NBER WORKING PAPER SERIES

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Working Paper 27779
http://www.nber.org/papers/w27779

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2020

We gratefully acknowledge funding from the Lab for Economic Applications and Policy and the Connaught Fund. The Texas Education Agency and a large urban school district provided invaluable administrative data for this project. We are grateful to Josh Angrist, Peter Blair, Roland Fryer, Brent Hickman, Caroline Hoxby, Asim Khwaja, Louis-Philippe Morin, Phil Oreopoulos, Sarah Reber, Alex Whalley, Wesley Yin, and seminar and conference participants at the NBER Summer Institute, IZA, Harvard, Brown, UCL, UBC, Purdue, Clemson, Collegio Carlo Alberto, the Ohlstadt workshop, CEA, University of Calgary, and UCLA for their helpful comments. We also thank Graham Beattie for his help with the newslibrary.com database. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 27779
September 2020
JEL No. I21,I24,J15,J24,J48

ABSTRACT

Racial affirmative action policies are widespread in college admissions. Yet, evidence on their effects before college is limited. Using four data sets, we study a U.S. Supreme Court ruling that reinstated affirmative action in three states. Using nationwide SAT data for difference-in-differences and synthetic control analyses, we separately identify the aggregate effects of affirmative action for whites and for underrepresented minorities. Using state-wide Texas administrative data, we measure the effect of affirmative action on racial gaps across the pre-treatment test score distribution. When affirmative action is re-instated, racial gaps in SAT scores, grades, attendance, and college applications fall. Average SAT scores for both whites and minorities increase, suggesting that reductions in racial gaps are driven by improvements in minorities' outcomes. Increases in pre-college human capital and college applications are concentrated in the top half of the test score distribution.

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1 Introduction

Economic opportunity is dramatically unequal across racial groups in the United States (Chetty et al., 2020; Derenoncourt, 2019; Darity Jr et al., 2018; Bayer and Charles, 2018). Access to higher education may be an important equalizing force (Chetty et al., forthcoming), but many minority groups are still underrepresented in American colleges, particularly at selective institutions. African Americans account for 15% of the college-aged population but only make up 6% of freshmen, and the underrepresentation of African Americans and Hispanics has *grown* over the last 35 years (Ashkenas et al., 2017).

Affirmative action (AA) policies that weigh race or ethnicity as one factor in the college admission process can address past inequities and create opportunities for these students. Indeed, motivating the creation of these policies in the 1960s, at a 1965 speech at Howard University, Lyndon B. Johnson explained, "You do not take a person who, for years, has been hobbled by chains and liberate him, bring him up to the starting line in a race, and then say, 'you are free to compete with all others,' and still justly believe that you have been completely fair."[1] However, these policies are also highly controversial and have repeatedly been challenged by court cases at the sub-national and national level.[2] Eight states have banned racial affirmative action at all public universities. Altogether, despite both the potential of affirmative action policies to address inequities and the controversy surrounding them, relatively little is known about whether these policies affect students *prior* to reaching college. This gap in the literature is important: closing racial achievement gaps in secondary school could be an effective tool for reducing inequities in long-term outcomes.

Affirmative action policies may affect pre-college human capital investment by incentivizing students, their parents, and their teachers to change their behavior in response to changes in the returns to pre-college human capital. Theoretically, affirmative action policies favoring students from underrepresented minority (URM) groups in college admissions have ambiguous average effects on human capital attainment prior to college entry. One the one hand, affirmative action policies may incentivize higher pre-college human capital attainment for URM students – particularly those who are on the margin of being accepted by a selective university – by increasing the probability that increased human capital will translate into

[1] Stulberg and Chen (2014) discuss the origins of AA initiatives in the United States. AA policies in higher education and in hiring practices are widespread in numerous countries, including Canada, Brazil, and India.

[2] Such cases include: *Regents of the University of California v. Bakke* in 1979, *Hopwood v. Texas* in 1996, *Grutter v. Bollinger* and *Gratz v. Bollinger* in 2003, *Fisher v. University of Texas* in 2013, *Schuette v. Coalition to Defend Affirmative Action* in 2014, *Fisher v. University of Texas* in 2016, and *Students for Fair Admissions v. Harvard* in 2019.

college admission (Fryer and Loury, 2005). Even if affirmative action does not directly affect students' perceptions of the likelihood of being admitted, by increasing the observed number of URM students admitted, it may increase aspirations or the perception that selective schools are welcoming to URMs, ultimately increasing pre-college human capital investment. On the other hand, for high-achieving URM students, affirmative action policies may reduce the returns to pre-college human capital investment by lowering the threshold for college admissions (Coate and Loury, 1993). Then, affirmative action could disincentivize human capital investments by these students (or their teachers and parents). Since the theoretical effects of affirmative action are ambiguous and may also depend on how far students are from the threshold for admission, we estimate the effects of affirmative action on both the average student and on students in different parts of the test score distribution.

To investigate the effects of racial affirmative action on the human capital investments of high school students, we exploit a natural experiment that induced a policy reversal in Texas, Louisiana, and Mississippi. In 2003, the Supreme Court decision in *Grutter v. Bollinger* ruled that race-conscious admissions processes that do not amount to quota systems are constitutional. This effectively reversed a 1996 lower court ruling in *Hopwood v. Texas* that prohibited the use of race in admissions in public universities in these three states. We exploit this exogenous policy change to estimate the effects of affirmative action on secondary school students' outcomes using two identification strategies in three administrative data sets and one survey data set. In cases where we have administrative data for all of Texas (secondary school attendance and college applications, admissions, and graduation) or part of the state (grades), we use a difference-in-differences strategy that compares the change in URM (Black and Hispanic) and white students' outcomes following the policy. This strategy can be interpreted as estimating the effect of affirmative action on the racial achievement gap. We further inspect trends in outcomes by race to ensure that positive effects on URMs are not driven by negative effects on whites. In cases where we have data across multiple states (SAT data), we separately compare the change in URMs' and whites' outcomes in states that were and were not affected by the policy using difference-in-differences and synthetic control group approaches. This second strategy allows us to identify potential spillover effects on whites and determine whether estimated effects on racial achievement gaps constitute lower or upper bounds on the aggregate effects of affirmative action on URMs' pre-college human capital. Finally, in the SAT data, we use an additional triple-differences strategy that interacts cohort, geographic, and racial variation. This strategy estimates the differential change in the racial achievement gap due to the policy and controls for any changes in treated states over time that affected both whites and URMs.

There are four main findings. First, across data sets and identification strategies, we find that URMs respond positively to affirmative action policies, including by increasing their pre-college human capital investment. URMs increase their number of applications to selective schools, as well as their SAT scores, grades, and attendance. Our estimates suggest that the re-instatement of affirmative action helped close the racial gap in applications to selective schools by 13%, the gap in math SAT scores by 5%, the gap in secondary school grades by 18%, and the gap in secondary school attendance by 8%. These positive effects are concentrated among high school students in the upper-half of the 6th grade test score distribution, who are more likely to be on the margin of admission to selective Texas public universities.

Second, we find no evidence that pre-college human capital investment fell for URMs anywhere in the test score distribution. The reintroduction of affirmative action policies in Texas had no detectable disincentivizing effect on effort by URMs. Third, we find that white students also experience small increases in their SAT scores in response to the policy, consistent with either positive spillovers from URMs or whites also responding to increased returns to effort due to greater competition for admissions. Thus, the reductions in the racial achievement gap we observe may be smaller than the aggregate effect of the policy on URMs' pre-college human capital investment. Fourth, we explore the effect of the affirmative action policy on the racial gap in college graduation with the caveat that the net effect conflates effects on pre-college human capital investment, college match, and college quality.[3] Following the policy, the gap in college graduation falls by 15% for students in the top quintile of the 6th grade test score distribution and remains unchanged for students in the bottom four quintiles.

Turning to mechanisms, several pieces of evidence suggest that students increase their effort in response to affirmative action policies. First, attendance – an outcome likely to be indicative of effort – increases. Second, in survey data, we find no evidence that parents or guidance counselors respond to the policy, but we do find that URMs spend 10% more time on homework. Third, the students whose outcomes improve the most are the same ones who experience increases in the returns to pre-college human capital.

Broadly, our results contribute to a large literature on the effects of affirmative action policies. This literature has focused primarily on affirmative action policies in higher education and their impact on college applications behavior (Card and Krueger, 2005), admissions and campus diversity (Bowen and Bok, 1998; Arcidiacono, 2005; Rothstein and Yoon, 2008;

[3]Mountjoy and Hickman (2020) find little evidence of differential match effects by race in Texas.

Hinrichs, 2012, 2016), major choice (Arcidiacono et al., 2016), and college graduation (Bowen and Bok, 1998; Hinrichs, 2012). Arcidiacono et al. (2015) reviews this literature.

This paper is most closely related to a smaller literature about the effects of affirmative action on students *prior* to college. In the United States, the evidence on educational outcomes from this literature is mixed.[4] Antonovics and Backes (2014) conclude that SAT scores and high school GPA changed little after California banned affirmative action by public universities. Caldwell (2010) finds that URM students' time spent studying and test scores worsened following affirmative action bans in California and Texas in the mid-1990s. Cotton et al. (2015) simulate affirmative action for younger students in a math competition between younger and older students and find that their intervention increases younger students' time spent studying and improves their math performance. Bodoh-Creed and Hickman (2018) structurally estimate the U.S. college admissions market. Their structural estimates indicate that moving to a race-blind counterfactual increases pre-college human capital for URMs with a low cost of effort and decreases it for those with a higher cost of effort.

We contribute to this literature in two ways. First, we exploit a policy experiment to directly estimate the effects of the reinstatement of a real affirmative action policy on students' outcomes in the U.S. Thus, we complement Cotton et al. (2015) and Bodoh-Creed and Hickman (2018) by providing less model-dependent, reduced-form estimates of affirmative action's effects as it is implemented in practice. Doing so does not require making assumptions about students' information or how they respond to incentives. Second, we exploit large and detailed administrative data sets, allowing us to go beyond average effects and trace out affirmative action's effects on a variety of dimensions across the distribution of pre-treatment test scores.

This study also relates to broader literatures on the incentive effects of college admissions (Cortes and Zhang, 2011; Leeds et al., 2017; Golightly, 2019) and the anticipatory effects of changes in the returns to human capital investment on children and their parents' investment decisions (Jayachandran and Lleras-Muney, 2009; Jensen, 2010, 2012; Oster and Steinberg, 2013; Leeds et al., 2017; Moeeni and Tanaka, 2020). While much of the evidence in the

[4]The evidence from abroad is also mixed. Outside of the U.S., Ferman and Assunção (2015) and Estevan et al. (2018) study the effects of race-based and SES-based university admissions quotas in Brazil on high school students, while Khanna (2016) and Cassan (2019) study the effects of affirmative action on pre-college education in India. Tincani et al. (2020) study the effect of a program that guaranteed admissions to students in the top 15% of their high school class in Chile. Ferman and Assunção (2015) find that affirmative action reduced student effort; Estevan et al. (2018) finds little effect on test preparation; and Khanna (2016) and Cassan (2019) finding positive effects on education. Tincani et al. (2020) find that the race-blind preferential admissions program they studied *decreased* human capital investment because students were overconfident of their ranking.

latter literature is from low-income countries, our results suggest that students also respond to changes in the returns to human capital investment in the United States.[5]

The remainder of this paper is organized as follows. Section 2 introduces the context in more detail. Section 3 presents our four data sets. Section 4 uses a simple conceptual framework to generate testable predictions. Section 5 reports our estimates of the average and distributional effects of affirmative action on student outcomes using both the nation-wide SAT data and Texas administrative data sets. Section 6 uses survey data to test which mechanisms drive the estimated effects, and Section 7 discusses whether alternative educational policies, such as No Child Left Behind, can explain our results. Section 8 concludes.

2 Context & Policy Change

In this section, we sketch out a brief time line of events during the study period (1997-2010) and describe the policy change that this paper studies. We then examine whether universities' stated commitment to affirmative action translated into changes in admissions.

Timeline of Events. In response to profound racial discrimination, affirmative action policies were first introduced in the United States in the 1960s to encourage firms competing for federal contracts to hire minorities (and subsequently women). Over time, the use of affirmative action policies as a tool to address racial discrimination expanded to universities, which sought to diversify their student bodies. However, these policies have been met with considerable opposition and have been repeatedly challenged in the courts (see Andrews and Swinton (2014) for a more detailed discussion).

In 1996, the U.S. Court of Appeals for the Fifth Circuit, which has jurisdiction over Texas, Louisiana, and Mississippi, ruled in *Texas v. Hopwood* that universities may not use race as a factor in deciding which applicants to admit. In the wake of this ruling, the Texas legislature passed the "Top 10% Rule" in 1997, which guaranteed admissions to *any* state-funded university in Texas to students graduating in the top 10% of their class. This law was passed as a means to promote diversity in universities by ensuring college access to high-achieving students from across Texas' somewhat segregated high schools. In June 2003, the Supreme Court ruled in *Grutter v. Bollinger* that a race-conscious admissions process that does not amount to a quota system is constitutional, effectively overturning *Texas*

[5]Several papers document educational investment responses to natural resources booms in the U.S. (Kovalenko, 2020; Cascio and Narayan, 2015) and Canada (Morissette et al., 2015).

v. Hopwood.[6] Thus, public universities in Texas, Louisiana, and Mississippi were unable to legally use race in the admissions process prior to 2003 and were able to do so again after 2003. We use this policy reversal to assess the effect of the introduction of race-based affirmative action on high school students' performance.[7]

The Top 10% Rule remained in place from 1997 onward. The only change occurred at the end of the study period in 2009, when the Texas legislature passed a law allowing UT Austin to cap the percent of its class admitted through the "Top 10% Rule" at 75%. Following the new law's implementation in 2011, only the top 7% of students were admitted to UT Austin.

Grutter v. Bollinger. The *Grutter v. Bollinger* ruling was a close 5-to-4 ruling, with the deciding vote cast by moderate justice Sandra Day O'Connor. Prior to the ruling, the outcome of the case was viewed as impossible to predict, with *USA Today* writing in 2002, "Both sides think it's their best chance of winning the AA battle...O'Connor is the 5th vote but her moderate history does not indicate her direction." Indeed, the Supreme Court majority opinion expressed ambivalence over affirmative action policies, striking down the ban on considering race holistically but banning assigning points for admissions based on race.[8] The decision was heavily covered by the media. Appendix Figure A1, which plots the number of articles in US newspapers mentioning affirmative action by day, shows the spike in coverage around the ruling. The policy was also heatedly discussed in Texas. On June 29, 2003 (5 days after the ruling), *every* letter to the editor published in the *Austin-American Statesman* was about the case.

Policy Response to *Grutter v. Bollinger*. On the day that the *Grutter v. Bollinger* decision was issued, UT Austin's president, Larry Faulkner, stated that the Texas flagship campus intended to return to considering race in the admissions process. This response was well-publicized, with Faulkner shown making comments to this effect on the NBC nightly

[6] As the ruling in *Grutter v. Bollinger* only established the constitutionality of affirmative action, states like California, Washington, and Florida, which had banned affirmative action due to ballot measures or executive orders, were unaffected.

[7] We don't focus on the earlier policy change in 1996 for two reasons. First, it combines a ban on race-based affirmative action and the introduction of the Top 10% Rule a year later. Therefore, the 1996 policy change does not provide a clean experiment for estimating the effects of an affirmative action ban on students' outcomes. Second, the scarcity of data from the pre-1996 period make credibly estimating the effect of the ban difficult.

[8] The majority ruling read, "The court takes the Law School at its word that it would like nothing better than to find a race-neutral admissions formula and will terminate its use of racial preferences as soon as practicable. The court expects that 25 years from now, the use of racial preferences will no longer be necessary to further the interest approved today."

news. Only the University of Texas Board of Regents could authorize the implementation of such a change, and in August 2003, the Board of Regents voted to allow all its campuses to return to considering race.[9] The Texas Tech University Board of Regents also outlined a plan to include race as an element in admissions in October 2003. Thus, following the 2003 Supreme Court ruling, it was clear that the state flagship university, UT Austin, and other public universities in Texas would return to using affirmative action.

Racial affirmative action co-exists with the Top 10% rule. Texas public universities first admit students who qualify for automatic admission through the 10% rule. Students who are not eligible for automatic admission are admitted based on a "holistic" review process. Following the policy change, race or ethnicity could again play a role in this process. Despite the Top 10% Rule, holistic admissions remain important. UT Austin, which has the highest percentage of freshmen admitted under the Top 10% Rule, admitted one-third of its freshman class through this process in 2003 (Office of the President, 2008).

Did Affirmative Action Policies Affect Admissions? To evaluate whether universities' stated commitment to affirmative action had real effects, we now consider how it affected both university composition and admissions. Appendix Figure A2 uses the IPEDS data to calculate the racial composition of UT Austin's Fall entering class by year. Following Fall 2003, there is a trend-break in the share of Blacks and Hispanics, with both rising precipitously. These descriptive statistics are consistent with the findings of Hinrichs (2012), who shows that affirmative action bans decrease the enrollment of URMs at selective universities. In contrast, the upward trend in the share of Asians, who are not considered an underrepresented minority, flattened from 2003 onward.

Similarly, the reversal of the ban appears to have affected UT Austin and other selective Texas universities' admissions behavior. Using administrative data from the Texas Education Agency, Appendix Figure A3 plots event study graphs of URM students' relative likelihood of being admitted to UT Austin, University of Houston, Texas Tech, and Texas A & M relative to whites by the year in which students attended 9th grade. Here, we focus on students in the top quintile of the test score distribution since these are the students most likely at the margin of admission to these institutions.[10] The estimation procedure for these event study graphs is identical to the one used to produce graphs for our outcome variables from the

[9] University of Texas campuses consist of Austin, Arlington, Dallas, El Paso, Rio Grande Valley, San Antonio, Tyler, and Permian Basin.

[10] Throughout our study period, 75% of students admitted to UT Austin were in the top quintile of their cohort's distribution of 6^{th} grade test scores.

Texas Education Agency data later in this paper and is described in detail in Section 5.2. Students who ended 9th grade in 2001 were the first group whose admissions were affected by the reinstatement of affirmative action, although these students would have had little time to change their pre-college human capital. URMs' likelihood of admission following 2003 grew at UT Austin, the University of Houston, and Texas Tech. For Texas A & M, which publicly stated that they would not use race-based affirmative action at the time of the ruling (Parker, 2018), there is no clear positive trend in URM admissions. Altogether, lifting the affirmative action ban appears to affect URM students' admissions probabilities at selective Texas universities.

3 Data

In this section, we describe our four data sets: (1) the panel of race-state-year SAT scores, (2) the administrative data for all Texas students from the Texas Education Agency (TEA), (3) the administrative data from a large urban school district (LUSD), and (4) the survey data from the Texas Higher Education Opportunity Project (THEOP).

SAT Data. To analyze the effects of the reinstatement of affirmative action on SAT scores, we collected data on mean math and verbal SAT scores and the number of test-takers at the state-race-year level from 1998 to 2010 from the College Board's publicly available reports. One important benefit of these data is the inclusion of states that were not affected by the policy change. This allows us to separately estimate the effect of *Grutter v. Bollinger* on URMs (Black and Hispanic students) and whites and to estimate the differential change in URMs' outcomes relative to whites in the treated states. The top panel of Appendix Table A1 reports summary statistics for the SAT data. There is a substantial racial achievement gap during the pre-treatment period, with whites scoring 91 points higher in math and 87 points higher in verbal on average, equivalent to about 0.8 standard deviations in both subjects.

