Affirmative Action and Pre-College Human Capital*

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Abstract

Race-based affirmative action policies are widespread in higher education. Despite the prevalence of these policies, there is limited evidence on whether they affect students *before* they reach college. We exploit the 2003 Supreme Court ruling in *Grutter v. Bollinger*, which overturned affirmative action bans in Texas, Louisiana, and Mississippi, but not in other states, to study the effect of affirmative action on high school students' outcomes. We analyze four data sets, including nationwide SAT data and administrative data for the entire state of Texas. The nation-wide data allow us to use state and time variation for difference-in-differences and synthetic control group analyses. Within Texas, variation in race, time, and ex ante ability further help us to isolate the effects of the policy change on college applications, grades, and attendance. Across data sets, outcomes, and identification strategies, the results all point toward reductions in racial achievement gaps. These gains were concentrated among students in the top of the ability distribution, who also experienced the largest increases in the returns to effort due to the policy change. This suggests that students increased their human capital investment in response to increases in the returns to effort. Thus, affirmative action appears to indirectly improve minority students' pre-college outcomes.

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1 Introduction

Affirmative action policies that weigh race or ethnicity as one factor in the college admissions process are widespread in higher education in numerous countries, including the United States, Canada, Brazil, and India. In the United States, affirmative action policies in public universities have repeatedly been challenged by court cases at the sub-national and national level,[1] and eight states have banned race-based affirmative action at all public universities. Despite the importance of race-based affirmative action policies and the controversy surrounding them, relatively little is known about whether or how affirmative action policies affect students *prior* to reaching college. Yet, the pre-college racial achievement gap is an important outcome in its own right as much of the racial wage gap can be traced to differences in cognitive skills gained in school (Neal and Johnson, 1996). Closing racial achievement gaps in secondary school may reduce 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 the college admissions process have ambiguous average effects on human capital attainment prior to college entry. On the one hand, for very high ability students, affirmative action policies may reduce the returns to pre-college human capital by lowering the threshold for college admissions (Coate and Loury, 1993). In this case, affirmative action may disincentivize human capital investments by these students (or their teachers and parents). On the other hand, affirmative action policies may incentivize higher pre-college human capital attainment for minority students – particularly those who are on the margin – by increasing the probability that increased human capital will translate into college admission (Fryer and Loury, 2005). Moreover, even if affirmative action doesn't directly affect students' perceptions of the likelihood of being admitted, by increasing the number of URMs students observe being admitted, it may increase aspirations or the perception that selective schools are welcoming to minority students, ultimately increasing human capital investment. Since the theoretical effects of affirmative action are ambiguous and may also depend on where students are in the ability distribution, we seek to empirically estimate the effects of affirmative action on both the average student and on students in different parts of the distribution.

[1] 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, and *Fisher v. University of Texas* in 2016.

To investigate the effects of affirmative action[2] on the human capital investment 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 a race-conscious admissions process that does not amount to a quota system is constitutional. This effectively reversed a 1996 lower court ruling in *Hopwood v. Texas* that had prohibited the use of race in the admissions process 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 main identification strategies. In cases where we have administrative data for all or part of Texas, we use a difference-in-differences strategy that compares the change in minority (black and Hispanic) and white students' outcomes following the policy.[3] This strategy can be interpreted as estimating the effects of affirmative action on the racial achievement gap. In cases where we have data across multiple states, we separately compare the change in minorities' and whites' outcomes in states that were and were not affected by the policy. This second strategy allows us to identify potential spillover effects on whites. Finally, we use a third triple-differences strategy and interact cohort, geographic, and racial variation. This strategy estimates the 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 minorities.

Using administrative data from all of Texas, we first investigate the effects of the policy change on minorities' college application behavior. This provides us with a "first stage" to test whether students (or those around them) are aware of and respond to the change in affirmative action policies. We find that minorities' college applications increase relative to whites' following the policy change. The effects are larger for higher ability minority students' applications to selective Texas universities. These are exactly the students who are on the margin of admission to selective Texas public universities and therefore, likely to be most informed about and affected by affirmative action. Event study graphs suggest that this result is not driven by pre-trends in minority students' application behavior.

We then examine the effect of the policy on student pre-college human capital attainment using a panel data set we constructed from publicly available data on state-by-race-by-year SAT scores. Using this data, we examine how minorities' and whites' SAT scores evolved in affected vs. non-affected states following the 2003 Supreme Court ruling. This difference-

[2] For simplicity, unless otherwise noted, we use "affirmative action" to refer to race-based affirmative action in the college admissions process, as opposed to "race-blind" affirmative action policies.

[3] The Texas "Top 10% Rule," which guarantees admission to any Texas public university to high school students graduating in the top 10% of their class, was held constant throughout our study period.

in-differences strategy indicates that whites' math SAT scores increased by 0.09sd, while minorities' increased by 0.18sd. Triple-differences estimates show that the effect on minorities is significantly greater than the effect on whites. While perhaps initially surprising, the positive effect on white students' outcomes is potentially consistent with a tournament model of affirmative action, where there are also increases in the returns to effort for white students and may also reflect positive spillovers due to greater minority student effort. These findings indicate that most of our measures, which focus on the racial achievement gap, may under-estimate rather than over-estimate the aggregate effects of affirmative action on pre-college human capital.

We next turn to other administrative measures of human capital. Since Texas state administrative data does not include grades, we draw on a supplemental administrative data set from a large urban, Texan school district. Using that data and our within-state difference-in-differences identification strategy, we find that affirmative action decreased the racial achievement gap in 11th grade by 0.1sd, with the largest effect on students in the top-third of the ability distribution. Replicating these analyses for attendance, a measure of human capital investment rather than attainment, in the full Texas administrative data set, we find that minorities attend more days of school relative to whites after the policy change. In both cases, we again find no evidence of positive pre-trends in the achievement gap in attendance that could bias our results.

In addition to the increase in attendance, two other pieces of evidence suggest that the increase in pre-college human capital is due to URM students responding to increased returns to human capital by increasing their effort. First, we use administrative admissions data to estimate the change in the effect of moving up in the ability distribution on admissions for URMs following the policy. We find that the returns to moving up in the ability distribution, which we interpret as the returns to effort, increase for the same quintiles of the ability distribution that increased their pre-college human capital.

Second, we use survey data from the Texas Higher Education Opportunity Project to shed light on additional potential mechanisms. Consistent with our administrative measures of pre-college human capital, we find that minorities increased daily time spent on homework after the policy change relative to whites and were more likely to apply to their first choice colleges. However, we find no evidence that parents or guidance counselors changed their behavior towards minority students. While these results are only suggestive, they are consistent with students changing their effort in response to the policy change.

Finally, in addition to examining the effects of affirmative action on pre-college human capital, we test whether affirmative action affects college graduation, with the caveat that this effect may capture both the effect of greater human capital accumulation in secondary school and the effect of attending a more selective institution. We find that college graduation increases by 1.4 percentage points for the top quintile in ability. This finding suggests that, at least in the context of Texas, mismatch effects due to affirmative action policies are not strong enough to reduce students' likelihood of completing a 4-year college degree (Arcidiacono et al., 2016).

Broadly our results contribute to a large literature studying the effects of affirmative action policies. This literature has focused primarily on affirmative action policies in higher education and their impact on college application behavior, college admissions, campus diversity, and college graduation. Examples of this extensive literature include Bowen and Bok (1998), Sander (2004), Card and Krueger (2005), Arcidiacono (2005), Rothstein and Yoon (2008), Hinrichs (2012), Arcidiacono et al. (2015), Arcidiacono et al. (2016), Arcidiacono and Lovenheim (2016), and Hinrichs (2016).

This paper is most closely related to a smaller literature about the implications of affirmative action for student behavior *prior* to college, which includes Antonovics and Backes (2014), Ferman and Assunção (2015), Cotton et al. (2015), Khanna (2016), Bodoh-Creed and Hickman (2018), Estevan et al. (2018), and Cassan (2019) on education and Venkataramani et al. (2019) on health behaviors. 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. In contrast, Cotton et al. (2015) estimate the effects of a field experiment that simulates both the tournament aspect of college admissions and the changes in incentives caused by affirmative action. In the field experiment, students are rewarded for rank-order math performance, and in the "affirmative action" intervention, initially weaker lower-grade students receive preferential treatment. Cotton et al. (2015) find that simulated affirmative action increases the disadvantaged group's investment in human capital on average. Most closely related to our paper, Bodoh-Creed and Hickman (2018) structurally estimate the U.S. college admissions market, modeling college admissions as an all-pay auction where pre-college human capital is a bid and taking into account how college admissions incentivizes student's pre-college human capital attainment. Bodoh-Creed and Hickman (2018) then

[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. 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.

generate counterfactuals estimates of student pre-college human capital under race-blind admissions. They find that moving to a race-blind counterfactual reduces pre-college human capital for minorities with a low cost of effort and increases 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 re-instatement of real affirmative action policies on students' outcomes in the U.S. Thus, we complement Bodoh-Creed and Hickman (2018) and Cotton et al. (2015) by providing model-independent, reduced-form estimates of affirmative action's effects as it is implemented in practice. Second, we exploit large and detailed administrative data sets, allowing us to examine affirmative action's effects on a variety of dimensions, and to trace out these effects across the ability distribution. Our findings are consistent with the results of Cotton et al. (2015) in the U.S. and Khanna (2016) and Cassan (2019) in India and confirm that affirmative action can increase minority students' human capital investment prior to the college admissions process. Our findings do deviate somewhat from Bodoh-Creed and Hickman (2018) since we do not find evidence that minorities anywhere in the ability distribution reduce their pre-college human capital. However, our policy experiment – reinstating affirmative action at a set of Texas public universities – is also different from the counterfactuals they study.

This study also relates to a literature on race-neutral affirmative action policies. Examples include Kapor (2016), Daugherty et al. (2014), Leeds et al. (2017), Golightly (2019), and especially Cortes and Zhang (2011). Cortes and Zhang (2011) study the incentive effects of the Texas Top 10% Rule, which guarantees admission to a public university for Texas students in the top 10% of their high school graduating class. They find that the plan incentivized students to increase their effort in high school. While these results are consistent with our's, the Top 10% Rule and race-based affirmative action are quite different.[5] Thus, separate studies are needed to determine how similar the incentive effects of these policies are.

Finally, we add to a broader literature on 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; Cortes and Zhang, 2011; Jensen, 2012; Oster and Steinberg, 2013; Leeds et al., 2017). While much of the evidence in this 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] Unlike race-based affirmative action, the Top 10% Rule is manipulable since students can switch schools to help ensure better outcomes (Cullen et al., 2013). Additionally, unlike race-based affirmative action, the Top 10% Rule has an explicit tournament structure with clear cutoffs.

The remainder of this paper is organized as follows. Section 2 introduces the context in more detail, and Section 3 discusses our different data sources. In Section 4, we report 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 5 provides suggestive evidence that the returns to investment in college admissions increased for the same set of students for whom we observe increases in pre-college human capital. 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 describe the Texas context and the policy change that this paper studies. We first sketch out a brief time line of events over the course of our study period (1997-2010) before describing the policy change and its effect on college admissions in more detail. In the last subsection, we consider whether universities' stated commitment to affirmative action translated into real changes in admissions.

Timeline of Events. 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 those 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. Then, 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. This Supreme Court decision overturned the previous decision banning the use of race as a factor in the admissions process in Texas, Louisiana, and Mississippi.[6] Thus, public universities in Texas, Louisiana, and Mississippi were unable to legally use race explicitly in the admissions process prior to 2003 and were able to do so again after 2003. We use this 2003 policy reversal to assess the effect of the introduction of race-based affirmative action on high

[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.

school students' performance.[7]

The Top 10% Rule remained in place with little change from 1997 onward, with the only change occurring at the very end of the study period. In 2009, the Texas legislature passed a law allowing UT Austin to cap the percent of its class admitted through the "Top 10% Rule" at 75%. As a result, 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." Consistent with this, the Supreme Court majority opinion expressed ambivalence over affirmative action policies, striking down the ban on considering race holistically while upholding a ban on 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* reader 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 news the same day of the ruling. Only the University of Texas Board of Regents could authorize the implementation of such a change and in August 2003, the Board of Regents

[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 student incentives. 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."

voted to allow all its campuses to return to considering race.[9] The Texas Tech University Board of Regents also outlined a plan in October 2003 to include race as an element in admitting prospective students. Thus, from the onset of 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 in the admissions process.

Race-based affirmative action co-existed 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 (i.e. are not in the top decile of their graduating class) are admitted based on a "holistic" review process. Following the policy change, race or ethnicity could again play a role in this admission process. While some portion of public university classes are admitted under the Top 10% Rule, the holistic admissions are also important. UT Austin, which has the highest percentage of freshmen admitted under the Top 10% Rule, admitted one-third of its freshman class through the holistic admissions process in 2003 (Office of the President, 2008). As described above, under current rules, UT Austin admits no more than 75% of its class based on high school ranking cut-offs.

Did Affirmative Action Policies Affect Admissions? To evaluate whether universities' stated commitment to affirmative action translated into different admissions decisions on the ground, we now consider how it affected both university composition and admissions. Appendix Figure A2 uses the IPEDS data to calculate the share of UT Austin's Fall, entering class by race and year. Following Fall 2003, there is a trend-break in the share of blacks and Hispanics, with both rising precipitously. These effects are consisting 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.

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 under-represented minority students' 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.[10] Students who ended 9th grade in 2001 were the first group whose admissions were affected

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

[10] The estimation procedure for these event study graphs is identical to that used to produce graphs for our outcome variables in the TEA later in this paper and is described in detail in Section 4.1.

by the re-instatement of affirmative action, although these students would have had little time to change their pre-college human capital. The likelihood of admissions for minorities following 2003 grew at UT Austin, the University of Houston, and Texas Tech. In contrast, there is no clear positive trend in minority admissions at Texas A & M, consistent with the fact that Texas A & M publicly stated they would not use race-based affirmative action in admissions (Parker, 2018). Altogether, these results suggest that lifting the affirmative action ban did affect minority students' admissions probabilities at selective Texas universities.

3 Data

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

3.1 Texas Education Agency (TEA) Administrative Data

Our first set of administrative data are based on individual-level administrative records on all Texas elementary, middle, and high school students from the Texas Education Agency. The records include yearly school attendance, test scores on standardized tests, as well as a host of demographic characteristics (e.g. race/ethnicity, gender, gifted status, socio-economic status). These data have several important advantages. First, one key feature of the TEA data is that the files are linked to (in-state) college administrative data, allowing us to study the impact of *Grutter v. Bollinger* on college applications *and* college completion. Thus, we observe which *Texan* universities a student applies to and whether they graduate from a *Texan* university.[11] Second, since they cover every student in Texas, they allow us to estimate the population average treatment effects of affirmative action. 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 capture information on students who actually enroll in college. Third, the large size of the TEA data set, as well as its panel structure, are important for estimating heterogeneity in the effects of affirmative action by ability. Because we observe 6th grade ability measures, in many cases we observe a student's location in the

[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).

ability distribution *before* she is affected by affirmative action. The scale of the data also allows us to estimate heterogeneous effects with statistical precision.

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. To examine the heterogeneous effects of affirmative action by academic ability, we collapsed these data at the school district-cohort-race-ability level.[12] Ability is determined by a student's 6th grade standardized test scores and students are classified into quintiles according to their rank in the cohort-specific test score distribution for the entire state of Texas.[13] Cohorts are defined using the academic year students first entered 9th grade. For most of our analysis, we focus on the 1997 to 2010 cohorts.[14] This analytical sample represents close to 3 million students. Thus, these data allow us to analyze the effects of affirmative action by ability on college applications, admissions, graduation, and school attendance.

While the TEA data also include data from Texas' state-wide standardized tests, these tests underwent a substantial version change at roughly the same time as affirmative action was re-instated. In 2003, the standardized exam changed from the TAAS to the TAKS.[15] As a result, we cannot disentangle the effects of affirmative action from the effects of the version change on minorities' test scores. Thus, to examine additional measures of human capital, such as grades and test scores, we turn to a complementary administrative data set from one Texas school district, which we will describe below.

Summary statistics in the top panel of Table 1 provide an overview of the students in the TEA data. These statistics are reported separately for whites and minorities and for cohorts that were and were not affected by *Grutter v. Bollinger*. The fraction of Texas students identified as URMs increases sharply over time, entirely driven by an increase in the Hispanic population. URMs are much more likely to be from poor households than whites (60% vs 12% in 1997-2000) and have lower 6th grade test scores (average decile of 4.4 vs 6.6). Prior to the re-instatement of affirmative action, 17% of URMs apply to any 4-year university (within 4 years after starting high school), whereas 29% of whites do. The gap is smaller in

[12] For confidentiality reasons, all cells with less than 5 students are dropped (7% of all students). For complementary robustness analyses, we also collapsed the data at the school-cohort-race level, in which case only 1% of students are dropped because they belong to small cells. For average effects, the coefficients produced using either data set are quantitatively and qualitatively similar.

