The Impacts of Removing College Entrance Exams: 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 2006 and 2014. Compared to schools that did not switch, test-optional schools witnessed around a 15 percent increase in the number of Black, Native American/Alaskan Native, and Hispanic enrollments and around a 7 percent 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. Institutions interested in adopting these policies should consider these possible unintended consequences.

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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, 2021). 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. 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 (Hiss and Neupane, 2004; Geiser and Santelices, 2007; Hiss and Franks, 2014) as two potential reasons test-optional policies could promote greater representation of low-income and minority students.

Beyond the question of changes in enrollment, there are worries that schools adopt test-optional policies to artificially increase an institution's position (Epstein, 2009; Belasco et al., 2015) and fears that removing a signal of academic ability will impact the academic performance of students admitted (Robinson and Monks, 2005; Saboe and Terrizzi, 2019). Opponents maintain that the best predictor of first-year college performance is the combination of high school grade point average and college entrance exam scores, leading to concerns that test-optional policies will result in the denial of applicants who are more likely to be successful (Mattern and Allen, 2016). However, if, in the absence of college entrance exam scores, test-optional schools require better performance on other high school measures for admission, the direction academic performance changes is uncertain. Schools must also consider that if test-optional policies change the composition of students enrolled, there may be a need to adjust the financial aid students receive. Unless schools receive additional funds, as more lower-income students enroll, the amount of aid they receive will decline.

In this paper, I address these questions by leveraging the differential timing of adoption of these policies to evaluate changes in admissions, enrollment, academic performance, and financial aid. I construct a detailed panel dataset on selective,[1] 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 2006 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 first examine the impact of test-optional policies on applications and admissions. Switching to test-optional admissions did not have an effect on the number of applications, admissions rate, or matriculation rate for adopting schools. I find no evidence that adopting schools are trying to increase their selectivity by deflating their admissions rate. However, test-optional schools did see changes in application behavior by witnessing a 16.55 p.p. decline in the percent of freshmen enrolled that submitted a college entrance exam. Importantly, this result suggests that these policies are far from completely eliminating test scores from the admissions process. A vast majority of students choose to submit scores even when it is not required to do so.

In terms of enrollment, I find that on average test-optional schools saw an increase of around 15 percent in their enrollment of first-time, full-time Black, Native American/Alaskan Native, and Hispanic (BNH) students and an increase of around 7 percent 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 19 percent and 23 percent, respectively. There is evidence that adopting schools expanded overall enrollment. However, the growth in total enrollment cannot fully explain the aforementioned results as the share of FTFT students identified as BNH increased by 1.39 percentage points (p.p.), and the share of undergraduates with a Pell Grant increased by 1.28 percentage points.

I further explore the effects of test-optional policies by evaluating changes in student academic performance. Schools that adopted a test-optional policy saw no statistically significant changes in the high school academic performance of their enrolled students. Furthermore, any changes in high school academic performance did not translate into meaningful changes in college academic performance. Retention rates fell at adopting schools by a statistically insignificant 0.19 p.p., and 6-year graduation rates increased by a statistically insignificant 0.08 p.p. When compared to the baseline mean (75.39 and 54.82, respectively), these results are small. Furthermore, I can rule out

[1] Specifically, institutions that are not open enrollment.

any effect size larger than a 1.09 p.p. decline in retention rates and any size larger than 1.14 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 not only plays a large role in the decision-making process for students (Bettinger et al., 2012; Dynarski et al., 2021), but acts as an accounting measure for institutions to ensure they can function. Schools that implemented a test-optional policy increased the number of FTFT students receiving any institutional grant aid by 7 percent, yet the average grant aid allotted to each student declined by 843 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 around 10 percent in the number of FTFT students taking an institutional loan, with the average amount of the loan increasing by around (a statistically insignificant) 181 dollars. Because the test-optional movement is recent and currently ongoing, it is too soon to know how these changes in 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 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, academic or student services, or changes in application fees. I also show that the results are robust to other difference-in-differences methods, including the standard two-way fixed-effect model, a model with state-year fixed-effects, and a mahabolonis-distance matching model. Finally, I show that my results are robust to alternate choices for the control group.

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 (Hiss and Neupane, 2004) 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

[2]Interviews I conducted with different directors of admissions at adopting schools further bolster this claim. 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.

an important reason why schools become test-optional.[3]

Three other studies 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 2006 and 2015. The author finds that enrollment of first-time, full-time BNH students increased by 12 percent, and the number of undergraduates receiving a Pell Grant increased by 3 percent. The author does not measure changes in enrollment by high school academic performance, retention/graduation, 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 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]

Saboe and Terrizzi (2019) find similar results on enrollment as Belasco et al. (2015) even as they examine a larger group of test-optional schools between 2009-2014. The authors also examine changes in retention and graduation rates and find no statistically significant impacts of the policy. My results differ from Saboe and Terrizzi (2019) in that I construct the retention and graduation rates to reflect the relevant cohorts rather than examine contemporaneous changes.[5] This change in variables better reflects the potential impacts on academic performance because we want to understand whether a cohort enrolled under a test-optional policy performs differently than those in the same cohort enrolled at a test-requiring institution.

My results complement these papers in several ways, beginning with my empirical strategy. I use 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. There is an open question of whether test-optional policies increase enrollment of typically underrepresented groups of students at the expense of other groups (including White and Asian students) or because schools expand when adopting test-optional policies. Third, I

[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 on the same set of schools they examine.

[5]For example, the 6-year graduation rate reported in 2013 is relevant for the entering 2007 cohort.

examine other outcome variables beyond enrollment including changes in academic performance and financial aid. These are important outcome variables to examine because test-optional policies are often adopted to address inequities in postsecondary outcomes. However, if students are enrolling in these institutions, but dropping out at a higher rate and/or accruing additional debt as a result of the policy, test-optional programs may contribute to the well-known differences in college completion rates by family income and racial group (Bailey and Dynarski, 2011; DeAngelo et al., 2011). It is critical to understand these potential unintended consequences in order to evaluate the effectiveness test-optional policies.

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, I 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, academic performance 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).[6] 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 (Hiss and Neupane, 2004; 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 21 in 2001 to just under 200 in 2018.[7] 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. When looking across the United States, test-optional schools are most commonly

[6] It is important to note that adoption of college entrance exams was not entirely race-neutral, particularly in the US South following a large increase in Black applicants in the mid-20th century (Baker, 2001).

[7] See Figure 1. The list of test-optional schools considered in this paper can be found in Appendix Table A1.

located in the Northeast, but there are number of adopting schools in the Midwest and South. Appendix Figure A1 shows the locations of test-optional schools that made the switched by 2018.

Schools often cite the desire to increase representation within their student body as the reason behind the change in policy and the message that there are greater barriers to access to college entrance 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 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 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-free 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.

[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 is not required.

The scope of the data has grown over time and as a result, there is often inconsistent time coverage 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.[11] 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 Department of Education is that the information is 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 1,073 colleges and universities that are not identified as a school for the arts or religious training program.[12] Schools that adopt a test-optional policy before 2006 or between 2015-2018 are not included in the sample.[13] Data on which schools switched to test-optional come from cross-referencing the list from the National Center of Fair & Open Testing (FairTest, 2020), which includes those that have additional requirements for non-submitters and the list from Bennett (2021). For each discrepancy, I verified adoption years from published articles. In cases where I could not verify an institution went test-optional according to FairTest (2020), I drop the school from the sample. A complete list of test-optional schools I use in my sample can be found in Appendix Table A1. I focus on schools adopting the test-optional policy between the years of 2006-2014 which gives me at least four years of data before and after the policy was implemented. In total, I identify 74 schools that adopt a test-optional policy between

[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.

[11] Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission.

[12] Importantly, the sample includes public schools. Many states have guaranteed admissions into a state university based on high school grade point average or class rank thresholds. However, almost all of the states with such programs still require the submission of a college entrance exam; therefore, for the purpose of this project unless a university is designated as test-optional by my definition it is still considered test-requiring even if it exists in one of these states. The results are similar when focusing only on private institutions and are reported in Appendix Table A9.

[13] 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 A4 for those results

2006-2014, of which, 69 meet the data requirements previously outlined.

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 (before any school adopted a test-optional policy). Test-optional schools are different from test-requiring schools on several 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 academic performance measures.[14] 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. Additionally, in each of my specifications I will control for the baseline level (2001) of each of the characteristics that statistically differ across the treated and control groups.[15]

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, $g = 2006$ represents the set of schools that adopted the policy in 2006, $g = 2007$ represents the set of schools that adopted the policy in 2007 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

[14] These patterns hold when examining additional years. See Appendix Table A2.

[15] I do not control for retention rate or % of freshman with HS class rank in the top-half because of data availability issues. Furthermore, I only include covariates at a point in time because my empirical strategy only uses these variables to create propensity score weighted groups. See Callaway and Sant'Anna (2021).