Texas Education Agency (TEA) Administrative Data. Our first set of student-level administrative data come from individual records for all Texas elementary, middle, and high school students from the Texas Education Agency. The files are linked to (in-state) college administrative data.[11] Altogether, the records include yearly school attendance, test scores

[11]In 2004, only 8% of Texan residents enrolled in an institution of higher education were enrolled in an institution outside of Texas (Center for Education Statistics, 2004).

on standardized tests, and demographic characteristics, as well as college applications, admissions, and graduation for Texas universities. Since the data cover the entire state, they allow us to estimate the population average treatment effects of affirmative action and estimate affirmative action's effects in different parts of the test score distribution with precision.[12]

Using the TEA data, we estimate the effects of affirmative action on attendance, college applications, admissions, and graduation. While the TEA data also includes standardized test scores in high school, we do not use these as an outcome measure because these tests underwent a substantial version change at roughly the same time as affirmative action was re-instated.[13] As a result, we cannot disentangle the effects of affirmative action from the effects of the version change on URMs' high school test scores.

Since use of the individual-level TEA data is restricted outside of a secure data room in Texas, we constructed a data set of aggregate observations for outside analysis from a sample of roughly 3 million students. To examine the heterogeneous effects of affirmative action by prior academic achievement, we collapsed these data at the school district-cohort-race-6th grade test score decile level.[14] By weighting cells by their size, we can then replicate the regression results we would attain with the individual-level data. In our main analyses, we classify students into quintiles according to their rank in their year-specific 6th grade statewide standardized test score distribution. To ensure 6th grade test scores are unaffected by the policy change, our main analysis focuses on cohorts in 6th grade before the policy (those in 9th grade from 1997 to 2006). In event study graphs, we do, however, include later cohorts to show trends over a longer horizon.[15]

The middle panel of Appendix Table A1 reports summary statistics by race and pre-/post-treatment cohort in the TEA data. The fraction of Texas students identified as URMs increases sharply over time, entirely driven by an increase in the Hispanic population.

Large Urban School District (LUSD) Administrative Data. Our second source of student-level administrative data is drawn from a large, urban school district in Texas. These

[12]In contrast, data sets like the SAT are restricted to students who take the exam. Data sets like the Integrated Post-Secondary Education Survey only have information on students who actually enroll in college.

[13]In 2003, the standardized exam changed from the TAAS to the TAKS. These tests differ meaningfully. First, TAAS was administered to grades 3-8 and grade 10. In contrast, TAKS is administered to grades 3-11, with the higher-stakes exit exam taking place in grade 11 instead of 10. Second, the TAKS high school version includes social studies while TAAS does not (Tutson, 2002).

[14]For confidentiality reasons, all cells with less than 5 students are dropped (7% of all students). The fraction of students with valid 6th grade test scores varies slightly across cohorts and is generally in the 70-75% range.

[15]A cohort is assigned the year it was in the Spring semester.

data consist of repeated cross-sections of all 11th graders in the school district between 2001 and 2008.[16] The data contain information on students' demographics, course grades, and test scores on the norm-referenced Stanford Achievement Test (hereafter, Stanford), a low-stakes achievement test that the school district has administered since 2000. Data on prior academic records for the three preceding years (e.g. course grades in 2003, 2004, and 2005 for students enrolled in 11th grade in 2006) also allow us to observe students' grades in 8th grade, as long as they were enrolled in the same district. We focus on grades as our main outcome in these data since the Stanford test underwent a minor version change from the Stanford 9 to the Stanford 10 in 2004, our first post-treatment year. While this change was less dramatic than the version change between the TAAKs and TAAS exams, we still view evidence from the Stanford test – which we report in Section 5.4 – as suggestive. The bottom panel of Appendix Table A1 reports summary statistics for the sample of 11th graders from this school district. The majority of students in the district are Black or Hispanic.

Texas Higher Education Opportunity Project Data. Our final data set, the Texas Higher Education Opportunity Project (THEOP) data, allows us shed light on what mechanisms may drive affirmative action's effects. THEOP surveyed high school seniors from a random sample of 105 public high schools in Texas in 2002 and 2004 regarding their demographics, college perceptions, parental involvement, and other activities in high school. The timing of the survey allows us to observe students' responses right before and after affirmative action was re-introduced, with the limitation that only observing two cross-sections of the data makes it impossible to evaluate pre-trends. THEOP records time spent on homework outside of school, a student-reported measure of effort. The survey also records whether the student applied to their first choice college, providing additional information on whether college applications behavior changed. Additionally, we combine a series of questions about parental behavior into a "parental involvement index," with values ranging from 5 to 20.[17] This index captures whether parents changed their behavior or educational investments in response to affirmative action. Finally, a question about whether the student discussed the

[16] We focused on 11th graders to reduce the substantial administrative burden of constructing the data set for the school district. We believed this group to be most likely to be affected by affirmative action, as they had not yet applied to college but were close enough to the college applications stage to make decisions based on college admissions policies.

[17] These questions ask "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" so that a higher index corresponds to more involvement along these dimensions.

college applications process with her/his guidance counselor captures changes in guidance counselor involvement. Appendix Table A2 reports summary statistics for these data.

4 Conceptual Framework

Before proceeding to the main results, we introduce a simple model that formalizes different mechanisms through which affirmative action affects different students' pre-college human capital. Based on this model, we predict that students for whom the returns to pre-college human capital increase the most should also increase their pre-college human capital the most. The second half of this section identifies where these students are in the pre-treatment test score distribution, generating testable predictions about where we should observe effects in Section 5.

4.1 Model

Let a student's perceived expected utility from college enrolment be

$$u_i = \beta_r^A q(h_i; r, A) - c_i(h_i; \mathbf{h}),$$

where i denotes a student, q is the expected payoff from pre-college human capital h_i in college admissions (e.g. the economic value of attending a college of a given quality), $\mathbf{h}$ is a vector of all other students' pre-college human capital, $r \in \{URM, W\}$ is the student's race, $A \in \{0, 1\}$ denotes the policy environment, and is 1 if there is affirmative action and 0 otherwise, c_i is a student-specific cost of human capital investment, which can be affected by peer effects through $\mathbf{h}$, as well as academic ability and socioeconomic status.

Let β_r^A be a race-specific scalar that represents the taste for/consumption value of attending college and may also capture systematically biased beliefs about the likelihood of being admitted to college (e.g. $\beta_{URM}^0 < \beta_W^0$). β_r^A can capture both the fact that students of a given race may have systematically negatively biased incorrect beliefs about admissions and that students of a given group may not value attending college as much if colleges are perceived as un-welcoming or having non-diverse student bodies. That is, a student with low β_r^A may view college as unattainable and not apply even if in fact she is likely to be admitted. To simplify exposition, let $c_i(h_i; \mathbf{h}) = h_i(\alpha_i - \delta \bar{h})$, where α_i is the portion of the cost of human capital that is unaffected by peers (academic ability and socioeconomic status) drawn from a distribution F_r, and $\bar{h}$ is the average human capital of the other students. F_r may vary across races, capturing differences in socioeconomic status and access to resources. To ensure an interior solution, assume $\frac{\partial q(h_i;r,A)}{\partial h} > 0$ and for h_i sufficiently high, $\frac{\partial^2 q(h_i;r,A)}{\partial h^2} < 0$. Assume that there are enough students that they don't internalize their own effects on average pre-college human capital. Then, a utility maximizing student's pre-college human capital investment is characterized by the first order condition

$$\beta_r^A \frac{\partial q(h_i^A; r, A)}{\partial h} = \alpha_i - \delta \bar{h}, \tag{1}$$

where h_i^A is the optimal choice of h_i under affirmative action policy A. Since h_i^A is our object of interest, we can use equation (1) to understand the different mechanisms through which the policy affects pre-college human capital.

1. **Effect of AA on Direct Returns to Pre-College Human Capital.** Denote the change in the payoff to an additional unit of pre-college human capital at h_i^0 from introducing affirmative action as $\Delta R(i,r) = \frac{\partial q(h_i^0;\mathbf{h},r,1)}{\partial h} - \frac{\partial q(h_i^0;\mathbf{h},r,0)}{\partial h}$. Since q has positive, diminishing marginal returns, if $\Delta R(i,r) > 0$, $h_i^1 > h_i^0$, and if $\Delta R < 0$, $h_i^1 < h_i^0$. $\Delta R(i,r)$ can be positive or negative depending on an individual's race and cost of pre-college human capital. To get intuition for this, consider a URM student who has a very low value of α_i, and was already selecting a h_i^0 that would result in admission to the most selective Texas public university with high probability. For this student, the human capital needed to be admitted to that school with a high probability falls due to affirmative action, so her returns to pre-college human capital at h_i^0 decline and $\Delta R < 0$. In contrast, for a student with a higher value of α_i, the student may have optimally chosen a low h_i^0 because increasing h enough to be admitted to a high quality college would be costly. If the returns to h become more steep for this student (e.g. colleges will take URM students with lower values of h with higher probability), $\Delta R > 0$. Similar logic applies to whites. Thus, the average effect of this mechanism by race and overall is ambiguous. In the next subsection, we will directly estimate $\Delta R(i,r)$ for URMs in different parts of the pre-policy test score distribution (a proxy for h_i^0) to obtain testable predictions about the direction and location of these effects on pre-college human capital.

2. **Aspirations Effect**: $\beta_r^1 > \beta_r^0$. If students view colleges as more welcoming and diverse (raising their value of attending college) or if affirmative action increases their aspirations, leading them to believe they can be admitted to a high quality institution, $\beta_{URM}^1 > \beta_{URM}^0$. This would unambiguously lead to an increase in h_i due to diminishing

marginal returns. The existence of an aspirations mechanism is consistent with work by Hoxby and Avery (2012), which shows that qualified, economically disadvantaged students often do not apply to elite schools.

3. **Spillover Effect**: $\bar{h}^1 > \bar{h}^0$ or $\bar{h}^1 < \bar{h}^0$. The spillover effect leads to a change in the cost of a unit of pre-college human capital $(\alpha_i - \delta\bar{h})$ due to an increase or decrease in $\bar{h}$ due to affirmative action. Increases in average human capital increase own human capital by reducing the cost of human capital, and reductions in average human capital increase own cost of human capital, reducing human capital. If both of the previous effects have average positive effects on pre-college human capital, we expect this effect to amplify the positive effects. If, on net, they have negative effects, we expect it to amplify negative effects. If URMs experience increases in the direct returns to human capital and whites experience losses, negative direct effects on whites can be mitigated or reversed by spillover effects due to the direct effects and aspirations effects on URMs.

Altogether, the model points to multiple mechanisms through which affirmative action could have positive or negative effects on pre-college human capital for both URMs and whites. We next use the data to develop predictions, based on the direct returns to human capital effect in the model, about where in the test score distribution we should observe effects on pre-college human capital.

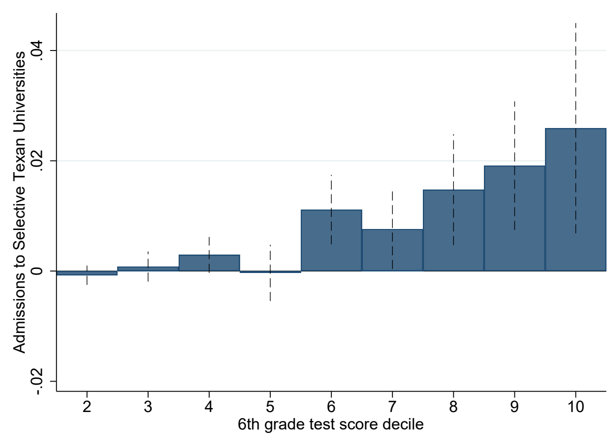
4.2 Changes in Returns to Pre-College Human Capital

In this subsection, we estimate the change in the returns to pre-college human capital for URMs relative to whites in different parts of the pre-treatment test score distribution following the introduction of affirmative action. To do so, we use TEA data on university admissions. Taking advantage of the fact that we observe test scores in 6th grade, we estimate changes in the marginal effect of moving up one *decile* in the test score distribution on admissions at selective public Texan universities. The estimating equation is

$$
\begin{aligned}
y_{dcea} = &\sum_j \beta_{1,j}(URM_e \times PartTreat_c \times \mathbf{I_a}^{a\geq j}) + \sum_j \beta_{2,j}(URM_e \times FullTreat_c \times \mathbf{I_a}^{a\geq j}) \\
&+ \mathbf{\Gamma X_{dcea}} + \epsilon_{dcea} \qquad (2)
\end{aligned}
$$

where a denotes a test score decile, c a cohort of 9^{th} graders, d a school district, and e a racial group. The outcome, y_{dcea}, is the number of selective universities that admit the student, and $\mathbf{I_a}^{a \geq j}$ is an indicator variable if a student's 6th grade test score decile a is greater than or equal to j. $\mathbf{X_{dcea}}$ is a vector of control variables that include average student characteristics for the observation cell,[18] cohort, district and race fixed effects, as well as their interactions with test score decile indicator variables. We allow changes over time to differ between partially treated cohorts ($PartTreat_c$) who were already in high school at the time of the policy change and fully treated cohorts ($FullTreat_c$) who started high school after the policy change. Thus, $\beta_{1,j}$ and $\beta_{2,j}$ capture the change in the marginal effect of moving from decile $j-1$ to j due to the policy for URMs who are partially and fully treated.[19] Since increased human capital investment can allow a student to move up in the distribution of test scores relative to her peers, we interpret these coefficients as a proxy for the change in the relative returns to pre-college human capital investment in university admissions.

Figure 1: Relative Change in Returns to Moving Up a 6th Grade Test Score Decile in Admissions to Selective Texan Public Universities



Notes: The outcome is the number of selective Texan public universities to which a student is admitted. Bars are the coefficients from equation (2), which capture the change in the marginal effect of moving up a 6th grade test score decile on college admissions. Dashed lines show 95% confidence intervals for standard errors clustered at the district level.

Figure 1 reports estimates of $\beta_{2,j}$. It shows that the returns mainly rise in the top half of the test score distribution. For instance, the returns to moving from the 9th to the 10th decile increased by 0.026 for URMs relative to whites among fully treated cohorts.[20]

[18]These consist of age, sex, immigrant status, low-income status, gifted, ESL, special education status, and limited English proficiency.

[19]Appendix Figure A4 shows the baseline, pre-AA, average number of selective schools a student is admitted to (unconditional on applications) by test score decile, separately for URMs and whites.

[20]The fact that there are strong increases in the returns for the top decile isn't inconsistent with the

Appendix Figure A5 further reports estimates of $\beta_{2,j}$ for admissions to each of four selective Texan universities separately. For UT Austin, University of Houston, and Texas Tech, which were free to practice affirmative action, there are increases in the returns to human capital in the top 30-50% of the test score distribution. Reassuringly, for Texas A&M, which does not practice affirmative action, there is no systematic effect on the returns to human capital.

Overall, these figures suggest that if students (or teachers and parents) respond to these changes in the returns to pre-college human capital, we would expect affirmative action to have (1) positive average effects on pre-college human capital that are (2) concentrated among students with 6th grade test scores in the top half of the distribution. We empirically examine both predictions in Section 5, focusing on SAT data to estimate averages effects by race and using Texas administrative data to uncover heterogeneous effects across the distribution of test scores.

5 Effects of Affirmative Action on Students' Outcomes

In this section, we estimate the effects of the reinstatement of affirmative action on several measures of students' behavior using our three non-survey data sets. We first report the effect of affirmative action on URMs' and whites' average SAT scores using difference-in-differences and synthetic control group approaches that compare changes in scores in states that re-instated affirmative action (Texas, Louisiana, Mississippi) to changes in unaffected states. Next, we turn to the Texas administrative data to examine the effects of affirmative action across the 6th grade test score distribution. We then complement these results with an analysis of a single large, urban Texas school district where we observe grades and standardized test scores in 11th grade. Finally, we estimate the effects of affirmative action on college completion, though we caution that this outcome is a function of both pre-college human capital and the college to which a student is matched.

5.1 Effect of Affirmative Action on SAT scores

To measure the effects of affirmative action on SAT scores, we exploit both time variation in whether students took the SAT after *Grutter v. Bollinger* and geographic variation in whether students lived in a state where *Grutter v. Bollinger* eliminated a previous ban on

existence of the Top 10% Rule. This is because the deciles do not accord with the cut-offs used by the rule: they are based on performance in 6th grade rather than at the end of high school and are across-school deciles rather than within-school deciles.

affirmative action. This difference-in-differences strategy allows us to estimate the effect of affirmative action *separately* for URMs and whites.

Event Study Graphs. We first use event study graphs to visually inspect the evolution of SAT scores in treated states relative unaffected states. We estimate the following equation separately for whites and URMs using a panel of average math and verbal SAT scores at the state-race-year level

$$
\begin{aligned}
y_{ket} = & \sum_{l=1998}^{2002} \beta_l (Treated\_State_k \times \mathbf{I_{tl}}) + \sum_{l=2004}^{2010} \beta_l (Treated\_State_k \times \mathbf{I_{tl}}) \\
& + \Gamma X_{kt} + \alpha_k + \alpha_t + \varepsilon_{ket},
\end{aligned} \tag{3}
$$

where k indexes a state, e indexes a racial group, and t indexes a year. Then, y_{ket} is the mean math or verbal test score for group e in state k and year t, $Treated\_State_k$ is an indicator variable equal to 1 if the observation belongs to a state that was treated, $\mathbf{I_{tl}}$ is an indicator variable equal to 1 if $t = l$, α_k is a state fixed effect and α_t is a year fixed effect. The omitted year is 2003, the year before the policy change. We weight race-state-year cells by the number of test-takers and cluster our standard errors at the state-level.

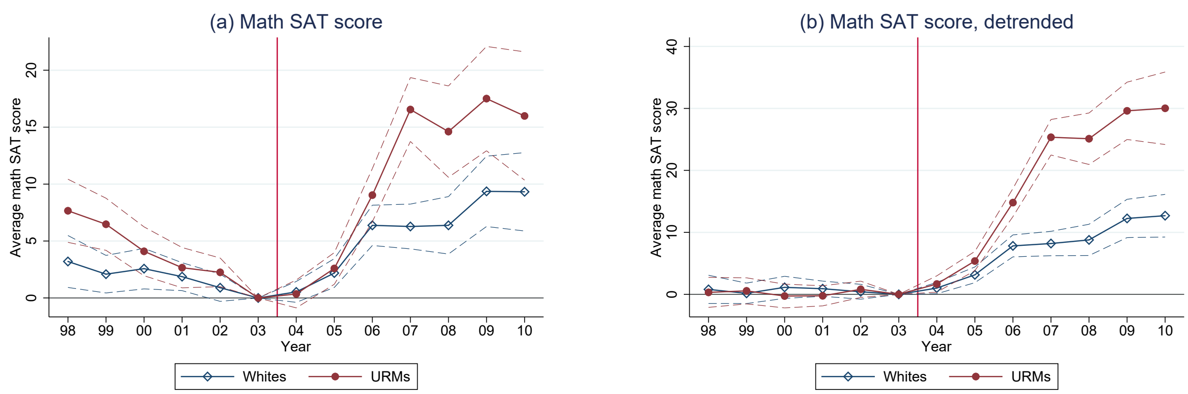
This event study specification estimates the differential effect of being in a treated state for each year, β_l. If pre-trends between treated and non-treated states are parallel, we expect that β_l should be small and insignificant prior to 2003. One potential concern is that our estimates might be contaminated by other affirmative action policy changes that occurred between 1998 and 2010 in the states that were not affected by *Grutter v. Bollinger*. To address this issue, we include a vector of controls X_{kt} for relevant policy changes in control states during our study period.[21]

Panel A of Figure 2 reports β_l for math separately for URM and white students. The plot shows a negative pre-trend in math SAT scores for students in treated states relative to those in non-treated states. That is, prior to *Grutter v. Bollinger*, students in treated states were falling behind the rest of the country on the math SAT.[22] Following the reinstatement of affirmative action, there is a reversal of fortunes, and the negative trend turns positive right after 2004. Importantly, the post-treatment positive trend for math scores appears to be considerably steeper for URM students than for whites. On the other hand, there is no clear change in verbal scores over time for either URMs or whites (see Appendix Figure A7).

[21] These policy changes are listed in Appendix A.

[22] Appendix Figure A6 shows that a similar pattern holds for Asians.