[13] The fraction of students with valid 6th grade test scores varies slightly across cohorts, generally hovering within the 70-75% range.

[14] Years are based on the Spring semester. For example, the 2000 cohort refers to students who were in 9th grade in the 1999-2000 academic year.

[15] 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).

the later period, with 26% of URMs applying and 34% of whites doing so. Racial gaps are even larger in terms of applications to selective universities. For example, for the 1997-2000 cohorts of 9th graders, the average number of applications sent to selective institutions by URM students is 0.06, while it is 0.21 for whites.[16] Finally, 11% of all (pre-AA) black and Hispanic students eventually obtain a college degree, while 25% of whites do.[17]

3.2 Large Urban School District (LUSD) Administrative Data

Our second source of administrative data is drawn from a large, urban school district in Texas. These data consist of repeated cross-sections of individual-level data for all 11th graders in the school district between 1997 and 2010. The data contain information on students' demographics (race/ethnicity, gender, age and zip code) and attendance rates. Importantly, courses and course grades, which are not available in the TEA data, are included in this data set. The data also includes test scores on the norm-referenced Stanford Achievement Test (hereafter, Stanford), a low-stakes achievement test that the school district has administered since 2000. The Stanford test administered by the school district did undergo a 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 therefore view evidence from the Stanford test as suggestive.

For students enrolled in 11th grade between 2000 and 2008, we also obtained prior academic records for the three preceding years (e.g. we obtained course grades in 2003, 2004 and 2005 for students enrolled in 11th grade in 2006). In most of our analyses, we restrict our sample to this shorter sample, which allows us to estimate the effect by or control for academic achievement prior to affirmative action policies.[18]

The bottom panel of Table 1 shows summary statistics for the sample of 11th graders from this school district. The majority of students in the district are black or Hispanic. In a typical campus, 85% of students are black or Hispanic, and these students have lower achievement than white students along all dimensions. Black and Hispanic students score

[16] The selective Texas universities to which we observe applications in our data are UT Austin, University of Houston, Texas A&M and Texas Tech.

[17] We exclude the 2007-2010 cohorts for college completion, since these later cohorts were less likely to have completed college by 2014, the last year of data we have. For instance, the overall college completion rate is less than 6% for the 2007 cohort of 9th graders.

[18] 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 application stage to make decisions based on college admissions policies.

significantly lower on the Stanford standardized test in terms of national percentile ranking compared to white students, have lower grades in their courses (both in 8th and 11th grade), and have lower attendance rates.

3.3 SAT Data

To analyze the effects of the re-instatement 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. As in our administrative data sets, we define underrepresented minorities (URMs) as Hispanic and black students and use white students as our comparison, non-minority group.

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 minorities and whites and to estimate the differential change in minorities' outcomes relative to whites in the treated states. Summary statistics of the SAT panel data are reported in Appendix Table A1. These summary statistics reveal a substantial racial achievement gap, with average math and verbal scores for whites of 530 and 528 respectively and for minorities of 493 and 441 over the 1998-2003 period.

3.4 Texas Higher Education Opportunity Project Data

Our final data set complements our administrative and SAT data with survey data from the Texas Higher Education Opportunity Project (THEOP). THEOP surveyed high school seniors from a random sample of 105 public high schools in Texas in 2002 and in 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 caveat that the fact we observe only two cross-sections of the data makes it impossible to assess whether pre-trends drive the results. Unfortunately, the two waves of the survey are not identical, but the set of questions that are consistent across these waves allow us to compare several outcomes that shed light on what mechanisms may drive affirmative action's effects.

THEOP records time spent on homework outside of school (in minutes), 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 application behavior changed. In addition, we combine a series of questions about parental behavior into a "parental involvement index," with values ranging from 5 to 20.[19] 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 college application process with his/her guidance counselor captures changes in guidance counselor involvement. Appendix Table A2 reports summary statistics for these data.

4 Effects of Affirmative Action on Students' Outcomes

In this section, we empirically investigate the effect 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 minorities' college application behavior relative to whites. As this outcome is the most malleable and the most directly connected to affirmative action policies, we view a positive effect of affirmative action on college applications as evidence that students were aware of and responded to the policy change. We then complement these results by estimating the effect of affirmative action on minorities' SAT scores using difference-in-differences, triple-differences, and synthetic control group approaches that compare trends in scores in states that re-instated affirmative action (Texas, Louisiana, Mississippi) to trends in unaffected states. Next, we focus on a single large, urban Texas school district where we observe grades and standardized test scores. Using these data, we estimate the effects of the reinstatement on the within-school-year racial achievement gap in standardized tests and course grades. As an additional measure of secondary school effort, we use also administrative data from the entire state of Texas to estimate the effects of affirmative action on attendance. Finally, we estimate the effects of the reinstatement of affirmative action on college completion, though we caution that this outcome is a function of both effort and the college to which a student is matched.

4.1 Impact of Affirmative Action on College Application Behavior

Difference-in-Differences Empirical Strategy. To assess the effects of affirmative action on students' college application behavior, we use Texas-wide administrative data. Our difference-in-differences strategy compares the change in minorities' college application be-

[19]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.

havior following the reinstatement of affirmative action to the change in whites' behavior. Recalling that an observation in this data is a race-ability quintile-district-cohort cell, we estimate

$$\begin{aligned} y_{dcea} = & \beta_1(Minority_e \times PartTreat_c) + \beta_2(Minority_e \times FullTreat_c) \\ & + \mathbf{\Gamma X_{dcea}} + \alpha_{dca} + \alpha_{dea} + \epsilon_{dcea} \end{aligned} \tag{1}$$

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 an ability quintile in the state standardized test in 6th grade. The variable $Minority_e$ is an indicator variable for underrepresented minority racial groups, and $\mathbf{X_{dcea}}$ is a vector of average student characteristics for the observation cell (age, sex, immigrant status, low-income status, gifted, ESL, special education status, and limited English proficiency). The dependent variable, y_{dcea}, is either the fraction of students who applied to *any* 4-year university[20] or the average number of applications sent to selective Texan institutions. Since the impact of affirmative action may not be immediate, we allow the effect to vary across cohorts. In our main parametric specification, 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. α_{dca} denotes district-cohort-ability fixed effects, and α_{dea} denotes district-ethnicity-ability fixed effects.

Our main coefficients of interest, β_1 and β_2 respectively represent the short and medium-run effects of affirmative action on college application 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. Thus, we expect $\beta_2 > \beta_1$.

In this difference-in-differences specification, the effect of affirmative action is identified by comparing minority students to non-minority students of the same ability, 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 ability levels, as long as they are not differential by race. The fixed effect α_{dea} accounts for any differences across races, districts, or ability levels (or any combination thereof), as long as these differences do not vary over time. To account for correlated outcomes in districts over time, we also cluster standard errors at the district-level.

[20] This measure includes non-selective institutions.

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 values for β_1 and β_2 in equation (1), even if minorities' behavior is unchanged. To assess whether this could be driving our results, we also separately graph trends in application behavior by race. Then, we can observe directly if minorities experience a jump or trend break when they are affected by the policy and whether whites are negatively affected.

Event Study Specification. In this difference-in-differences empirical strategy, identification of the causal effect of affirmative action relies on the assumption that the college application behavior of minority and comparable non-minority students would have evolved the same way in the absence of the ruling. To examine the plausibility of this assumption, we plot the effect of being a minority on college application behavior separately for each cohort. Doing so allows us to establish if trends in college applications for minorities and whites were parallel prior to the re-introduction of affirmative action. Plotting these point estimates also allows us to observe whether the treatment effects of affirmative action accumulate, justifying our decision to separate partially and fully treated cohorts. To do so, we estimate the following model:

$$\begin{aligned} y_{dcea} = &\sum_{t=1997}^{1999} \beta_t (Minority_e \times \mathbf{I_{ct}}^{Grade\ 9}) + \sum_{t=2001}^{2010} \beta_t (Minority_e \times \mathbf{I_{c}}^{Grade\ 9}) + \mathbf{\Gamma X_{dcea}} \\ &+ \alpha_{da} + \alpha_{ca} + \alpha_{ea} + \epsilon_{dcea}, \end{aligned} \tag{2}$$

where $\mathbf{I_{ct}}^{Grade\ 9}$ is an indicator variable equal to 1 if cohort c was in 9th grade in year t. We therefore estimate a separate, cohort-level "treatment effect," β_t, which captures the relative effect of being a minority on college applications for each cohort. We calculate these effects relative to 2000, which is the base cohort. We choose this cohort since it is the last never treated cohort. Students in this group finished high school in the Spring of 2003. As before, standard errors are clustered at the district-level. To allow us to estimate cohort-specific effects more precisely, the fixed effects included in equation (2) are slightly less conservative than those in equation (1). That said, they still account for level differences across districts, ethnic groups, ability groups, and over time.

If the parallel trends assumption is valid, for $t < 2000$, we expect that β_t will be indistinguishable from zero. 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. Additionally, 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 2000.

Difference-in-Differences Results. We report coefficients from equation (1) 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 dependent variable is the probability of applying to any college. In panel A, on average, fully treated minority students apply to 0.02 more selective Texas colleges, a 13% increase on a base of 0.16 applications.[21] Turning to Panel B, on average, lifting the ban on affirmative action increased minorities' probability of applying to at least one college relative to whites by 0.8 percentage points for cohorts who were in high school at the time of *Grutter v. Bollinger* and by 2.9 percentage points for cohorts who entered high school after the ban was lifted. They indicate that, over the long-term, the policy closed the pre-AA racial achievement gap in applying by 25%. In both panels, the estimates are precisely estimated and statistically significant at the 1% level.

For both Panels A and B, these average effects mask substantial heterogeneity. The remaining columns of the table estimate the effects for students in different ability quintiles. While we do find a small positive effect on application to any college (0.0101) for fully treated students in the bottom quintile of the ability distribution, the effect is five times larger among the highest ability students (0.0545). While bottom quintile students are no more likely to apply to selective institutions, top quintile students apply to 0.05 more selective institutions and the second highest quintile applies to 0.03 more selective institutions. This heterogeneity accords with where we would expect affirmative action to have the strongest effects on college applications, as affirmative action is most likely to affect admissions for students already on the margin of being admitted. The small, positive effects we estimate for lower quintile students could reflect both noise in the quintile assignment, which is based on 6th grade test scores, and positive spillovers from higher ability students.

Additionally, we examine applying to any of the campuses of the University of Texas (UT Arlington, UT Austin, UT Dallas, UT El Paso, UT Permian Basin, UT Rio Grande, UT San Antonio, UT Tyler). Since the University of Texas Board of Regent promptly allowed its campuses to consider race in admissions, the effects of affirmative action should materialize for these institutions. Appendix Table A5 and Figure A7 respectively report the difference-

[21] Results are reported separately for black and Hispanic students in Appendix Tables A3 and A4, respectively.

in-differences and event study results for applications to any UT campus. The patterns are comparable to results for the other applications outcomes.

Finally, to verify that the positive effects reported in Table 2 are not driven by declining applications by whites, we plot application behavior separately by race in Appendix Figure A4. The unadjusted figures plot the cohort effect on applications (normalized to the cohort in 9th grade in 2000) without controls, while the adjusted figures include the full set of controls from equation (1), as well as race-ability and district-ability fixed effects. Appendix Figure A5 further shows the results by racial group for the top quintile. Taken together, these figures show that there is a trend break in minorities' behavior around the reintroduction of affirmative action, and that the difference-in-differences estimates are not due to reduced applications by whites.

Event Study Results. We now turn to the event study graphs based on equation (2) to examine whether pre-trends drive our findings. Figure 1 plots the effects on applications sent to selective universities only. Cohorts between the solid and dashed vertical lines are partially treated, whereas cohorts to the right of the dashed vertical line are fully treated. For this outcome, we examine time trends separately for students in the top and bottom quintiles of the ability distribution.[22] This is driven by our finding that the effect of affirmative action on selective college applications was concentrated in higher ability cohorts. The point estimates for bottom quintile students are indeed 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 generally not statistically significant. Overall, there is no evidence that a pre-trend could drive the positive estimated effect of affirmative action. Additionally, a positive trend emerges directly following the policy change. The fact that the trend break coincides with the policy change further suggests that the policy change itself is driving the growth in minority students' applications to selective institutions.

Figure 2 plots year-specific coefficients β_t for the probability of applying to any university in Texas, and 95% confidence intervals are shown using dashed lines. Again, results are shown separately for students in the top and bottom quintiles of the ability distribution. For bottom quintile students, there is a small upward trend in minority college applications relative to non-minorities prior to the policy change, but most year-specific coefficients are close to zero and statistically indistinguishable from the base year. The 2001 cohort of 9th graders is the

[22] Figure A6 shows average effects for the full sample.

first cohort to apply to university following *Grutter v. Bollinger*. The point estimate for this cohort indicates that the probability of applying to university of minority students increases at the time of the policy change, but the jump is considerably more pronounced for top quintile students. The positive effect of affirmative action then grows over time, suggesting that fully treated cohorts were more affected than partially treated cohorts.

In both graphs, the treatment effects appear to accumulate over time, with affirmative action having a larger effect on fully treated 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 effort, a hypothesis that we begin to investigate in the next subsection.

Robustness. Before moving on to assessing the effects of affirmative action on pre-college human capital, we first ensure that our results are robust to an additional test. One concern is that in later-cohorts our 6th grade ability measure, which we use to estimate the heterogeneous effects of the policy, is observed after the policy change. Thus, observed ability may be endogenously changing as a result of the reintroduction of affirmative action. To ensure this is not effecting our results, in Appendix Table A6, we re-estimate equation (1), dropping cohorts who were in 6th grade following the policy change. The pattern in the results is the same as before: affirmative action has its strongest positive effects on minorities in the upper part of the ability distribution.

4.2 Impact of Affirmative Action on SAT scores

Difference-in-Differences Empirical Strategy. To measure whether affirmative action affected students' human capital, we now examine whether it affected students' SAT scores. To measure the effects of affirmative action, 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 affirmative action. This difference-in-differences strategy allows us to estimate the effect of affirmative action *separately* for minorities and whites.

To implement this strategy, we use a panel of average math and verbal SAT scores at the state-race-year level. Using this data, for minorities and whites, we separately estimate

$$y_{ket} = \beta(Treated\_State_k \times Post2003_t) + \alpha_k + \alpha_t + \alpha_e + \varepsilon_{ket}. \tag{3}$$

where k indexes a state, t indexes a year, and e indexes a racial group. Then, y_{ket} is either 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 (Texas, Louisiana, and Mississippi), $Post2003_t$ is an indicator variable equal to 1 if the year is greater than 2003, α_t is a year fixed effect, α_k is a state fixed effect, and α_e is a race fixed effect. Additionally, we weight race-state-year cells by the number of test-takers and cluster our standard errors at the state-level.

Triple-Differences Empirical Strategy. In addition to exploiting time and geographic variation to estimate the effect of affirmative action, we also use a triple-differences strategy. Since we expect minorities to be more affected by affirmative action, we use 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. 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 URM's outcomes if the policy also affected whites. To estimate the differential effect of affirmative action on minority students relative to non-minority students, we estimate

$$y_{ket} = \beta_1(Treated\_State_k \times Post2003_t \times Minority_e) + \alpha_{ke} + \alpha_{et} + \alpha_{kt} + \varepsilon_{ket}, \tag{4}$$

where $Minority_e$ is an indicator variable equal to 1 if the individual belongs to a minority group, 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 racial status.

Event Study. As with college applications, we also use event study graphs to assess whether the parallel trends assumption of our difference-in-differences strategy is violated.

To do so, we estimate the following equation separately for whites and URMs

$$y_{ket} = \sum_{c=1998}^{2002} \beta_c(Treated\_State_k \times \mathbf{1_t}\{t \in c\}) + \sum_{c=2004}^{2010} \beta_c(Treated\_State_k \times \mathbf{1_t}\{t \in c\})$$
$$+ \alpha_k + \alpha_e + \alpha_t + \varepsilon_{ket}, \tag{5}$$

where $\mathbf{1_t}\{t \in c\}$ is an indicator variable equal to 1 if an observation is from year c. The omitted year is 2003, the year before the policy change. This event study specification estimates the differential effect of a test-taker being in a treated state for each year, β_c. If pre-trends between treated and non-treated states are parallel, we expect that β_c should be small and insignificant prior to 2003.

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 different 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).[23] We match the pre-treatment values of the number of minority and white test takers, the math SAT scores of minority and white students, and the verbal SAT scores of minority 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.

To assess the significance of our estimates, we use permutation tests. More specifically, for all possible combinations of three control states, we apply the synthetic control method and calculate the post/pre-treatment ratio of root mean squared prediction errors (RMSPE).[24] We then plot the distribution of these ratios and examine the rank of the real treatment unit in that distribution.

[23]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 the Online Appendix, we show that our results are robust to using fewer pre-treatment years to construct the synthetic control group.