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 $g = 2006$ corresponds to the year 2008. 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.[16] 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 ad-

[16] 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.

justment 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.[17] Formally, this is represented by[18]

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

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 and admissions. I then extend the analysis in Section V.B to examine the effects on enrollment. Finally, in Section V.C, I present the effects of test-optional policies on the academic performance of students and their financial aid receipt.

V.A 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).[19]

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

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

[18] 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.

[19] Appendix Table A5 presents the results in table format.

that schools are trying to increase their selectivity by deflating their admission rate. On average, schools that switched to a test-optional policy saw a statistically insignificant increase of 2 percent in the number of applications, a 1.42 p.p. increase in the admissions rate, and a 0.06 p.p. decrease in the matriculation rate.

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 16.55 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.[20] This result is also important because it highlights how the move to "test-optional" is far from the move that some institutions have recently made (e.g. University of California system) to "test-blind"/"test-free" admissions. Under the "test-optional" policies, a vast majority of students are still submitting their test scores. It is not the case that that these policies completely remove the consideration of college entrance exam scores in the admissions process.

My results on applications and admissions contrast that of Belasco et al. (2015) and Saboe and Terrizzi (2019) and suggest that test-optional policies may only be effective at increasing applications in the short-run. Figure 2 shows a slight increase in log applications in the year after the policy was adopted, but the effects quickly reverse. Since there are no statistically significant increases in the admission or matriculation rates, in order for the results in the following section to hold, its either the case that when institutions adopt the policy they either are choosing different students to admit and/or different students decide to attend the school. Unfortunately data on admissions by demographic group (other than gender) is unavailable. Future work is necessary to disentangle these two possibilities.

V.B Enrollment

V.B.1 Total Enrollment, 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, I next present the results on enrollment in Table 2. Each cell presents the single, aggregated average treatment on the treated effect (ATT) for

[20]The control mean is below 100 percent because of how the variables are coded in the dataset. Rather than indicating what percent of students submitting either the SAT or ACT, the data measures the percent of students submitting the SAT/ACT separately.

the separate estimations. Column (1) presents the results for overall first-time, full-time enrollment (FTFT), Column (2) presents the results for first-time, full-time Black, Native American/Alaskan Native and Hispanic enrollment and Column (3) presents the results for number of undergraduates receiving a Pell Grant. I separate the table to show the results when the variables are measured levels (Panel A), logs (Panel B) and as a percent of enrollment (Panel C).

The results in Column (1) suggest that after switching to a test-optional policy, adopting schools saw a statistically significant increase of 24.74 first-time, full-time (or 4 percent) students when compared to the control group. The potential increase in the overall number of students attending test-optional schools highlights the importance of examining the impacts of enrollment by race as a share. If schools are simply expanding as a result of the policy, it may be unfair to conclude that schools that have gone test-optional accomplished their often stated goal of increasing representation within their student body.

Columns (2) and (3) examine whether adopting schools see changes in the enrollment of Black, Native American/Alaskan Native and Hispanic students and Pell Grant recipients. The results show consistent and statistically significant effects of the adoption of test-optional policy on these outcome variables of interest. The results of Column (2) suggest that adopting a test-optional policy is associated with increased enrollment of FTFT BNH students. Specifically, after the switch to a test-optional policy, adopting schools saw an increase of 13.50 (or around 15 percent) FTFT BNH students enrolled. Panel C confirms that the increase in overall fist-time, full-time enrollment 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 1.39 percentage points, suggesting that test-optional schools are changing the composition of students on their campus rather than just expanding.[21] Column (3) 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 48.14 (or around 7 percent) Pell Grant recipients, which translated to a statistically significant increase of 1.28 p.p. in the percent of students receiving a Pell Grant.

Together, these results suggest that over the entire post-period, adopting schools saw changes in the enrollment composition of their student body which is in line with previous work on test-optional policies (Bennett, 2021). However, it is important to contextualize these findings. The baseline levels of first-time, full-time Black, Native American/Alaskan Native and Hispanic students are relatively low at test-optional institutions. Furthermore, Figure 7 shows that switching to test-optional allows

[21] Similar patterns hold when I examine changes in full-time Black, Native American/Alaskan Native and Hispanic enrollment rather than focusing on solely first-time students. Results for these specifications can be found in Appendix Figure A3 and Appendix Table A3.

these schools to catch up to their peer and nearby institutions. Whether test-optional policies will have similar effects for schools that better represent the average 4-year college/university is still an open question.

Figure 3 examines the timing of these effects to give a sense of when enrollment patterns changed and if preexisting trends are driving the results.[22] For first-time, full-time enrollment (Panel A) there is an immediate jump following the adoption of the policy. However, the effect quickly fades as the coefficients are statistically insignificant following the first year. For first-time, full-time BNH enrollment, the impact of the policy is immediate and slightly increases as it remains in place. For Pell Grant enrollment (Panel C), the effect of the policy becomes statistically significant starting two years after the policy was adopted. 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.

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.[23] 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 around a 19 percent

[22] Appendix Figure A2 shows the results using levels and percentages for results on FTFT BNH enrollment and Pell Grant Recipients. Appendix Figure A2 shows the results on undergraduate enrollment.

[23] Appendix Figure A4 shows the event-study results for enrollment by race. Appendix Figure A3 plots the coefficients when the estimation is run on levels and percentages. The results of Appendix Figure A3 show a flat or increasing percentage for each group displayed which is inline with the (statistically insignificant) increase in full-time enrollment. However, these results do not rule out the possibility that test-optional schools see declines in the number of international students, the number of students with an unknown race or the number of students reporting two or more races.

increase in the number of FTFT Black women and around a 23 percent increase in the number of FTFT Hispanic women. Adopting schools also saw statistically significant increases in the enrollment of Black men, but most estimates for Hispanic and Native American/Alaskan Native men are statistically insignificant. 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 some evidence that switching to a test-optional policy increases the number of FTFT White women and men enrolled, and the point estimate in the bottom panel of the figure suggests that FTFT Asian Women enrollment increased by around 15 percent 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.[24] Therefore, I refrain from interpreting this result as a definite increase in the enrollment of first-time, full-time Asian women. Importantly, these results suggest that the increase in BNH enrollment is not at the expense of other groups, which has been a concern expressed for other policies adopted to increase the diversity of colleges campuses such as affirmative action (Riley, 2012; Kang and Chen, 2019).

V.C Results on Academic Performance and Financial Aid Receipt

V.C.1 High School and College Academic Performance

There are concerns that test-optional policies could reduce the academic performance of enrolled students, since these policies eliminate a signal admissions committees can use to differentiate students. Therefore, in this next section I examine the impacts of test-optional policies school-level changes in the academic preparation of students enrolled and subsequent retention and graduation rates. Table 3 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), re-

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

tention rates (column 3) and 6-year graduation rates (column 4). I separate the table to show the results when the variables are measured as rates (Panel A), levels (Panel B) and logs (Panel C).

Performance in high school classes is consistently ranked as one of the most important factors in college admissions decisions.[25] 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 increases the percent of enrolled freshmen with a high school GPA above 3.0 by a statistically insignificant 1.48 p.p., or by 2.03 percent. 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 with a slight increase a few years after the policy was adopted. 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.

There is also no evidence that adopting schools saw changes 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 insignificant 0.26 p.p., or by 0.32 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.[26] However, they do not see significant changes in the other attributes of student's portfolio.

While schools may not have seen changes in the high school performance of their enrolled freshmen, there may be still be a concern of students' performance during college. Adopting schools saw a statistically insignificant decrease of 0.19 p.p. (or 0.25 percent) in their retention rates and a increase of 0.08 p.p.(0.14 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 these results 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. It is important to note that based on the years of available data the results on graduation rates are driven

[25] 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 institutions place either "considerable" or "moderate" importance on high school GPA when considering admissions decisions (Clinedinst and Patel, 2018).

[26] 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).

by early adopters. Future work will explore if schools that went test-optional in the mid-2010's saw differential changes in their graduation rates.

My findings complement the results found in Saboe and Terrizzi (2019) that also examines changes in the retention/graduation of students after adopting a test-optional policy. The authors use a standard difference-in-differences design to quantify the impacts for schools that made the switch between 2009 and 2014. The authors find that after adopting the policy, schools saw no statistically significant changes in the contemporaneous retention or graduation rates compared to the control group. My findings are in-line with their results, but I show that cohorts enrolled under a test-optional policy do not retain or graduate at rates differently from than those in the same cohort enrolled at a test-requiring institution.

