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Essays on the Impacts of Affirmative Action

A dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Economics

by

Ben Backes

Committee in charge:

Professor Julie Cullen, Chair
Professor Kate Antonovics
Professor Terrance August
Professor Zoltan Hajnal
Professor Valerie Ramey

2012

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The dissertation of Ben Backes is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

Chair

University of California, San Diego

2012

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Chapter 1 will be published in the *Journal of Human Resources* in Spring 2012.

Chapter 2 was co-authored with Kate Antonovics and has been submitted for publication.

Chapter 3 was co-authored with Kate Antonovics and Valerie Ramey and is being prepared for submission for publication.

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ABSTRACT OF THE DISSERTATION

Essays on the Impacts of Affirmative Action

by

Ben Backes

Doctor of Philosophy in Economics

University of California, San Diego, 2012

Professor Julie Cullen, Chair

The first chapter, "*Do Affirmative Action Bans Lower Minority College Enrollment and Attainment? Evidence From Statewide Bans*" (forthcoming, *Journal of Human Resources*, Spring 2012) examines whether minority students were less likely to enroll in a four-year public college or receive a degree following a statewide affirmative action ban. Previous studies of affirmative action focus on drops in the enrollment of under-represented minorities (URMs) at top public institutions after the elimination of racial preferences. Using institutional data, I show that the effects of the bans were limited to these large drops at elite public institutions. Furthermore, students displaced from top universities were mostly absorbed by other campuses, resulting in very small drops in the number of minority students enrolling in college.

Finally, I provide evidence that fewer black and Hispanic students graduated from four-year public universities after the bans.

The second chapter, "*Were Minority Students Discouraged From Applying to University of California Campuses After the Affirmative Action Ban?*" (with Kate Antonovics), examines college applications. We show that the most highly qualified minority UC applicants were no less likely to apply to Berkeley or UCLA after Proposition 209, despite sharp drops in admissions probability. The middle 50 percent of URMs reduced their application rate to top UC campuses after Proposition 209, but also experienced large drops in admissions probability and shifted applications to less selective UCs. Given these findings, we conclude that there is little evidence of reduced minority interest in the UC system after the removal of racial preferences.

In the third and final chapter, "*Effort Levels of Minority High School Students Under Affirmative Action: Evidence From Bans in California and Texas*" (with Kate Antonovics and Valerie Ramey), we examine whether minority high school students spent less time investing in human capital after affirmative action bans. To evaluate these possibilities, we use both test scores and other measures of effort and find little evidence that minority students reacted to the bans by decreasing effort.

Chapter 1

Do Affirmative Action Bans Lower Minority College Enrollment and Attainment? Evidence from Statewide Bans

Using institutional data on race-specific college enrollment and completion, I examine whether minority students were less likely to enroll in a four-year public college or receive a degree following a statewide affirmative action ban. As in previous studies, I find that black and Hispanic enrollment dropped at the top institutions; however, there is little evidence that overall black enrollment at public universities fell. Finally, while there is evidence that fewer blacks and Hispanics graduated from college following a ban, the effects on graduation rates are very noisy.[1]

[1]Scheduled for publication Spring 2012, *JHR*. The author is grateful to Kate Antonovics, Prashant Bharadwaj, Julie Cullen, Gordon Dahl, Marjorie Flavin, Valerie Ramey, Rick Sander, two anonymous referees and various seminar participants for extremely helpful discussions and comments. Any remaining errors are my own.

1.1 Introduction

Affirmative action remains a divisive subject in the United States. Proponents deem it necessary to equalize opportunities available to different races, citing racial disparities in educational attainment or earnings. Others attack affirmative action as a policy that perpetuates inequality and stereotypes by devaluing the achievements of those who benefit from the policy, in addition to being unfair to other groups. In response to court and citizen challenges, between 1997 and 2004, six states – Texas, California, Washington, Florida, Georgia and Michigan – banned public institutions from using race when considering applications. This paper measures the change in the racial composition of students enrolling in and graduating from public unversities after these affirmative action bans.