Figure 2: Effect of AA on Math SAT Scores



Notes: The outcome is average math SAT scores at the state-year level. Dots indicate coefficients of regressions of the outcome on year dummies interacted with an indicator variable for the three treated states, estimated separately for white and URM students. Cells are weighted by the number of SAT test takers. Dashed lines show 95% confidence intervals for standard errors clustered at the state level.

As shown by Panel B, which plots the de-trented event study graph, the standard difference-in-differences estimates that do not account for the observed negative pre-trends understate the positive effect of AA on math SAT scores.[23] Accounting for a linear trend eliminates all the negative pre-trends, and the positive effects on math SAT scores are now larger.

Difference-in-Differences and Triple-Differences Empirical Strategies. The difference-in-differences analogue to the event study graphs is given by

$$y_{ket} = \beta_{DD}(Treated\_State_k \times Post2003_t) + \Gamma X_{kt} + \alpha_k + \alpha_t + \alpha_e + \varepsilon_{ket}. \tag{4}$$

where $Post2003_t$ is an indicator variable equal to 1 if the year is greater than 2003. Since we expect URMs to be more affected by affirmative action, we also implement a triple-differences strategy that uses race as a third difference. This identifies the change in the racial achievement gap due to the policy, in line with our within-Texas results in the next section. This approach controls for any time-varying shocks in states affected by the policy but may under or over-estimate the policy's effects on URMs' outcomes if the policy also affected whites. To estimate the differential effect of affirmative action on URMs relative to

[23]To construct this figure, we first estimate linear pre-treatment (1998-2003) trends separately for each ethnicity and treatment group, and subtract these time trend terms from the full panel.

non-URMs, we estimate

$$y_{ket} = \beta_{DDD}(Treated\_State_k \times Post2003_t \times URM_e) + \Gamma X_{ket} + \alpha_{ke} + \alpha_{et} + \alpha_{kt} + \varepsilon_{ket}, \quad (5)$$

where URM_e is an indicator for underrepresented minority groups, α_{ke} is a state-race fixed effect, α_{et} is a race-year fixed effect, and α_{kt} is a state-year fixed effect. While the triple-differences strategy requires us to include controls for all three sources of variation and their double interactions, these are subsumed by the fixed effects in this specification.

This strategy controls for all the same potential sources of bias as the difference-in-differences strategy. Both strategies use fixed effects to account for level differences in SAT scores between states and over time. In addition, the triple-differences strategy includes the fixed effect α_{kt}, which controls for any state-specific differences over time. Thus, this triple-differences strategy is valid even if Texas, Louisiana, and Mississippi have different time trends from other states, as long as those time trends also don't vary by race.

Difference-in-Differences and Triple-Differences Results. Table 1 reports the coefficients from equation (4) (panels A, B and C) and equation (5) (panel D) for SAT scores. Column (1) shows that math scores for both URMs and whites improved in treated states following 2003, though URMs' test scores improved by twice as much (8 points or 0.07 sd relative to 4 points or 0.035 sd). There is no effect on math scores for Asians and no effect on verbal scores for any group.

Whites' human capital appears to be positively affected by the abrogation of the ban. As described in the conceptual framework, this could occur if whites increased their pre-college human capital in response to intensifying competition or if positive spillovers from URM students cancelled out any negative effects on the returns to pre-college human capital. If the distribution of the cost of pre-college human capital investment is different for whites than URMs, the introduction of affirmative action could increase the returns to pre-college human capital investment for both the average white and URM student.[24] Altogether, the findings in Table 1 indicate that, since the policy increased both whites' and URMs' pre-college human capital investment, estimates of the effect of the policy on the racial achievement gap are likely to under-estimate its aggregate effects on pre-college human capital.

In the last panel of Table 1, we report the results of the triple-differences specification. We find that URMs' math SAT scores improved relative to whites' in treated states by

[24]This mechanism is consistent with both the theoretical model and empirical findings of Cotton et al. (2015), who show that students who do not benefit from a simulated affirmative action policy may also be incentivized to increase their effort.

Table 1: Effect of AA on SAT Scores for URMs and Whites

	Math	Verbal	# Test takers	% Test takers
	(1)	(2)	(3)	(4)
	Panel A: URMs			
DD coefficient	8.009***	-0.634	545.9	0.002
	(1.544)	(1.784)	(1195.1)	(0.005)
Observations (cells)	1904	1901	1904	1114
R^2	0.844	0.796	0.802	0.872
State, year and race FE	X	X	X	X
	Panel B: Whites			
DD coefficient	4.048***	0.0342	1588.7	0.005
	(0.984)	(0.888)	(1262.0)	(0.005)
Observations (cells)	663	663	663	561
R^2	0.969	0.971	0.987	0.978
State, year and race FE	X	X	X	X
	Panel C: Asians			
DD coefficient	0.658	-0.176	-2086.3	-0.010
	(1.827)	(2.753)	(2038.4)	(0.007)
Observations (cells)	663	663	663	556
R^2	0.944	0.929	0.975	0.783
State, year and race FE	X	X	X	X
	Panel D: Triple-Difference (URMs vs Whites)			
DDD coefficient	4.155***	1.260	-437.5	-0.002
	(0.828)	(0.753)	(1058.6)	(0.003)
Observations (cells)	2555	2552	2555	1675
R^2	0.998	0.998	0.999	0.993
State-year FE	X	X	X	X
State-race FE	X	X	X	X
Race-year FE	X	X	X	X

Notes: This table reports differences-in-difference and triple-differences estimates of the effect of affirmative action on SAT scores. Each observation is a state-race-year group. In columns (1) and (2), cells are weighted by the number of test-takers in a group. In column (3), cells are weighted by the average number of test-takers in the pre-treatment years 1998-2000. In column (4), cells are weighted by the number of 17-19 year olds in the population group (from ACS), and the dependent variable is (# of test-takers)/(# of 17-19 years old). In Panels A, B and C, the DD coefficient reports the interaction of an indicator variable for belonging to a treated state (Texas, Louisiana, Mississippi) and being tested after *Grutter v. Bollinger* (post 2003), and the regressions include controls for policy changes in control states. In Panel D, the coefficient is on the interaction between being a URM, being tested post 2003, and belonging to a treated state, and the regressions include controls for policy changes in control states interacted with being a URM. Standard errors are clustered at the state-level.

a statistically significant 4 points, equivalent to 0.035sd or a 5% reduction in the racial achievement gap. The triple-differences results further confirm that the positive difference-in-differences estimates are not merely due to differential time trends in states that were not affected by *Grutter v. Bollinger*.

In the last two columns, we evaluate whether the policy change affected test-taking. In column (3), the outcome is the raw number of SAT test-takers, and cells are weighted by the average number of test-takers in the first three years of the panel, 1998-2000. In column (4), we generate a measure of the probability of taking the SAT by dividing the number of test-takers by the number of 17-19 year-olds in each cell using yearly ACS population counts to account for fluctuations in the number of test takers due to changes in population size. Both metrics suggest there was no significant change in the probability of taking the SAT in treated states relative to untreated states.

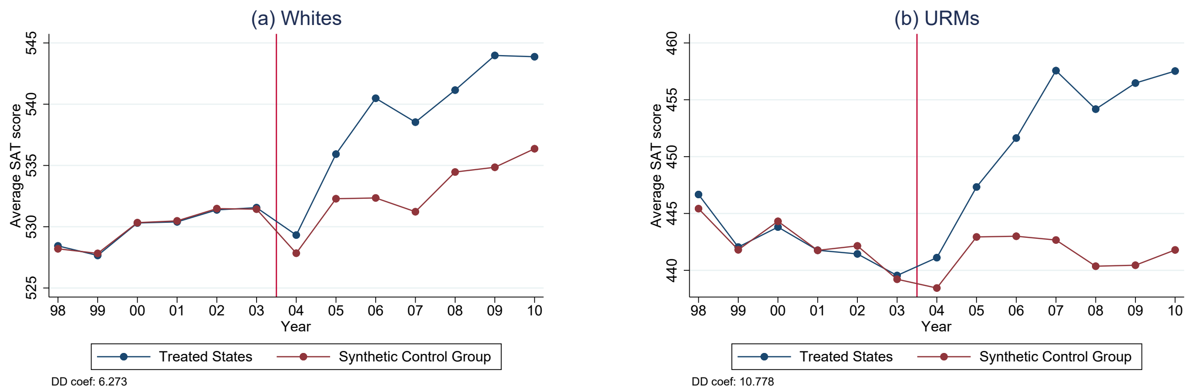
In Appendix A, we report the results of a variety of additional robustness tests for the SAT results. Motivated by Hinrichs (2012), who argues that Louisiana and Mississippi were less affected by the original *Hopwood v. Texas* ruling than Texas, we show that the results are unchanged if we drop Lousiana and Mississippi from the analysis, or if we drop states that implemented affirmative action bans.

Synthetic Control Group Strategy. While event study graphs help us to assess the appropriateness of the parallel trends assumption, synthetic control group methods provide us with an alternative way of verifying that our results are robust to accounting for differential time trends. Based on these methods, developed by Abadie and Gardeazabal (2003) and Abadie et al. (2010), we construct a synthetic control group of states by matching those states' pre-trends in test scores to the pre-trends of the treated unit (the weighted average of Texas, Mississippi, and Louisiana).[25] We match the pre-treatment values of the number of URM and white test takers, the math SAT scores of URM and white students, and the verbal SAT scores of URM and white students. Our estimated effect of the reinstatement of affirmative action is then the difference between the change in test scores in the weighted average of the treated states and the synthetic control. Given the null results for verbal scores found previously, we focus on math SAT scores.

[25]When generating the synthetic control groups, we exclude South Dakota, North Dakota, Wyoming, and Washington DC from the pool of potential controls because SAT scores are missing for some ethnic groups in some years in these states due to small samples. We follow the standard practice of minimizing the mean squared prediction error of our outcome variable over the entire pre-treatment period. In Appendix B, we show that our results are robust to using fewer pre-treatment years to construct the synthetic control group and to using an alternative nearest-neighbor matching approach.

To assess the significance of our estimates, we use permutation tests. For all possible combinations of three untreated states, we apply the synthetic control method and calculate the post/pre-treatment ratio of root mean squared prediction errors (RMSPE).[26] We then calculate the rank of the real treatment unit in the distribution of RMSPE ratios.

Figure 3: Synthetic Control Estimates of the Effect of AA on SAT Math Scores



Notes: This figure reports synthetic control analyses separately for whites and URMs. The left figure shows SAT math scores for the treated states (Texas, Mississippi and Louisiana) for whites, while the right panel shows them for URMs. For whites, weights on control units are 42.5% (California), 40.8% (Florida), 8.3% (Pennsylvania), 6.2% (New York), and 2.2% (Indiana). All other states have a weight of zero. For URMs, weights on control units are 33.2% (Oregon), 28.4% (New Jersey), 20.6% (California), and 17.8% (Pennsylvania).

Synthetic Control Results. Panel A of Figure 3 shows the evolution of SAT math scores over time for our treatment unit and the associated synthetic control group separately for white and URM students. Interestingly, for both URMs and whites, California, which banned affirmative action in public universities prior to our study period, contributes to the control group. In both cases, the synthetic control group closely tracks the treatment unit prior to the reinstatement of affirmative action, and the two trends diverge considerably from 2004 onward. This is true for both groups, but the divergence is greater in magnitude for URMs. The implied treatment effects are larger than our baseline difference-in-differences estimates, consistent with the evidence of negative time trends. Whites' math scores increase by 6.3 points, and URMs' increase by 10.8 points. The placebo tests suggest that these results are not due to chance. The treatment unit's post-pre ratio of RMSPE is at the 99.2th percentile

[26]Since the donor pool contains 44 control units, the number of possible combinations of three states is 13,244.

of the distribution for whites and at the 96.6th percentile for URMs. Appendix B provides further robustness tests of the synthetic control estimates, including dropping states that enacted AA bans during the study period from the donor pool.

Appendix Figure A8 is a synthetic control plot of the differences between treated and untreated units separately for white and URM students. For both racial groups, the differences are close to zero prior to treatment and then exhibit large increases following *Grutter v. Bollinger*, with larger effects for URMs. The implied difference in the gains between URMs and whites is 4.5 points, which is similar to our conventional triple-differences estimate.

Having found evidence that students respond to affirmative action by improving their SAT scores, we next investigate whether students also increase other dimensions of their human capital. Since SAT scores may just reflect better SAT-specific test-taking skills, examining other outcomes allows us to evaluate if affirmative action affects human capital more broadly.

5.2 Impact of Affirmative Action on College Applications

Motivated by the conceptual framework, to assess the effects of affirmative action throughout the test score distribution, we now use Texas-wide administrative data. As a first step, we examine students' college applications behavior and compare the change in URMs' college applications behavior following *Grutter v. Bollinger* to the change in whites'.

Event Study Graphs. The key identifying assumption is that the college applications behavior of URM and comparable non-URM students would have evolved the same way in the absence of the ruling. We first examine the plausibility of this assumption using an event study approach in which we plot the relative effect of being a URM on college applications separately for each cohort. Doing so allows us to establish if trends in college applications for URMs and whites were parallel prior to the re-introduction of affirmative action. Recalling that an observation in these data is a race-(pre-treatment) test score quintile-district-cohort cell, we estimate the following model:

$$\begin{aligned} y_{dcea} = &\sum_{t=1997}^{1999} \beta_t (URM_e \times \mathbf{I_{ct}}) + \sum_{t=2001}^{2010} \beta_t (URM_e \times \mathbf{I_{ct}}) + \mathbf{\Gamma X_{dcea}} \\ &+ \alpha_{da} + \alpha_{ca} + \alpha_{ea} + \epsilon_{dcea}, \end{aligned} \tag{6}$$

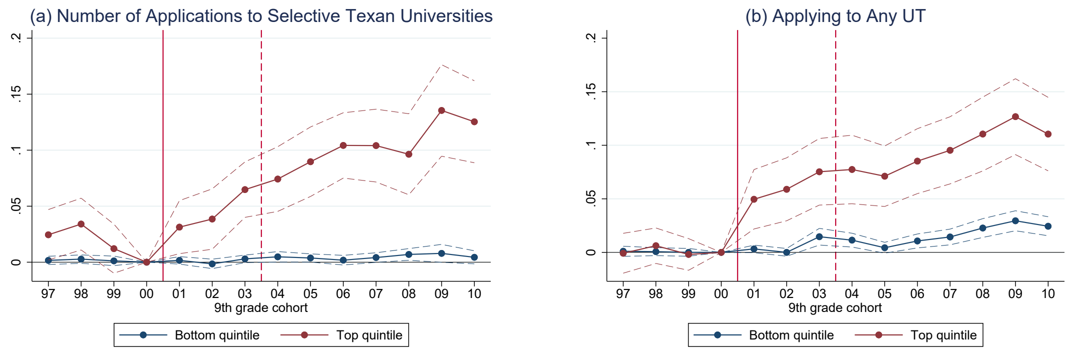
where d indexes a school district, c indexes a 9th grade cohort (the year the student entered 9th grade), e indexes an ethnicity, and a indexes quintiles in the 6th grade state standardized test distribution. The variable URM_e is an indicator variable for belonging to a URM group, $\mathbf{I_{ct}}$ is an indicator variable equal to 1 if $t = c$, and $\mathbf{X_{dcea}}$ is a vector of average student characteristics. We estimate the model both separately by test score quintile as well as pooling all test score quintiles together. In all cases, we include district, cohort, and race fixed effects, as well as all two-way interactions with test score quintile fixed effects (α_{da}, α_{ca}, and α_{ea}) when test score quintiles are pooled. The dependent variable, y_{dcea}, is either the average number of applications sent to selective Texan institutions or fraction of students who applied to any of the campuses of the University of Texas. We focus on the University of Texas because the University of Texas Board of Regents promptly allowed its campuses to consider race in admissions. Standard errors are clustered at the district-level.

If the parallel trends assumption is valid, for $t < 2000$, we expect that β_t will be indistinguishable from zero. If the effects we estimate in the difference-in-differences strategy are due to affirmative action, we expect to see an increase in the values of β_t soon after the 2000 9th grade cohort. Additionally, if the effects of affirmative action accumulate over time as students have more time to adjust their behavior, we expect that after 2000, β_t will generally be greater for greater values of t. To provide richer evidence on dynamic effects, we include all cohorts from 1997 to 2010.

Before progressing to the event study results, we note that one important limitation of this strategy is that whites' outcomes may also be affected by affirmative action. If, for example, whites decrease their college applications in response to the reinstatement of affirmative action, we would estimate positive effects of affirmative action, even if URMs' behavior was unchanged. To assess whether this could be driving our results, we also separately graph trends in unconditional applications behavior by race in Appendix Figure A9. This figure plots raw trends in applications (normalized to the cohort in 9th grade in 2000) separately for the full sample and for top quintile students. The figure shows that there is a positive trend break in URMs' behavior around the reintroduction of affirmative action, with no clear evidence of differential trends across groups prior to the event. Whites do not appear to reduce their applications.

Figure 4 plots the year-specific coefficients β_t for the number of applications to selective universities (Panel A) and for the probability of applying to any University of Texas (UT) institution (Panel B). Since the increase in the returns to human capital is concentrated at the top of the test score distribution, we examine trends separately for students in the top and

Figure 4: College Application Behavior of URMs Relative to Whites



Notes: The University of Texas system consists of UT Arlington, UT Austin, UT Dallas, UT El Paso, UT Permian Basin, UT Rio Grande, UT San Antonio, and UT Tyler. Dots indicate coefficients from a regression of the outcome on year dummies interacted with URM status separately for students in the bottom and top quintiles of the 6^{th} grade cohort-specific test score distribution. All regressions condition on cohort, race and district fixed effects, as well as means of individual characteristics at the district-cohort-ethnicity-test score quintile level. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

bottom quintiles.[27] Cohorts between the solid and dashed vertical lines were in high school at the time of the reinstatement of AA, while cohorts to the right of the dashed vertical line started high school in the new policy environment.

For applications to selective universities, the point estimates for bottom quintile students are very small and statistically insignificant both before and after the policy change. For top-quintile students, there appears to be a weak negative pre-trend, but these year-specific coefficients are small and not systematically statistically significant, and a strong positive effect emerges directly after the policy change. In panel B, coefficients prior to the policy change are all close to zero and statistically insignificant. For top quintile students, there is an increase in applications for the 2000 cohort, and then applications grow slowly over time. For bottom quintile students, a small positive effect appears in later years.[28]

[27] Appendix Figure A10 shows average effects pooling all test score quintiles.

[28] For completeness, Appendix Figure A11 reproduces the event study graph for the probability of applying to any 4-year public Texan university. There is a small upward trend in URM college applications relative to whites prior to the policy change, but most year-specific coefficients are close to zero and statistically indistinguishable from the base year. The results indicate that the probability of applying to a university for URM students increases at the time of the policy change, and the jump is considerably more pronounced for top quintile students.

Difference-in-Differences Empirical Strategy. The difference-in-differences estimates for the number of applications are given by

$$y_{dcea} = \beta_1(URM_e \times PartTreat_c) + \beta_2(URM_e \times FullTreat_c) + \mathbf{\Gamma X_{dcea}} + \alpha_{dca} + \alpha_{dea} + \epsilon_{dcea}, \tag{7}$$

where we distinguish between partially treated cohorts who were already in high school at the time of the policy change and fully treated cohorts who started high school after the policy change. Thus, $PartTreat_c$ is equal to 1 if a student was in 9th grade between 2000 and 2003, while $FullTreat_c$ is equal to 1 if a student was in 9th grade after 2003. To ensure that our test score quintiles are exogenous to the policy, we restrict the sample to cohorts who completed 6th grade prior to *Grutter v. Bollinger*. In this specification, the effect of affirmative action is identified by comparing URM students to non-URM students with similar test scores in 6th grade, in the same cohort and the same school district. The fixed effect α_{dca} accounts for any time trends that may vary across districts or test score levels, as long as they are not differential by race. The fixed effect α_{dea} accounts for any differences across races, districts, or test score levels (or any combination thereof), as long as these differences do not vary over time. We cluster the standard errors at the district-level.