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 4 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 around 7 percent, yet the average amount of the institutional grants decreased by 844 dollars (2010 $). These results suggest that schools have had to respond to the change in financial need of their enrolled cohorts. Figure 5 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 5(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 5(b)) 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 around 10 percent 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 181 (2010 $) dollars in the post-period, but when looking at the event-study estimates in Figure 5(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 around 68 dollars.[27]

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 has meaningful impacts on the composition of students attending and the financial aid they receive. There remain, however, four 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, (3) the results may be sensitive to the use of a specific estimation strategy and (4) the estimates may be biased upwards if students are moving from control schools to adopting schools.

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.[28] In all the years before adoption the 95 percent confidence intervals contains zero for both log first-time, full-time 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 FTFT BNH enrollment, but in the three years leading up to adoption

[27] Students could be receiving additional funds from other sources, such as state aid or federal loans, but that data is not available until 2008.

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

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 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 5, 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 5 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 or student services expenditures after the switch to test-optional.[29] If adopting schools are simultaneously increasing their outreach to different communities or expanding the academic/student services available we may expect these changes in expenditures to attract a new group of students that could explain the results. However, the results in Columns (2), (3) and (4) of Table 5 suggest that adopting a test-optional policy is not associated with any increases in academic, public or student services expenditures. Across each of these specifications, the coefficient are in the opposite direction of what we would expect if adopting schools were using public outreach or academic/student services to attract new students. Furthermore, the sign and significance of the estimates are not consistent across how the variables are measured.

I also 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;

[29]Public service expenditures are defined as the sum of all operating expenses associated with activities established primarily to provide non-instructional services beneficial to individuals and groups external to the institution. Academic support expenditures include the sum of all operating expenses associated with activities and services that support the institution's primary missions of instruction, research, and public service. Student services expenditures are defined as the sum of all operating expenses associated with admissions, registrar activities, and activities whose primary purpose is to contribute to students' emotional and physical well-being and to their intellectual, cultural, and social development outside the context of the formal instructional program.

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 (5) of Table 5 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. One may expect that hiring new faculty may not occur simultaneously with changes in admissions policies. However, the event-study results, shown in Appendix Figure A6, do not show any evidence that adopting schools beginning hiring new BNH faculty after the policy was adopted.

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 2, 3, and 4 using three alternative estimation strategies. Specifically, I re-run the results using the standard two-way fixed-effects model, two-way fixed-effects model with state-year fixed effects, and a mahabolonis-distance matching model. I find consistent evidence that, irrespective of the estimation strategy, the signs and general significance levels of the estimated treatment effect are maintained as shown in Table 6.

Finally, to address the concern that test optional colleges may be drawing their new enrollments from other four-year colleges included in my sample, I re-estimate my main results with two alternative control groups. Appendix Table A8 reports the results of this exercise. Panel A reports the results when the control group does not include peer institutions (as defined by the National Center for Education Statistics based on institutional characteristics) of any test-optional school in my sample. The estimates in Panel B come from a sample that removes any control schools that are within 50 miles of a test-optional institution in my sample. Each of these specifications is meant to remove control schools where test-optional institutions are likely to gain enrollment from. Across both panels, we see the results remain relatively unchanged. This exercise does not completely rid the concern of an upward bias of my results as I do not have information on what schools students' consider when making the enrollment decision. A further discussion about where new enrollments for test-optional school come from is found in the following section.

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 in the academic performance 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 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 6, I examine three sources from which test-optional schools could be receiving new students. First, I consider whether test-optional schools shift students away from their peer institutions.[30] In Panel A of Figure 6, 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 school. Panel B of Figure 6 plots the average FTFT BNH enrollment for ever test-optional schools and the nearest 4-year institution. Again, test-optional schools do not seem to draw students from this source; if anything, they are catching up to surrounding colleges and universities. Finally, in Panel C of Figure 6, 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 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.

[30] Peer institutions are identified from the National Center for Education Statistics based on institutional characteristics.

(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.

Third, there is a question of 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.

Fourth, there is a question of the mechanisms driving the results on enrollment. Ideally, information on applications, admissions and matriculation by race would be available to allow researchers to understand how BNH enrollments increase. Without such information it is impossible to distinguish between the following explanations: (1) more BNH students applying that hadn't before because college entrance exams served as a barrier, (2) BNH students are admitted at a higher rate because lower exam scores reduced their application quality, or (3) test-optional polices change the matriculation rate of different groups of students. My results show slight (albeit often statistically insignificant) increases in applications, admissions and enrollment; therefore, it is likely that all three channels are at play for the schools that made the switch during my sample period. Previous case studies have shown that test-optional policies can increase the number of applications by groups of students that would not have otherwise applied (Hiss and Neupane, 2004; Robinson and Monks, 2005), but future work with more expansive data is needed to address this question further.

A final question of interest is how does going test-optional compare to other changes in admissions

[31] Mean plots are available upon request.

policies. Previous work has explored how changes in application fees (Pallais, 2015; Smith et al., 2015), essay requirements (Smith et al., 2015) and the move to a centralized application system (Knight and Schiff, 2022) impacts the number of applications schools receive and the types of students enrolled. Overall these studies show that reducing application frictions increases both the number of applications and the share of non-White students enrolled. Test-optional policies fit well into this literature because college entrance exams are another dimension to a student's application. My results show that the impact of test-optional policies on the racial composition of enrolled students is of similar magnitude compared to the complementary literature. However, I show that adopting schools see meaningful increases in the number of Pell grant recipients. The literature on changes in application fees, essay requirements and centralization finds either no impact on the share of students receiving a Pell grant (Smith et al., 2015) or a slight decrease in low-income students (Knight and Schiff, 2022). The contrast in these results suggests that its college entrance exams that serve as the barrier for these students to enroll rather than small changes in fees or effort that is takes to complete the application. Institutions with the goal of increasing representation on this dimension need to consider this point when assessing how to adjust their application process.

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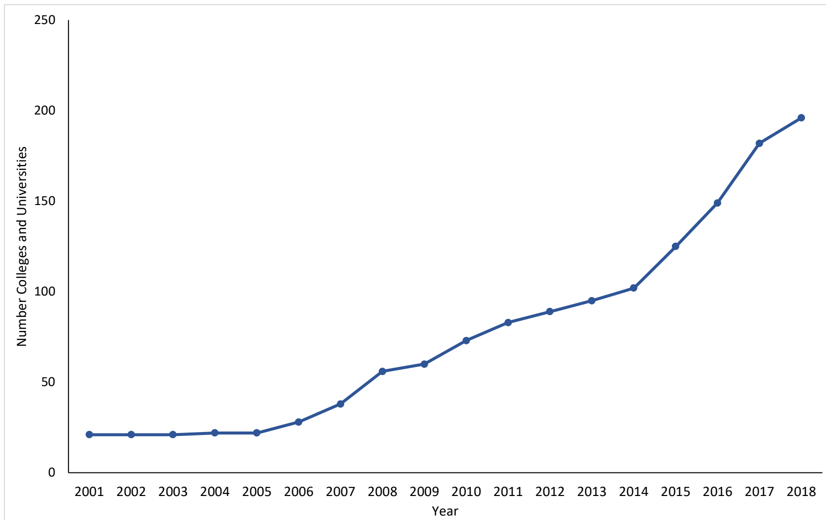
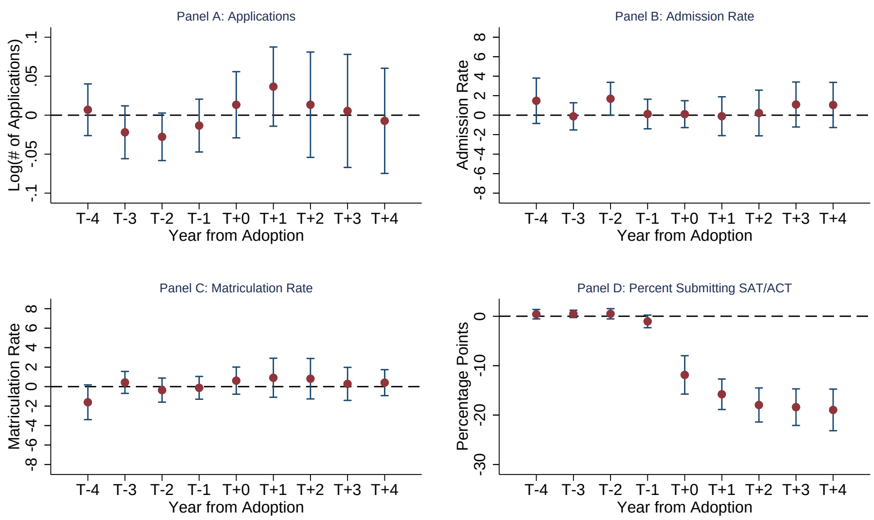
Figures

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



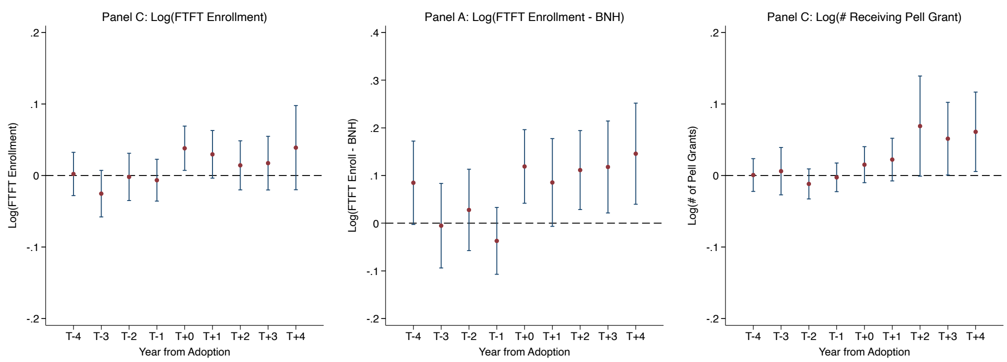
Notes: This figure presents the cumulative number of schools that have adopted a test-optional policy from 2001 to 2018. Data on the timing of adoption comes from FairTest (2020) and Bennett (2021).