Previous studies of affirmative action bans have focused on application and enrollment, many using selected samples of universities or states, but little is known about effects on the university system as a whole or on graduation. Card and Krueger [2005] examine college application behavior in Texas and California and find no evidence that highly qualified minority applicants reduced their rate of applying to top state institutions in response to affirmative action bans. Furthermore, Antonovics and Sander [2011] find no evidence that enrollment rates of minority applicants in the UC system were lowered by the ban in California. Long [2007] examines the impact of affirmative action bans on seven selective state universities and finds that the elimination of race-based preferences led to a statistically significant increase in minority under-representation (defined by comparing enrollment to the statewide racial makeup of the relevant age group) of five percentage points.[2] Additionally, he finds that top-x% programs (which guarantee graduating seniors in the top x% of their class admission to a state university) offset some of the decline in minority students at some institutions. Like this paper, Hinrichs [2012] uses institutional data to an-

[2]There are many documented benefits of attending a selective university: Kane [1998], Loury and Garman [1995] and Hoekstra [2009] find that graduates of more selective colleges have higher earnings, while Bowen and Bok [1998] show that attending a more selective college improves other outcomes, such as the likelihood of (not getting a) divorce.

alyze the effect of affirmative action on the minority share of enrollment. However, the enrollment analysis is not as detailed – there is little attempt to estimate effects at various points of the university selectivity distribution, or to assess whether minority students were absorbed by private, out of state or two-year institutions. While Hinrichs [2012] is the only paper I'm aware of attempting to measure the effect on college completion, it uses demographic data, which (as discussed later) is not ideal. As shown in this paper, institutional data can provide more informative estimates of the impact on college completion.

The main way this analysis adds to the previous literature is that it includes a comprehensive sample of institutions, rather than a chosen sample of the most selective universities. This allows for an assessment of whether fewer minority students enrolled in college following a ban, rather than simply documenting what happened at high tier institutions. In addition, the paper attempts to assess the impact of the bans on later outcomes; specifically, college completion. While previous studies have mainly focused on the effect of the bans on college application behavior, it is college attainment that is ultimately of interest. In the sample, little more than half of first time students go on to graduate, and graduation rates are even smaller for minority students. Since there is slippage between college application and enrollment, and between college enrollment and graduation, an analysis of college completion, rather than simply enrollment, is needed to determine the welfare implications of the policy change.

This paper investigates college enrollment and completion at four-year public universities in states which banned affirmative action. Estimates reveal little change in the share of students enrolling in four-year public institutions who were black, and a decrease in those who were Hispanic. Furthermore, there is evidence that the average college graduate was less likely to be black or Hispanic after a ban, especially in the top decile of institutions. Additional analysis shows that it is not likely that private institutions, two-year institutions or neighboring states absorbed additional minority students. All in all, although the effect sizes were modest, estimates show that there were fewer black and Hispanic students graduating from four-year, public universities following the bans, and those that did graduate tended to do so from less prestigious universities.

1.2 Dataset

The data for this analysis come from the Integrated Postsecondary Education Data System (IPEDS), a panel of university-level data for public institutions from 1990 to 2009.[3] There are two measures that will be used as outcome variables: first, enrollment; and second, the number of students from each entering class who graduated. Data for each of these measures are available by year and racial group. The enrollment data will be used to capture the effect of affirmative action on enrollment of minority students at universities of different selectivity. The data on graduation is only available for the entering classes of 1996 through 2003 and gives the number of students from each entering class (by race) who attained a degree in six years or less.

The main disadvantages of the dataset are lack of information on post-college outcomes and on specific individuals, making an examination of the mechanisms of the effects difficult.[4]

In order to measure the effects on the intended targets of the policy, the sample is restricted to four-year, public institutions. This restriction could result in misleading results if there were large impacts on out-of-sample institutions, such as private or two-year universities. In a later section, I will show that there is no evidence that either type of institution offset the effects of the affirmative action bans.

1.3 Estimation Strategy and Overview of Data

Estimates are based on a comparison of the pre- and post- policy change cohorts, controlling for general nationwide trends using time dummies. For the share of race j at institution i located in state s at time t, a very simple specification can be written as

[3] For a more complete discussion of the data used, see the Data Appendix.