Our main coefficients of interest, β_1 and β_2 capture the short and medium-run effects of affirmative action on college applications behavior. Later cohorts may have had greater opportunities to adjust their human capital investment in high school in response to the re-instatement of affirmative action. This in turn may have affected their likelihood of being accepted to college and therefore their propensity to apply in the first place, relative to earlier treated cohorts, suggesting that $\beta_2 > \beta_1$.

Difference-in-Differences Results. We report coefficients from equation (7) in column (1) of Table 2. In panel A, the outcome is is the average number of applications to selective institutions, and in panel B, the outcome is the probability of applying to any University of Texas institution. In panel A, on average, fully treated URM students apply to 0.02 more selective Texas colleges, indicating that affirmative action closed the racial gap by 13%. Turning to Panel B, on average, lifting the ban on affirmative action increased URMs' probability of applying to at least one UT campus relative to whites' by 0.7 percentage points for cohorts who entered high school after the ban was lifted. In both panels, the estimates are precisely estimated and statistically significant at the 5% level.

For both panels, these average effects mask substantial heterogeneity. The remaining

Table 2: Effect of AA on College Applications Behavior for URMs Relative to Whites

		Percentile of 6th grade test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Applications to selective universities					
Partial treatment	0.0097***	0.0018	0.0022	0.0024	0.0145**	0.0297***
	(0.0027)	(0.0019)	(0.0025)	(0.0035)	(0.0067)	(0.0085)
Full treatment	0.0187***	0.0019	0.0046	0.0151***	0.0304***	0.0449***
	(0.0038)	(0.0016)	(0.0030)	(0.0049)	(0.0073)	(0.0105)
Observations (cells)	68509	12933	14515	14809	14145	12107
R^2	0.913	0.469	0.630	0.738	0.800	0.837
Mean dependent variable	0.1484	0.0079	0.0331	0.0877	0.1994	0.4158
Test: quintile q = top quintile						
Partial treatment p-value		0.0013	0.0022	0.0028	0.1265	
Full treatment p-value		0.0000	0.0003	0.0046	0.1356	
	Panel B: Application to any UT institution					
Partial treatment	0.0030	0.0031**	-0.0025	0.0021	-0.0002	0.0148***
	(0.0019)	(0.0014)	(0.0023)	(0.0030)	(0.0043)	(0.0057)
Full treatment	0.0074**	0.0004	0.0022	0.0089**	0.0066	0.0198***
	(0.0029)	(0.0017)	(0.0022)	(0.0041)	(0.0054)	(0.0074)
Observations (cells)	68509	12933	14515	14809	14145	12107
R^2	0.905	0.825	0.872	0.878	0.864	0.846
Mean dependent variable	0.1072	0.0212	0.0514	0.0879	0.1390	0.2363
Test: quintile q = top quintile						
Partial treatment p-value		0.0578	0.0023	0.0384	0.0171	
Full treatment p-value		0.0087	0.0219	0.1067	0.0539	
Demographic controls	X	X	X	X	X	X
District-cohort-test score FE	X	X	X	X	X	X
District-ethnicity-test score FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on URMs' college applications behavior. The regressions use the TEA data, an observation is at the district-cohort-race-test score quintile level, where test score quintile is assigned based on 6th grade (pre-AA) test scores on the state standardized test. The sample is restricted to students who were in 9th grade between 1997 and 2006. Cells are weighted by the number of student-years in a cell. Partial treatment is the coefficient on the interaction between an indicator for being a URM and an indicator variable for entering high school after 2001 and before 2003. Full treatment is the coefficient on the interaction between entering high school after 2003 and being a URM. The outcome variable in panel A is the average number of selective universities to which students applied. The outcome variable in Panel B is the fraction of students in a cell that applied to any institution of the University of Texas system (UT Arlington, UT Austin, UT Dallas, UT El Paso, UT Permian Basin, UT Rio Grande, UT San Antonio, UT Tyler). Standard errors are clustered at the district-level.

columns of the table estimate the effects for students in different quintiles of the 6th grade test score distribution. In Panel A, while bottom quintile students are no more likely to apply to selective institutions, top quintile students apply to 0.04 more selective institutions, and the second highest quintile applies to 0.03 more selective institutions. We can reject at the 1% level that the coefficients for top quintile students are equal to those of any of the first three quintiles. Turning to Panel B, we do find a very small positive effect on applying to any UT campus (0.0031) for partly treated students in the bottom quintile of the test score distribution, which rapidly fades out. The partial treatment effect is five times larger among students with top test scores (0.0148). This positive effect on top quintile students persists, with a coefficient of 0.0198 for fully treated students. In both cases, this heterogeneity accords with where we would expect affirmative action to have the strongest effects on college applications, given the estimates in Section 4. Among fully treated students, there are no significant effects in the bottom 40% of the distribution.

For all outcomes, the treatment effects appear to be larger for later cohorts. Thus, allowing students to have more years to adjust in response to the affirmative action policy appears to strengthen the policy's effect. This could be because students respond to these policies by increasing their pre-college human capital. We investigate this hypothesis in the next subsection.

5.3 Impact of Affirmative Action on Grades

Empirical Strategy. In this subsection, we turn to our data from the large, urban Texas school district (LUSD) to examine the effect of affirmative action on students' grades in 11th grade. Our econometric specification is similar to equation (7), with some alterations to accommodate the different structure of the school district's administrative data. In particular, unlike our Texas-wide regressions, which use aggregate district-year-race-test score quintile data, for the LUSD, an observation is an individual. The specification is

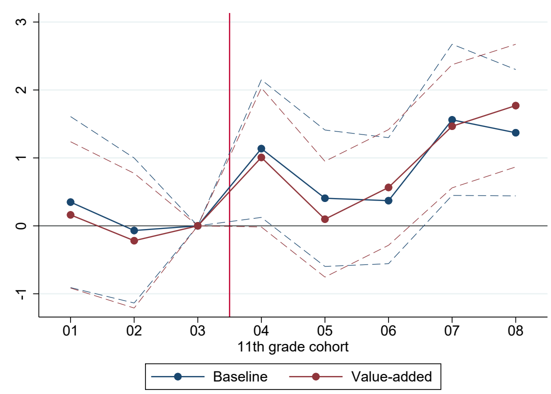
$$y_{isec} = \beta\left(URM_i \times Post2003_i\right) + \mathbf{\Gamma X_i} + \alpha_{sc} + \alpha_e + \epsilon_{isec} \tag{8}$$

where i denotes an individual, s denotes a school, e denotes a racial group, and c denotes a cohort.[29] The treatment variable $Post2003_i$ is an indicator variable equal to 1 if the outcome is realized after the policy change, so a student is observed in 11th grade after 2003. α_{sc}

[29]Since the LUSD data consists of repeated cross-sections of 11th graders, in this data set, a cohort refers to the year students attended 11th grade.

denotes a school-cohort fixed effect, and α_e is a race-specific fixed effect. We include α_{sc} to account for the fact that grades may not be comparable across schools or across years.[30] Thus, the effect of affirmative action in this regression is identified by comparing URM and white students in the same school in the same year. The basic controls $\mathbf{X_i}$ consists of age, sex, and home zip code fixed effects. Additionally, in a more conservative, "value-added" specification, we control for a lagged measure of school grades (8th grade course grades).[31] This control accounts for any changes in the achievement distributions of URMs and whites over time that might otherwise be attributed to affirmative action (such as changes due to cohort composition or migration). As before, the coefficient of interest, β, represents the effect of affirmative action on URM students relative to non-URM students. We also estimate cohort-specific coefficients and plot them in an event study graph. To do so, we simply alter equation (8) to estimate a different coefficient on the variable URM_i for every cohort.

Figure 5: Raw and Value-Added Estimates of AA's Effect on Mean Grades for URMs Relative to Whites



Notes: The outcome is mean grades across subjects in 11^{th} grade. Dots indicate the coefficients from regressions of the outcome on year dummies interacted with an indicator variable for URM status. The regression also includes school-cohort, race, and ZIP code fixed effects, as well as controls for age and gender. The value-added specification additionally controls for 8th grade grades. Dashed lines show 95% confidence intervals for standard errors clustered at the school-cohort level.

Results. Figure 5 reports year-specific coefficients on the URM_i indicator variable when the outcome is mean grades. There are no significant pre-trends, with the racial gap in school grades remaining constant over the 2001-2003 period. Grades for URM students improve

[30] For example, this would be the case if course offerings or grading standards are changing over time.

[31] The fact that we use 6th grade test scores in the TEA data and 8th grade test scores in the LUSD simply reflects differences in the availability of lagged scores across the two data sets.

relative to their non-URM peers upon the reinstatement of affirmative action and remain at this higher level through 2008. Results under the value-added specification, which controls for 8th grade test scores, are extremely similar.

The associated difference-in-differences estimates are reported in Table 3. The point estimates confirm that affirmative action had a positive effect on school grades in 11th grade. Our baseline estimates of equation (8) in column (1) indicate that grades increased by 0.9 points (on a 0-100 scale), equivalent to 0.1 sd, following the reinstatement of affirmative action, closing the pre-AA racial gap by 18%. In column (2), we report the results of the value-added specification. The coefficient is almost identical and remains statistically significant.

In column (3), we re-arrange the data set into a panel that includes two entries per student (one for 11th grade and one for 8th grade) and estimate a specification with student fixed effects. In this model, our main explanatory variable becomes a triple-difference interaction term $(URM_e \times Treat_c \times I_g^{11th\,Grade})$, where $I_g^{11th\,Grade}$ is an indicator variable equal to 1 when a student is enrolled in 11th grade. Here, the effect of affirmative action is identified from within-student changes in outcomes between 8th and 11th grade for students who were in 11th grade after the policy change and in 8th grade before the change. This alternative specification further accounts for any unobserved changes in URM students' characteristics across cohorts that might otherwise bias our estimate of the effect of affirmative action. The results of this alternative specification are nearly identical to our previous results.

In columns (4) to (6), we examine whether the effects are heterogeneous by prior school performance. To do so, we calculate school-by-cohort specific terciles of the distribution of grades in 8th grade. We focus on terciles instead of quintiles, as we did in the TEA data, because of the much smaller sample size. We then re-estimate equation (8) separately for students in the bottom, middle, and top terciles. While the point estimates for the effect of affirmative action are positive for all terciles, they are particularly large for top-achieving students (an effect of 1.4 percentage points or 0.2 sd). This is consistent with our estimated distribution of the increases in the returns to pre-college human capital.

5.4 Impact of Affirmative Action on the Stanford Exam

The data from the large, urban school district also allows us to estimate the effects of affirmative action on the standardized Stanford test, a low-stakes exam that the school district itself administered. To estimate the effects on the Stanford exam, we use the same difference-in-differences strategy as we did for grades in Section 5.3. The outcome variable is a student's mean percentile on the Stanford exam, where percentiles are based on the national distribu-

Table 3: Effect of AA on School Grades for URMs Relative to Whites

	All students			Grades in 8th grade		
				Bottom tercile	Middle tercile	Top tercile
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.8770***	1.0024***	0.9552***	0.8816*	0.3996	1.3859***
	(0.3086)	(0.2979)	(0.3114)	(0.5102)	(0.3906)	(0.4207)
Lagged dep. var. (grade 8)		0.5552***				
		(0.0092)				
Observations	61089	46346	92847	15874	15621	14776
R^2	0.226	0.345	0.784	0.189	0.224	0.208
Mean dependent variable	78.67	79.48	81.11	75.79	79.49	83.46
S.D. dependent variable	8.67	7.80	7.37	7.43	6.99	6.97
Test: tercile q = top tercile						
p-value				0.4412	0.0784	
School-year FE	X	X	X	X	X	X
Ethnicity FE	X	X		X	X	X
Demographic controls	X	X		X	X	X
Student FE			X			
Grade-year FE			X			
Grade-ethnicity FE			X			

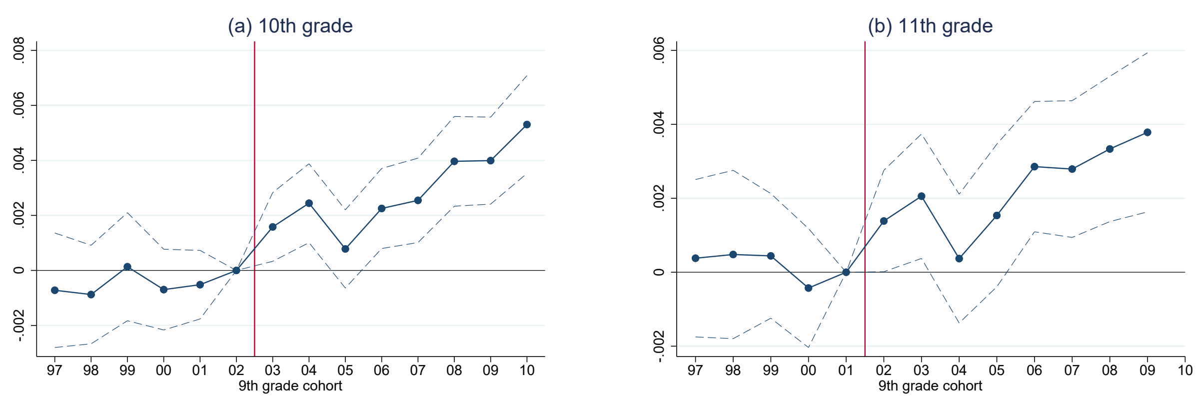
Notes: This table reports difference-in-differences estimates of the effect of affirmative action on grades in a large urban school district. An observation is a student, and the sample consists of repeated cross-sections of 11th graders. "Treated" is the coefficient on the interaction between being a URM and being observed post 2003. Achievement terciles are assigned based on 8th grade average school grades. Standard errors are clustered at the school level.

tion. Appendix Figure A12 plots the event study graph. We see little evidence of pre-trends, and there is an immediate positive effect of affirmative action on URMs' test scores at the time of the policy change. Appendix Table A4 reports the point estimates. On average, Stanford test scores increase by 4.78 percentiles for URMs relative to whites (equivalent to 0.2 sd). The effect is largest for the top tercile, which has gains of 7.47 percentiles (0.3 sd).

5.5 Impact of Affirmative Action on Attendance

Having shown that grades and test scores increase as a result of affirmative action, we now consider a more direct measure of student effort. Returning to the TEA data, we test whether affirmative action affects URM students' attendance. Our empirical strategy is identical for the strategy estimating affirmative action's effects on college applications (see equation (7)).

Figure 6: Effect of AA on Attendance for URMs Relative to Whites



Notes: The outcomes are attendance rates in grades 10 and 11. Dots indicate coefficients from a regression of the outcome on year dummies interacted with URM status. All regressions condition on cohort-test score quintile, race-test score quintile and district-test score quintile fixed effects, where test score quintiles are from the cohort-specific distribution of 6^{th} grade standard test scores. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

Figure 6 reports the event study plots for attendance in 10th and 11th grade. For these outcomes, because our data is organized in cohort-time, the first treated cohort for 10th grade attendance is the 2003 cohort, and the first treated cohort for 11th grade attendance is the 2002 cohort. Reassuringly, the timing of increases in attendance rates is consistent with a positive treatment effect at the time affirmative action was re-instituted rather than simple differences in attendance rates across cohorts. Attendance rates for the 2002 cohort of 9th graders are greater than for the 2001 cohort in 11th grade but not in 10th grade.

Overall, the plots show no discernible pre-trend and suggest that there was a positive effect on attendance.

Table 4: Effect of AA on School Attendance for URMs Relative to Whites

		Percentile of 6th grade test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Attendance in grade 10					
Treated	0.0022***	0.0036***	0.0002	0.0023***	0.0026***	0.0025***
	(0.0005)	(0.0013)	(0.0009)	(0.0008)	(0.0005)	(0.0007)
Observations (cells)	68480	12910	14515	14805	14143	12107
R^2	0.761	0.628	0.617	0.595	0.604	0.635
Mean dependent variable	0.9460	0.9222	0.9377	0.9477	0.9562	0.9655
Test: quintile q = top quintile						
p-value		0.4473	0.0182	0.8013	0.9239	
	Panel B: Attendance in grade 11					
Treated	0.0015**	0.0018	-0.0000	0.0014	0.0014**	0.0034***
	(0.0006)	(0.0015)	(0.0010)	(0.0010)	(0.0007)	(0.0006)
Observations (cells)	68431	12865	14509	14807	14143	12107
R^2	0.715	0.574	0.580	0.592	0.611	0.648
Mean dependent variable	0.9402	0.9190	0.9316	0.9406	0.9492	0.9598
Test: quintile q = top quintile						
p-value		0.2731	0.0020	0.0392	0.0089	
Demographic controls	X	X	X	X	X	X
District-cohort-test score FE	X	X	X	X	X	X
District-ethnicity-test score FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on URMs' school attendance. The regressions use the TEA data, an observation is at the district-cohort-race-test score quintile level, where test score quintile is assigned based on 6th grade (pre-AA) test scores on the state standardized test. Cells are weighted by the number of student-years in a cell. The reported coefficient is the coefficient on the interaction between an indicator for being a URM and an indicator variable for being observed after 2003. The outcome variables in Panels A and B are the average percent of days students in a cell attended school in 10th and 11th grade respectively. Standard errors are clustered at the district-level.

Table 4 reports the associated regression results for 10th and 11th grade attendance. Difference-in-differences estimates indicate a positive average effect on the fraction of days present of 0.22 percentage points in 10th grade (panel A) and of 0.15 percentage points in 11th grade (panel B). The latter effect is equivalent to 8% of the pre-AA racial gap in attendance rates.[32] While the effects on attendance occur throughout the distribution in

[32]Results are quantitatively similar, but considerably less precise, in the LUSD.

10th grade, for 11th grade they are again concentrated in the top part of the distribution.

5.6 Affirmative Action and College Completion

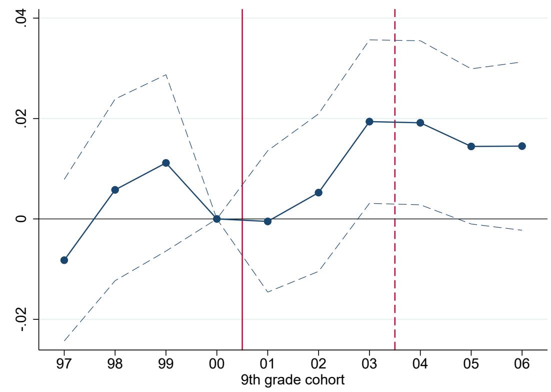
Thusfar, our analyses have documented the positive effects of affirmative action on URMs' college applications and human capital prior to reaching college. In this subsection, we estimate the effect of affirmative action on the probability of completing a college degree using administrative data from the TEA.

In Section 5.2, we showed that more URM students applied to college as a result of the reinstatement of affirmative action. However, this need not result in an increase in the fraction of URM students who obtain a post-secondary degree. On the one hand, if marginal students are now matched to colleges for which they are not prepared, they may be less likely to complete their degrees than they would have been absent affirmative action. This is the mismatch argument of Sander (2004). Then, affirmative action might reduce the fraction of degree holders among URMs. On the other hand, if increased effort in high school contributes to the accumulation of human capital, the probability of completing a 4-year college degree may increase. Additionally, if students are matched to better schools that have higher returns to education, incentivizing students to graduate, or that are more able to ensure students graduate, graduation rates may increase. To measure the direction of the effect of affirmative action on college graduation, we employ the same empirical strategy that we used in the TEA data to measure college application behavior (see equation (7)).

Figure 7 is an event study plot of the effect of the reinstatement of affirmative action on the relative probability of top-quintile URMs completing a 4-year college degree. For these high test score students, the relative probability of graduating college appears to increase post-policy change. Graduation rates vary noisily around zero for cohorts that were never treated (i.e. who would have started college prior to the court ruling), appear to start increasing with cohorts that were partially treated (i.e. who were in 9^{th} grade between 2001 and 2003), and stabilize at higher values for cohorts who started high school post 2003. This pattern is consistent with increased pre-college human capital: cohorts who had more time to adjust their human capital investment experience larger increases in college graduation.