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



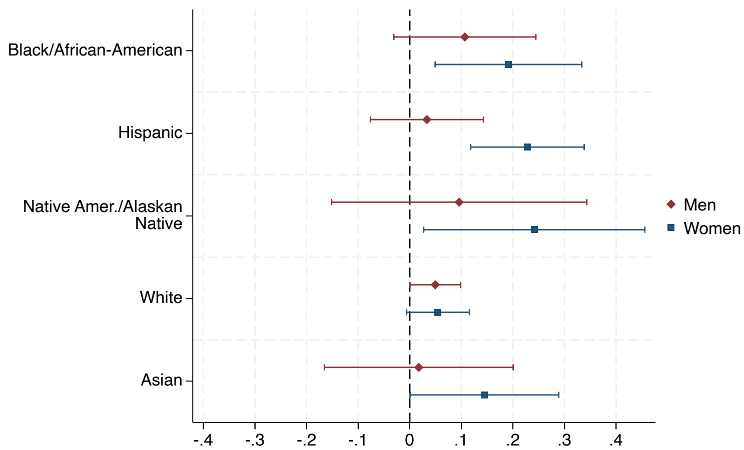
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 2(a) plots the estimates for the log number of applications, Figure 2(b) plots the estimates for the admissions rate, Figure 2(c) plots the estimates for the matriculation rate, and Figure 2(d) 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on applications, admissions and enrollment come from IPEDS. Data on SAT/ACT submissions comes from the College Board's *Annual Survey of Colleges*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. See Appendix Table A5 for the number of observations and baseline means.

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



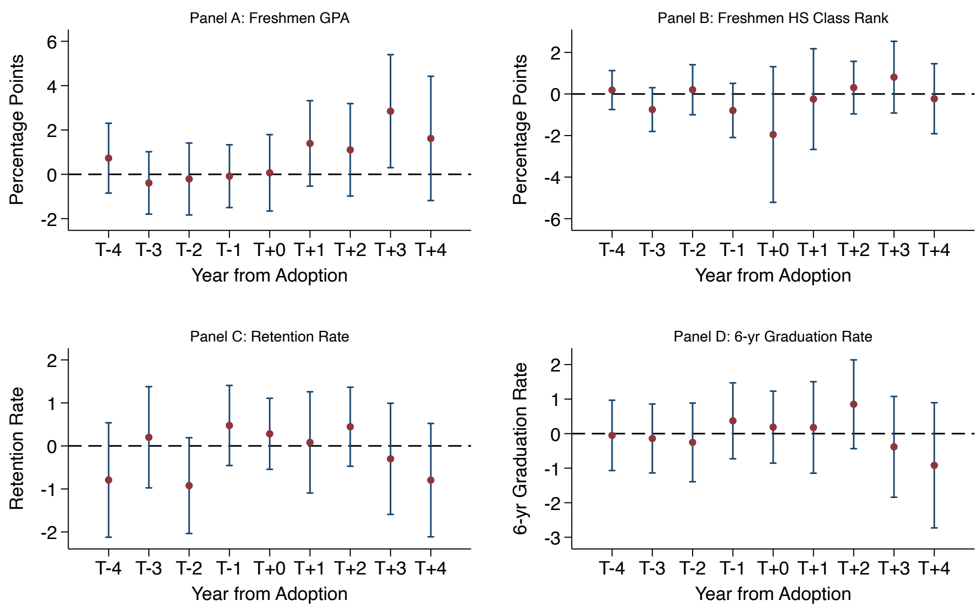
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 3(a) plots the estimates for the log number of first-time, full-time students enrolled, Figure 3(b) plots the estimates for the log number of first-time, full-time Black, Native American/Alaskan Native and Hispanic 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on enrollment comes from IPEDS. Data on Pell Grant Recipients comes from the Department of Education. See Table 2 for observations and control means.

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



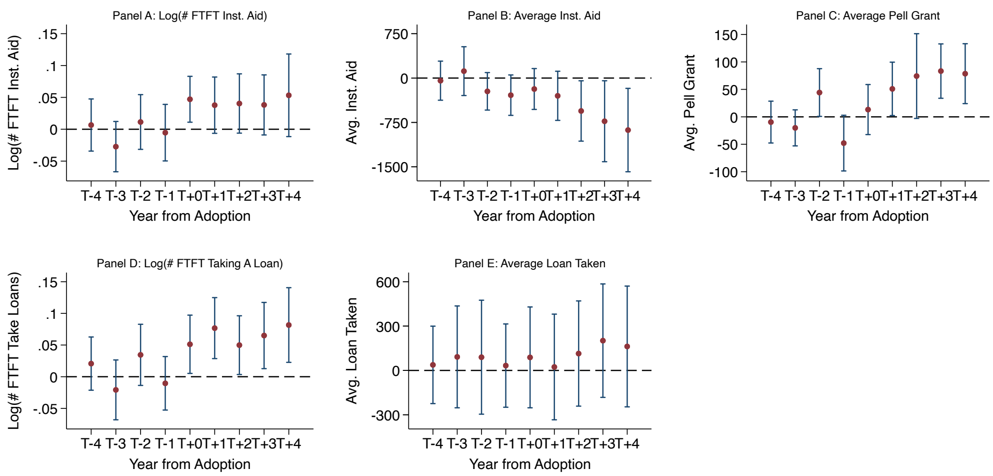
Notes: This figure presents single, aggregated average treatment on the treated effects for first-time, full-time enrollment of Black/African-American, Hispanic, Native American/Alaskan Native, White and Asian students, separately for men and women. Each point is the result of a separate estimation. 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on enrollment comes from IPEDS. See Appendix Table A6 for information on control means and observations.

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



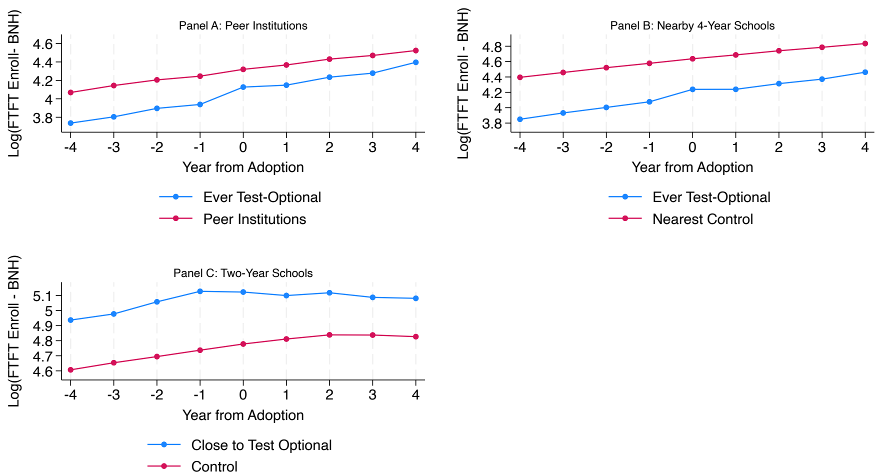
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure 5(a) plots the estimates for the percent of freshmen enrolled with a HS GPA $>$ 3.0, Figure 5(b) plots the estimates for the percent of freshmen enrolled with a HS Class rank in the top half, 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on high school performance comes from the College Board's *Annual Survey of Colleges*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. Data on retention and graduation come from IPEDS. See Table 3 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 5(a) plots the estimates for the log number of first-time, full-time (FTFT) students receiving institutional aid, Figure 5(b) plots the estimates for the log number of FTFT taking an institutional loan, Figure 5(c) plots the estimates for the average amount of institutional aid FTFT students receive (in 2010 \$), 5(s) plots the estimates for the average amount of the institutional loans FTFT students take (in 2010 \$), 5(e) 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on institutional aid and loans come from IPEDS. Data on Pell grants come from the Department of Education. See Table 4 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 first-time, full-time Black, Native American/Alaskan Native and Hispanic enrollment across different event times in the sample. Figure 6(a) plots enrollment for ever-test optional schools (blue) and their peer institutions as indicated by the National Center for Educational Statistics (red), Figure 6(b) plots enrollment for ever test-optional schools (blue) and the nearest 4-year college or university (red), Figure 6(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	6.4	19	21	9.8***
# of Applications	3,647	2,835	4,403	5,700	756
Full-Time Enrollment	2,537	2,166	4,838	5,769	2,301***
Published In-State Tuition	19,904	4,885	11,163	6,939	-8,741***
% Students with a Pell Grant	17	6.2	28	13	10***
Admission Rate	66	12	69	19	3
% Freshmen w/ HS Class Rank in Top Half	87	12	80	14	-7.1***
Retention Rate	83	8.1	75	11	-7.1***
6-Year Graduation Rate	70	13	55	17	-15***
N	69		1,004		1,073

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*. Source: Annual Survey of Colleges 2020. Data on Pell Grant Receipt comes from the U.S. Department of Education. FTFT BNH - First-time, full-time Black, Native American/Alaskan Native and Hispanic Enrollment.