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

Difference-in-Differences and Triple-Differences Results. Table 3 reports the coefficients from equation (3) (panels A and B) and equation (4) (panel C) for SAT scores measured in standard deviations. Estimates for math scores are reported in column (1), and verbal scores are in column (2). Column (1) shows that math scores for both minorities and whites improved in treated states following 2003, but minorities' test scores improved by almost twice as much (0.18sd relative to 0.09sd). While at first surprising, this finding may be because whites' human capital was also positively affected by the ban. This would be the case if whites increased their effort in response to intensifying competition. Indeed, this 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. In contrast, there is no effect on verbal SAT scores (column (2)).

In the last panel of Table 3, we report the results of the triple-differences specification. We find that minorities' SAT math scores improved relative to whites in treated states by a statistically significant 0.09sd. These results suggest that the reinstatement of affirmative action helped close the racial achievement gap in treated states. They also suggest that the difference-in-differences estimates are not merely biased by differential time trends in states that were not affected by *Grutter v. Bollinger*.

In the last two columns, we verify that our results are not driven by changes in the composition of SAT test-takers. 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 pre-treatment period, 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). Both metrics suggest there was no significant change in the probability of taking the SAT in treated states relative to untreated states.

One potential concern is that our estimates might be contaminated by other policy changes that occurred between 1998 and 2010 in otherwise untreated states. For instance, four states moved to banning affirmative action in college admission during these years. In the Online Appendix, we verify that our results are robust to controlling for these other policy changes.

Event Study Results. As for college completion, we also visually inspect trends in SAT scores. Figure 3 reports year-specific coefficients from equation (5) separately for minority and non-minority students. 95% confidence intervals are shown using dashed lines. 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 in terms of performance on SATs.[25] The reinstatement of affirmative action coincides with a dramatic reversal of fortunes, with the negative trend turning strongly positive right after 2004. Importantly, the post-treatment positive trend for math scores appears considerably steeper for minority students than for whites. Consistent with the point estimates in Table 3, the patterns are less clear for verbal scores, with no clear change over time for either minorities or Whites. For URMs, there is a steep negative pre-trend, but there is some evidence that this decline in relative performances comes to a halt following the policy change. Altogether, Figure 3 provides further evidence that our estimates are not driven by differential time trends between treated and untreated states.

Synthetic Control Results. The top panel of Figure 4 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. In both cases, the synthetic control group closely tracks the treatment unit prior to the re-instatement of affirmative action, and the two trends diverge considerably from 2004 onward. This is true both for white and minority students, but the divergence is greater in magnitude for the latter group. The implied treatment effects are larger than our baseline difference-in-differences estimates. We find a 0.14sd increase in test scores for whites, and a 0.24sd increase for minorities. The placebo tests suggest that these results are not due to chance. The treatment unit's post-pre ratio of RMSPE falls at the 99.2th percentile of the distribution of whites, and at the 96.6th percentile for minorities.

In Appendix Figure A9, we also report a synthetic control plot of the differences between treated and untreated units separately for white and minority students. For both racial groups, the differences are close to zero prior to treatment and then exhibit large increases following *Grutter v. Bollinger*. Again, the effect is greater for URMs than whites. The implied triple-differences estimate from differencing out the whites' difference from the URMs' is 0.10sd, which is very close 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. SAT scores may only reflect better SAT-specific test-taking skills. Thus, examining other measures of student effort allows us to evaluate if affirmative action affects human capital more broadly.

[25] Appendix Figure A8 shows that a similar pattern holds for Asians.

4.3 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 (1), 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-ability data, for the LUSD, an observation is an individual. The specification is

$$y_{isec} = \beta(Minority_i \times Post2003_i) + \mathbf{\Gamma X_i} + \alpha_{sc} + \alpha_e + \epsilon_{isec} \tag{6}$$

where i denotes an individual, s denotes a school, e denotes a racial group, and c denotes a cohort.[26] 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} 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.[27] Thus, the effect of affirmative action in this regression is identified by comparing minority and white students in the same school in the same year. The basic controls $\mathbf{X_i}$ consists of controls for age, sex, and home zip code fixed effects. Additionally, in a more conservative, "value-added" specification, we control for a lagged measure of ability (8th grade test scores).[28] This control accounts for any changes in the ability distribution of minorities 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 minority students relative to non-minority students.

In addition to using this difference-in-differences approach to estimate the effect of affirmative action, we also estimate cohort-specific coefficients and plot them in an event study graph. To do so, we simply alter equation (6) to estimate a different coefficient on the variable $Minority_i$ for every cohort. As in our previous analyses, the event study graph sheds light on whether the results we observe are driven by pre-trends.

[26] 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.

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

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

Difference-in-Differences Results. The difference-in-differences estimates from equation (6) are reported in Table 4. The point estimates confirm that affirmative action had a positive effect on school grades in 11th grade. Our baseline estimates of equation (6) in column (1) indicate that grades increased by 0.9 points (on a 0-100 scale) following the reinstatement of affirmative action. This is a 0.1 s.d. effect, a magnitude similar to the differential effect of affirmative action on minorities' SAT scores. In column (2), we estimate the value-added specification where we control for school grades in middle school (8th grade). The difference-in-differences coefficient is almost identical and remains strongly 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 ($Minority_e \times Treat_c \times I_g^{11th\,Grade}$), 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 effort between 8th and 11th grade. This alternative specification accounts for any unobserved changes in minority students' characteristics across cohorts that might otherwise bias our estimate of the effect of affirmative action. Again, 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 ability. To do so, we calculate school-by-cohort specific terciles of the distribution of grades in 8th grade within school-years. In this data, 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 (6) separately for students in the bottom, middle, and top terciles. While the point estimates for the effect of affirmative action are positive for all three ability categories, they are particularly large for top-ability students (an effect of 1.4 percentage points or 0.2 sd). This is what one would expect if these students are most likely to apply to selective colleges and therefore to benefit from the policy change.

Appendix Tables A8 and A9 re-estimate the specifications in Table 4 separately for math and English grades. The effect sizes for both math and English grades are similar, and in both cases, the largest effects are on students in the top tercile of the student population.

Event Study Results. The top panel of Figure 5 reports year-specific coefficients on the $Minority_i$ indicator variable when the outcome is mean student grades. There appear to be no significant pre-trends, with the racial gap in school grades remaining constant over the 2001-2003 period. School grades for URM students improve relative to their non-URM peers upon the re-instatement of affirmative action and remain at this higher level through 2008. The bottom panel of Figure 5 reports the year-specific coefficients under the value-added specification, which controls for 8th grade test scores. The results across the two specifications are very similar.

4.4 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 follow the exact same difference-in-differences strategy as we did for grades in Section 4.3. The only difference is that the outcome variable is now a student's mean percentile on the Stanford exam, where percentiles are based on the national distribution. Appendix Table A7 reports the estimates. On average, Stanford test scores increase by 4.78 percentiles for minorities relative to whites (equivalent to 0.2 s.d.). The effect is again largest for the top tercile, who experience gains of 7.47 percentiles (0.3 s.d.). Appendix Figure A10 plots the event study graph for the Stanford exam. We again see little evidence of pre-trends and the immediate positive effect of affirmative action on minorities' test scores at the time of the policy change.

4.5 Impact of Affirmative Action on Attendance

Having shown that grades and test scores increase as a result of affirmative action, we now consider more direct measures of student effort. Returning to the Texas-wide administrative TEA data, we test whether affirmative action affects minority students' attendance. Our empirical strategy for examining attendance in the TEA data follows our strategy for estimating effects on college applications (see equation (1)).

Table 5 reports the regression results for 10th and 11th grade attendance. Difference-in-differences estimates indicate a positive average effect on the fraction of days present of 0.0036 percentage points in 10th grade (panel A) and of 0.0024 percentage points in 11th grade (panel B). While the effects on attendance occur throughout the distribution in grade 10, for grade 11, they are concentrated again in the top part of the distribution.

Figure 6 reports the event study plots for attendance in grades 10 and 11. 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 (both cohorts were in 10th grade before *Grutter v. Bollinger*, but only the 2002 cohort was in 11th grade post-treatment). Overall, the plots show no discernible pre-trend, and they suggest that there was a positive effect on attendance in high school.

4.6 Affirmative Action and College Completion

Thus far, our analyses have documented the positive effects of affirmative action in undergraduate college admissions on application behavior and human capital prior to reaching college. In this section, we estimate the effect on the probability of completing a college degree using administrative data from the TEA.

In Section 4.1, we showed that more minority 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 minority 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. This is essentially the mismatch argument of Sander (2004). Then, affirmative action might reduce the fraction of degree holders. On the other hand, if increased effort in high school contributes to the accumulation of human capital, the probability of completing a 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, this may also increase graduation rates. 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 (1)).

Table 6 reports the 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%). As in our other analyses, in columns (2) to (6) we estimate the effect separately for each quintile of the ability distribution. We find no significant evidence of gains for partially treated cohorts for any of the quintiles, though the estimate is positive for top ability students. For fully treated cohorts, the point estimates are positive throughout the ability distribution, but are much larger for the top ability quintile. The estimates indicate that students in the top quintile of the ability distribution who started high school post-policy experienced a 1.4 percentage point increase (4%) in the probability of completing college.

In Figure 7, we present an event study plot of the reinstatement of affirmative action on the probability of completing a college degree. We plot the effects separately for students in the bottom and top quintiles of the ability distribution. The probability of completing a college degree is roughly flat for low-ability students throughout the study period, as one might expect. For high ability 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 suggestive evidence in favor of the human capital accumulation channel. The cohorts who had the most time to adjust their human capital investment in secondary school appear to benefit the most from the change in admission rules in terms of their college graduation outcomes.

Taking all our results together, higher ability minority students increased their effort in high school as measured by attendance, increased their pre-college human capital, 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 in parts of the distribution 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.

5 Changes in Returns to Pre-College Human Capital

The previous section provided evidence that the effects of affirmative action on minority students' pre-college human capital are concentrated in the top half of the ability distribution. If students are indeed responding to changes in the returns to human capital investment caused by the policy, then these should also be the students for whom the returns increased. In this section, we provide suggestive evidence that this is the case.

To do so, we return to the TEA data on university admissions. Taking advantage of our 6th grade ability measure, we estimate the change in the marginal effect of moving up an ability *decile* on university admissions. The estimating equation is

$$y_{dcea} = \sum_k \beta_{1,k}(Minority_e \times PartTreat_c \times \mathbf{I_a}^{a \geq k}) + \sum_k \beta_{2,k}(Minority_e \times FullTreat_c \times \mathbf{I_a}^{a \geq k})$$
$$+ \mathbf{\Gamma X_{dcea}} + \alpha_{dca} + \alpha_{dea} + \epsilon_{dcea} \tag{7}$$

where a denotes a decile, y_{dcea} is a college admissions outcome, and $\mathbf{I_a}^{a \geq k}$ is an indicator variable if a student's ability decile a is greater than or equal to k. The controls are the same as in equation (1) except that they are now fully interacted with ability decile fixed effects. Thus, $\beta_{1,k}$ and $\beta_{2,k}$ capture the change in the marginal effect of moving from decile $k-1$ to k due to the policy for those who are partially and fully treated. Since increased human capital investment can allow a student to move up in the distribution relative to her peers, we interpret these coefficients as a proxy for the change in the returns to investment in university admissions, with the caveat that part of the increased admissions may be due to changes in students' applications behavior due to the policy.

Figure 8 reports $\beta_{2,k}$ for admission to any college and for the number of selective Texas universities to which a student is admitted. Consistent with the fact that affirmative action increases effort and human capital in the top half of the ability distribution, we see that the returns mainly rise in the top half. For instance, the returns to moving from the 9th to the 10th ability decile, in terms of number of admissions to selective universities per student, increased by 0.026 for minorities relative to whites in the long-term. The fact that there are strong increases in the returns for the top decile isn't inconsistent with the 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.

Appendix Figure A11 further reports the effects on the "returns to human capital" for admissions to four specific schools: UT Austin, University of Houston, Texas Tech, and Texas A&M. For the first three schools, which were free to practice affirmative action, there are increases in the "returns to human capital" in the top half of the ability distribution (or in the top 30% in the case of Houston). Reassuringly, for Texas A&M, which does not practice affirmative action, there is no systematic effect on the returns to human capital. Given that human capital investment responded to affirmative action exactly among the students with the greatest increase in its returns, the results provide evidence that students increased their effort in response to the change in the returns to effort.

6 Suggestive Evidence on Mechanisms

So far, we have provided evidence that affirmative action narrowed the achievement gap between whites and minorities 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 admissions. Still, teachers may have also become more lenient toward minorities after the policy change or teachers may have focused more on improving minority students' outcomes. While the relative improvement in standardized test scores cannot be explained by teachers grading minorities more leniently, this does not rule out the possibility that they focused more attention on improving minorities' learning.[29] Another alternative explanation is that the change in affirmative action policy changed parents' or guidance counselors' perception of a student's returns to human capital investment and led them to become more involved with the students. To provide suggestive evidence on the drivers of minority students' improved outcomes, we analyze students' responses from the THEOP survey.

As mentioned previously, the THEOP survey asked two cross-sections of high school seniors across Texas about their demographics, college application 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, 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:[30]

$$y_{iet} = \beta_1 Post2003_i + \beta_2 Minority_i \times Post2003_t + \alpha_e + \varepsilon_{it}, \tag{8}$$

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

Table 7 reports the results. As column (1) shows, after the implementation of affirmative action, minority high school seniors spend 5 minutes more on homework a day, equivalent to

[29] However, our findings for the SAT suggest that if this is the case, it did not have negative spillovers for whites' outcomes.

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

8% more time on homework outside of school relative to white students. Minority students are also 5 percentage points more likely to apply to their first choice college after the policy change compared to whites, consistent with our findings in the TEA data. However, we do not see any changes in an index of variables measuring parental involvement or the likelihood of discussing college applications with guidance counselors after affirmative action is put in place. Overall, Table 7 provides suggestive evidence that students directly responded to the change in affirmative action policies by changing their behavior. For these measures, we don't find any evidence that schools or parents changed their human capital investments in minority students.

7 Alternative Policies

This section discusses alternative educational policies that were enacted in the early 2000s and argues that they are unlikely to explain our findings. The first sub-section discusses No Child Left Behind (NCLB). The second subsection discusses charter school expansions.

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 minority 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 George 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 minorities specifically in Texas, Louisiana, and Mississippi.[31] Third, we find 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. Consistent with this, Neal and Schanzenbach (2010) show that NCLB and similar policies increased test scores among the middle of the test score distribution.

[31] 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 minorities in Texas, Louisiana and Mississippi will be under-estimates due to NCLB.

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 minority students' outcomes. However, 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).[32] Nonetheless, as a robustness test, we also omit Houston and Dallas, the two areas with by far the largest number of students enrolled in charter schools today (Texas Charter Schools Association, 2016), from the analysis and re-run our college application results. These results are reported in Appendix Table A10, and are again very similar to the main estimates.

8 Conclusion

In this paper, 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 colleges, high school attendance, and college graduation by minorities in Texas. The policy also reduced the racial achievement gap for math SAT scores by 5% in the affected states. Comparing minority (black and Hispanic) and white students in the same schools in a large, urban school district in Texas, we also find that this reinstatement substantially reduced the racial achievement gap in grades, causing it to fall by approximately 20%. The effects we observe are concentrated among higher ability students. We also verify that these students experience the greatest change in the effects of moving up an ability decile on college admissions following the policy. Thus, the students whose returns to human capital investment increase the most also appear to be those who respond the most. Finally, our findings suggest that any negative mismatch effects of affirmative action on college graduation were swamped by the net effects of increased human capital accumulation and attending better universities.

Altogether, given the positive effects on attendance, the distribution of the treatment effects, and the evidence from the survey data, our findings suggest that minority students

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

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 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 minority students to perceive admission to a selective school as more attainable, it may change both their application behavior and their pre-college human capital investment.

Finally, the meaningful effect sizes we estimate on a variety of dimensions suggest that policy debates that ignore the pre-college incentive effects of affirmative action policies ignore a significant effect of these policies. Given the role the racial achievement gap may play in determining gaps in long-term outcomes (Neal and Johnson, 1996), reductions in the achievement gap may translate into substantial reductions in the wage gap.