Table 5 reports the associated difference-in-differences estimates. Pooling all students together (column (1)), we find no effect of affirmative action on students who had little opportunity to adjust their level of effort in high school (the partially treated cohorts). For fully treated cohorts, the probability of graduating increases by 0.46 percentage points (3%). We find no significant evidence of gains for partially treated cohorts for any of the quintiles,

Figure 7: Effect of AA On College Graduation for Top Quintile URMs Relative to Whites



Notes: The outcome is college graduation. Dots indicate coefficients from a regression of the outcome on year dummies interacted with URM status. The sample consists of students in the top quintile of the 6th grade test score distribution. All regressions condition on cohort, race and district fixed effects, as well as means of individual characteristics. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

though the estimate is positive for top quintile students. For fully treated cohorts, the point estimates are positive throughout the test score distribution, but positive effects are concentrated in the top quintile. Fully treated top-quintile URMs are 1.4 percentage points (4%) more likely to complete college.

Taking all the results in this section together, high-achieving URM students increased their effort in high school as measured by attendance, increased their pre-college human capital as measured by school grades, increased the number of applications they sent to selective institutions, and became more likely to graduate from college. For college graduation, any decrease in match-quality that may have resulted from the reinstatement of affirmative action was more than made up for by positive effects on effort, application rates, and college quality.

6 Suggestive Evidence on Mechanisms

So far, we have provided evidence that affirmative action narrowed the achievement gap between URMs and whites for an array of outcomes. A natural next question is what channels led to these effects. One possibility that is consistent with both the evidence presented in the previous section and the effects on attendance is that high school students changed their behavior in direct response to perceived changes in their likelihood of college admission. Still,

Table 5: Effect of AA on College Completion for URMs Relative to Whites

		Percentile of 6th grade test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
Partial treatment	-0.0009	-0.0011	-0.0011	-0.0055	-0.0022	0.0098
	(0.0022)	(0.0018)	(0.0030)	(0.0036)	(0.0037)	(0.0063)
Full treatment	0.0046*	0.0006	0.0023	0.0033	0.0054	0.0141**
	(0.0025)	(0.0023)	(0.0031)	(0.0041)	(0.0049)	(0.0071)
Observations (cells)	68509	12933	14515	14809	14145	12107
R^2	0.890	0.556	0.640	0.690	0.708	0.707
Mean dependent variable	0.1688	0.0202	0.0695	0.1415	0.2398	0.3714
Test: quintile q = top quintile						
Partial treatment p-value		0.0955	0.0902	0.0214	0.0568	
Full treatment p-value		0.0592	0.1295	0.1443	0.2122	
Demographic controls	X	X	X	X	X	X
District-cohort-test score FE	X	X	X	X	X	X
District-ethnicity-test score FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on URMs' college graduation. The regressions use the TEA data, and an observation is at the district-cohort-race-test score quintile level. The test score quintile is assigned based on 6th grade (pre-AA) test scores on the state standardized test. Cells are weighted by the number of student-years in a cell. Partial treatment is the coefficient on the interaction between an indicator for being a URM and an indicator variable for entering high school after 2001 and before 2003. Full treatment is the coefficient on the interaction between entering high school after 2003 and being a URM. The outcome variable is the fraction of students in a cell who completed college. Standard errors are clustered at the district-level.

teachers, guidance counselors, and parents may have also changed their behavior if affirmative action change their perceived returns to investing in/providing attention to URM students. To provide suggestive evidence on the drivers of URM students' improved outcomes, we analyze students' responses from the THEOP survey.

Table 6: Student and Parent Behavior and Affirmative Action

	(1) Time on Homework	(2) Applied to First Choice College	(3) Parental Involvement	(4) Guidance From Counselor
$URM \times Post2003$	5.452**	0.048**	0.176	-0.025
	(2.496)	(0.023)	(0.166)	(0.018)
Mean Whites Pre-2003	51.585	0.732	10.635	0.614
N	13,452	9,993	13,558	13,699
Adjusted R^2	0.061	0.024	0.038	0.026
Race Fixed Effects	X	X	X	X
Year control	X	X	X	X

Notes: This table reports differences-in-differences analyses using survey data from two cohorts, both in their senior year, of the Texas Higher Education Opportunity Project (THEOP). The earlier cohort was surveyed in 2002, and the later cohort was surveyed in 2004. For the measure of how many minutes per day students spend on homework, students were asked how many hours per day they spent on their homework and were given the options zero hours, less than 1 hour, 1 to 2 hours, 3 to 4 hours, and 5+ hours. We convert these to minutes so that 0 hours is 0 minutes, less than 1 hour is 30 minutes, 1 to 2 hours is 90 minutes, and so on. The parental involvement index is constructed using questions that ask "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" in a way that a higher index corresponds to more involvement along these dimensions. Standard errors are heteroskedasticity robust.

As mentioned previously, the THEOP survey asked two cross-sections of high school seniors across Texas about their demographics, college applications behavior, and high school activities in 2002 (pre-affirmative action) and then again in 2004 (post-affirmative action). While the two waves of the survey are not identical, the questions that are consistent across waves allow us to measure student effort in terms of time spent on homework, as well as parental involvement, and guidance counselor involvement. For each outcome, we run the following regression, which closely mirrors our difference-in-differences strategies in the TEA and LUSD data:[33]

$$y_{iet} = \beta_1 Post2003_i + \beta_2 \left(URM_i \times Post2003_t \right) + \alpha_e + \varepsilon_{iet}, \tag{9}$$

where i denotes an individual, e denotes an ethnicity, and t denotes a survey round. $Post2003_i$ is an indicator variable equal to 1 for seniors surveyed in 2004, while α_e is an ethnicity fixed effect. This regression compares the change in outcomes between URM and white seniors from 2002 to 2004.

Table 6 reports the results. After the implementation of affirmative action, URM high school seniors spend 5 (10%) minutes more on homework a day relative to white students. They are also 5 percentage points more likely to apply to their first choice college compared to whites, consistent with our findings in the TEA data. However, we do not see any changes in parental involvement or the likelihood of discussing college applications with guidance counselors. Overall, students appear to directly respond to affirmative action by changing their behavior.

7 Alternative Policies

This section discusses whether two alternative educational policies that were enacted in the early 2000s (No Child Left Behind and charter schooling) can explain our findings.

No Child Left Behind. One threat to the validity of our findings is that a major national educational policy, No Child Left Behind (NCLB), was signed into law in 2002. NCLB may have also differentially affected URM students' outcomes, confounding our estimates. We believe that this is unlikely to be the case for several reasons. First, as documented by Dee and Jacob (2011) and Deming et al. (2016), Texas has had high-stakes school accountability policies since 1993. These policies, which were adopted under Governor George Bush, served as the later basis for the NCLB policies enacted when Bush was president (Deming et al., 2016). Second, our SAT results exploit geographic variation in the reinstatement of affirmative action policies. Since NCLB was a national law, we do not expect it to differentially positively affect URMs specifically in Texas, Louisiana, and Mississippi.[34] Third, we find

[33]In this analysis, we cannot include campus fixed effects because we do not observe the campus to which the student belongs.

[34]If anything, given that Texas should be *less* affected by NCLB due to its pre-existing policies, we should expect our estimates of the change in SAT scores for URMs in Texas, Louisiana and Mississippi will be under-estimates due to NCLB.

that affirmative action had the largest effects on high-achieving students who would have been on the margin of college admissions. In contrast, NCLB incentivized schools to ensure students passed relatively low proficiency cut-offs. Neal and Schanzenbach (2010) show that NCLB and similar policies increased test scores in the middle of the test score distribution. Thus, the distribution of effects we estimate is inconsistent with NCLB's incentive system and with past estimates of the effects of the NCLB program.

Charter School Expansion. In the early 2000s, charter schools expanded rapidly in Texas. Since these charter schools typically serve disadvantaged populations, they may have also differentially affected URMs' outcomes. We believe this expansion is unlikely to drive our results since, at the time of the policy change (2003-2004), charter school enrollment made up only 1% of total enrollment in Texas (Texas Education Agency, 2004).[35] Nonetheless, in Appendix Table A5, we show that the college applications results are robust to omitting Houston and Dallas, the two areas with the largest number of students enrolled in charter schools today.

8 Conclusion

We study the effects of a 2003 U.S. Supreme Court ruling that effectively reinstated race-based affirmative action policies in public universities in Texas, Louisiana, and Mississippi. We find that the policy increased applications to selective universities, high school attendance, and college graduation by URMs in Texas. The policy also reduced the racial achievement gap in math SAT scores by 5% and grades (in a large, Texan school district) by 18%.

These effects are concentrated among students in the top half of the test score distribution, who also experienced the greatest change in the gains of moving up a decile in the test score distribution in terms of admissions to selective universities. Consistent with students responding to increased returns to pre-college human capital, the students whose returns to human capital investment increased the most are also those whose pre-college human capital investment was most affected by the policy.

Altogether, given the positive effects on attendance, the distribution of the treatment effects, and the evidence from the survey data, our findings suggest that URM students respond to the affirmative action policy by changing their college aspirations and adjust their effort accordingly. We speculate that these results are consistent with work by Hoxby

[35] In 2003-2004, there were 60,833 students in charter schools in Texas and 4,328,028 enrolled overall (Texas Education Agency, 2004).

and Avery (2012) and Hoxby and Turner (2013), which shows that qualified, disadvantaged students are less likely to apply to highly selective four-year institutions. If affirmative action leads URM students to perceive admission to a selective school as more attainable or colleges as more welcoming, they may change their behavior.

Finally, our estimates suggest that policy debates that ignore the pre-college incentive effects of affirmative action policies ignore a significant benefit of these policies. Affirmative action policies do not merely affect admissions among a given pool of applicants; they affect the composition of the pool of university applicants itself. Given the importance of the racial achievement gap for determining gaps in long-term outcomes, reductions in the achievement gap may translate into substantial changes in welfare and inequality later in life.

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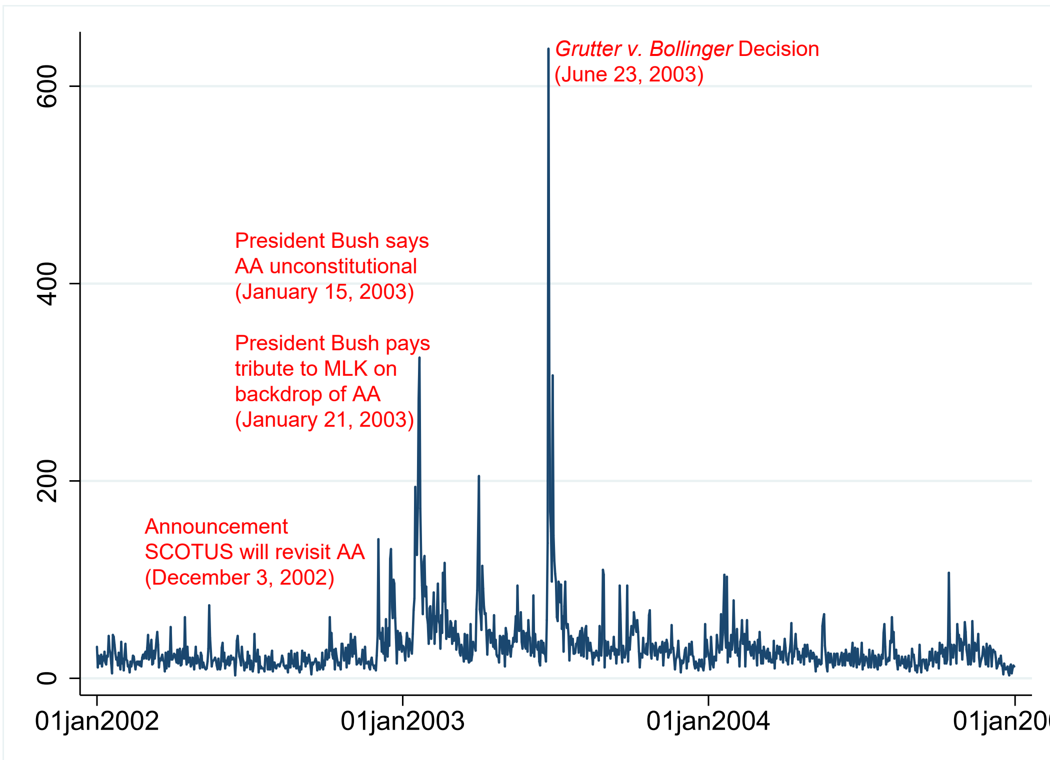
Appendix for Online Publication

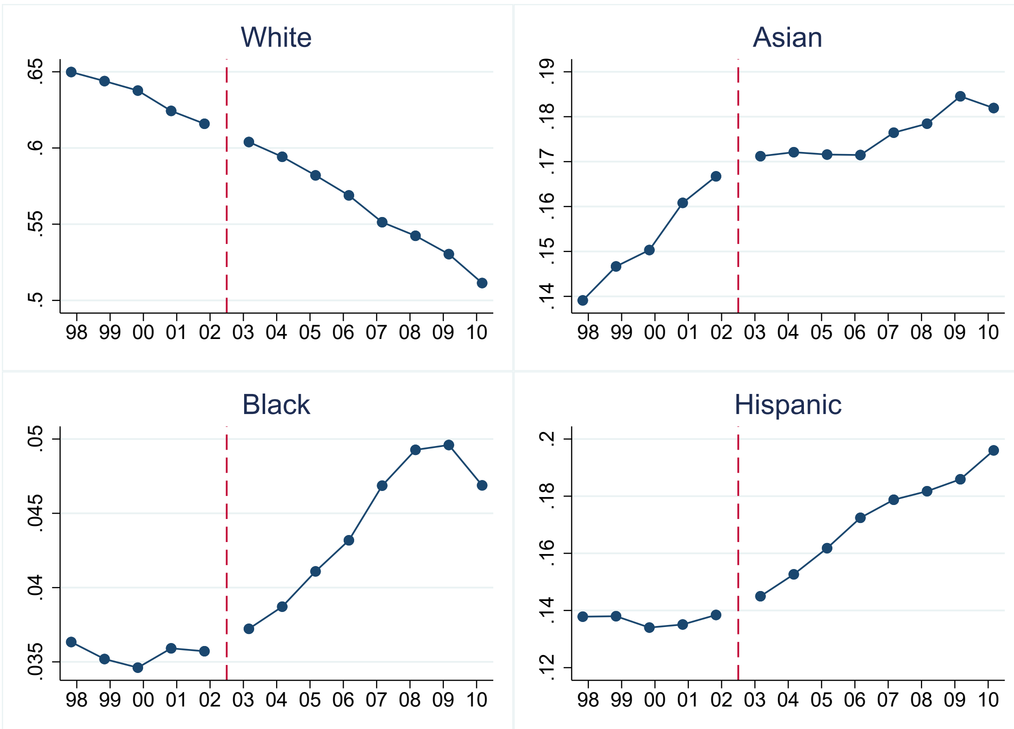
Appendix Figures

Figure A1: Number of Articles Mentioning Affirmative Action by Day, 2002-2004



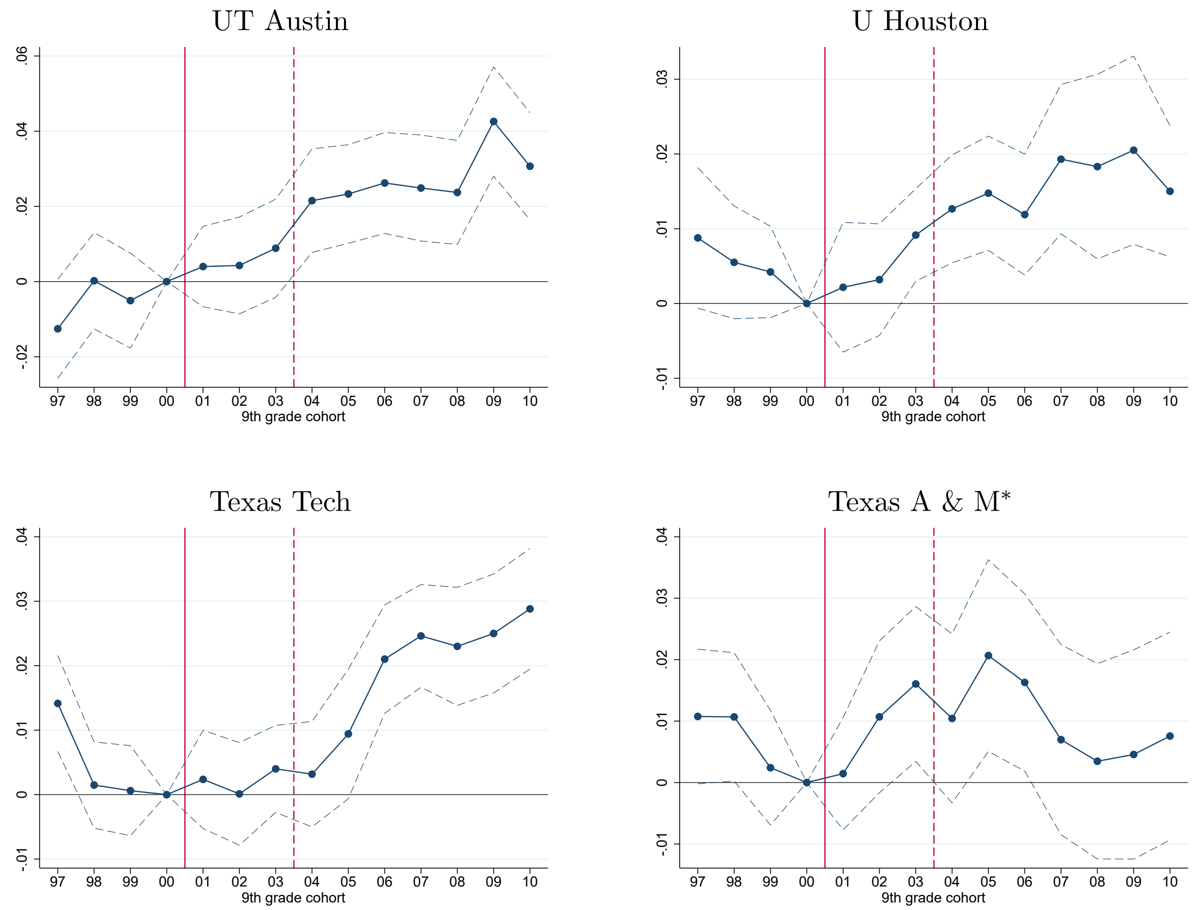
Notes: This figure reports the number of US newspaper articles by day that contained the phrase “affirmative action” on newslibrary.com.

Figure A2: Racial Composition of UT Austin by Year



Notes: This figure reports the racial composition of UT Austin's fall enrollment by year using data from the Integrated Postsecondary Education Data System (IPEDS).

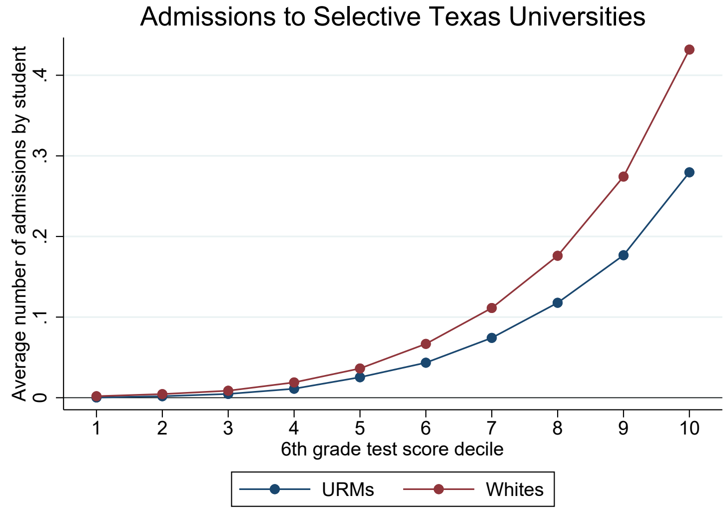
Figure A3: Effect of AA on Admissions to Selective Institutions for URMs Relative to Whites



Notes: This figure reports event study graphs for the probability of a URM student receiving admission to each institution relative to a white student within 4 years of starting 9^{th} grade. The regressions use the TEA data. The sample is restricted to students in the top quintile of the test score distribution where quintiles are measured using the of cohort-specific distribution of 6^{th} grade standard test scores. Dotted lines report 95% confidence intervals with standard errors clustered at the district level.