Table 2: Results on Enrollment

	Total First-Time, Full-Time (1)	FTFT BNH (2)	Pell Grant Recipients (3)
Panel A: Levels			
ATT	24.74**	13.50***	48.14
	(12.50)	(4.53)	(35.90)
Number of Obs.	19,312	19,312	18,790
Control Mean	1,020	184	1,673
Panel B: Logs			
ATT	0.04**	0.15***	0.07***
	(0.020)	(0.043)	(0.025)
Number of Obs.	19,312	19,153	18,790
Control Mean	6.39	4.31	6.84
Panel C: Percent			
ATT	-	1.39***	1.28***
	-	(0.52)	(0.46)
Number of Obs.	-	19,312	18,789
Control Mean	-	18.96	27.79

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment covariates include baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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 High School and College Academic Performance

	% of Freshmen w/ HS GPA $\geq$ 3.0 (1)	% of Freshmen w/ HS Class Rank in Top Half (2)	Retention (3)	Six Year Graduation (4)
Panel A: Rates				
ATT	1.48	-0.26	-0.19	0.08
	(0.90)	(0.80)	(0.46)	(0.54)
Number of Obs.	12,997	13,549	17,877	13,944
Control Mean	72.77	80.01	75.39	54.82
Panel B: Levels				
ATT	-	-	16.04	12.08
	-	-	(12.042)	(9.179)
Number of Obs.	-	-	13,949	13,944
Control Mean	-	-	859	612
Panel C: Logs				
ATT	-	-	0.029	0.027
	-	-	(0.024)	(0.022)
Number of Obs.	-	-	13,944	13,944
Control Mean	-	-	6.13	5.72

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard errors are calculated using a bootstrap technique and are clustered at the school level. Pre-treatment covariates include baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Each coefficient is the result of a separate estimation. Information on high school performance comes from the College Board's *Annual Survey of Colleges*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. 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 Department of Education cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table 4: Results on Financial Aid Receipt

	Levels (1)	Logs (2)	Percent (3)	Avg. Amount (4)
Panel A: FTFT Receiving Institutional Aid				
ATT	33.33**	0.07**	2.21*	-843.97**
	(13.73)	(0.03)	(1.17)	(342.31)
Number of Obs.	19,311	19,285	19,311	19,288
Control Mean	468	5.69	62.06	5,392.30
Panel B: FTFT Taking a Loan				
ATT	3939.43***	0.10***	3.12**	181.47
	(13.35)	(0.03)	(1.38)	(194.34)
Number of Obs.	19,311	19,187	19,311	19,194
Control Mean	496	5.79	58.06	3,779.75
Panel C: Receiving a Pell Grant				
ATT	48.14	0.07***	1.28***	67.95***
	(47.70)	(0.03)	(0.47)	(24.60)
Number of Obs.	19,168	19,168	19,167	19,168
Control Mean	1,672	6.84	27.79	2,200.86

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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. FTFT represents First-time, full-time.

Table 5: Other Possible Policy Innovations

	(1) Application Fees ($)	(2) Exp. Acad. Serv (in Millions)	(3) Exp. Pub. Serv. (in Millions)	(4) Exp. Student Serv. (in Millions)	(5) # of BNH New Hires
Panel A: Levels					
ATT	-0.64	-2.42	-0.08	-2.33***	-0.62**
	(1.72)	(3.99)	(0.32)	(0.85)	(0.30)
Number of Obs.	18,780	18,077	18,077	18,077	15,504
Control Mean	29.58	13.43	9.29	6.87	3.70
Panel B: Logs					
ATT	-0.01	-0.03	0.19*	0.01	-0.07
	(0.02)	(0.04)	(0.10)	(0.03)	(0.14)
Number of Obs.	18,780	18,077	18,077	18,077	15,504
Control Mean	3.33	1.55	0.54	1.75	1.11
Panel C: Per-Pupil					
ATT	-	-226.17	15.928	-303.68*	-
	-	(884.97)	(56.98)	(161.85)	-
Number of Obs.	-	18,077	18,077	18,077	-
Control Mean	-	2,441.07	706.38	2,658.64	-

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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. BNH represents Black, Native American/Alaskan Native and Hispanic.

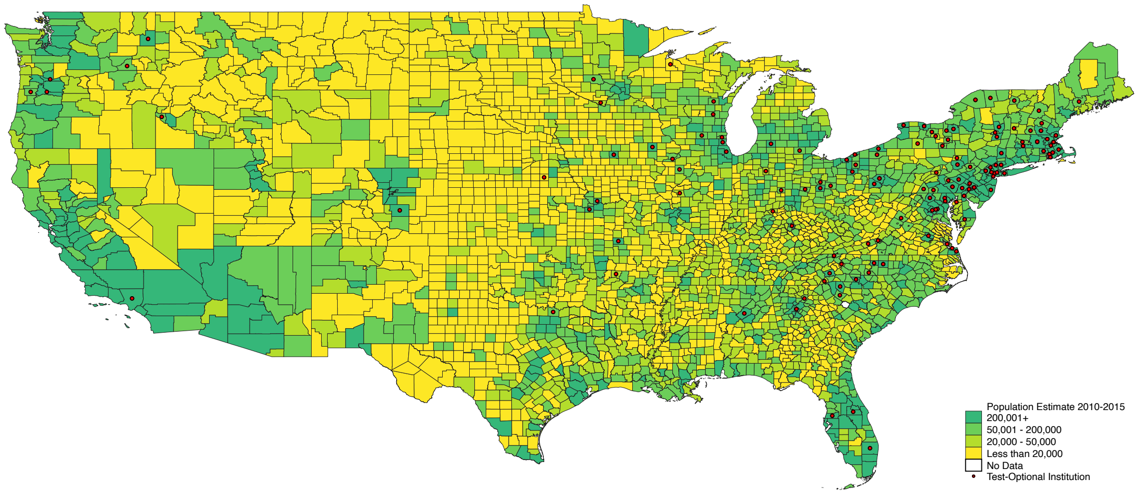
Table 6: Results Using Alternative Estimation Strategies

	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	0.14***	0.063***	-1.31**	0.05**
	(0.04)	(0.02)	(0.66)	(0.02)
Observations	18,055	18,180	12,862	18,164
Control Mean	4.61	6.95	79.60	5.91
Panel A: TWFE with State-Year Fixed Effects				
ATT	0.13***	0.03	-1.00	0.01
	(0.04)	(0.02)	(0.65)	(0.03)
Observations	18,037	18,162	12,832	18,146
Control Mean	4.61	6.95	79.60	5.91
Panel C: Mahabolonis Distance Matching-DiD				
ATT	0.12	0.03	-1.35*	0.04
(0.07)	(0.04)	(0.81)	(0.04)	
Observations	6,089	6,074	4,550	6,202
Control Mean	3.87	6.10	83.01	5.65

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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 when using a difference-in-differences design with state-year fixed effects. Panel C presents the estimates using a Mahabolonis Distance Matching method combined with the standard difference-in-differences design. 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 Department of Education cover up to the 2018-2019 academic year. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. Some institutions are missing data in particular years as described in the data section. FTFT represents first-time, full-time. BNH represents Black, Native American/Alaskan Native and Hispanic.