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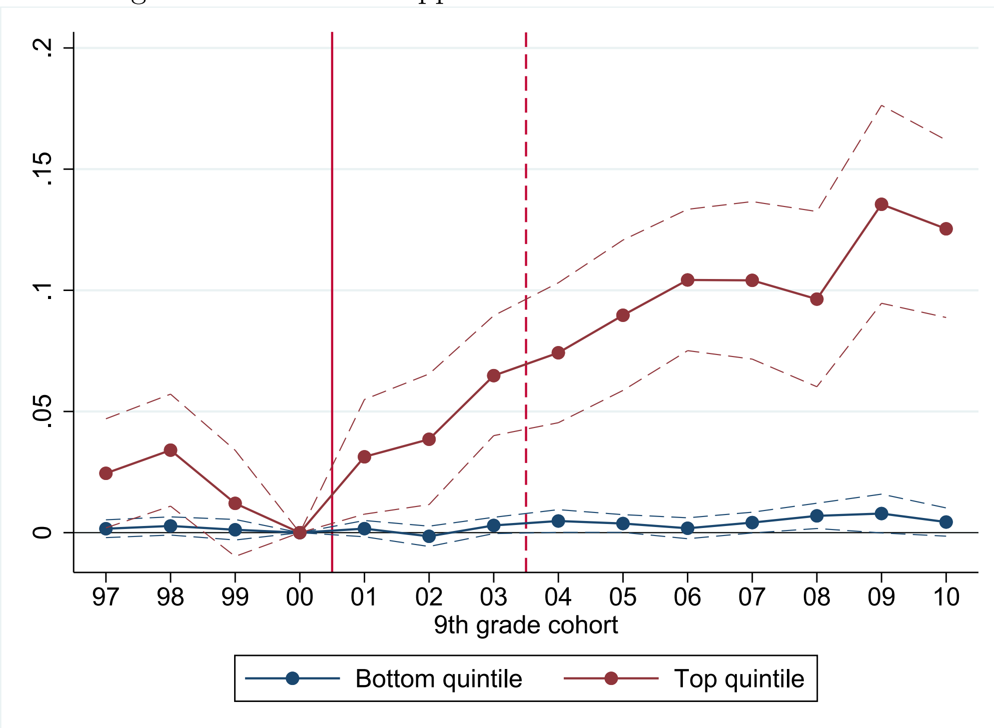
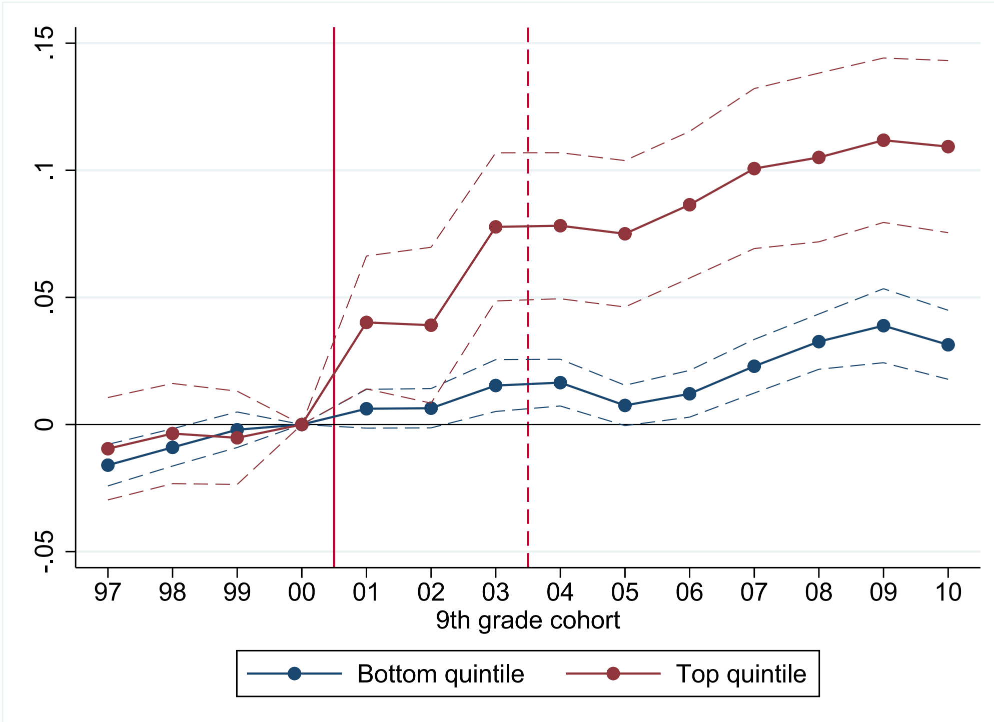
Figures

Figure 1: Number of Applications to Selective Universities



Notes: The outcome is the average number of applications sent to selective universities by students within 4 years after starting 9^{th} grade. Dots indicate coefficients of regressions of the outcome on year dummies interacted with minority status separately for students in the bottom and top quintiles of the ability distribution, where ability is given by deciles of cohort-specific distribution of 6^{th} grade standard test scores. 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.

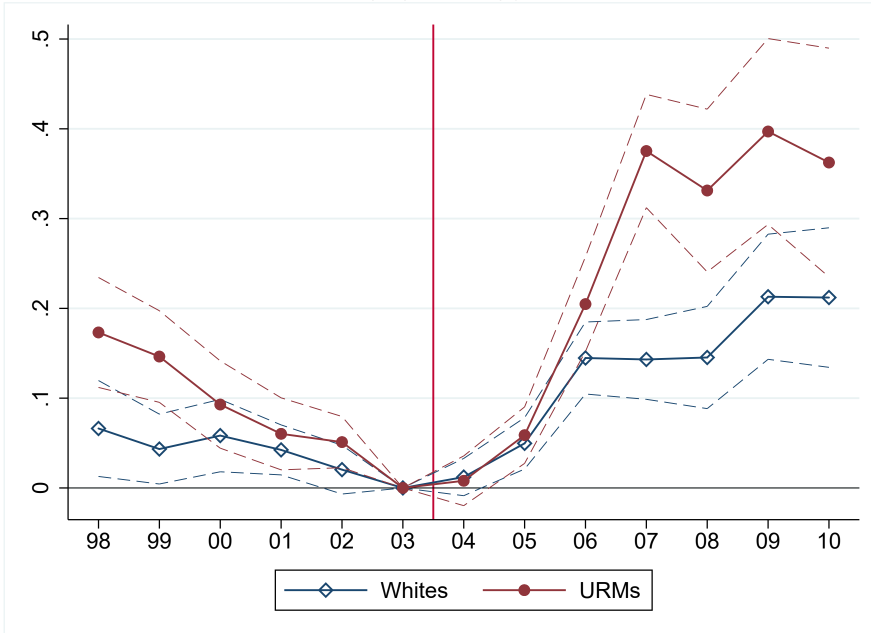
Figure 2: Probability of Applying to Any University



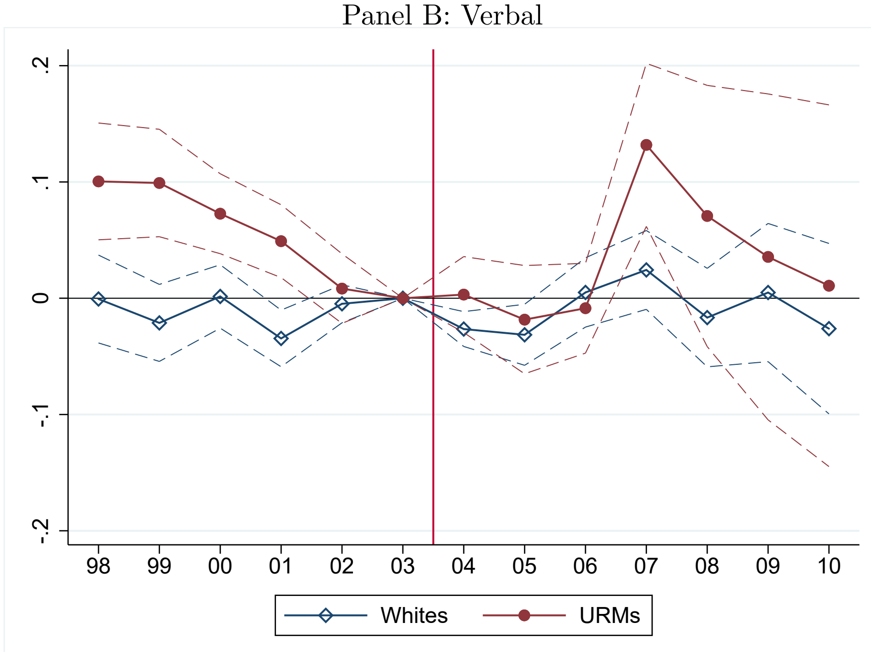
Notes: The outcome is the probability of applying to any university within 4 years after starting 9^{th} grade. Dots indicate coefficients of a regression of the outcome on year dummies interacted with minority status separately for students in the bottom and top quintiles of the ability distribution, where ability is given by deciles of cohort-specific distribution of 6^{th} grade standard test scores. 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.

Figure 3: SAT Scores

Panel A: Math

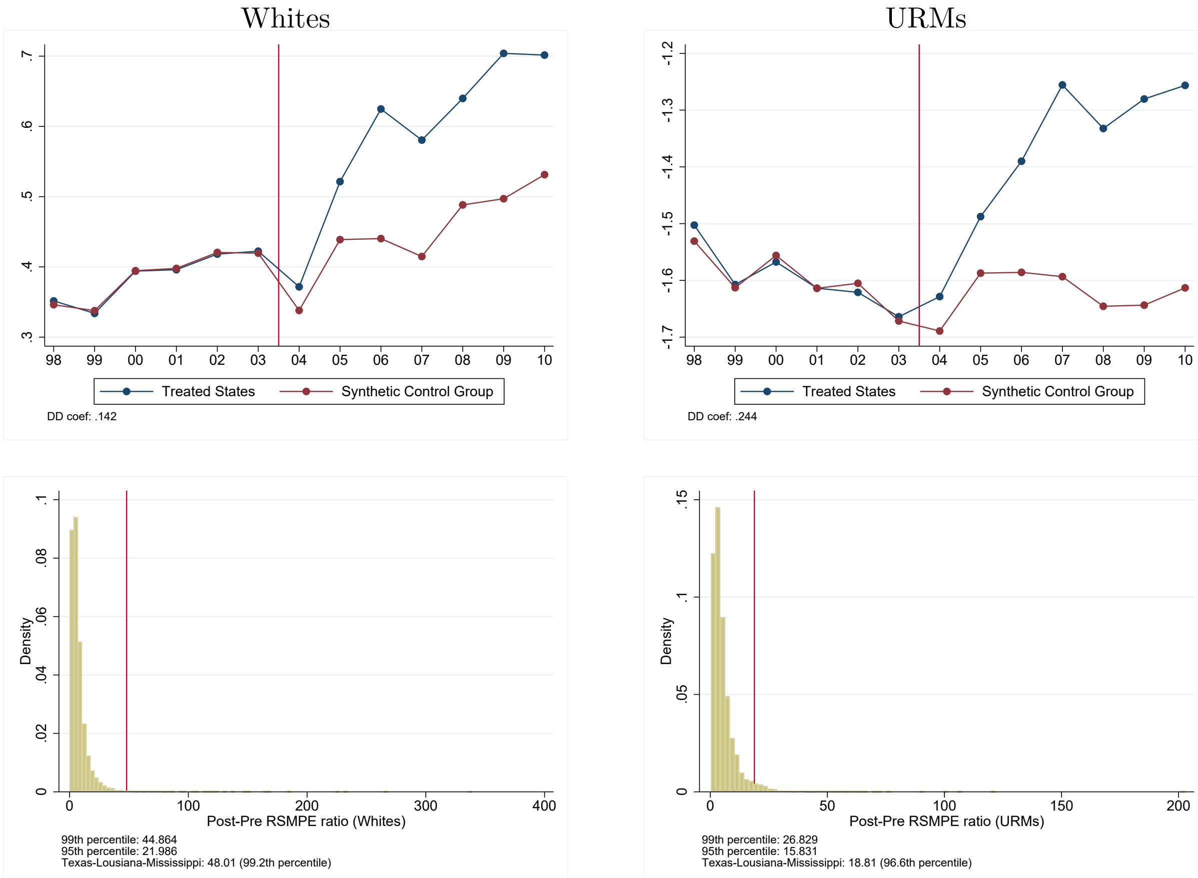


Panel B: Verbal



Notes: The outcome is average 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, separately for white and minority 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.

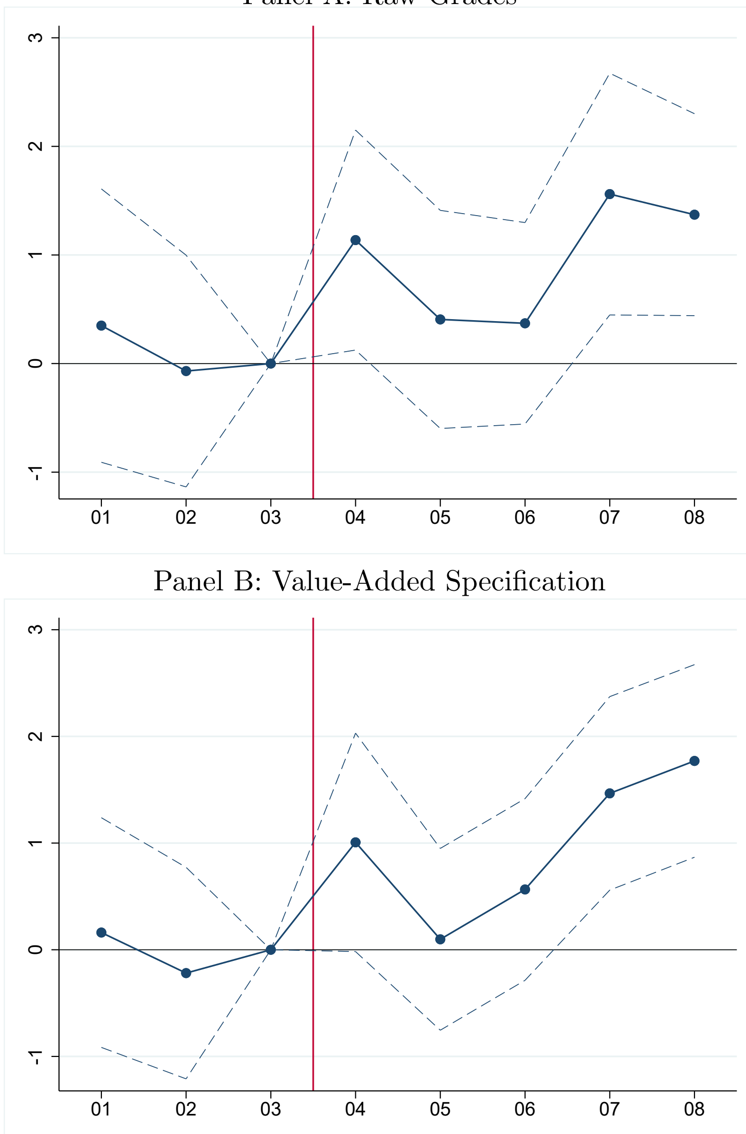
Figure 4: SAT Math Scores: Synthetic Control Approach



Notes: This figure reports synthetic cohort analyses separately for whites and minorities. The top panel shows SAT math scores for the treated states (Texas, Mississippi and Louisiana) and for the synthetic control group. The bottom panel shows the distribution of post/pre RMSPE ratio for placebo estimates. The vertical red line in the bottom panels indicates the post/pre RMSPE ratio for the treated states. 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 minorities, weights on control units are 33.2% (Oregon), 28.4% (New Jersey), 20.6% (California), and 17.8% (Pennsylvania).