[4] Another way to measure the impact of the bans would be to use individual-level data to compare later life outcomes of individuals who finished high school before the bans to those who finished after the bans. However, using the ACS or any other dataset not restricted to those likely to enroll in college is problematic because college attainment rates of blacks and Hispanics are very low, making a change difficult to pick up in the data.

$$\text{Enrollment share}_{jist} = \beta_0 + \beta_1 \cdot \text{X}_{st} + \theta \cdot \text{AA ban}_{st} + \gamma_i + \gamma_t + \epsilon_{ist} \tag{1.1}$$

$$\text{Graduate share}_{jist} = \beta_0 + \beta_1 \cdot \text{X}_{st} + \theta \cdot \text{AA ban}_{st} + \gamma_i + \gamma_t + \epsilon_{ist} \tag{1.2}$$

where "AA ban" is equal to 1 if an affirmative action ban is in place, γ_i denotes institution fixed effects, X_{st} represent time-varying state level controls and γ_t are year fixed effects.[5] θ is the measured impact of an affirmative action ban and represents the expected change in the outcome variable when an institution becomes affected by a statewide ban.[6] Due to the heterogeneous nature of the treatment, the coefficients should be interpreted as the average treatment effect across all treated institutions. An alternative interpretation is that the model is a test of the null hypothesis that each treatment has zero effect.

In this specification, the parameter of interest θ is identified if affirmative action policies are uncorrelated with unobserved factors which affect the outcome variables, such as time varying statewide conditions. The vector X_{st} is designed to control for two factors which could potentially influence college attainment outcomes and be correlated with bans of affirmative action: first, statewide economic characteristics at the time of application to college; and second, general statewide attitudes and policies towards education. For economic characteristics, I use yearly averages of the state unemployment rate, fraction of individuals with a high school degree, fraction of individuals with a college degree, and average income (these averages are taken over individuals above 25 years old). The education policy variables include whether a state instituted a consequential accountability system prior to the relevant year, whether an exam was required for graduating high school and whether a top $x\%$ program had been instituted.[7] The sources for these variables are detailed more thoroughly in the Data Appendix.

[5] This can be thought of as a difference-in-differences approach with multiple policy change years. The "AA ban" term would be the interaction term in a traditional DD specification, with the level effects being absorbed by the institution and year dummies.

[6] Affirmative action bans could change high school effort levels. However, the effects of the bans appear roughly constant over time, so the estimation results likely reflect changes in applications and admissions rather than in pre-college behavior (which presumably should have a time-varying component as students are given more time to adjust effort).

[7] None of the results in this paper are sensitive to including these controls.

In addition, to assess how university selectivity affects the response to an affirmative action ban, institutions are divided into three SAT selectivity groups: the decile of most selective universities, universities in the second and third deciles, and the remainder.[8] Selectivity groups are based on test scores of incoming students in 2007. The choice of 2007 would be problematic if the selectivity measure changed in response to the treatment. However, the hierarchy of institutions is remarkably constant over time (see Hoxby [2009]), so using test scores from the pre-treatment period (if available) likely would not change the groupings.[9] The reasoning behind the number of group definitions is Kane's (1998) finding that blacks and Hispanics enjoy large admissions advantages at institutions whose mean SAT scores were in the top fifth of all four-year schools. As a result, the initial analysis used regressions for each of the top two deciles. However, there are few institutions in the second decile in the policy change states, resulting in very large standard errors if these are not grouped with the third decile. In any case, most of the effects of the policy change occur in the top decile. Furthermore, I find little impact of the policies on the institutions below the 70th percentile of SAT scores and as a result combine these institutions into one group. There are a small number of institutions with neither SAT nor ACT scores reported. Since these schools are observationally similar to low selectivity schools, I add the missing selectivity group schools to the low selectivity category; this has no impact on results.

I define a state as being affected by an affirmative action ban when it is deemed (by a court case, referendum, or law passed) that in-state public universities must change their admissions procedures. As a result, I code Texans as first being affected in 1997 since it was the first year Texan universities conducted admissions after the conclusion of the *Hopwood* case. For Florida, I use 2001 since it was the first year affirmative action was banned, even though the top 20% plan was introduced in 2000. The other states are more straightforward: California in 1998, Washington in 1999, Georgia in 2002 and Michigan in 2004.