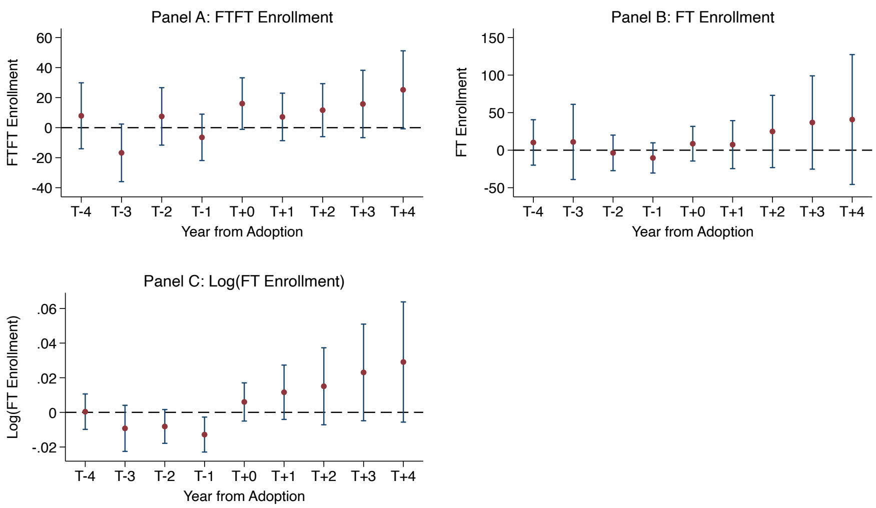
Appendix Figures

Figure A1: Test-Optional Colleges and Universities Across the United States



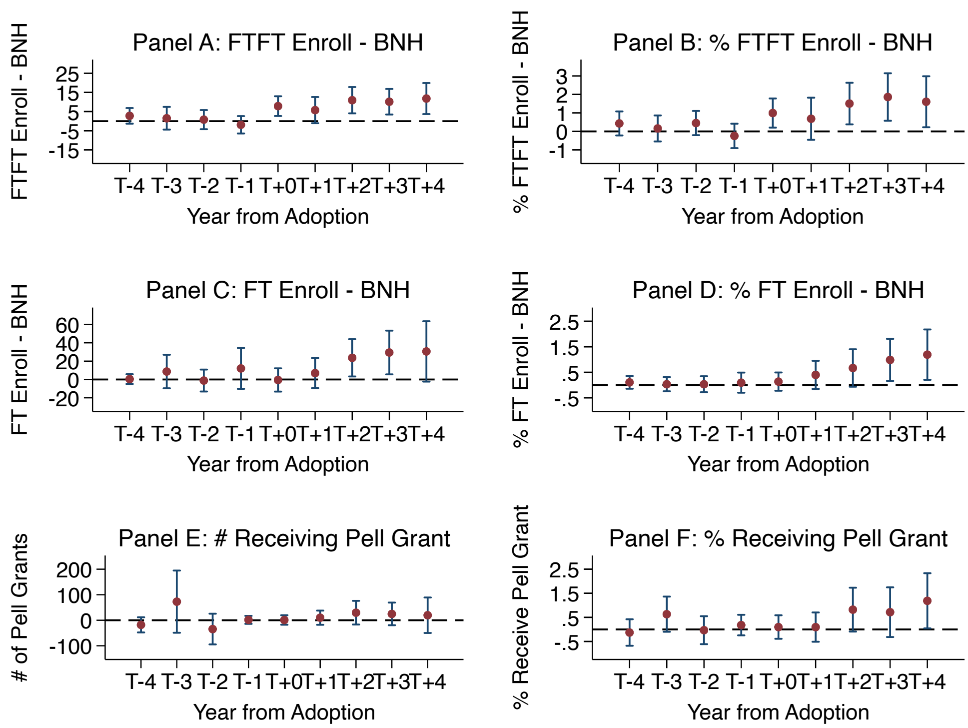
Notes: This figure presents the locations of every test-optional institution that adopted the policy by 2018 against the county population estimates from 2010-2015. Data on the location of test-optional institutions comes from IPEDS and population count data come from the U.S Census Bureau (2016).

Figure A2: Changes in Total Enrollment



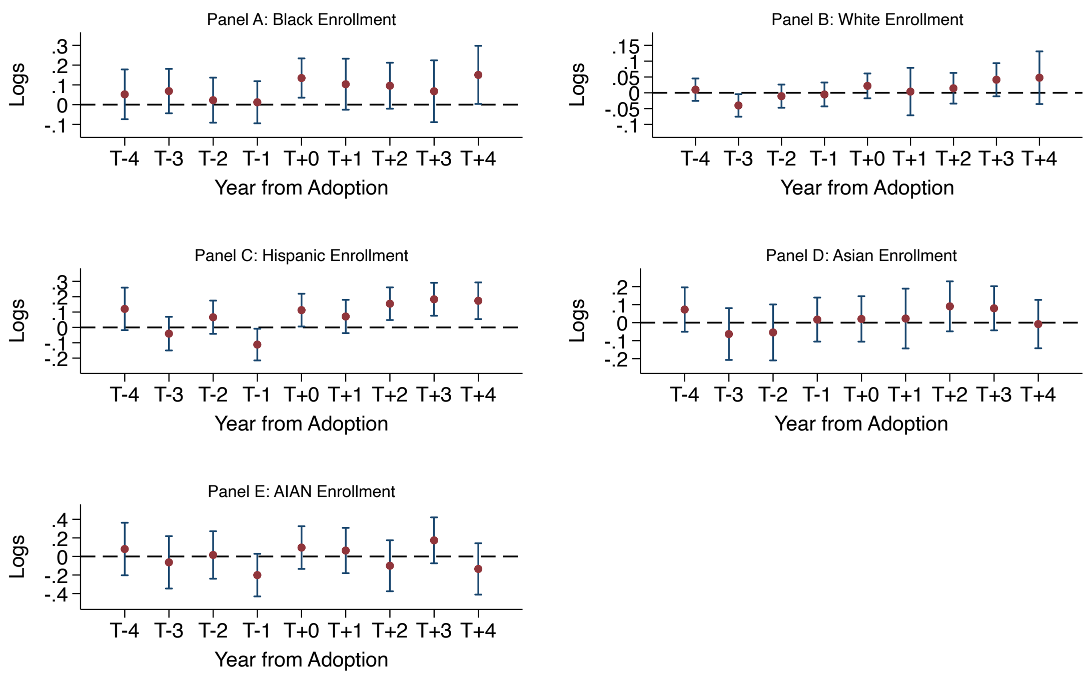
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A2(a) plots the estimates for the number of first-time, full-time students enrolled, Figure A2(b) plots the estimates for the number of full-time students enrolled, and Figure A2(c) plots the estimates for the log number of full-time students enrolled. 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate.

Figure A3: Effects of Test-Optional Policies on Enrollment - Levels and Percents



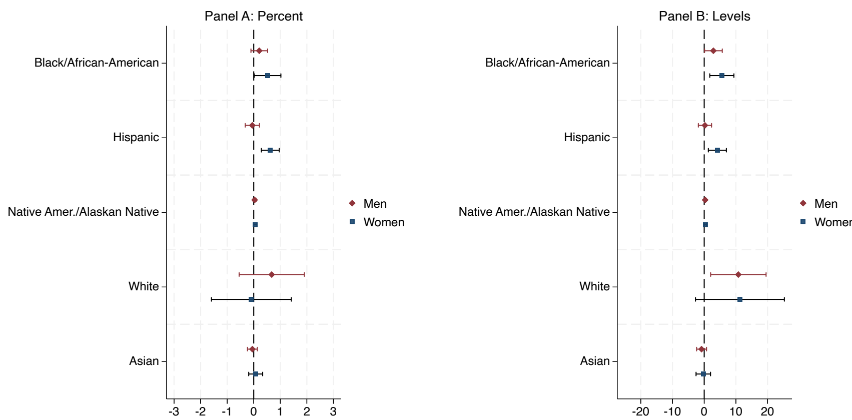
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A3(a) plots the estimates for the number of first-time, full-time Black, Native American/Alaskan Native and Hispanic (FTFT BNH) students enrolled, Figure A3(b) plots the estimates for the percent of full-time (FT) BNH students enrolled, Figure A3(c) plots the estimates for the number of FT BNH students enrolled, Figure A3(d) plots the estimates for the percent of FTFT BNH students enrolled, Figure A3(e) plots the estimates for the number of students receiving a Pell Grant and Figure A3(f) 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on enrollment comes from IPEDS. Data on Pell Grant Recipients comes from the Department of Education.