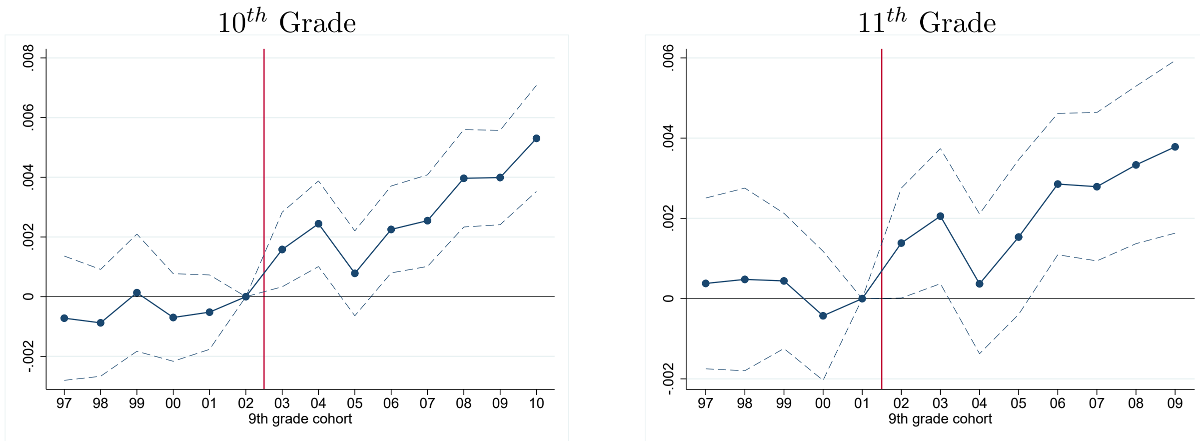
Figure 5: Mean Grades: Raw and Value-Added

Panel A: Raw Grades



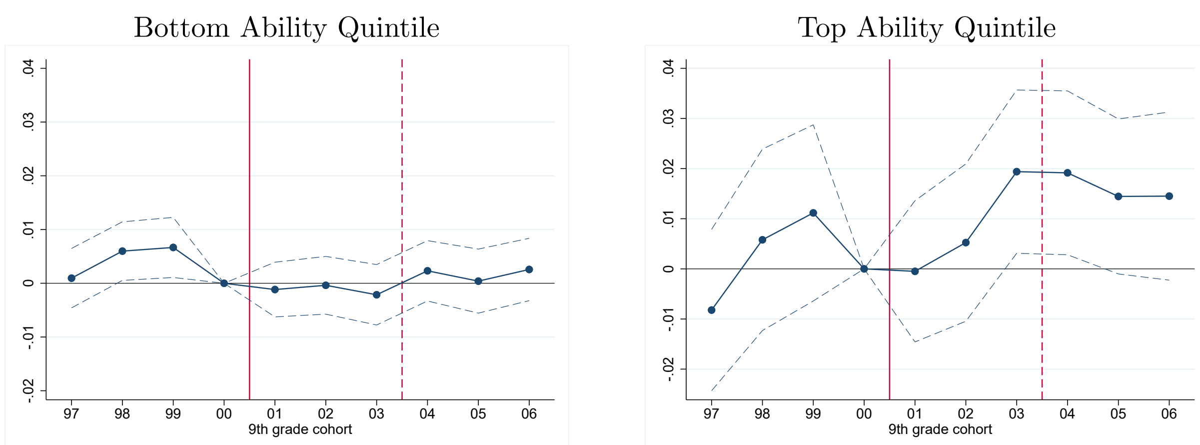
Notes: The outcome in both figures is mean grades in 11^{th} grade. Dots indicate coefficients of regressions of the outcome on year dummies interacted with an indicator variable for minority status. The regression also includes school-by-cohort, race, ZIP code fixed effects, as well as controls for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

Figure 6: Attendance



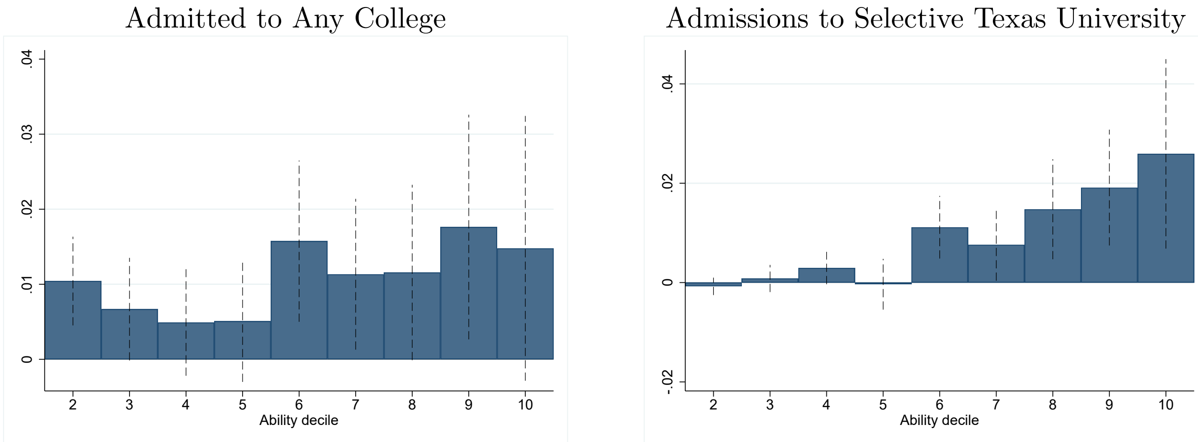
Notes: The outcomes are mean attendance rates. Dots indicate coefficients of a regression of the outcome on year dummies interacted with minority status. All regressions condition on cohort-by-ability, race-by-ability and district-by-ability fixed effects, where ability is given by deciles of cohort-specific distribution of 6^{th} grade standard test scores. Dashed lines show 95% confidence intervals for standard errors clustered at the district level.

Figure 7: College Graduation



Notes: The outcome is the probability of graduating from college. Dots indicate coefficients of regressions of the outcome on year dummies interacted with minority status, separately for students in the bottom and top quintiles of the ability 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.

Figure 8: Returns to Moving Up an Ability Decile in College Admissions



Notes: The outcome in the left graph is admission to any Texas college. The outcome in the right graph is the number of selective Texas schools to which a student is admitted. Bars indicate the coefficients from equation (7), which capture the change in the marginal effect of moving up an ability decile on college admissions. Dashed lines show 95% confidence intervals for standard errors clustered at the district level.

Tables

Table 1: Summary Statistics

	URMs		Whites	
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
Ability (decile)	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
University application rate (within 4 years)	0.1734	0.2583	0.2900	0.3350
Applications to selective universities (within 4 years)	0.0603	0.1012	0.2098	0.2384
College graduation rate	0.1126	0.0969	0.2488	0.2265
District-by-cohort-ability 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 from the Texas Education Agency (TEA) administrative data and the administrative data from a large, urban school district (LUSD). An observation in the TEA data is a district-ability-cohort cell. The LUSD data consists of repeated cross-sections of 11th graders, and an observation is a student.

Table 2: Effect of AA on College Application Behavior

		Percentile of grade 6 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 colleges				
Partial treatment	0.0095***	0.0017	0.0020	0.0022	0.0145**	0.0276***
	(0.0027)	(0.0019)	(0.0025)	(0.0034)	(0.0066)	(0.0086)
Full treatment	0.0190***	0.0016	0.0044*	0.0145***	0.0344***	0.0429***
	(0.0033)	(0.0014)	(0.0025)	(0.0040)	(0.0057)	(0.0099)
Observations (cells)	97121	18380	20681	20974	19960	17126
R^2	0.913	0.492	0.646	0.738	0.798	0.838
Mean dependent variable	0.1584	0.0100	0.0376	0.0941	0.2120	0.4426
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]			0.0032 [8.7649]			
Full treatment: p-value [F-stat]			0.0000 [18.2058]			
		Panel B: Application to any college				
Partial treatment	0.0078***	0.0086***	0.0046	0.0011	0.0086*	0.0222***
	(0.0026)	(0.0027)	(0.0035)	(0.0044)	(0.0052)	(0.0075)
Full treatment	0.0286***	0.0101***	0.0132***	0.0263***	0.0432***	0.0545***
	(0.0035)	(0.0027)	(0.0035)	(0.0051)	(0.0054)	(0.0086)
Observations (cells)	97121	18380	20681	20974	19960	17126
R^2	0.915	0.798	0.824	0.814	0.803	0.781
Mean dependent variable	0.2785	0.0789	0.1595	0.2505	0.3708	0.5330
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]			0.0902 [2.8770]			
Full treatment: p-value [F-stat]			0.0000 [28.8031]			
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

This table reports difference-in-differences estimates of the effect of affirmative action on minorities' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 minority 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 minority. The outcome variable in panel A is the average number of selective colleges students applied to. Standard errors are clustered at the district-level. The outcome variable in Panel B is the fraction of students in a cell that applied to any college.

Table 3: Effect of AA on SAT Scores

	Math	Verbal	# Test takers	% Test takers
	(1)	(2)	(3)	(4)
		Panel A: URMs		
DD coefficient	0.181***	-0.0197	531.8	0.0026
	(0.0340)	(0.0444)	(1162.0)	(0.0053)
Observations (cells)	1904	1901	1904	1116
R^2	0.844	0.795	0.802	0.877
State, year and ethnicity FE	X	X	X	X
		Panel B: Whites		
DD coefficient	0.0940***	0.0006	1546.0	0.0052
	(0.0225)	(0.0222)	(1268.7)	(0.0045)
Observations (cells)	663	663	663	561
R^2	0.968	0.971	0.987	0.978
State, year and ethnicity FE	X	X	X	X
		Panel C: Triple-Difference		
DDD coefficient	0.0901***	0.0274	-379.4	-0.0021
	(0.0198)	(0.0208)	(1071.8)	(0.0025)
Observations (cells)	2555	2552	2555	1677
R^2	0.998	0.998	0.999	0.993
State-by-year FE	X	X	X	X
State-by-ethnicity FE	X	X	X	X
Ethnicity-by-year FE	X	X	X	X

This table reports differences-in-difference and triple-differences effects 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 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 and B, 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 C, the coefficient is on the interaction between being a minority, being tested post 2003, and belonging to a treated state. Standard errors are clustered at the state-level.

Table 4: Effect of AA on School Grades

	All students			Ability distribution		
				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: Bottom tercile = Top tercile						
p-value [F-stat]			0.4412 [0.5948]			
School-by-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-by-year FE			X			
Grade-by-ethnicity FE			X			

This table reports the 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. The reported treated effect is the coefficient on the interaction between being a minority and being observed post 2003. Ability terciles are assigned based on 8th grade scores on the Stanford test. Standard errors are clustered at the school-level.

Table 5: Effect of AA on School Attendance

		Percentile of grade 6 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	0.0036***	0.0045***	0.0024***	0.0039***	0.0036***	0.0035***
	(0.0005)	(0.0012)	(0.0008)	(0.0008)	(0.0005)	(0.0006)
Observations (cells)	97071	18340	20677	20970	19958	17126
R^2	0.757	0.629	0.617	0.597	0.604	0.634
Mean dependent variable	0.9464	0.9238	0.9386	0.9479	0.9561	0.9653
Test: Bottom quintile = Top quintile p-value [F-stat]	0.4208 [0.6488]					
	Panel B: Attendance in grade 11					
Treated	0.0024***	0.0019	0.0012	0.0028***	0.0024***	0.0038***
	(0.0006)	(0.0014)	(0.0009)	(0.0009)	(0.0007)	(0.0006)
Observations (cells)	89849	16910	19120	19438	18532	15849
R^2	0.713	0.577	0.585	0.589	0.607	0.647
Mean dependent variable	0.9405	0.9199	0.9322	0.9409	0.9494	0.9596
Test: Bottom quintile = Top quintile p-value [F-stat]	0.1569 [2.0076]					
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

This table reports difference-in-differences estimates of the effect of affirmative action on minorities' school attendance. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 minority 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 6: Effect of AA on College Completion

		Percentile of grade 6 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: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]				0.0955 [2.7856]		
Full treatment: p-value [F-stat]				0.0592 [3.5714]		
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

This table reports difference-in-differences estimates of the effect of affirmative action on minorities' college graduation. The regressions use the TEA data, and an observation is at the district-cohort-race-ability quintile level. The ability 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 minority 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 minority. The outcome variable is the fraction of students in a cell who completed college. Standard errors are clustered at the district-level.

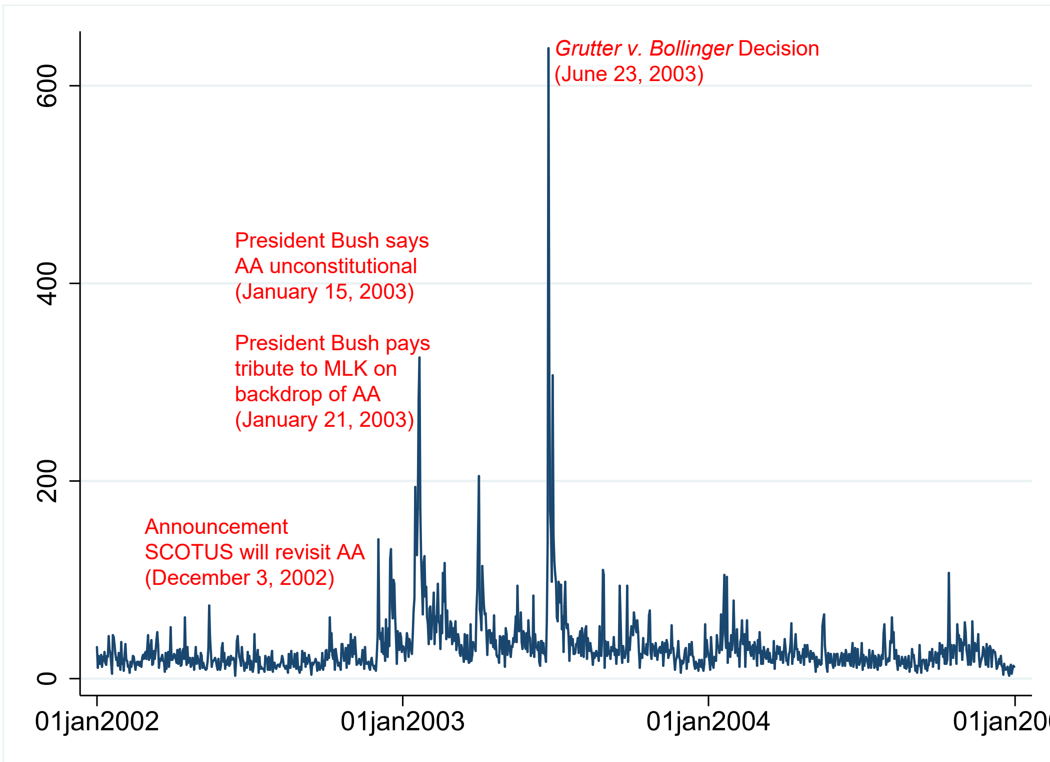
Table 7: Student and Parent Behavior and Affirmative Action

	(1) Time on Homework	(2) Applied to First Choice College	(3) Parental Involvement	(4) Guidance From Counselor
$Minority \times Post2003$	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

This table presents 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 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. Standard errors are heteroskedasticity robust.