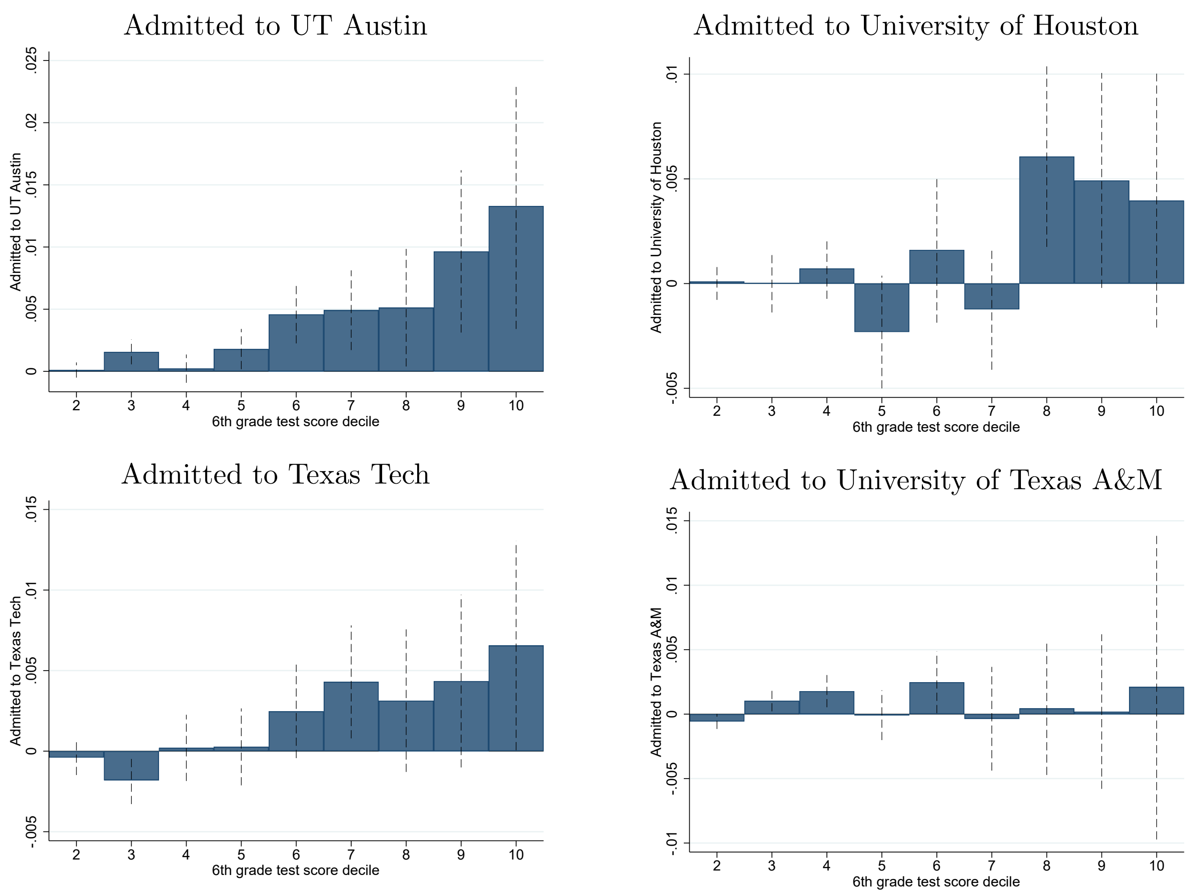
*Texas A & M publicly announced that it would *not* use race (Parker, 2018).

Figure A4: Pre-AA Admissions to Selective Institutions for URMs and Whites, by Test Score Decile



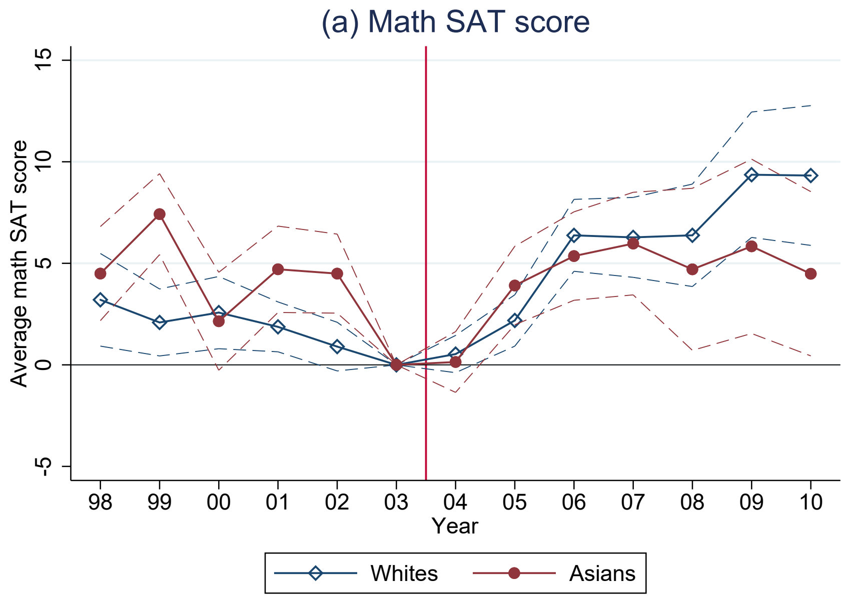
Notes: This figure reports the average number of admissions to selective Texan public universities for the pre-AA, 1997-2000 cohorts by decile of the 6^{th} grade test score distribution. The statistics are calculated separately for URM and white students.

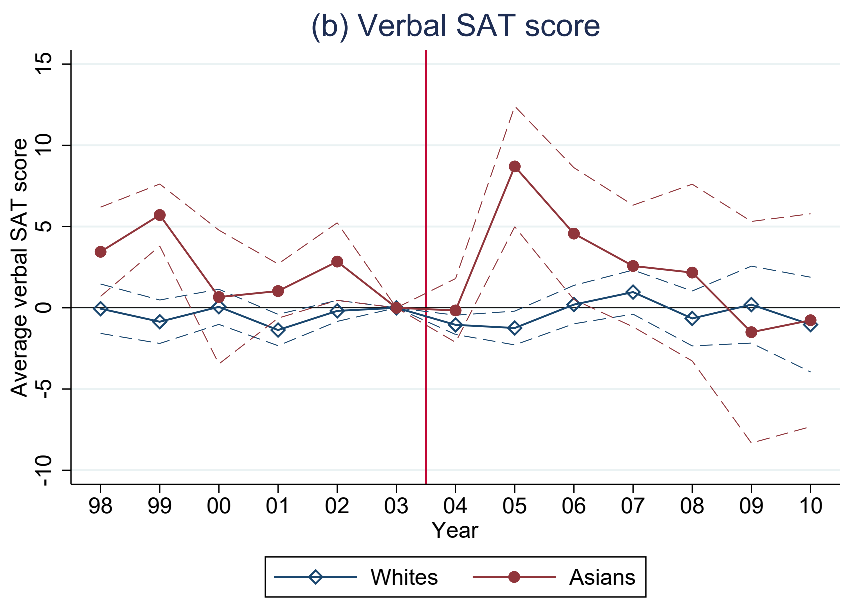
Figure A5: Change in Relative Returns to Moving Up a Test Score Decile on Admissions to Specific Texas Universities



Notes: The outcomes are admission to each of 4 selective Texas universities. Bars indicate the coefficients from equation (2), which capture the change in the marginal effect of moving up a test score decile on college admissions for URMs relative to whites. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

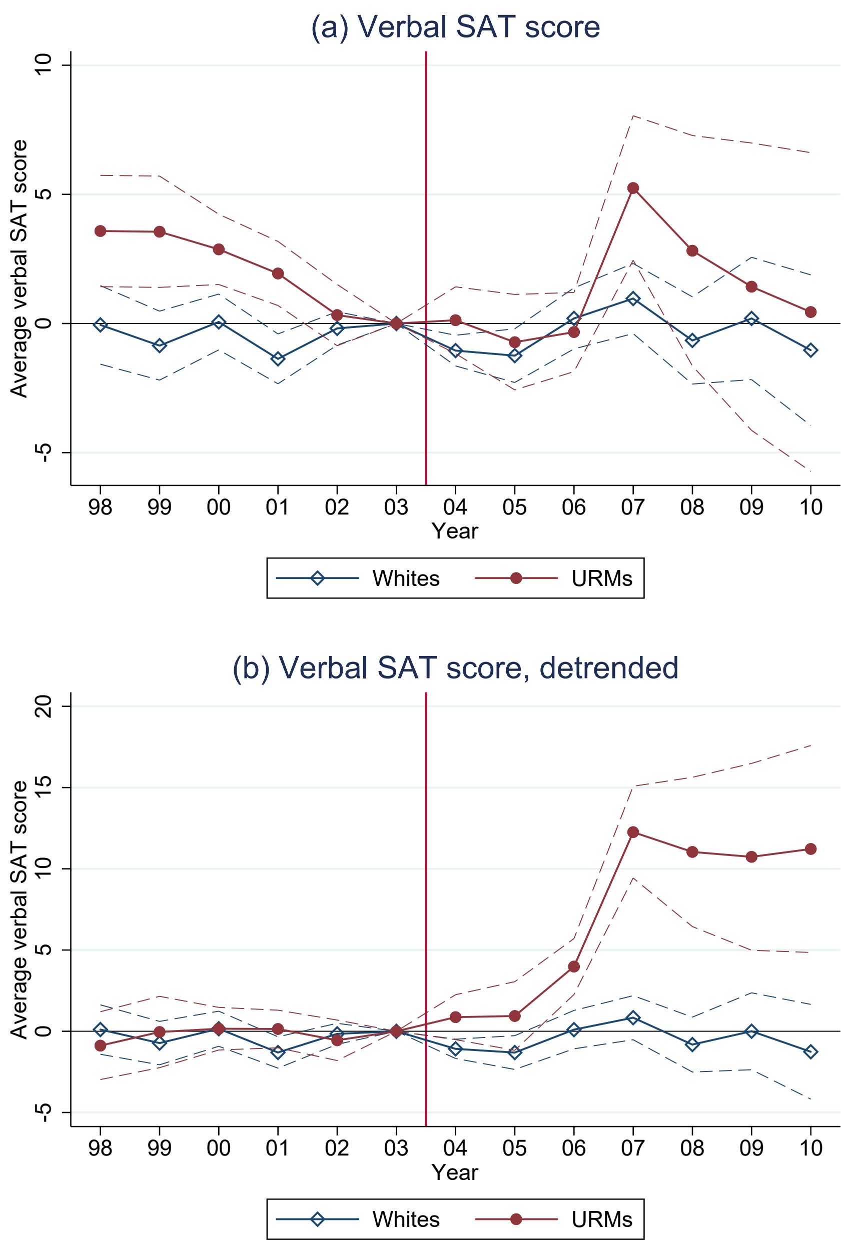
Figure A6: Effect of AA on SAT Scores for Asians and Whites





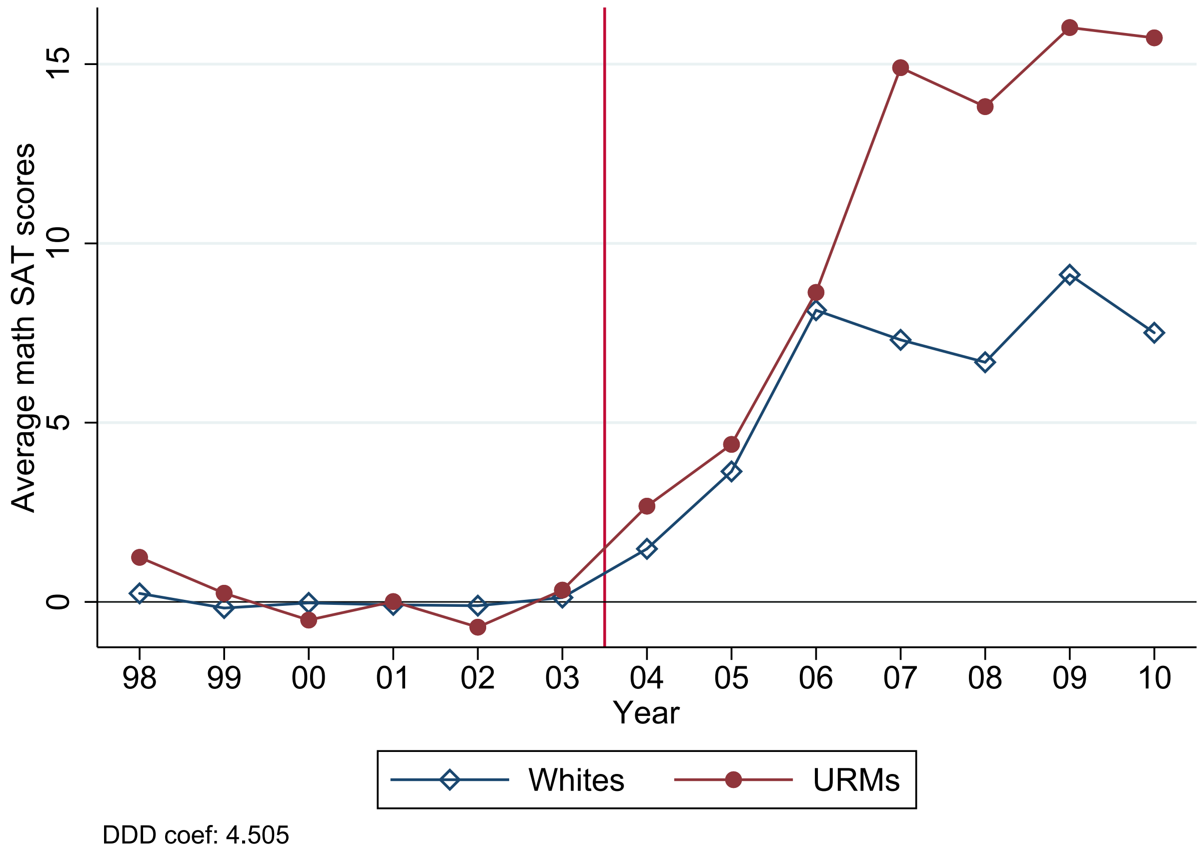
Notes: The outcomes are state-year average SAT math and verbal scores. Dots indicate coefficients from a regression of the outcome on year indicator variables interacted with an indicator variable for the three treated states, estimated separately for Asian and white students. Cells are weighted by the number of SAT test takers. Dashed lines show 95% confidence intervals for standard errors clustered at the state-level.

Figure A7: Effect of AA on Verbal SAT Scores



Notes: To construct this figure, we first estimate linear trends separately for each treatment group (race-by-treatment status) using pre-AA years only (1998 to 2003). We then partial out these linear pre-trends from the full panel. We use these de-trended series as the outcome. Dots indicate coefficients of regressions of the outcome on year dummies interacted with an indicator variable for the three treated states, estimated separately for white and URM students. Cells are weighted by the number of SAT test takers. Dashed lines show 95% confidence intervals for standard errors clustered at the state-level.

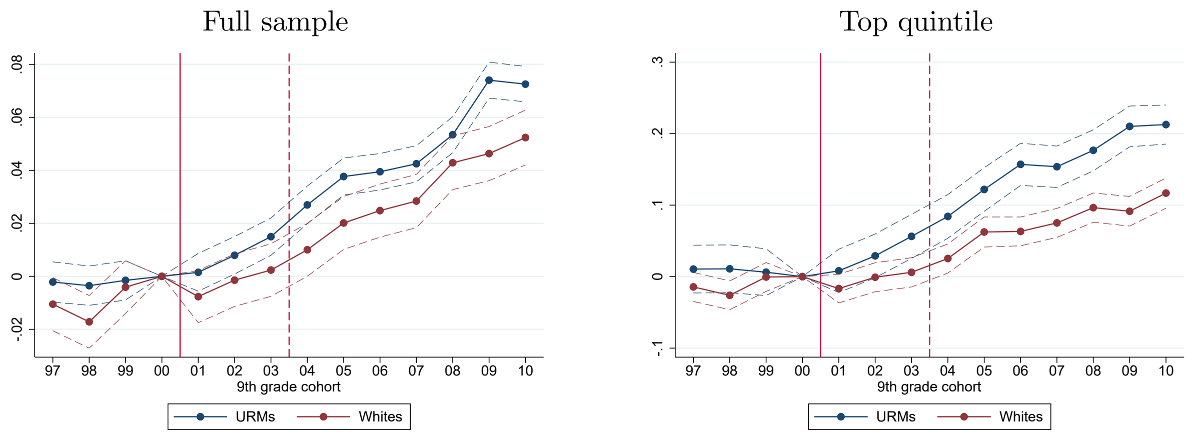
Figure A8: Difference Between Treated and Control States in the Synthetic Control Estimates



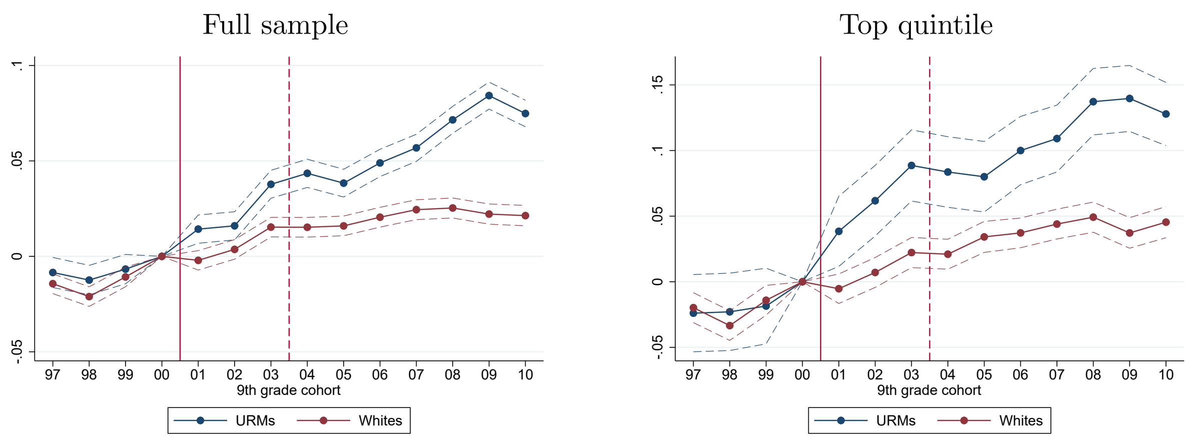
Notes: This figure reports differences in SAT math scores between treated states and synthetic control groups, separately for URMs and white students.