Although this paper uses shares of enrollment and graduation, there are three

[8]I use test scores, rather than admissions rates, since admissions rates are not necessarily informative about selectivity; see Hoxby [2009].

[9]As an alternative measure of university rankings, I created a group of the top 50 US News public schools. Results from the US News group are nearly identical to those in the top decile of test scores.

possible functional forms of the outcome variables: levels (for example, the number of black students enrolling in a given year), logs or shares of the total. Results will be presented using race-specific shares as the outcome variable, although all three specifications give very similar results. In each regression, universities are weighted by their total enrollment or total graduates in 1996 (the final year in which no state had adopted a ban), depending on the outcome variable. Reported standard errors are robust to clustering at the state level.

1.3.1 Summary Statistics

Institutional summary statistics for four-year, public universities are presented in Table 1.1. Numbers for enrollment and graduates are institutional averages for the pre-policy change period: the three years before a ban in affected states and 1994-1996 for non-ban states.

The first three columns shows the entire sample of 526 institutions in all states, followed by institutions in ban states and non-ban states. About 20 percent of universities were in the six states affected by affirmative action bans. Universities in ban states tend to be larger and have a higher proportion of Hispanic and Asian students. Other measures, such as test scores and statewide economic characteristics, are similar across the state groupings. The last three columns show summary statistics for the three selectivity groups *for institutions located in ban states*. The most selective universities have students with higher test scores (by construction), have much higher enrollment and have a higher fraction of Asians and lower fraction of blacks. Finally, a comparison of average enrollment and average graduates shows that graduation rates increase as university selectivity increases.

1.3.2 Graphical Representation of the Effect of the Bans

A first look at enrollment over time in affected states is shown in Figure 1.1a, which shows enrollment shares of blacks and Hispanics aggregated by state and selectivity group in a time window around each state's ban year.[10] The figure shows

[10]Note that these figures do not use a common scale, since it would make changes in states with low black or Hispanic populations very hard to detect.

clear drops in minority enrollment at each state's most selective schools. The bottom graph of control states is an aggregation of states adjacent to affirmative action banning states.[11] For these states, it appears there may have been a decrease in black enrollment at the most selective institutions following the bans.

Figure 1.1a underscores the importance of accounting for time trends, since some states have steady pre-existing trends in minority enrollment. A simple difference-in-difference regression comparing highly selective universities in Florida to those in non-ban states would suggest that the affirmative action ban *increased* the share of Hispanic students enrolling, since average enrollment was higher in Florida after the ban and roughly constant in non-ban states. Later in the paper I will give evidence that adding a state-specific linear time trend removes the possible bias from these increases over time.

Define the number of graduates *from each entering class* to be the number of students from that class who would go on to graduate in six years or fewer. Figure 1.1b shows the shares of black and Hispanic graduates by enrollment year. Each point on the graph shows, for a given enrollment class, what percentage of the cohort's graduates were black or Hispanic.[12] Figure 1.1b reveals similar patterns for graduation as enrollment, although the changes are not as pronounced. For most states, there is little evidence of an effect for the middle or low selectivity institutions for either graduation or enrollment.

Table 1.2 shows average means of black and Hispanic enrollment for each of the selectivity groups for the three years before and after each state's ban. These basic means show the same patterns in black enrollment as in Figure 1.1a: falls at the most selective institutions and little change in the other two groups. As shown in the graphs, there were fewer blacks enrolled in the most selective universities in the group of control states after the bans. However, the standard deviations for the group

[11] The group of adjacent control states includes New Mexico, Oklahoma, Arkansas, Louisiana, Oregon, Nevada, Arizona, Idaho, Alabama, North Carolina, South Carolina, Tennessee, Indiana, Ohio, Wisconsin, and Illinois.