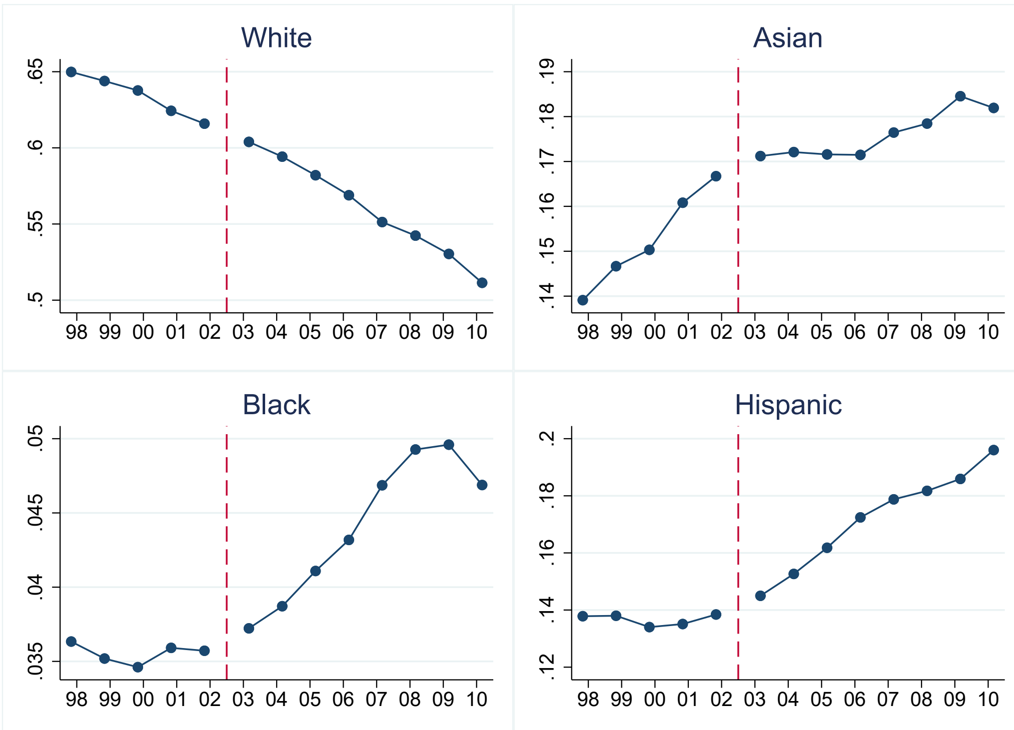
Appendix Figures

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



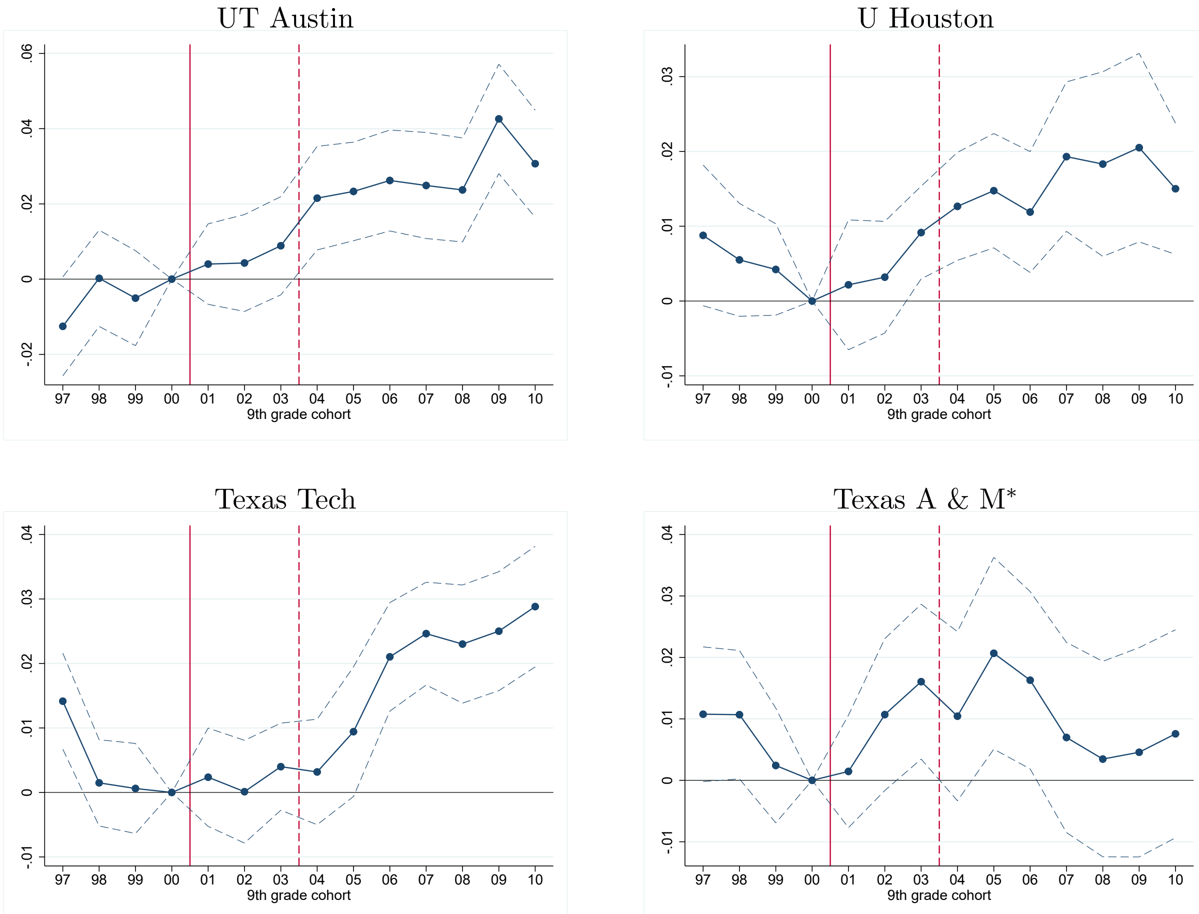
Notes: This figure reports the number of US newspaper articles by day that contained the phrase "affirmative action" on newslibrary.com.

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: Average Admissions to Selective Institutions

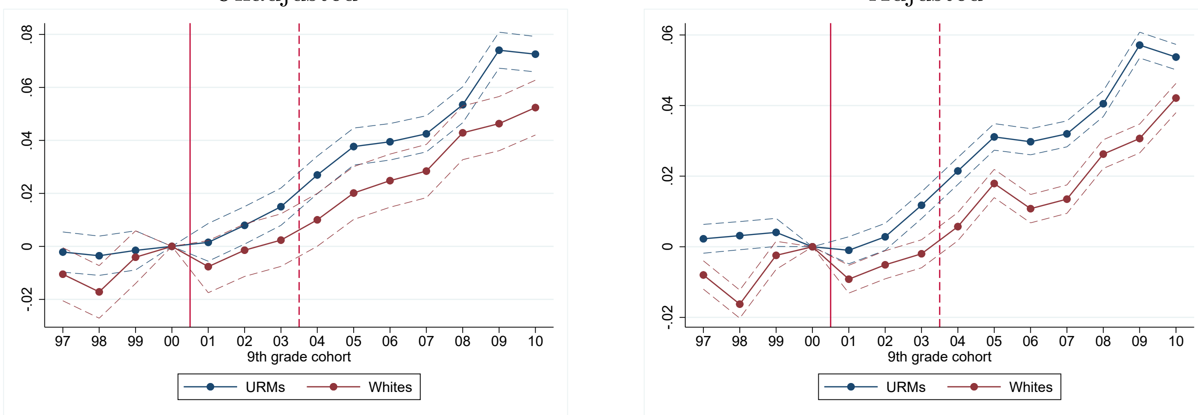


Notes: This figure reports event study graphs for the probability of a minority student receiving admissions to each institution relative to a white student by students' 9th grade cohort. The regressions use the TEA data. Dotted lines report 95% confidence intervals. *Texas A & M publicly announced that it would *not* use race (Parker, 2018).

Figure A4: Trends in College Application Behavior

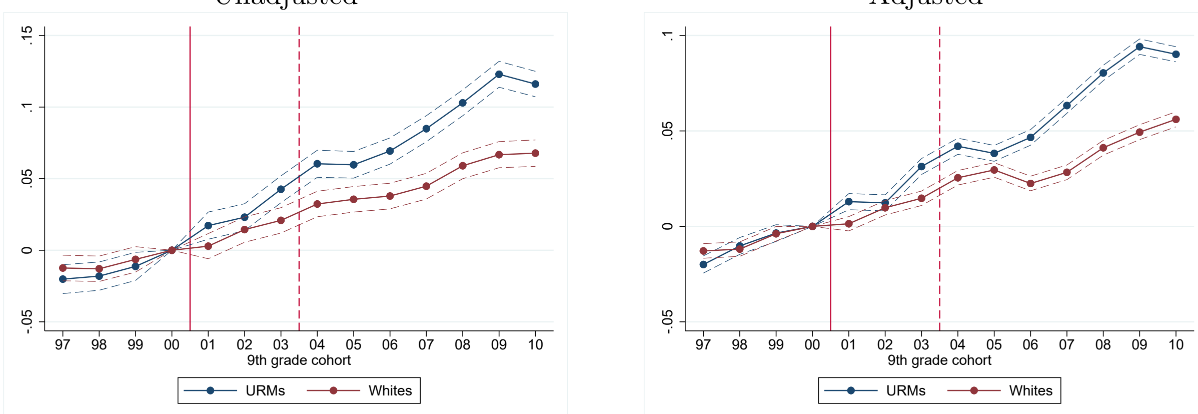
Panel A: Number of Applications to Selective Universities

Unadjusted | Adjusted



Panel B: Probability of Applying to Any University

Unadjusted | Adjusted

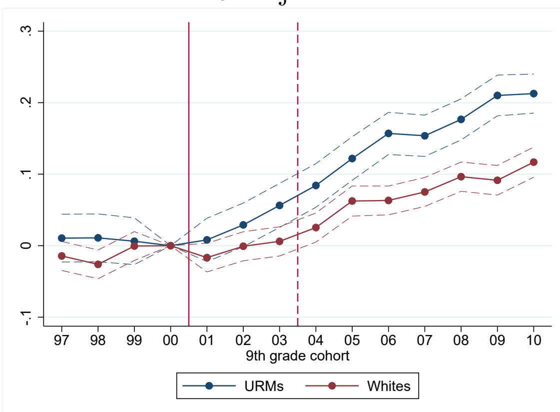


Notes: This figure reports trends in college application behavior in our analytical sample. Time series are normalized relative to base cohort 2000. Unadjusted figures directly plot raw averages. Adjusted figures are residuals from regressions on individual characteristics, race-by-ability fixed effects and district-by-ability fixed effects.

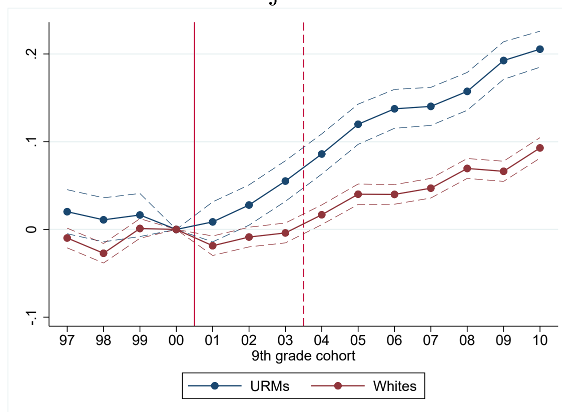
Figure A5: Trends in College Application Behavior: Top Ability Quintile

Panel A: Number of Applications to Selective Universities

Unadjusted

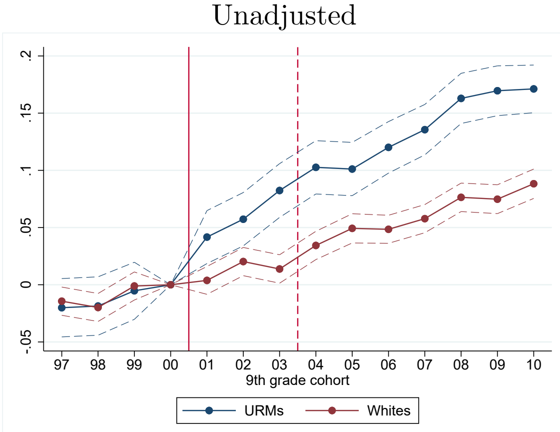


Adjusted

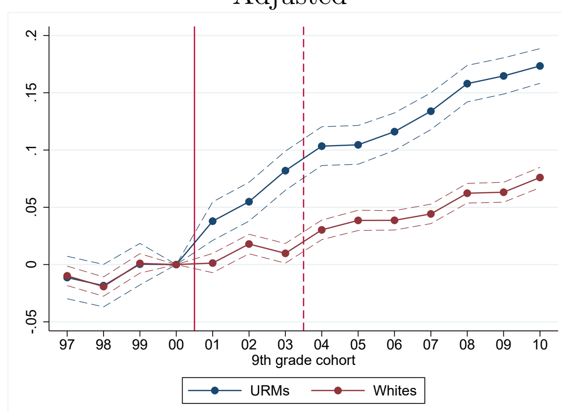


Panel B: Probability of Applying to Any University

Unadjusted



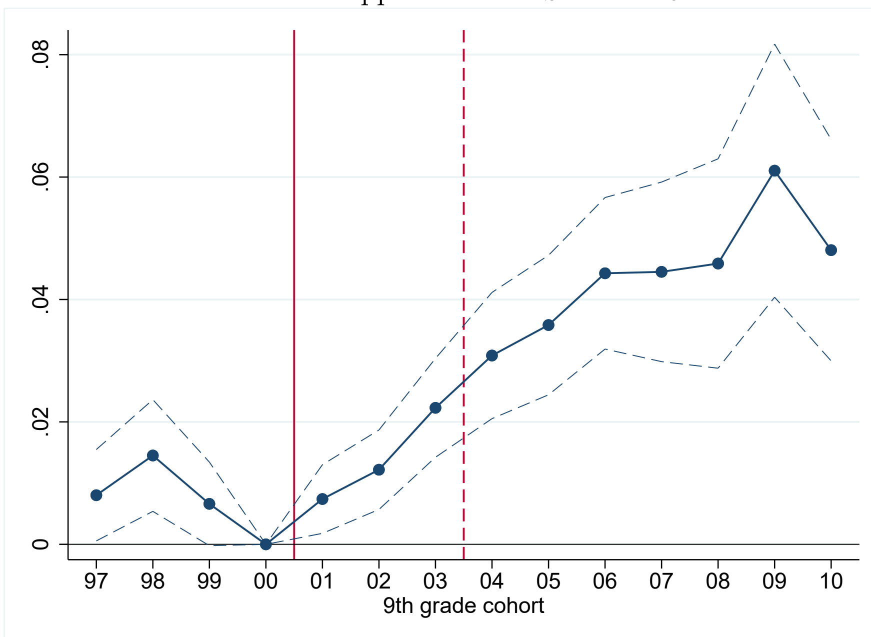
Adjusted



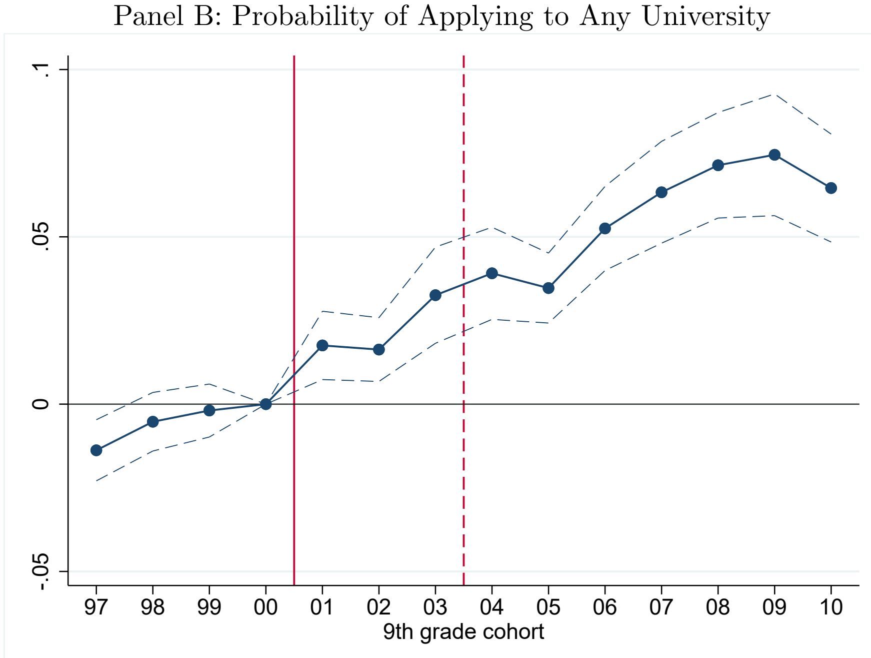
Notes: This figure reports trends in college application behavior in our analytical sample. Time series are normalized relative to base cohort 2000. Unadjusted figures directly plot raw averages. Adjusted figures are residuals from regressions on individual characteristics, race-by-ability fixed effects and district-by-ability fixed effects.

Figure A6: Event-Study: College Application Behavior

Panel A: Number of Applications to Selective Universities

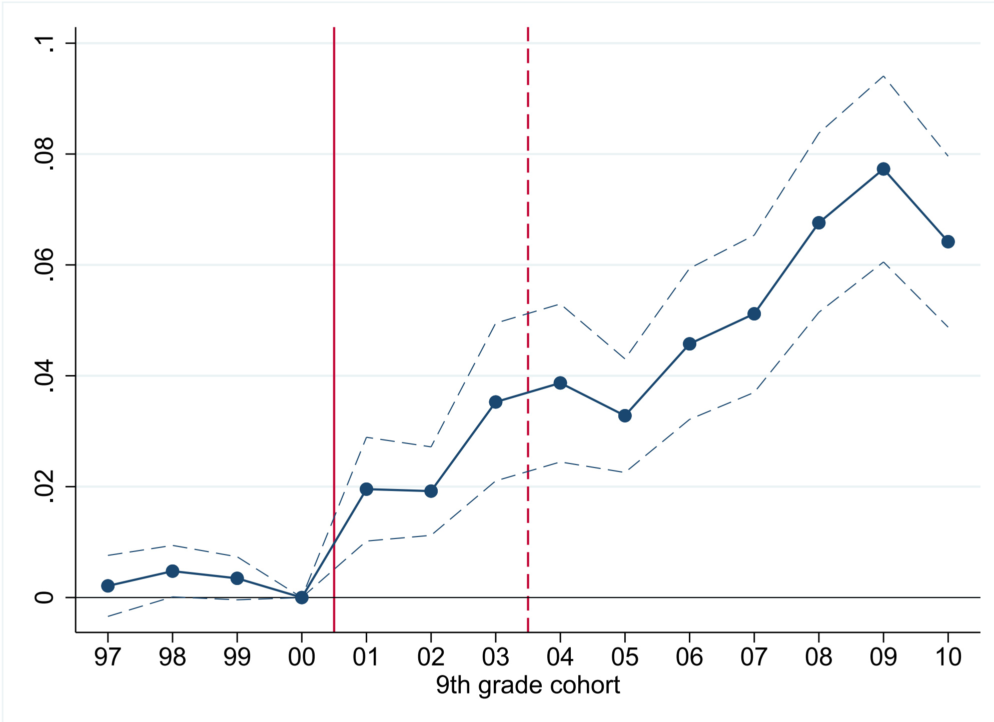


Panel B: Probability of Applying to Any University



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 university in Panel B. Dots indicate coefficients of a regression of the outcome on year dummies interacted with minority status. All regressions condition on cohort-by-ability, race-by-ability and district-by-ability fixed effects, as well as means of individual characteristics, where ability is given by deciles of cohort-specific distribution of 6^{th} grade standard test scores. Dashed lines show 95% confidence intervals for standard errors clustered at the district level.