Figure A9: Raw Trends in College Applications by Race

Panel A: Number of Applications to Selective Universities



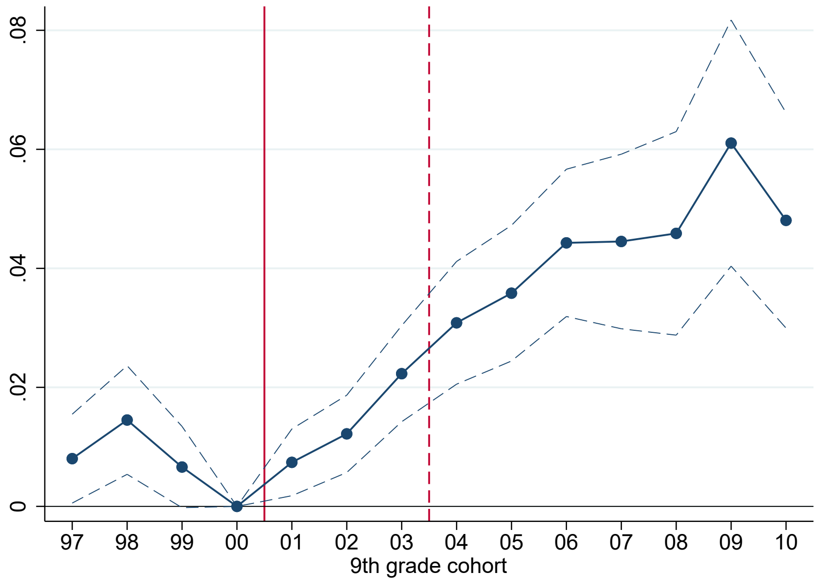
Panel B: Probability of Applying to Any UT Institution



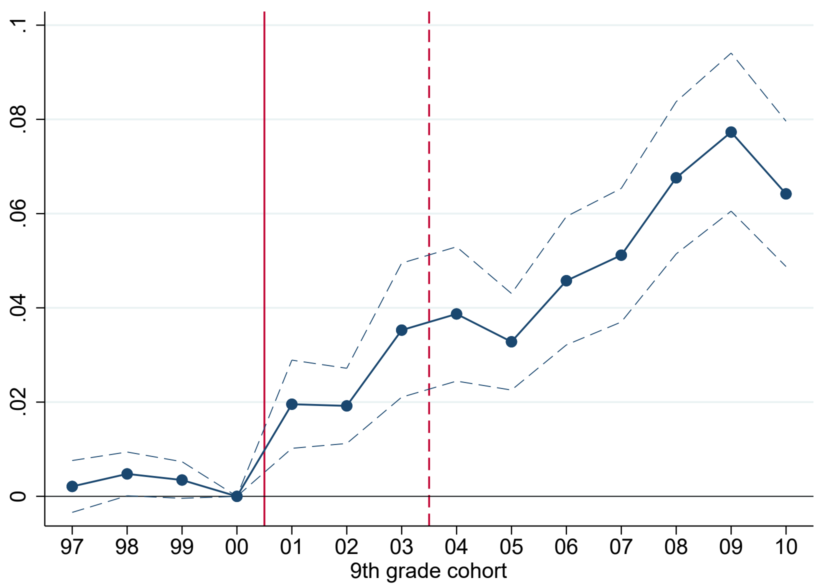
Notes: This figure reports raw average trends in college applications behavior in our analytical sample. Time series are normalized relative to the cohort that entered 9th grade in 2000.

Figure A10: Average Effect of AA on College Applications for URMs Relative to Whites

Panel A: Number of Applications to Selective Universities

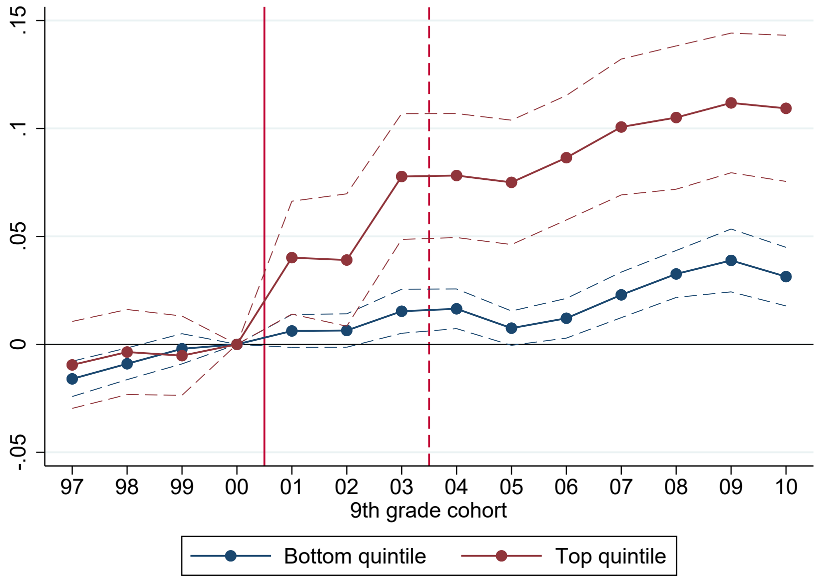


Panel B: Probability of Applying to Any UT Institution



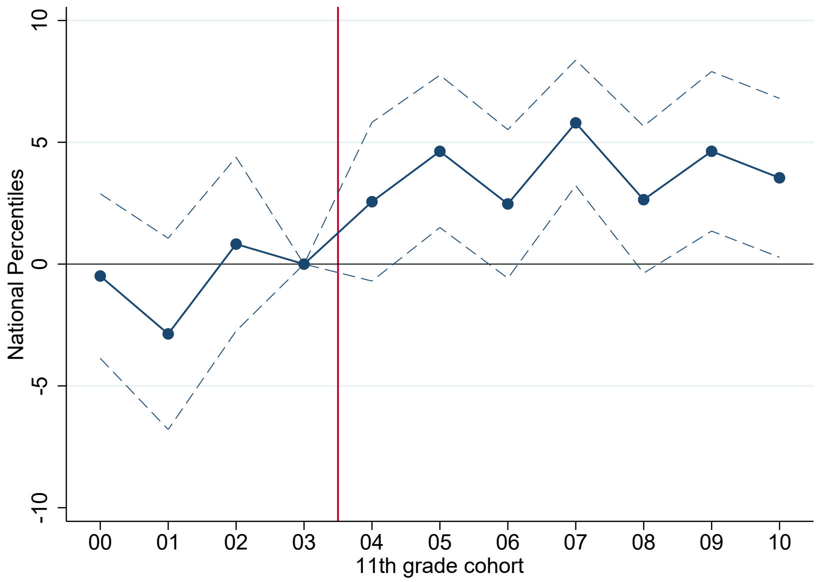
Notes: The outcome is the average number of applications sent to selective universities by students in Panel A and the probability of applying to any institution in the University of Texas System in Panel B. Dots are coefficients from a regression of the outcome on year dummies interacted with URM status. All regressions condition on cohort-test score, race-test score and district-test score fixed effects, as well as means of individual characteristics, where test score quintiles are from the cohort-specific distribution of 6^{th} grade standard test scores. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

Figure A11: Effect of AA on Probability of Applying to Any Texan Public University for URMs Relative to Whites



Notes: The outcome is the probability of applying to any Texan 4-year public university within 4 years of starting 9^{th} grade. Dots are coefficients from a regression of the outcome on year dummies interacted with URM status. All regressions condition on cohort-test score, race-test score and district-test score fixed effects, where where test score quintiles are from the cohort-specific distribution of 6^{th} grade standard test scores. Dashed lines show 95% confidence intervals for standard errors clustered at the district-level.

Figure A12: Effect of AA on Mean Stanford Scores for URMs Relative to Whites



Notes: The outcome is the mean percentile rank on the Stanford test in 11^{th} grade, where the mean is taken across 5 subjects (reading, math, language, science, social science). Dots indicate coefficients from regressions of the outcome on year dummies interacted with an indicator variable for URM status. The regressions also include school-cohort, race, and ZIP code fixed effects, as well as controls for age and gender. Dashed lines show 95% confidence intervals for standard errors clustered at the school-cohort level.

Appendix Tables

Table A1: Summary Statistics

	URMs		Whites	
SAT Data				
Years	1998-2003	2004-2010	1998-2003	2004-2010
Verbal scores	440.9	441.7	527.7	528.4
Math scores	438.7	443.4	530.1	534.7
Number of cells	878	1,026	306	357
Number of SAT takers	1,194,067	2,159,747	4,136,869	5,634,200
TEA Administrative Data				
Cohorts (grade 9)	1997-2000	2001-2010	1997-2000	2001-2010
Age (grade 9)	14.2684	14.1983	14.1648	14.1412
Limited English Proficiency (LEP)	0.0671	0.0477	0.0004	0.0004
Special Ed status	0.0772	0.0521	0.0784	0.0590
English as a Second Language (ESL)	0.042	0.0379	0.0001	0.0002
Gifted	0.0771	0.0852	0.1583	0.1599
Immigrant	0.0051	0.0014	0.0010	0.0004
Poor	0.6022	0.6628	0.1246	0.1570
Female	0.508	0.5079	0.4988	0.4963
Test score decile (grade 6)	4.3648	4.5541	6.6283	6.6263
Attendance rate (grade 10)	0.9343	0.9405	0.9541	0.9554
Attendance rate (grade 11)	0.9305	0.9335	0.9493	0.9494
Applications to selective universities (within 4 years)	0.0603	0.1012	0.2098	0.2384
UT system application rate (within 4 years)	0.0752	0.1325	0.0969	0.1242
University application rate (within 4 years)	0.1734	0.2583	0.2900	0.3350
College graduation rate	0.1126	0.0969	0.2488	0.2265
District-cohort-test score cells	12,492	36,462	17,414	41,614
Number of students	357,973	1,176,595	405,005	971,850
Number of districts	522	680	803	844
LUSD Administrative Data				
Cohorts (grade 11)	2001-2003	2004-2008	2001-2003	2004-2008
Age (grade 11)	16.3936	16.4087	16.2100	16.2234
Female	0.5377	0.5346	0.5057	0.5202
Mean school grades (grade 11)	77.3440	78.1689	82.2364	83.4534
Mean school grades (grade 8)	82.4995	81.9075	86.6246	86.8627
Attendance rate (grade 11)	0.9286	0.9274	0.9431	0.9482
Stanford test percentile rank (grade 11)	36.1245	49.7647	69.2039	77.8087
Number of students	17,620	34,107	3,623	5,779
Number of schools	42	49	36	42

Notes: This table reports summary statistics (means) from the SAT data, the Texas Education Agency (TEA) administrative data and the administrative data from a large, urban school district (LUSD). An observation in the SAT data is a race-year-state cell. An observation in the TEA data is a district-test score decile-cohort cell. The LUSD data consists of repeated cross-sections of 11th graders, and an observation is a student.

Table A2: Summary Statistics for THEOP Survey Data

Panel A: Summary Statistics

	Full Sample		Whites		URMs	
	Mean	SD	Mean	SD	Mean	SD
Time (Minutes) Spent on Homework	64.54	56.69	56.06	53.60	70.56	56.26
Applied to First Choice College	0.65	0.48	0.70	0.46	0.60	0.49
Parental Involvement Index (0-15)	5.98	3.87	5.94	3.78	6.18	3.96
Discussed College App. w. Counselor	0.67	0.47	0.65	0.48	0.70	0.46

Panel B: Total Numbers

	N
Total Students	13,938
Whites	6,406
URMs	7,532
Students in 2002	11,098
Students in 2004	2,840

Notes: This table reports summary statistics for the Texas Higher Education Opportunity Project (THEOP) survey data for two cohorts of seniors, one in 2002 and one in 2004. For the measure of how many minutes per day students spend on homework, students were asked how many hours per day they spent on their homework and were given the options zero hours, less than 1 hour, 1 to 2 hours, 3 to 4 hours, and 5+ hours. We convert these to minutes so that 0 hours is 0 minutes, less than 1 hour is 30 minutes, 1 to 2 hours is 90 minutes, and so on. The parental involvement index is also constructed using several questions that ask "How often do your parents ... (i) give you special privileges because of good grades, (ii) try to make you work harder if you get bad grades, (iii) know when you are having difficulty in school, (iv) help with your school work, and (v) talk with you about problems in school." Students' responses range from "very rarely" (1) to "almost all the time" (4). We sum across the answers to these questions to construct the "parental involvement index" in a way that a higher index corresponds to more involvement along these dimensions and renormalize the measure by subtracting 5 so that the minimum score is 0 rather than 5.

Table A3: Effect of AA on Application to Any Texan Public University for URMs Relative to Whites

		Percentile of 6th grade test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Application to any university					
Partial treatment	0.0082***	0.0085***	0.0047	0.0016	0.0090*	0.0238***
	(0.0026)	(0.0027)	(0.0036)	(0.0044)	(0.0052)	(0.0073)
Full treatment	0.0169***	0.0031	0.0048	0.0125**	0.0254***	0.0438***
	(0.0035)	(0.0030)	(0.0040)	(0.0058)	(0.0059)	(0.0085)
Observations (cells)	68509	12933	14515	14809	14145	12107
R^2	0.915	0.788	0.815	0.810	0.802	0.781
Mean dependent variable	0.2603	0.0659	0.1414	0.2312	0.3499	0.5107
Test: quintile q = top quintile						
Partial treatment p-value		0.0552	0.0141	0.0051	0.0610	
Full treatment p-value		0.0000	0.0000	0.0001	0.0166	
Demographic controls	X	X	X	X	X	X
District-cohort-test score FE	X	X	X	X	X	X
District-ethnicity-test score FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on URMs' college applications behavior. The regressions use the TEA data, an observation is at the district-cohort-race-test score quintile level, where quintiles are assigned based on 6th grade (pre-AA) test scores on the state standardized test. The sample is restricted to students who were in 9th grade between 1997 and 2006. Cells are weighted by the number of student-years in a cell. Partial treatment is the coefficient on the interaction between an indicator for being a URM and an indicator variable for entering high school after 2001 and before 2003. Full treatment is the coefficient on the interaction between entering high school after 2003 and being a URM. The outcome variable is the fraction of students in a cell that applied to any Texan public university. Standard errors are clustered at the district-level.

Table A4: Effect of AA on Stanford Test Scores for URMs Relative to Whites

		Grades in 8th grade		
	All students	Bottom tercile	Middle tercile	Top tercile
	(1)	(2)	(3)	(4)
	Dependent variable: Stanford Test Scores (grade 11)			
Treated	4.7801***	4.2109***	4.6267***	7.3731***
	(1.1352)	(1.2879)	(1.5649)	(1.4315)
Observations	58096	15486	15347	14620
R^2	0.444	0.455	0.487	0.464
Mean dependent variable	49.40	42.24	50.49	59.99
S.D. dependent variable	25.74	23.38	24.00	23.76
Test: tercile q = top tercile				
p-value		0.0981	0.1184	
School-year FE	X	X	X	X
Ethnicity FE	X	X	X	X
Demographic controls	X	X	X	X

Notes: This table reports the difference-in-differences estimates of the effect of affirmative action on mean Stanford test scores (in national percentile ranks) in a large, urban school district. Mean Stanford test scores are the average across 5 subjects (reading, math, language, science, social science). An observation is a student, and the sample consists of repeated cross-sections of 11th graders. "Treated" is the coefficient on the interaction between being a URM and being observed post 2003. Achievement terciles are assigned based on 8th grade average school grades, and the sample is restricted to years 2001 to 2008. Standard errors are clustered at the school-level.

Table A5: Effect of AA on College Applications Behavior – Excluding Houston & Dallas

		Percentile of 6th grade test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Applications to Selective Universities					
Partial treatment	0.0096***	0.0026	0.0010	0.0022	0.0146**	0.0312***
	(0.0028)	(0.0018)	(0.0024)	(0.0037)	(0.0071)	(0.0087)
Full treatment	0.0213***	0.0028*	0.0042	0.0156***	0.0355***	0.0519***
	(0.0032)	(0.0015)	(0.0030)	(0.0050)	(0.0064)	(0.0099)
Observations (cells)	67909	12813	14395	14689	14025	11987
R^2	0.911	0.466	0.625	0.731	0.797	0.836
Mean dependent variable	0.1511	0.0079	0.0321	0.0861	0.1980	0.4169
Test: quintile q = top quintile						
Partial treatment p-value		0.0016	0.0007	0.0020	0.1074	
Full treatment p-value		0.0000	0.0000	0.0003	0.1133	
	Panel B: Application to any UT Institution					
Partial treatment	0.0029	0.0035**	-0.0032	0.0016	0.0021	0.0133**
	(0.0020)	(0.0014)	(0.0024)	(0.0032)	(0.0042)	(0.0061)
Full treatment	0.0088***	0.0007	0.0019	0.0095**	0.0098*	0.0239***
	(0.0027)	(0.0017)	(0.0021)	(0.0040)	(0.0051)	(0.0074)
Observations (cells)	67909	12813	14395	14689	14025	11987
R^2	0.903	0.827	0.871	0.879	0.864	0.846
Mean dependent variable	0.1108	0.0229	0.0537	0.0903	0.1407	0.2372
Test: quintile q = top quintile						
Partial treatment p-value		0.1337	0.0068	0.0753	0.0700	
Full treatment p-value		0.0016	0.0041	0.0376	0.0549	
Demographic controls	X	X	X	X	X	X
District-cohort-test score FE	X	X	X	X	X	X
District-ethnicity-test score FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on URMs' college applications behavior. The regressions use the TEA data, an observation is at the district-cohort-race-test score quintile level, where quintiles are assigned based on 6th grade (pre-AA) test scores on the state standardized test. The sample is restricted to students who were in 9th grade between 1997 and 2006. The sample excludes the Houston Independent School District and the Dallas Independent School District. Cells are weighted by the number of student-years in a cell. Partial treatment is the coefficient on the interaction between an indicator for being a URM and an indicator variable for entering high school after 2001 and before 2003. Full treatment is the coefficient on the interaction between entering high school after 2003 and being a URM. The outcome variable in panel A is the average number of selective universities to which students applied. The outcome variable in Panel B is the fraction of students in a cell that applied to any institution of the University of Texas system. Standard errors are clustered at the district-level.

Appendix A: Robustness of Difference-in-Differences SAT Results

Accounting for AA Bans. Several states implemented affirmative action bans in university admissions during our study period. Washington, Michigan, and Nebraska passed affirmative action bans through ballot initiatives in November 1998, 2006 and 2008, respectively. Governor Jeb Bush issued an executive order banning affirmative action in Florida in November 1999.[36] We do not use these bans to estimate the effects of affirmative action because, as Hinrichs (2012) points out, the effect of affirmative action bans cannot be disentangled from the effect of percent plans; these two policies were almost always enacted concurrently. This would certainly be an issue for estimating the effect of bans in our SAT sample. In this sample, Florida would drive most of the variation in the AA ban indicator because of its large population size and the fact that Michigan and Nebraska are both ACT states and have fewer SAT test-takers. Florida implemented a very aggressive "percent plan" under which students in the top 20% of their high school graduating class were guaranteed admission to a state public university shortly after the ban.

In our main empirical specification, we control for affirmative action bans in control states.[37] In this subsection, we verify that our difference-in-differences estimates of the effect of *Grutter v. Bollinger* on students in Texas, Louisiana and Mississippi are robust to alternative ways of accounting for these affirmative action bans. Robustness tests for math SAT scores are reported in Appendix Table A6 and, for completeness, corresponding results for verbal scores are reported in Appendix Table A7. Column (1) reports results for our baseline specification. The coefficient on the AA ban indicator is statistically insignificant in all three difference-in-differences specifications (panels A to C). In the triple-differences model (panel D), the coefficient on the AA ban indicator interacted with a URM dummy is positive and significant (5.7 points), which suggests that URM students' SAT scores increased relative to white students' in the states that implemented bans on affirmative action. However, as discussed above, we strongly caution against interpreting this coefficient as the causal effect of AA bans. In column (2), we omit the controls for the effect of affirmative action bans. Our estimates of the effect of *Grutter v. Bollinger* on whites (4 points) and on URMs (8 points) are unaffected by the exclusion of an indicator for AA bans as a control variable.

[36]Following our study period, Arizona (2010), New Hampshire (2011) and Oklahoma (2012) also banned affirmative action in college admissions.

[37]The indicator turns on after 1999 in Washington, after 2000 in Florida, after 2007 in Michigan, and after 2009 in Nebraska. It is zero in all years for all others states.

Failing to control for AA bans yields a slightly smaller triple-differences estimate of 4 points. In column (3), we drop the four states that banned affirmative action between 1998 and 2010 from the estimating sample. Again, our estimates of the effect of the reinstatement of affirmative action are virtually unchanged.

Accounting for Potential Non-Compliance. There is some evidence that Louisiana and Mississippi may have continued to use race in university admissions to some extent in 1998-2003 despite the *Hopwood v. Texas* ruling due to pre-existing rulings that required them to de-segregate their institutions of higher education (Hinrichs, 2012). Thus, we also drop these two states from the sample and estimate the effects of the policy change on Texas alone relative to the control states in column (4) of Appendix Table. Dropping these two states has little effect on the estimates.