[12] Another possible way of measuring the effect of affirmative action on the number of graduates would be to measure the amount of BAs awarded by race and year. However, this method is not ideal because for any given year, it is not possible to tell when the students who graduated enrolled (and, thus, whether they were affected by affirmative action bans or not). Finally, when using race-specific graduation *rates*, the effects are very imprecisely estimated.

of control institutions are large, which isn't surprising as it represents an average of a heterogeneous sample of states. For Hispanics, the simple sample averages don't show a clear patter due to states which experienced gradual rises in Hispanic enrollment (such as Florida and Georgia), but some states, such as Texas and California, had much lower Hispanic enrollment at their most selective institutions.

1.4 Estimation Results

A central question concerns the effect of affirmative action bans on the enrollment shares of minority students at universities of different levels of selectivity. Results for different specifications of Equation 1 are shown in Table 1.3 as a way to test for sensitivity. Specifications tested include changing the nature of the fixed time trends (linear state trends, linear university trends or squared university trends), restricting the sample to include a narrower window around the bans and restricting the non-ban state sample to only states adjacent to a banning state. Reported coefficients are expressed in percentage terms: a coefficient of one represents a 1 percentage point change.

The first row of regressions shows the estimated effect of affirmative action bans on the black share of total enrollment for the pooled sample of all universities for the various specifications. Other than the first column, which does not include any time trends, the point estimates across specifications are similar. The coefficients are small (around -0.4) and insignificant or marginally significant, suggesting that if affirmative action lowered the share of enrolling students in four-year public institutions who are black, the effects were modest.[13] The specifications which include some sort of fixed time trend – whether state- or university- specific – give very similar results: Columns 2-4 and 7 are nearly identical. Columns 5 and 6, which either restrict the years included or the set of control states, give similar estimates but tend to have larger standard errors, which is not surprising since the sample is smaller.[14]

The next row restricts the sample to the decile of the most selective univer-

[13]When the outcome variable used is the level of black enrollment or its log, the coefficient is not significant.

[14]The restricted years include 1995-2001, designed to capture a period in which many states changed their affirmative action laws.

sities. Again, as long as a time trend is included, results are very similar across specifications. Results show a highly significant drop of about 1.6 percentage points in the number of enrolling students who are black, consistent with previous studies of affirmative action bans at selective universities.

For institutions in the medium selectivity group (the second and third deciles of SAT scores), there is a marginally significant increase of about 0.4 in the percentage of incoming students who are black. This number is much smaller in magnitude than the decrease in enrollment at the top tier of institutions, so it appears that the response of the medium selectivity institutions was less pronounced than at the top – the capacity of medium selectivity schools did not expand to fully offset the drops at high selectivity schools, even after taking into account the greater number of schools in the medium group.[15] Finally, there is no evidence that the remaining low selectivity institutions experienced a change in black enrollment following the bans.

To summarize, across the specifications, the predicted fall in black enrollment at the top universities following an affirmative action ban is about 1.6 percentage points of enrollment. Additionally, there were possible increases in the second tier of institutions and very small, non significant decreases in the bottom 70 percent of universities. Since results are similar regardless of the specification of the time trend, I will take the simple state trends (Column 2) to be the preferred specification and use them for the rest of the paper.

Table 1.4 shows a summary of regressions using the specification used in Table 1.3, Column 2 for various samples. Thus, the first panel of Column 1 is identical to Table 1.3's Column 2. Column 1, Panel 2 shows the same regressions for the share of Hispanic enrollment. Coefficients are qualitatively similar to those for blacks but consistently larger in magnitude, which is not surprising due to the higher initial levels of Hispanic enrollment in banning states. The first row shows that at the average institution, the share of Hispanic enrollment fell by about 1.4 percentage points, although this is only significant at the 10% level. Furthermore, the enrollment drops at the top selectivity group was larger for Hispanics than for blacks – about 2.9

[15] To compare effects across selectivity groups, coefficients should be weighted by the total number of students in each group. Using the Table 1.1 to perform a rough calculation yields about 46,000 (12 schools $*$ 3835 students/school) students enrolling in the high group and 55,000 (20 $*$ 2750) students enrolling in the medium group each year in affected states.

percentage points. There were no significant changes in the second tier and non-significant, though somewhat large, falls at the lowest tier.