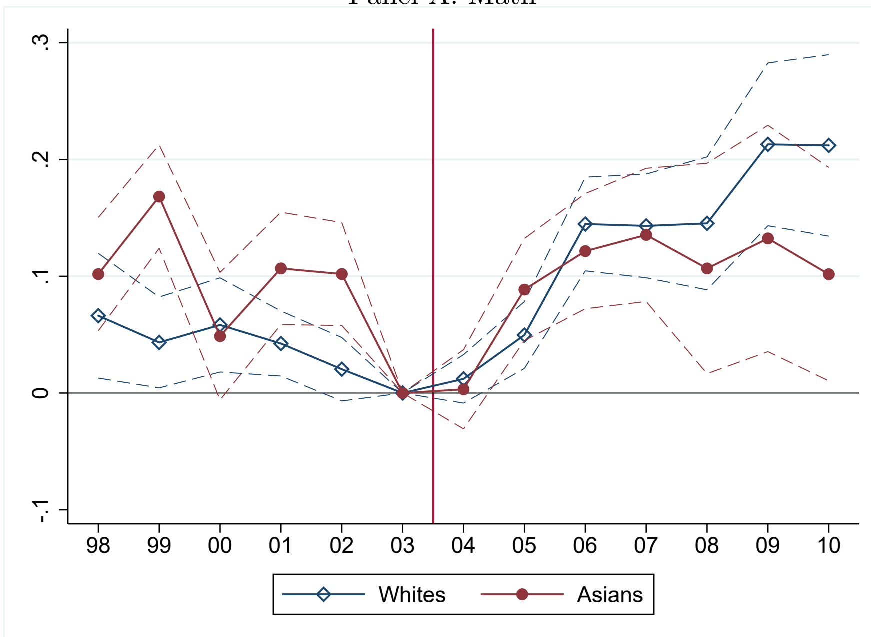
Figure A7: Probability of Applying to Any UT Institution



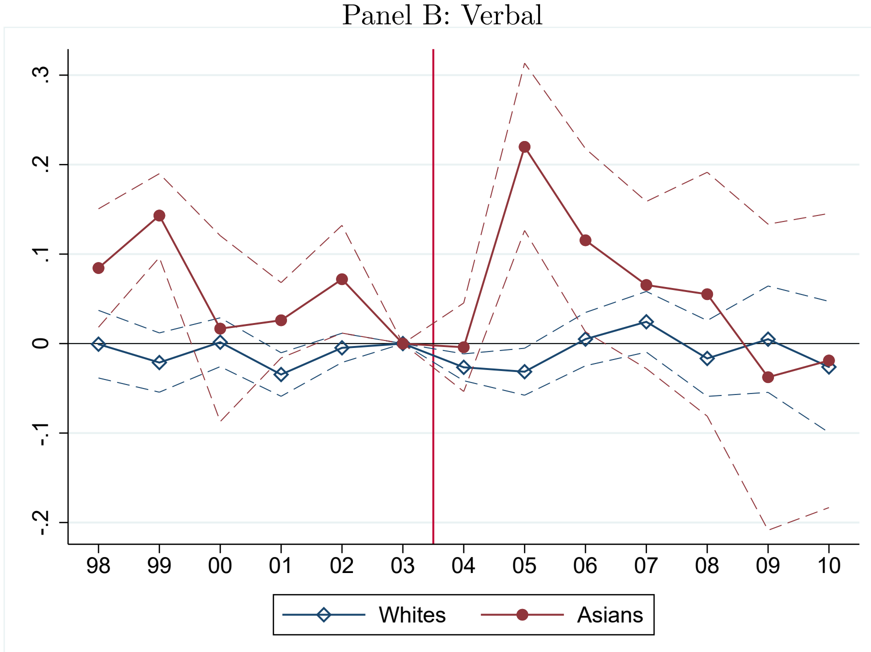
Notes: The outcome is the probability of applying to any university in the University of Texas System within 4 years after starting 9^{th} grade. Dots indicate coefficients of a regression of the outcome on year dummies interacted with minority status. All regressions condition on cohort-by-ability, race-by-ability and district-by-ability fixed effects, where ability is given by deciles of 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 A8: SAT Scores – Asians and Whites

Panel A: Math



Panel B: Verbal

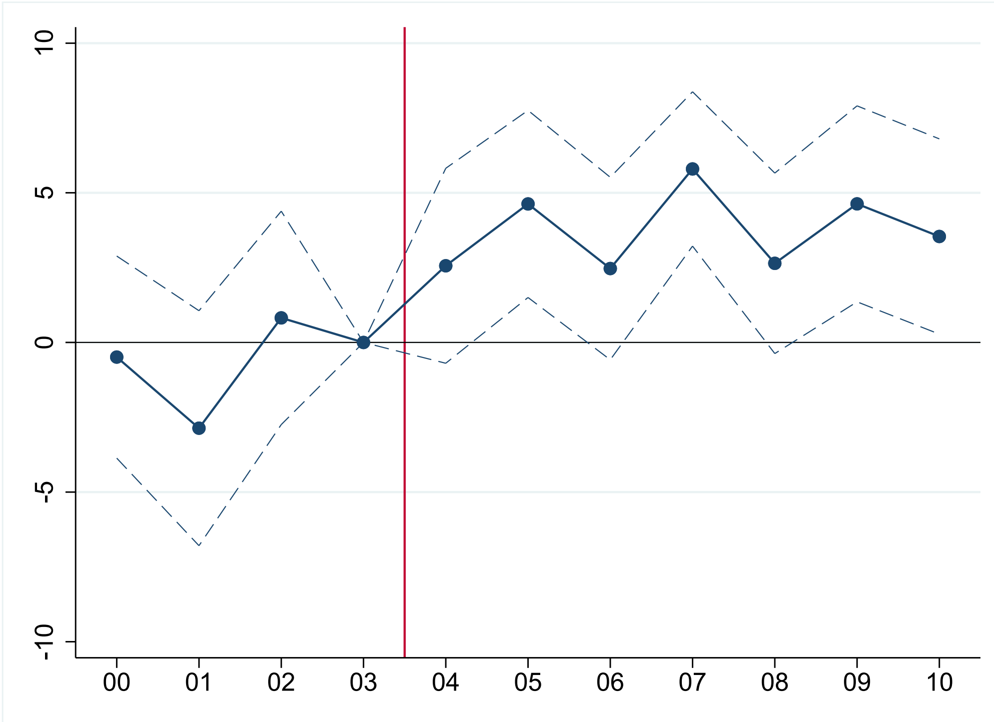


Notes: The outcome is the average SAT scores. Dots indicate coefficients of regressions of the outcome on year dummies interacted with an indicator variable for the three treated states, separately for asian and white students. Cells are weighted by the number of SAT test takers. Dashed lines show 95%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

Figure A9: Differences in SAT math scores: Synthetic Control Approach

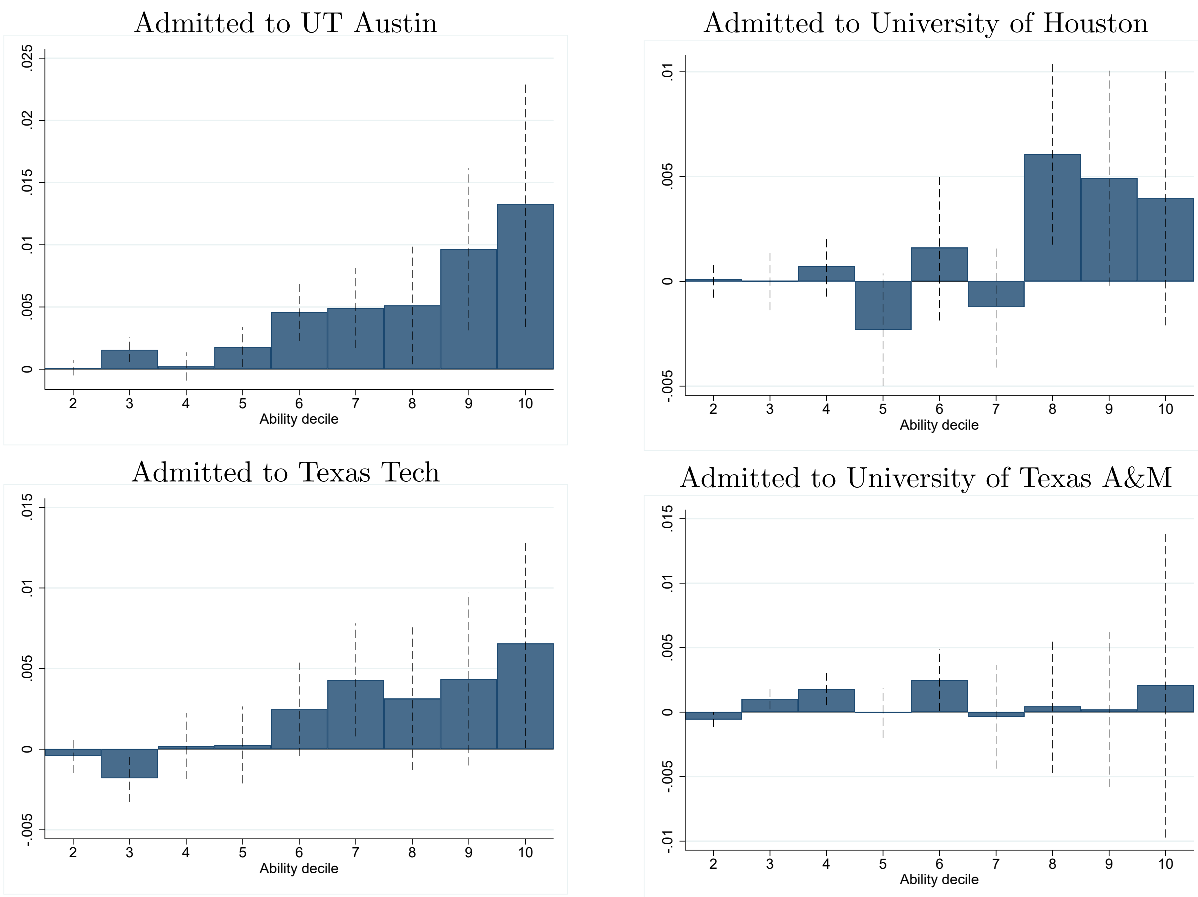
Notes: This figure reports differences in SAT math scores between treated states and synthetic control groups, separately for minorities and White students.

Figure A10: Mean Stanford Scores



Notes: The outcome is the mean percentile rank on the Stanford test in 11^{th} grade. Dots indicate coefficients from a regression of the outcome on year dummies interacted with an indicator variable for minority status. The regression also includes school-by-cohort, race, 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.

Figure A11: Returns to Moving Up an Ability Decile on Admissions to Specific Texas Universities



Notes: The outcomes are admissions to each of the 4 Texas universities. Bars indicate the coefficients from equation (7), which capture the change in the marginal effect of moving up an ability decile on college admissions. Dashed lines show 95% confidence intervals for standard errors clustered at the district level.

Appendix Tables

Table A1: Summary Statistics for SAT Data

	URMs		Whites	
Years	1998-2003	2004-2010	1998-2003	2004-2010
Verbal scores (mean)	440.9	441.7	527.7	528.4
Verbal scores (standard deviation)	21.5	21.7	18.7	19.8
Math scores (mean)	438.7	443.4	530.1	534.7
Math scores (standard deviation)	23.9	23.7	20.2	19.0
Number of cells	878	1,026	306	357
Number of SAT takers	1,194,067	2,159,747	4,136,869	5,634,200

Notes: This table reports summary statistics for the SAT data. An observation is a race-year-state cell.

Table A2: Summary Statistics for THEOP Survey Data

Panel A: Summary Statistics	Full Sample		Whites		Minorities	
	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
Minorities	7,532
Students in 2002	11,098
Students in 2004	2,840

Notes: This table presents 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 College Application Behavior – Black Students

	All students	Percentile of grade 6 test score distribution: Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Application to any college					
Partial treatment	0.0240***	0.0184***	0.0276***	0.0129	0.0225***	0.0581***
	(0.0039)	(0.0040)	(0.0054)	(0.0085)	(0.0084)	(0.0116)
Full treatment	0.0414***	0.0220***	0.0359***	0.0397***	0.0583***	0.0745***
	(0.0044)	(0.0039)	(0.0059)	(0.0068)	(0.0090)	(0.0139)
Observations (cells)	64017	10546	12868	13882	13963	12758
R^2	0.910	0.716	0.771	0.781	0.789	0.791
Mean dependent variable	0.3152	0.0857	0.1640	0.2534	0.3731	0.5374
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.0012 [10.5951]					
Full treatment: p-value [F-stat]	0.0002 [14.4416]					
	Panel B: Applications to selective colleges					
Partial treatment	0.0181***	0.0040	0.0093**	0.0100	0.0216**	0.0679***
	(0.0041)	(0.0030)	(0.0045)	(0.0070)	(0.0104)	(0.0145)
Full treatment	0.0270***	0.0062***	0.0121***	0.0254***	0.0515***	0.0654***
	(0.0052)	(0.0022)	(0.0038)	(0.0059)	(0.0107)	(0.0197)
Observations (cells)	64017	10546	12868	13882	13963	12758
R^2	0.922	0.522	0.664	0.754	0.819	0.860
Mean dependent variable	0.2070	0.0153	0.0482	0.1112	0.2348	0.4681
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.0000 [18.6942]					
Full treatment: p-value [F-stat]	0.0019 [9.6814]					
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on blacks' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 black students 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 an indicator for black students. The outcome variable in Panel A is the fraction of students in a cell that applied to any college. For Panel B, it is the average number of selective colleges students applied to. Standard errors are clustered at the district-level.

Table A4: Effect of AA on College Application Behavior – Hispanic Students

		Percentile of grade 6 test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Application to any college					
Partial treatment	0.0000	0.0037	-0.0072**	-0.0041	0.0013	0.0095
	(0.0030)	(0.0028)	(0.0035)	(0.0042)	(0.0060)	(0.0087)
Full treatment	0.0217***	0.0010	0.0021	0.0206***	0.0377***	0.0482***
	(0.0041)	(0.0028)	(0.0031)	(0.0059)	(0.0061)	(0.0103)
Observations (cells)	81946	14155	16963	17864	17493	15471
R^2	0.927	0.847	0.845	0.830	0.821	0.802
Mean dependent variable	0.2783	0.0668	0.1408	0.2339	0.3609	0.5309
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.5222 [0.4100]					
Full treatment: p-value [F-stat]	0.0000 [22.1933]					
	Panel B: Applications to selective colleges					
Partial treatment	0.0045	-0.0003	-0.0018	-0.0022	0.0102	0.0128
	(0.0029)	(0.0019)	(0.0024)	(0.0033)	(0.0074)	(0.0101)
Full treatment	0.0141***	-0.0019	0.0007	0.0102**	0.0272***	0.0349***
	(0.0036)	(0.0016)	(0.0024)	(0.0045)	(0.0059)	(0.0107)
Observations (cells)	81946	14155	16963	17864	17493	15471
R^2	0.925	0.538	0.687	0.778	0.826	0.857
Mean dependent variable	0.1662	0.0077	0.0327	0.0888	0.2095	0.4441
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.1878 [1.7374]					
Full treatment: p-value [F-stat]	0.0005 [12.3546]					
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on Hispanics' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 Hispanic students 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 an indicator for Hispanic students. The outcome variable in Panel A is the fraction of students in a cell that applied to any college. For Panel B, it is the average number of selective colleges students applied to. Standard errors are clustered at the district-level.

Table A5: Effect of AA on Applications to University of Texas System

	All students	Percentile of grade 6 test score distribution: Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Application to any UT					
Partial treatment	0.0029	0.0030**	-0.0025	0.0020	-0.0004	0.0145**
	(0.0019)	(0.0014)	(0.0023)	(0.0030)	(0.0043)	(0.0057)
Full treatment	0.0152***	0.0040**	0.0083***	0.0161***	0.0191***	0.0291***
	(0.0028)	(0.0016)	(0.0017)	(0.0038)	(0.0048)	(0.0065)
Observations (cells)	97121	18380	20681	20974	19960	17126
R^2	0.911	0.857	0.886	0.890	0.875	0.855
Mean dependent variable	0.1191	0.0280	0.0613	0.0991	0.1534	0.2549
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]				0.0587 [3.5847]		
Full treatment: p-value [F-stat]				0.0001 [15.6335]		
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on minorities' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 minority 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 minority. The outcome variable is the fraction of students in a cell that applied to any UT institution (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.

Table A6: Effect of AA on College Application Behavior – Exogenous Ability Sample

		Percentile of grade 6 test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Application to any college					
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: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.0552 [3.6870]					
Full treatment: p-value [F-stat]	0.0000 [23.2199]					
	Panel B: Applications to selective colleges					
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: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]	0.0013 [10.4874]					
Full treatment: p-value [F-stat]	0.0000 [17.9979]					
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

Note: This table reports difference-in-differences estimates of the effect of affirmative action on minorities' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 sample excludes the 2007-2010 cohorts. Partial treatment is the coefficient on the interaction between an indicator for being a minority 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 minority. The outcome variable in Panel A is the fraction of students in a cell that applied to any college. For Panel B, it is the average number of selective colleges students applied to. Standard errors are clustered at the district-level.