Figure A4: Results on Enrollment by Race



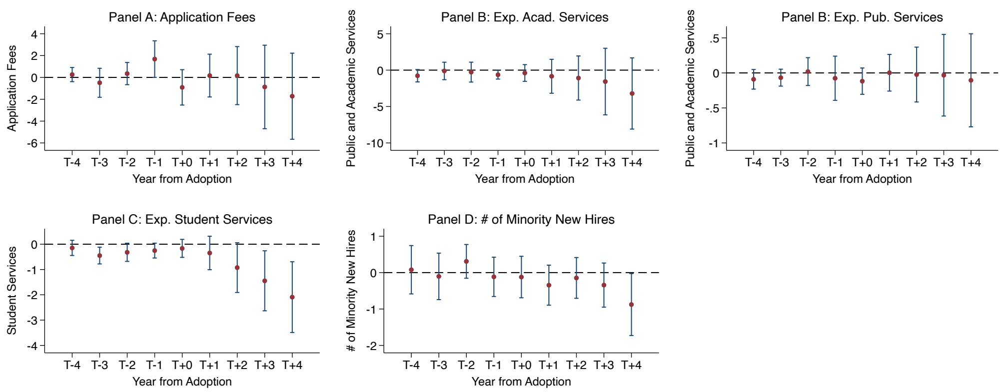
Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A4(a) plots the estimates for the log number of Black students enrolled, Figure A4(b) plots the estimates for the log number of White students enrolled, Figure A4(c) plots the estimates for the log number of Hispanic students enrolled, Figure A4(d) plots the estimates for the log number of Asian students enrolled, and Figure A4(e) plots the estimates for the log number of Native American/Alaskan Native students enrolled. 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate.

Figure A5: Results on Enrollment by Race and Gender (Levels and Percent)



Notes: This figure presents single, aggregated average treatment on the treated effects for first-time, full-time enrollment of Black/African-American, Hispanic, Native American/Alaskan Native, White and Asian students, separately for men and women. Each point is the result of a separate estimation. Panel A reports the results as measured in percents and Panel reports the results as measured in levels. 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on enrollment comes from IPEDS. See Appendix Table A6 for information on control means and observations.

Figure A6: Results on Other Policy Innovations



Notes: This figure presents the event-study estimates from the Callaway and Sant'Anna (2021) procedure. Figure A6(a) plots the estimates for application fees, Figure A6(b) plots the estimates for expenditures on public and academic services as measured in 2010 dollar, Figure A6(c) plots the estimates for expenditures on student services as measured in 2010 dollars, and Figure A6(d) plots the estimates for the number of Black, Native American/Alaskan Native and Hispanic faulty new hires. 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Data on application fees, expenditures and new hires comes from IPEDS.

Appendix Tables

Table A1: Test-Optional Schools 2006-2014

Adoption Year	Institution Name	Institution Type	Adoption Year	Institution Name	Institution Type
2006	Chatham University	Private not-for-profit (no religious affiliation)	2010	American University	Private not-for-profit (religious affiliation)
2006	College of the Holy Cross	Private not-for-profit (no religious affiliation)	2010	Assumption College	Private not-for-profit (religious affiliation)
2006	Knox College	Private not-for-profit (religious affiliation)	2010	Connecticut College	Private not-for-profit (no religious affiliation)
2006	Lawrence University	Private not-for-profit (no religious affiliation)	2010	Fairfield University	Private not-for-profit (religious affiliation)
2006	Salisbury University	Private not-for-profit (no religious affiliation)	2010	Illinois College	Private not-for-profit (religious affiliation)
2006	St Lawrence University	Private not-for-profit (religious affiliation)	2010	Loyola University Maryland	Private not-for-profit (religious affiliation)
2007	Bennington College	Private not-for-profit (no religious affiliation)	2010	Marlboro College	Private not-for-profit (no religious affiliation)
2007	George Mason University	Public	2010	Sacred Heart University	Private not-for-profit (religious affiliation)
2007	Gustavus Adolphus College	Private not-for-profit (religious affiliation)	2010	SUNY College at Potsdam	Public
2007	Hobart William Smith Colleges	Private not-for-profit (religious affiliation)	2010	Washington & Jefferson College	Private not-for-profit (no religious affiliation)
2007	King's College	Private not-for-profit (no religious affiliation)	2011	Manhattanville College	Private not-for-profit (no religious affiliation)
2007	Lake Forest College	Private not-for-profit (religious affiliation)	2011	Marist College	Private not-for-profit (no religious affiliation)
2007	Lebanon Valley College	Private not-for-profit (religious affiliation)	2011	Saint Anselm College	Private not-for-profit (religious affiliation)
2007	Mitchell College	Private not-for-profit (religious affiliation)	2011	Saint Michael's College	Private not-for-profit (religious affiliation)
2007	Nazareth College	Private not-for-profit (no religious affiliation)	2011	Salve Regina University	Private not-for-profit (religious affiliation)
2007	Providence College	Private not-for-profit (no religious affiliation)	2011	University of Rochester	Private not-for-profit (no religious affiliation)
2008	Augustana College	Private not-for-profit (religious affiliation)	2011	Virginia Wesleyan University	Private not-for-profit (religious affiliation)
2008	Denison University	Private not-for-profit (religious affiliation)	2011	Wagner College	Private not-for-profit (religious affiliation)
2008	Drew University	Private not-for-profit (no religious affiliation)	2012	Anna Maria College	Private not-for-profit (religious affiliation)
2008	Georgian Court University	Private not-for-profit (religious affiliation)	2012	Bryant University	Private not-for-profit (no religious affiliation)
2008	Gettysburg College	Private not-for-profit (religious affiliation)	2012	DePaul University	Private not-for-profit (religious affiliation)
2008	Goucher College	Private not-for-profit (religious affiliation)	2012	Earlham College	Private not-for-profit (religious affiliation)
2008	Green Mountain College	Private not-for-profit (no religious affiliation)	2012	Nichols College	Private not-for-profit (no religious affiliation)
2008	Keuka College	Private not-for-profit (religious affiliation)	2012	Saint Leo University	Private not-for-profit (religious affiliation)
2008	Merrimack College	Private not-for-profit (religious affiliation)	2013	Catawba College	Private not-for-profit (religious affiliation)
2008	Rollins College	Private not-for-profit (religious affiliation)	2013	Clark University	Private not-for-profit (no religious affiliation)
2008	Stonehill College	Private not-for-profit (no religious affiliation)	2013	Ithaca College	Private not-for-profit (no religious affiliation)
2008	Washington College	Private not-for-profit (religious affiliation)	2013	Lees-McRae College	Private not-for-profit (religious affiliation)
2008	Whitworth University	Private not-for-profit (no religious affiliation)	2013	Roger Williams University	Private not-for-profit (no religious affiliation)
2008	Wilson College	Private not-for-profit (religious affiliation)	2013	William Jewell College	Private not-for-profit (religious affiliation)
2008	Wittenberg University	Private not-for-profit (religious affiliation)	2014	Brandeis University	Private not-for-profit (no religious affiliation)
2008	Worcester Polytechnic Institute	Private not-for-profit (religious affiliation)	2014	Hood College	Private not-for-profit (no religious affiliation)
2009	Albright College	Private not-for-profit (no religious affiliation)	2014	Lynn University	Private not-for-profit (no religious affiliation)
2009	Baldwin Wallace University	Private not-for-profit (religious affiliation)	2014	Ohio Wesleyan University	Private not-for-profit (religious affiliation)
2009	Smith College	Private not-for-profit (religious affiliation)	2014	Presbyterian College	Private not-for-profit (religious affiliation)
2009	Wake Forest University	Private not-for-profit (no religious affiliation)	2014	Regis College	Private not-for-profit (religious affiliation)
2010	Agnes Scott College	Private not-for-profit (no religious affiliation)	2014	Saint Joseph's University	Private not-for-profit (religious affiliation)

This table presents the list of test-optional schools I identify in my sample. The list of schools and their adoption year is compiled from FairTest (2020) and Bennett (2021).

Table A2: Summary Statistics in Various Years

	Test Optional		Test Requiring		**Control - Treated**
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.
Panel A: Summary Statistics 2005					
% FTFT Enroll - BNH	9.5	6.3	19	21	9.9***
# of Applications	3,815	2,934	4,624	5,942	810*
Full-Time Enrollment	2,580	2,225	4,923	5,894	2,343***
Published In-State Tuition	21,887	5,338	12,235	7,591	-9,652***
% Students with a Pell Grant	16	6.5	26	13	10***
Admission Rate	64	14	69	19	5.1**
% Freshmen w/ HS Class Rank in Top Half	87	12	80	14	-6.8***
Retention Rate	82	8.4	75	11	-6.6***
6-Year Graduation Rate	69	13	55	17	-14***
Panel B: Summary Statistics 2007					
% FTFT Enroll - BNH	11	6.9	20	21	9.7***
# of Applications	4,484	3,393	5,250	6,829	766
Full-Time Enrollment	2,647	2,292	5,057	6,061	2,409***
Published In-State Tuition	2,6472	6,317	14,795	9,180	-11,677***
% Students with a Pell Grant	17	6.6	27	13	9.7***
Admission Rate	61	15	66	18	4.8*
% Freshmen w/ HS Class Rank in Top Half	87	13	80	15	-7.5***
Retention Rate	83	7.7	75	11	-7.4***
6-Year Graduation Rate	69	13	55	17	-14***
Panel C: Summary Statistics 2009					
% FTFT Enroll - BNH	13	8.1	22	21	8.8***
# of Applications	4,772	3,420	6,030	7,898	1,259*
Full-Time Enrollment	2,759	2,533	5,296	6,368	2,537***
Published In-State Tuition	30,090	7,090	16,902	10,307	-13,187***
% Students with a Pell Grant	21	7.9	29	12	7.9***
Admission Rate	62	13	65	19	2.7
% Freshmen w/ HS Class Rank in Top Half	88	9.2	80	15	-8.2***
Retention Rate	82	10	76	11	-6***
6-Year Graduation Rate	70	13	56	17	-13***
N	69		1,004		1,073