For selective institutions, the regression coefficients represent large changes in the outcome variable. For black students at the most selective universities, average enrollment share in the post policy change period was 4 percent (author's calculation). According to the regression results, black enrollment share at a given university would have been 1.6 percentage points higher had affirmative action not been banned. Thus, the bans led black enrollment to be 1.6/(4+1.6)=29 percent lower at top institutions than it would have been in the absence of a policy change. At the average institution, the total number of black students fell by about .38/11.7 = 3 percent – a relatively small change that is only marginally significant in some specifications. For Hispanics, enrollment share at the top institutions fell by 2.9 percentage points. Compared to the post-policy change mean of 11.3 percent (author's calculation), this is about a 20 percent change. Note that while in absolute terms, the number of Hispanics at the top institutions fell by more than for blacks; however, since there are many more Hispanics, the percentage change in Hispanic enrollment is smaller. For Hispanics at the average institution, the total number of students fell by about 8 percent.[16] In other words, it does appear that the total enrollment of Hispanics in four-year public institutions was reduced by affirmative action bans (and possibly a very slight reduction for blacks as well).

Since each of the coefficients represents the percentage of total enrollment, it is important to note that total enrollment was unaffected by the policy change, as shown in Panel 3, Column 1 of Table 1.4. Coefficients reported are from regressions with log total enrollment as the outcome variable. The coefficients are consistently small and insignificant: it appears that the policy change did not affect total enrollment at the average institution or at the top tier.

Next, I briefly turn to white and Asian enrollment. One drawback to the regressions for Asians and whites is that there was an increase in students who did not report their race, and the majority of these students are likely white or Asian

[16]For both blacks and Hispanics, using log enrollment as the outcome variable yields coefficients of similar magnitude to these rough calculations of percentage change in enrollment, so these results are robust to changes in how the outcome variable is measured.

(discussed in a later section). These results should be treated with caution and are not the emphasis of this paper. Effects on white enrollment are shown in Table 1.5, Column 1, Panel 1. The results reveal that the total share of white students enrolling increased by about 0.9 percentage points and that there were large increases in enrollment – over 3 percentage points – at the top institutions.

Panel 2 shows that the bans had little impact on the Asian share of enrollment at the more selective institutions. When state trends are not included (regressions not shown), affirmative action bans are predicted to have a large positive impact on Asian enrollment, but this result completely disappears once the time trends are added. This is not surprising as many top-tier institutions, especially in California, have experienced persistent rises in Asian enrollment. Once these fixed time trends are accounted for, a ban does not predict any deviation in Asian enrollment from this trend.

1.4.1 Graduation

To see how the effects on enrollment shares translates into graduate shares, I regress race-specific graduate shares on whether an affirmative action ban is in place, in the same manner as above. Recall that data are only available for the entering classes between 1996 and 2003, so the panel is shorter than for enrollment. Column 2 of Table 1.4 displays results for the shares of black and Hispanic graduates at public universities.

The first row of Panel 1 shows a decrease in the total share of black college graduates following a ban, mostly coming from the reductions at the most selective institutions. The average institution's share of black graduates fell by about 0.6 percentage points following a ban. As with enrollment, the effects are largest at the most selective universities. The second row of estimates shows a 1.2 percentage point decrease in the share of graduates who are black at the most selective institutions. For the medium selectivity institutions, estimates are similar to enrollment – a .37 percentage point increase. Finally, the least selective institutions show a non significant decrease in graduate shares.

For Hispanics, Column 2 shows a non significant drop in the share of graduates at the average institution of about 0.6, smaller than the drop in enrollment in Column 1 and similar to overall fall in the share of black graduates. The share of Hispanic graduates at the top decile fell by about 1.9 percentage points.

When comparing the enrollment and graduation, if lower quality students were the ones displaced by the bans, one would expect the effects on graduates to be less pronounced than on enrollment, since these students would have been less likely to graduate. Looking at the three selectivity groups for blacks and Hispanics shows this to be the case for Hispanics in each group and for blacks in all groups except the lowest tier, where enrollment fell by -0.18 and graduation fell by -0.55 percentage points. The changes in this group presumably lead to the graduation effect for blacks being larger than the enrollment effect for the entire sample (-0.63 and -0.38), but the limitations of institutional-level data make it difficult to say more, especially since the coefficients for the lowest tier are not very precisely estimated.