Table A7: Effect of AA on Stanford Test Scores

		Ability distribution		
	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)	(1.5648)	(1.4314)
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: Bottom tercile = Top tercile				
p-value [F-stat]		0.0981 [2.7535]		
School-by-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 a large, urban school district. An observation is a student, and the sample consists of repeated cross-sections of 11th graders. The reported treated effect is the coefficient on the interaction between being a minority and being observed post 2003. Ability terciles are assigned based on 8th grade scores on the Stanford test. Standard errors are clustered at the school-level.

Table A8: Effect of AA on School Grades (Math)

	All students			Ability distribution		
				Bottom tercile	Middle tercile	Top tercile
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.7389*	0.7274*	0.2932	0.2845	0.4446	1.7302***
	(0.4263)	(0.4293)	(0.4272)	(0.6580)	(0.5309)	(0.6590)
Lagged dep. var. (grade 8)		0.4538***				
		(0.0112)				
Observations	55595	41724	83590	14314	14641	13947
R^2	0.148	0.228	0.729	0.136	0.156	0.162
Mean dependent variable	76.12	76.68	79.07	72.67	76.52	81.19
S.D. dependent variable	10.79	10.11	9.41	9.66	9.39	9.54
Test: Bottom tercile = Top tercile						
p-value [F-stat]			0.0753 [3.1850]			
School-by-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-by-year FE			X			
Grade-by-ethnicity FE			X			

Notes: This table reports the difference-in-differences estimates of the effect of affirmative action on math grades in a large urban school district. An observation is a student, and the sample consists of repeated cross-sections of 11th graders. The reported treated effect is the coefficient on the interaction between being a minority and being observed post 2003. Ability terciles are assigned based on 8th grade scores on the Stanford test. Standard errors are clustered at the school-level.

Table A9: Effect of AA on School Grades (English)

	All students			Ability distribution		
				Bottom tercile	Middle tercile	Top tercile
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	1.1597***	1.5617***	1.6601***	1.4907**	0.7296	1.3799***
	(0.4225)	(0.4414)	(0.4032)	(0.6882)	(0.5912)	(0.5035)
Lagged dep. var. (grade 8)		0.3521***				
		(0.0098)				
Observations	58649	43522	87197	15058	15255	14503
R^2	0.200	0.234	0.713	0.188	0.195	0.169
Mean dependent variable	79.03	79.93	81.76	76.02	79.90	83.61
S.D. dependent variable	10.38	9.47	8.90	9.66	8.95	8.35
Test: Bottom tercile = Top tercile						
p-value [F-stat]			0.8893 [0.0194]			
School-by-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-by-year FE			X			
Grade-by-ethnicity FE			X			

Notes: This table reports the difference-in-differences estimates of the effect of affirmative action on English grades in a large urban school district. An observation is a student, and the sample consists of repeated cross-sections of 11th graders. The reported treated effect is the coefficient on the interaction between being a minority and being observed post 2003. Ability terciles are assigned based on 8th grade scores on the Stanford test. Standard errors are clustered at the school-level.

Table A10: Effect of AA on College Application Behavior – Excluding Houston & Dallas

		Percentile of grade 6 test score distribution				
	All students	Bottom quintile	2nd quintile	3rd quintile	4th quintile	Top quintile
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Application to any college					
Partial treatment	0.0079***	0.0096***	0.0037	0.0013	0.0092*	0.0226***
	(0.0027)	(0.0027)	(0.0036)	(0.0046)	(0.0055)	(0.0073)
Full treatment	0.0295***	0.0106***	0.0118***	0.0280***	0.0449***	0.0581***
	(0.0034)	(0.0027)	(0.0035)	(0.0051)	(0.0054)	(0.0082)
Observations (cells)	96281	18212	20513	20806	19792	16958
R^2	0.913	0.792	0.817	0.807	0.799	0.779
Mean dependent variable	0.2808	0.0768	0.1564	0.2480	0.3700	0.5345
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]			0.1097 [2.5643]			
Full treatment: p-value [F-stat]			0.0000 [35.7148]			
	Panel B: Applications to selective colleges					
Partial treatment	0.0094***	0.0025	0.0008	0.0020	0.0146**	0.0295***
	(0.0028)	(0.0018)	(0.0024)	(0.0036)	(0.0070)	(0.0087)
Full treatment	0.0213***	0.0022*	0.0041*	0.0162***	0.0381***	0.0495***
	(0.0028)	(0.0013)	(0.0025)	(0.0038)	(0.0052)	(0.0093)
Observations (cells)	96281	18212	20513	20806	19792	16958
R^2	0.911	0.470	0.623	0.720	0.790	0.835
Mean dependent variable	0.1598	0.0094	0.0356	0.0910	0.2092	0.4422
Test: Bottom quintile = Top quintile						
Partial treatment: p-value [F-stat]			0.0029 [8.9553]			
Full treatment: p-value [F-stat]			0.0000 [25.4628]			
Demographic controls	X	X	X	X	X	X
District-by-cohort-by-ability FE	X	X	X	X	X	X
District-by-ethnicity-by-ability FE	X	X	X	X	X	X

Notes: This table reports difference-in-differences estimates of the effect of affirmative action on minorities' college application behavior. The regressions use the TEA data, an observation is at the district-cohort-race-ability quintile level, where ability 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 sample excludes the Houston Independent School District and the Dallas Independent School District. Partial treatment is the coefficient on the interaction between an indicator for being a minority 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 minority. The outcome variable in Panel A is the fraction of students in a cell that applied to any college. For Panel B, it is the average number of selective colleges students applied to. Standard errors are clustered at the district-level.

SAT Robustness Appendix

A Policy changes in non-treated states

Several states implemented affirmative action bans in university admissions throughout our study period. The states of 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. Shortly thereafter, Florida implemented a very aggressive "percent plan" whereas students in the top 20% of their high school graduating class are guaranteed admission to a state public university.[33]

In this section, we verify that our difference-in-differences estimates of the effect of *Grutter v. Bollinger* on students in Texas, Louisiana and Mississippi are not confounded by these affirmative action bans. Results for math SAT scores are shown in Table A11 and, for completeness, corresponding results for verbal scores are shown in Table A12. Column (1) reports our baseline estimates. In column (2), we explicitly control for the effect of affirmative action bans by including a dummy that takes a value of 1 for post-AA bans years in the associated states.[34] Our estimates of the effect of *Grutter v. Bollinger* on whites (0.092sd) and on minorities (0.182sd) are unaffected by the inclusion of an indicator for AA bans as a control variable. Controlling for AA bans yields a slightly higher triple-difference estimate of 0.0942sd. The coefficient on the AA ban indicator is statistically insignificant in both difference-in-differences specifications. In the triple-difference model, the coefficient on the AA ban indicator interacted with a minority dummy is positive and significant (0.13sd, s.e. 0.261), which would suggest that white students' SAT scores decreased relative to minority students' in the states that implemented bans on affirmative action. However, we warrant against interpreting this coefficient as the causal effect of AA bans. As Hinrichs (2012) points out, the effect of affirmative action bans cannot be disentangled from the effect of percent plans as these 2 policies are generally enacted concomitantly. This is particularly true in Florida, the one state that is largely driving most of the variation in the AA ban indicator given its population size, and the fact that Michigan and Nebraska are both ACT states, hence have fewer SAT test takers and therefore receive less weight in our regressions. In

[33]Arizona (2010), New Hampshire (2011) and Oklahoma (2012) later also banned affirmative action in college admissions.

[34]The indicator turns on in 1999 in Washington, in 2000 in Florida, in 2007 in Michigan, and in 2009 in Nebraska. It takes a value of zero in all years for all others states.

column (3) of Table A11, we then drop the four states that moved to banning affirmative action between 1998 and 2010 from the estimating sample. Again, our estimates of the effect of the re-instatement of affirmative action is very robust. Finally, in column (4), we drop Louisiana and Mississippi since these two states somewhat continued to use race in university admissions in 1998-2003 despite the *Hopwood v. Texas* ruling (Hinrichs, 2012). Dropping these two treated states does not materially affect our estimates. In our main specification, the bulk of the weight among treated states is put on Texas because it is the only SAT state among the three (Louisiana and Mississippi are both ACT states).

Table A11: Effect of AA on Math SAT Scores: Controlling for AA Bans

	Baseline	Control for AA bans	Drop AA ban states	Drop Mississippi and Louisiana
	(1)	(2)	(3)	(4)
	Panel A: URMs			
DD coefficient	0.181***	0.182***	0.184***	0.183***
	(0.0340)	(0.0350)	(0.0384)	(0.0338)
AA Ban Indicator		0.0071		
		(0.0611)		
Observations (cells)	1904	1904	1748	1830
R^2	0.844	0.844	0.839	0.844
State, year and ethnicity FE	X	X	X	X
	Panel B: Whites			
DD coefficient	0.0940***	0.0918***	0.0870***	0.103***
	(0.0225)	(0.0223)	(0.0231)	(0.0195)
AA Ban Indicator		-0.0744		
		(0.0605)		
Observations (cells)	663	663	611	637
R^2	0.968	0.969	0.967	0.968
State, year and ethnicity FE	X	X	X	X
	Panel C: Triple-Difference			
DDD coefficient	0.0901***	0.0942***	0.0964***	0.0880***
	(0.0198)	(0.0188)	(0.0185)	(0.0195)
AA Ban Indicator × Minority dummy		0.130***		
		(0.0261)		
Observations (cells)	2555	2555	2347	2455
R^2	0.998	0.998	0.998	0.998
State-by-year FE	X	X	X	X
State-by-ethnicity FE	X	X	X	X
Ethnicity-by-year FE	X	X	X	X

This table reports differences-in-difference 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 and B, 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 C, the coefficient is on the interaction between being a minority, being tested post 2003, and belonging to a treated state. Standard errors are clustered at the state-level.

Table A12: Effect of AA on Verbal SAT Scores: Controlling for AA Bans

	Baseline	Control for AA bans	Drop AA ban states	Drop Mississippi and Louisiana
	(1)	(2)	(3)	(4)
	Panel A: URMs			
DD coefficient	-0.0197	-0.0160	-0.00430	-0.0200
	(0.0444)	(0.0451)	(0.0487)	(0.0446)
AA Ban Indicator		0.104*		
		(0.0572)		
Observations (cells)	1901	1901	1745	1828
R^2	0.795	0.796	0.788	0.793
State, year and ethnicity FE	X	X	X	X
	Panel B: Whites			
DD coefficient	0.0006	0.0009	0.00002	-0.0009
	(0.0222)	(0.0224)	(0.0241)	(0.0222)
AA Ban Indicator		0.0081		
		(0.0803)		
Observations (cells)	663	663	611	637
R^2	0.971	0.971	0.970	0.970
State, year and ethnicity FE	X	X	X	X
	Panel C: Triple-Difference			
DDD coefficient	0.0274	0.0318	0.0364**	0.0292
	(0.0208)	(0.0190)	(0.0160)	(0.0211)
AA Ban Indicator $\times$ Minority dummy		0.142***		
		(0.0373)		
Observations (cells)	2552	2552	2344	2453
R^2	0.998	0.998	0.998	0.998
State-by-year FE	X	X	X	X
State-by-ethnicity FE	X	X	X	X
Ethnicity-by-year FE	X	X	X	X

This table reports differences-in-difference 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 and B, 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 C, the coefficient is on the interaction between being a minority, being tested post 2003, and belonging to a treated state. Standard errors are clustered at the state-level.

B Synthetic control specification

In our baseline synthetic control results, we find an appropriate control group by minimizing the mean squared prediction errors in 1998-2003 using the following set of variables as predictors: number white SAT test takers, number of minority SAT test takers, white math SAT scores, minority math SAT scores, white verbal SAT scores, and minority verbal SAT scores. Each of these variables is averaged over 1998-2000 and over 2001-2003 in the matching

Here, we verify that our results are robust to using fewer pre-treatment years in the construction of the synthetic control group. Figure A12 shows time series of math SAT scores for synthetic control groups based on 4, 5 and 6 years of pre-treatment data.[35]

For whites, the results are insensitive to model specification, with the 3 synthetic groups tracking each other very closely. Interestingly, when using fewer years of pre-treatment data to construct the synthetic control group, the gap between treated and untreated states during pre-treatment years remain very small, even in years that were not used in the construction 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 (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%).

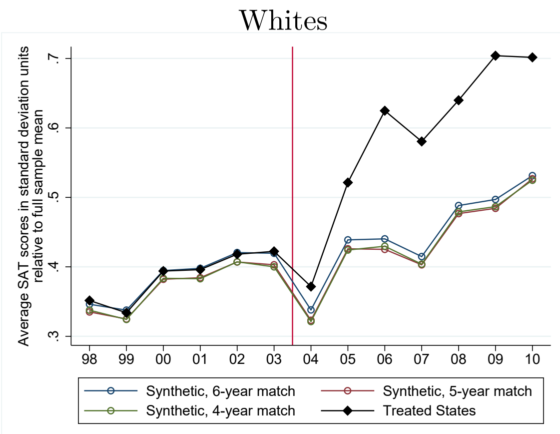
SAT scores of minorities are more volatile and, as a result, the composition of the synthetic control group is more sensitive to the number of pre-treatment years over which MSPE are minimized. While the SAT scores of the 6-year match synthetic control group track our treated group very closely for all 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 three synthetic groups converge, with SAT scores significantly below that 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%).

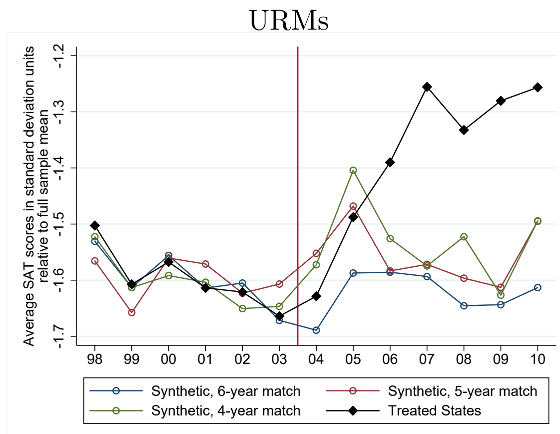
As a final robustness check, we drop from the donor pool the four states that banned affirmative action between 1998 and 2010 (Florida, Nebraska, Michigan and Washington). Results are shown in Figure A13. In the pre-treatment years, the synthetic control group tracks the treated states fairly closely, albeit with more volatility than under our baseline

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

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 minorities are California (70.8%), Pennsylvania (20.1%), and New Hampshire (9.1%).

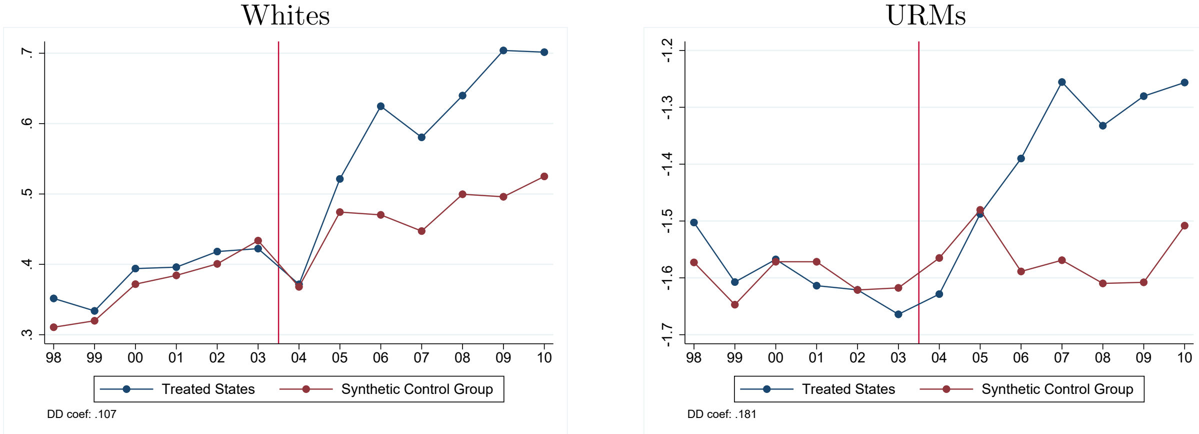
Figure A12: SAT Math Scores: Alternative Synthetic Control Approaches





Notes: This figure reports synthetic cohort analyses separately for whites and minorities. 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 mean squared prediction error (MSPE) over the 1998-2003 period. For "Synthetic, 5-year match", the MSPE are minimized over the 1998-2002 period, and for "Synthetic, 4-year match" they are minimized over the 1998-2001 period.

Figure A13: SAT Math Scores: Synthetic Control Approach without AA Ban States



Notes: This figure reports synthetic cohort analyses separately for whites and minorities. 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 are omitted from the donor pool.