This table presents summary statistics for schools that will adopt a test-optional policy to those that maintain a test requirement in the years 2005, 2007 and 2009. 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*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. Data on Pell Grant Receipt comes from the U.S. Department of Education

Table A3: Results on Full-Time Enrollment

	Total Full-Time Enrollment	Full-Time BNH Enrollment
	(1)	(2)
Panel A: Levels		
ATT	71.54	40.82*
	(55.89)	(24.74)
Number of Obs.	19,312	19,312
Control Mean	4,838	850
Panel B: Logs		
ATT	0.03*	0.12***
	(0.016)	(0.033)
Number of Obs.	19,312	19,184
Control Mean	7.91	5.80
Panel C: Percent		
ATT	-	1.01***
	-	(0.38)
Number of Obs.	-	19,312
Control Mean	-	18.27

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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 A4: 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	-13.99***	0.11***	0.07***
	(1.86)	(0.04)	(0.02)
Number of Obs.	2,351	2,772	2,742
Baseline Mean	90.27	3.70	6.13
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	0.20	0.32	0.51
	(0.84)	(0.43)	(0.63)
Number of Obs.	2,157	2,572	2,002
Baseline Mean	84.39	79.79	65.13
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	0.04*	264.22	0.07***
	(0.03)	(333.60)	(0.03)
Number of Obs.	2,772	2,772	2,772
Baseline Mean	5.82	8,426.60	5.72

*** 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 covariates include baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. 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 Department of Education cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table A5: Results on Applications and Admissions

	Log(# of Applications)	Admissions Rate	Matriculation Rate	% Freshmen Submit Test
	(1)	(2)	(3)	(4)
ATT	0.02	1.42	-0.06	-16.55***
	(0.03)	(1.03)	(0.76)	(1.87)
Number of Obs.	19,250	19,250	19,250	15,822
Control Mean	7.75	68.73	42.34	87.01

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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*. Source: Annual Survey of Colleges 2020. Copyright © 2020 The College Board. This material may not be copied, published, rewritten or redistributed without permission. 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 Department of Education cover up to the 2018-2019 academic year. Some institutions are missing data in particular years as described in the data section.

Table A6: 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	8.47***	5.57***	2.90**	0.13**	0.19***	0.11	0.73**	0.52**	0.21
	(2.93)	(1.94)	(1.43)	(0.06)	(0.07)	(0.07)	(0.36)	(0.26)	(0.16)
Number of Obs.	19,312	19,312	19,312	19,035	18,495	18,205	19,312	19,312	19,312
Control Mean	105	63	43	3.62	3.02	2.89	12.11	7.11	5.00
Panel B: FTFT Hispanic Enrollment									
ATT	4.38**	4.14***	0.24	0.18***	0.23***	0.03	0.57**	0.62***	-0.05
	(2.03)	(1.5)	(1.07)	(0.05)	(0.06)	(0.06)	(0.25)	(0.17)	(0.14)
Number of Obs.	19,312	19,312	19,312	18,808	18,250	17,717	19,312	19,312	19,312
Control Mean	70	41	29	3.02	2.58	2.40	6.06	3.60	2.46
Panel C: FTFT AIAN Enrollment									
ATT 0.65***	0.35*	0.30**	0.09	0.24**	0.10	0.09*	0.06	0.03	
	(0.22)	(0.18)	(0.15)	(0.10)	(0.11)	(0.13)	(0.05)	(0.04)	(0.03)
Number of Obs.	19,312	19,312	19,312	14,969	12,577	11,405	19,312	19,312	19,312
Control Mean	8	4	3	1.44	1.14	1.06	1.06	0.78	0.44
Panel D: FTFT White Enrollment									
ATT	22.05**	11.27	10.78**	0.06**	0.06*	0.05**	0.59	-0.09	0.68
	(10.62)	(7.15)	(4.46)	(0.03)	(0.03)	(0.03)	(1.02)	(0.77)	(0.63)
Number of Obs.	19,312	19,312	19,312	19,142	18,897	18,590	19,312	19,312	19,312
Control Mean	704	381	322	5.87	5.30	5.08	69.70	38.72	30.97
Panel E: FTFT Asian Enrollment									
ATT	-1.12	-0.29	-0.83	0.08	0.15**	0.02	0.02	0.07	-0.05
	(1.71)	(1.17)	(0.80)	(0.06)	(0.07)	(0.09)	(0.17)	(0.13)	(0.10)
Number of Obs.	19,312	19,312	19,312	17,969	16,875	16,180	19,312	19,312	19,312
Control Mean	69	37	32	2.83	2.35	2.90	4.65	2.57	2.07

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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 A7: Results Varying the Method of Including Controls

	DRIMPW	DRIMP	STIDPW	IPW
	(1)	(2)	(3)	(4)
Panel A: Log # of FTFT BNH Enrolled				
ATT	0.147***	0.146***	0.147***	0.148***
	(0.042)	(0.045)	(0.046)	(0.043)
Number of Obs.	19,153	19,153	19,153	19,153
Panel B: Log # of Pell Grants				
ATT	0.072***	0.072***	0.072***	0.072***
	(0.027)	(0.025)	(0.027)	(0.026)
Number of Obs.	18,790	18,790	18,790	18,790
Panel C: % Freshmen with HS Class Rank in Top Half				
ATT	-0.257	-0.255	-0.246	-0.245
	(0.827)	(0.832)	(0.808)	(0.823)
Number of Obs.	13,549	13,549	13,549	13,549
Panel D: Retention Rate				
ATT	-0.191	-0.190	-0.184	-0.184
	(0.490)	(0.452)	(0.495)	(0.506)
Number of Obs.	17,877	17,877	17,877	17,877
Panel E: 6-Year Graduation Rate				
ATT	0.075	0.072	0.080	0.080
	(0.551)	(0.507)	(0.555)	(0.524)
Number of Obs.	13,944	13,944	13,944	13,944
Panel F: Log(# FTFT Receiving Inst. Aid)				
ATT	0.065**	0.065**	0.066**	0.066**
	(0.028)	(0.027)	(0.029)	(0.029)
Number of Obs.	19,285	19,285	19,285	19,285
Panel G: Average Inst. Aid Received				
ATT	-843.97**	-834.00**	-863.36***	-863.68**
	(371.785)	(349.087)	(328.236)	(351.001)
Number of Obs.	19,288	19,288	19,288	19,288
Panel H: Log(# Students Taking A Loan)				
ATT	0.101***	0.102***	0.102***	0.102***
	(0.027)	(0.029)	(0.030)	(0.026)
Number of Obs.	19,187	19,187	19,187	19,187

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. 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 A8: Results with Alternate Control Groups

	Log(FTFT Enroll - BNH)	Log(# Pell Recipients)	6-yr Graduation Rate	Log(# Inst. Aid)
	(1)	(2)	(3)	(4)
Panel A: Removing Peer Institutions				
ATT	0.19***	0.09***	-0.40	0.09**
	(0.05)	(0.03)	(0.74)	(0.04)
Number of Obs.	10,535	10,404	7,719	10,667
Panel B: Removing Nearby Institutions				
ATT	0.19***	0.08***	-0.23	0.08***
	(0.05)	(0.02)	(0.54)	(0.03)
Number of Obs.	13,244	13,006	9,578	13,256

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Each coefficient is the result of a separate estimation. Data for this table come from IPEDS. Some institutions are missing data in particular years as described in the data section.

Table A9: Results on Private Schools

	Log(FTFT Enroll - BNH)	Log(# Pell Recipients)	6-yr Graduation Rate	Log(# Inst. Aid)
	(1)	(2)	(3)	(4)
ATT	0.14***	0.07**	-0.02	0.05*
	(0.05)	(0.03)	(0.61)	(0.03)
Number of Obs.	11,864	11,785	8,679	12,013

*** 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 baseline levels of all statistically significant differences between the adopting and control schools shown in Table 1 except % enrolled freshman with HS class rank in the top half and retention rate. Each coefficient is the result of a separate estimation. Data for this table come from IPEDS. Some institutions are missing data in particular years as described in the data section. The sample only includes private schools.