Accounting for Group-Specific Pre-Trends. It is apparent in Figure 2 that Texas, Louisiana and Mississippi were falling behind the rest of the country prior to the reinstatement of AA. To account for these differential pre-trends, we estimate a linear trend term separately for each racial group and treatment group using only the pre-treatment year, and partial out this linear trend from the full panel. We use the resulting de-trended data as our outcome variable in column (5). The point estimates are generally twice as large as they are in our baseline specifications. This suggests that, if anything, our main estimates put a lower bound on the effect of affirmative action on SAT scores.

Table A6: Robustness of Effect of AA on Math SAT Scores to Controlling for AA Bans

	Baseline	No control for AA bans	Drop AA ban states	Drop Mississippi and Louisiana	De-trended data
	(1)	(2)	(3)	(4)	(5)
			Panel A: URMs		
DD coefficient	8.009***	7.998***	8.122***	8.092***	20.02***
	(1.544)	(1.498)	(1.694)	(1.538)	(1.522)
AA Ban Indicator	0.312			0.290	-0.114
	(2.696)			(2.704)	(2.490)
Observations (cells)	1904	1904	1748	1830	1904
R^2	0.844	0.844	0.839	0.844	0.693
State, year and ethnicity FE	X	X	X	X	X
			Panel B: Whites		
DD coefficient	4.048***	4.145***	3.835***	4.455***	7.208***
	(0.984)	(0.995)	(1.021)	(0.849)	(0.984)
AA Ban Indicator	-3.282			-3.279	-3.382
	(2.668)			(2.670)	(2.669)
Observations (cells)	663	663	611	637	663
R^2	0.969	0.968	0.967	0.969	0.968
State, year and ethnicity FE	X	X	X	X	X
			Panel C: Asians		
DD coefficient	0.658	0.658	0.447	0.362	6.659***
	(1.827)	(1.813)	(1.962)	(1.788)	(1.828)
AA Ban Indicator	-0.0191			-0.0268	-0.177
	(6.710)			(6.725)	(6.716)
Observations (cells)	663	663	611	637	663
R^2	0.944	0.944	0.939	0.944	0.939
State, year and ethnicity FE	X	X	X	X	X
			Panel D: Triple-Difference (URMs vs Whites)		
DDD coefficient	4.155***	3.975***	4.253***	4.059***	10.17***
	(0.828)	(0.872)	(0.816)	(0.817)	(1.263)
AA Ban Indicator	5.714***			5.689***	4.979***
$\times$ URM dummy	(1.153)			(1.158)	(1.137)
Observations (cells)	2555	2555	2347	2455	2555
R^2	0.998	0.998	0.998	0.998	0.989
State-year FE	X	X	X	X	X
State-ethnicity FE	X	X	X	X	X
Ethnicity-year FE	X	X	X	X	X

Notes: This table reports difference-in-differences and triple-differences effects of affirmative action on SAT scores. Each observation is a state-race-year group. In all specifications, cells are weighted by the number of test-takers in a group. In Panels A, B and C, the DD coefficient reports the interaction of an indicator variable for belonging to a treated state (Texas, Louisiana, Mississippi) and being tested after *Grutter v. Bollinger* (post 2003). In Panel D, the coefficient is on the interaction between being a URM, being tested post 2003, and belonging to a treated state. Standard errors are clustered at the state-level.

Table A7: Robustness of Effect of AA on Verbal SAT Scores to Controlling for AA Bans

	Baseline	No control for AA bans	Drop AA ban states	Drop Mississippi and Louisiana	De-trended data
	(1)	(2)	(3)	(4)	(5)
			Panel A: URMs		
DD coefficient	-0.634	-0.779	-0.170	-0.649	8.000***
	(1.784)	(1.756)	(1.929)	(1.795)	(1.935)
AA Ban Indicator	4.131*		0	4.112*	3.150
	(2.264)		(.)	(2.269)	(2.412)
Observations (cells)	1901	1901	1745	1828	1901
R^2	0.796	0.795	0.788	0.793	0.717
State, year and ethnicity FE	X	X	X	X	X
			Panel B: Whites		
DD coefficient	0.0342	0.0247	0.000796	-0.0255	-0.179
	(0.888)	(0.878)	(0.955)	(0.888)	(0.888)
AA Ban Indicator	0.321		0	0.332	0.328
	(3.182)		(.)	(3.183)	(3.182)
Observations (cells)	663	663	611	637	663
R^2	0.971	0.971	0.970	0.970	0.971
State, year and ethnicity FE	X	X	X	X	X
			Panel C: Asians		
DD coefficient	-0.176	-0.145	-0.456	-0.426	5.088*
	(2.753)	(2.709)	(2.880)	(2.743)	(2.751)
AA Ban Indicator	-1.408		0	-1.411	-1.546
	(3.531)		(.)	(3.540)	(3.508)
Observations (cells)	663	663	611	637	663
R^2	0.929	0.929	0.925	0.928	0.920
State, year and ethnicity FE	X	X	X	X	X
			Panel D: Triple-Difference (URMs vs Whites)		
DDD coefficient	1.260	1.083	1.440**	1.331*	7.137***
	(0.753)	(0.825)	(0.634)	(0.763)	(1.081)
AA Ban Indicator	5.604***		0	5.575***	4.810***
× URM dummy	(1.478)		(.)	(1.476)	(1.382)
Observations (cells)	2552	2552	2344	2453	2552
R^2	0.998	0.998	0.998	0.998	0.990
State-year FE	X	X	X	X	X
State-ethnicity FE	X	X	X	X	X
Ethnicity-year FE	X	X	X	X	X

Notes: This table reports difference-in-differences and triple-differences effects of affirmative action on SAT scores. Each observation is a state-race-year group. In all specifications, cells are weighted by the number of test-takers in a group. In Panels A, B and C, the DD coefficient reports the interaction of an indicator variable for belonging to a treated state (Texas, Louisiana, Mississippi) and being tested after *Grutter v. Bollinger* (post 2003). In Panel D, the coefficient is on the interaction between being a URM, being tested post 2003, and belonging to a treated state. Standard errors are clustered at the state-level.

Appendix B: Robustness of SAT Results to Alternative Synthetic Control Specifications

In our baseline synthetic control results, we choose the control group by minimizing the mean squared prediction errors in 1998-2003 using the following set of variables as predictors: number of white SAT test takers, number of URM SAT test takers, white math SAT scores, URM math SAT scores, white verbal SAT scores, and URM verbal SAT scores. Each of these variables is averaged over 1998-2000 and over 2001-2003 in the matching process. Here, we verify that our results are robust to using alternative sets of pre-treatment variables and alternative donor pools.

Matching on SAT Taking Rates. Using the actual number of SAT test takers may put too much weight on state size, as opposed to student SAT-taking behavior. Appendix Figure A13 shows results when we substitute SAT taking rates for number of SAT test takers. To calculate SAT taking rates, we divide the number of test takers by the number of 17-19 years olds in each state-by-race-by-year cell, which we obtain from ACS/Census data. Note that these population counts are not available in 1998 and 1999. Hence, we here match on fewer values. Also, population counts at such a disaggregated level are quite volatile, which may introduce measurement error in the process. Nevertheless, the observed patterns under this alternative approach are quite similar to our baseline results.

Matching on Fewer Pre-treatment Cohorts. Here, we verify that our results are robust to using fewer pre-treatment years in the construction of the synthetic control group. Appendix Figure A14 shows time series of math SAT scores for synthetic control groups based on 4, 5 and 6 years of pre-treatment data.[38]

For whites, the results are insensitive to the model's specification, with the 3 synthetic groups tracking each other very closely. Reassuringly, when fewer years of pre-treatment data are used to construct the synthetic control group, the gap between treated and untreated states during pre-treatment years remains very small, even in years that were not used in the construction of the synthetic control group. The states included in the 5-year match synthetic control group are California (46.4%), Florida (39.5%), Indiana (6.1%) and Pennsylvania

[38]When minimizing the RMSPE over 4 years, we average the predictors over 1998-1999 and 2000-2001. When minimizing the RMSPE over 5 years, we average the predictors over 1998-2000 and 2001-2002.

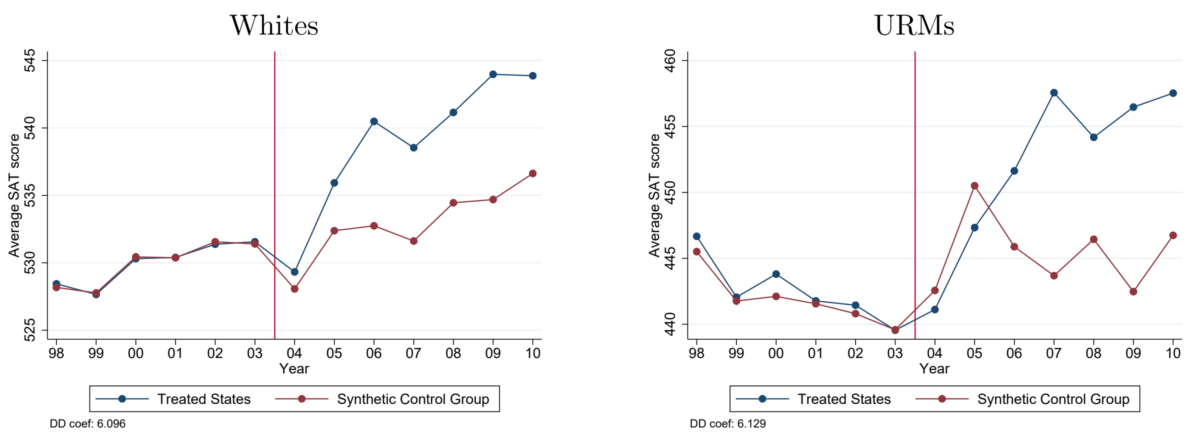
(7.9%). The states included in the 4-year match synthetic control group are California (44.9%), Florida (41.9%), Indiana (10.7%), North Carolina (1.8%) and Pennsylvania (0.6%).

There are fewer URMs than whites taking the SAT in a state in a given year. As a result, yearly mean SAT scores for URMs are more volatile, and the composition of the synthetic control group is more sensitive to the number of pre-treatment years over which the RMSPE is minimized. While the SAT scores of the 6-year match synthetic control group track our treated group very closely for all pre-treatment years, the 4-year and 5-year match groups do not replicate the treated states' negative pre-trend as closely. In both cases, an upward movement in SAT scores of the synthetic group appears around 2002, whereas the treated states are still on a downward trend at that time. By 2007, however, the outcomes of the three synthetic control groups are similar, and in all cases, the control groups' SAT scores are significantly below those of the treated states. The states included in the 5-year match synthetic control group are California (84.3%), Pennsylvania (11.6%) and New Hampshire (4%). The states included in the 4-year match synthetic control group are California (82.4%), West Virginia (14.8%), and Pennsylvania (2.7%).

Excluding States That Banned AA During the Study Period. Next, we drop the four states that banned affirmative action between 1998 and 2010 (Florida, Nebraska, Michigan and Washington) from the donor pool. The results are shown in Appendix Figure A16. In the pre-treatment years, the synthetic control group tracks the treated states fairly closely, albeit with more volatility than under our baseline approach. Overall, our results on the effect of *Grutter v. Bollinger* are not significantly affected by this sample restriction. The states included in the synthetic control group for whites are California (35.1%), Pennsylvania (38.7%), New York (15.2%), Utah (4.5%), New Hampshire (2.8%), Minnesota (2.5%), and Montana (1.1%). The states included in the synthetic control group for URMs are California (70.8%), Pennsylvania (20.1%), and New Hampshire (9.1%).

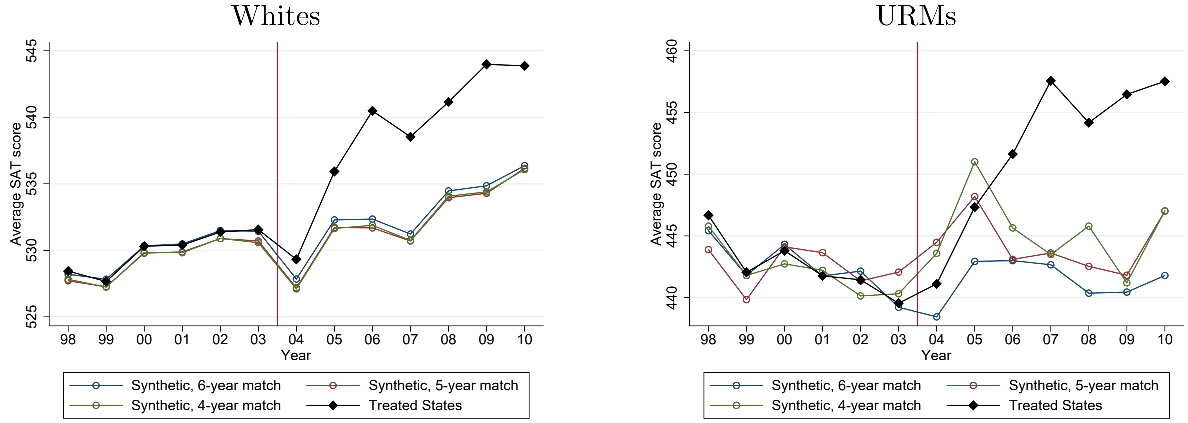
Static Matching. As a final robustness check, we consider an alternative matching algorithm. Here, we use a nearest-neighbor matching approach and match treatment and control states using cross-sectional variation in observable characteristics. First, we extract several average characteristics of 17-54 years olds in each state from the 2000 census, separately by ethnicity (White, Black, Hispanic). We then match treated and control states on the following observable characteristics, all measured for year 2000 and entered separately for the three ethnicities: fraction immigrant, fraction employed, fraction who has moved in the past 5 years, average occupational income score, average age, fraction of the state's population, the SAT taking rate of 17-19 year olds, their average SAT math score, and their average SAT verbal score. We run the matching algorithm twice: once weighting states by their number of white residents, and once weighting states by their number of URM residents. We thereby retrieve matching weights separately for whites and URMs. 11 states have positive weights in the matched control group for whites, with the most weight being put on California (26.3%), Pennsylvania (21.3%), Ohio (20.2%), Missouri (9.4%) and Arizona (5.2%). 8 states have positive weights in the matched control group for URMs, with the most weight being put on California (48.7%), New York (21.2%), Florida (18.2%), Ohio (4.8%) and Arizona (4.2%). The matched control groups do not track the treatment group's pre-trend as closely as the synthetic control groups do, but the results still indicate positive effects of AA on both whites and URMs, with a significantly larger effect on URMs.

Figure A13: Synthetic Control Estimates of the Effect of AA on Math SAT Scores, Matching on SAT Taking Rates



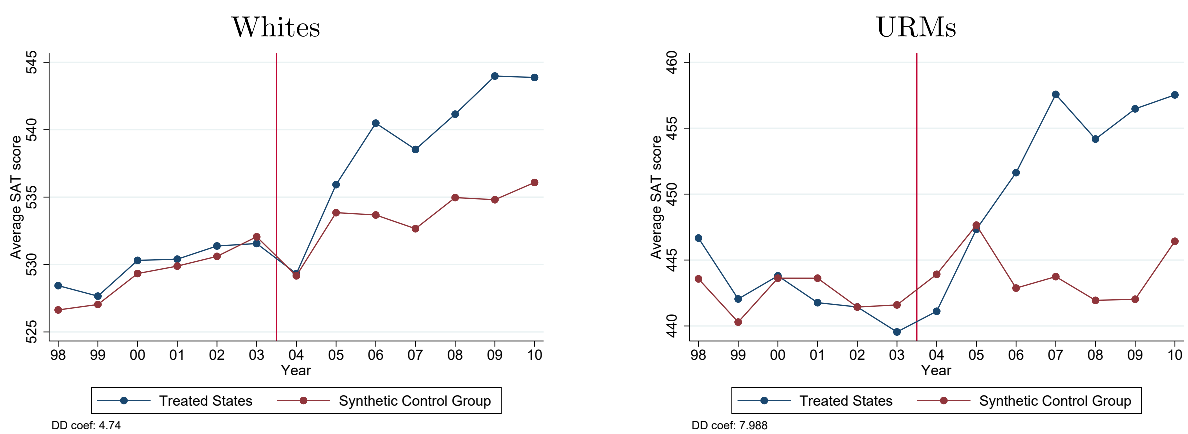
Notes: This figure reports synthetic cohort analyses separately for whites and URMs. It shows SAT math scores for the treated states (Texas, Mississippi and Louisiana) and for the synthetic control group. In constructing the control group, we use pre-treatment estimated SAT taking rates rather than number of test takers as match variables. SAT-taking rates are not available in 1998 and 1999.

Figure A14: Synthetic Control Estimates of the Effect of AA on Math SAT Scores, by Number of Pre-treatment Years Used in Match



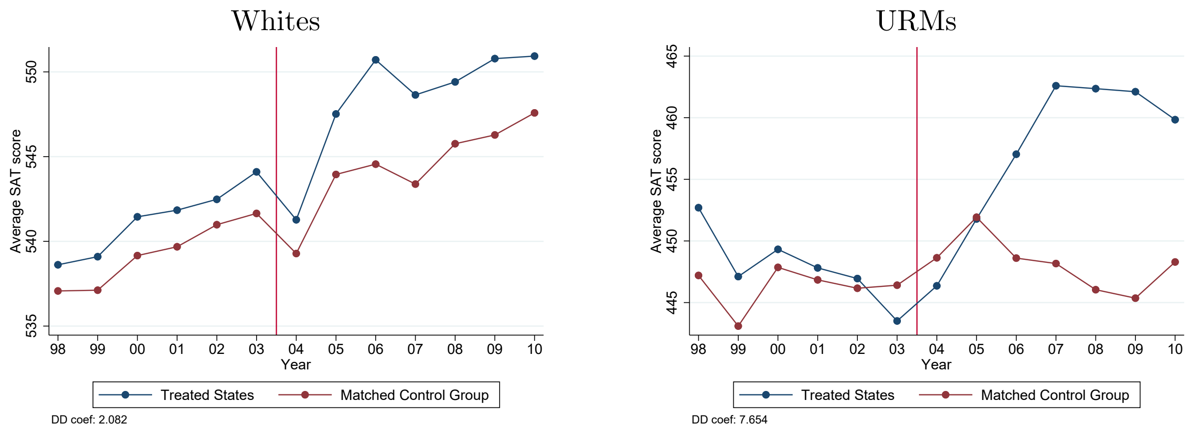
Notes: This figure reports synthetic cohort analyses separately for whites and URMs. It shows SAT math scores for the treated states (Texas, Mississippi and Louisiana) and for the synthetic control group under alternative matching specifications. The control group "Synthetic, 6-year match" is obtained by minimizing the root mean squared prediction error (RMSPE) over the 1998-2003 period. For "Synthetic, 5-year match," the RMSPE is minimized over the 1998-2002 period, and for "Synthetic, 4-year match," it is minimized over the 1998-2001 period.

Figure A15: Synthetic Control Estimates of the Effect of AA on Math SAT Scores, Dropping AA Ban States



Notes: This figure reports synthetic cohort analyses separately for whites and URMs. It shows SAT math scores for the treated states (Texas, Mississippi and Louisiana) and for the synthetic control group. In constructing the control group, Florida, Nebraska, Michigan and Washington were omitted from the donor pool.

Figure A16: Matching Estimates of the Effect of AA on Math SAT Scores



Notes: This figure reports matching analyses separately for whites and URMs. It shows weighted average SAT math scores for the treated states (Texas, Mississippi and Louisiana) and for the matched control group. The control group is constructed by matching on the following observables characteristics measured separately for Whites, Blacks and Hispanics in each state: fraction immigrant (2000 census), fraction employed (2000 census), fraction who moved in pasted 5 years (2000 census), mean occupational income score (2000 census), mean age (2000 census), fraction of the population (2000 census), SAT taking rate in 2000, average SAT math score in 2000 and average SAT verbal score in 2000.