To summarize, the previous tables reveal a mostly clear picture of the effect of the affirmative action bans. Confirming previous findings, there were large drops in the black and Hispanic share of students enrolling and graduating from the top tier of institutions. However, at the average institution, the drops in the black share of enrollment were very small – there is little evidence that the overall number of blacks enrolling in college changed following the affirmative action bans. For the black share of university graduates, the marginal effects at the average university are significant and negative, although not large in magnitude, so there may have been a drop in the likelihood of blacks graduating from college. At the average institution, the Hispanic share of both enrollment and graduates dropped, although the coefficient for graduates is not significant. Overall, the results for blacks and Hispanics are generally similar, with both groups being pushed out of the most selective institutions but having smaller changes for the university system as a whole.

1.4.2 Private Institutions, Two-year Institutions, Out of State Students, Non Race Reporters

One way the effects of affirmative action could have been mitigated would be if private universities absorbed some of the students who would have otherwise attended public school. The previous analysis using institutional data suggests that black and Hispanic enrollment fell at the most selective universities. If the number of black and Hispanic students at the most selective private institutions rose following a ban, the implications would be quite different – the top students would have been absorbed by the best private institutions, rather than being forced to attend lower tier public institutions. Results for a sample consisting of private institutions are reported in Column 3 of Table 1.4. For blacks, the coefficient for the top selectivity group is very small. For Hispanics, results are somewhat mixed, with some negative, significant coefficients. In any case, there certainly does not appear to be an increase in the share of minority private school enrollment – either overall or at the most selective institutions – following the bans.

Another possible effect of the ban could have been to push minority students from four-year to two-year institutions. Previous results showed a decrease in Hispanic enrollment at the average four-year university following a ban. Column 4 of Table 1.4 shows enrollment results for the sample of two-year institutions. Institutions are not split into selectivity groups because very few schools report SAT scores, so only regressions for the average institution are shown. For blacks, there is no evidence of any change in two-year enrollment following the bans. For Hispanics, there is a small, non significant decrease in enrollment share.

Another factor which could affect the welfare implications would be induced interstate migration – perhaps black and Hispanic students reacted to the bans by attending institutions in other states. Unfortunately, IPEDS does not report out of state enrollment by race, so I use two somewhat indirect methods of testing. One test is to regress minority share of enrollment on whether an adjacent state has enacted an affirmative action ban. The basic idea of this regression is to see whether, for example, universities in Oregon had an increase in the minority share of enrollment after 1998 – the year California enacted its ban. Coefficients from these regressions (not reported) are small and not significant – evidence that blacks and Hispanics did not react to the bans by attending college in nearby states. The second test is seeing whether states adjacent to banning states experienced increases in the total share of out of state students. Again, regression results indicate that this is not the case.

Finally, one problem with the IPEDS data is that there are a substantial number of students whose race is not reported (unknown). If the failure to report race is affected by the bans, this could bias results. For example, in 1998 there was a sharp increase in the number of students who did not report race at institutions in the University of California system. If the problem only occurs in the first year of a ban (as appears to be the case in the California universities), then dropping the first year of a ban provides a check on the results. When dropping the initial ban year, results for blacks and Hispanics are very similar to the full sample, suggesting that it was individuals of other races who changed their likelihood of reporting race following the bans. Furthermore, Antonovics and Sander (2010) provide convincing evidence that, in California, the rise in unknowns is due to an increase in whites and Asians not reporting race (and in fact classify unknowns as white/Asian in their paper). If these unknowns are indeed white or Asian, then drawing conclusions about what happened to the enrollment shares of whites and Asians would be faulty due to these unknowns. However, the evidence presented about blacks and Hispanics would still be valid.

1.4.3 Model Specification Checks

An example of an endogeneity problem would be if the bans were passed in response to rising minority enrollment in public universities. If the bans were as good as randomly assigned, conditional on the controls, they should not be able to predict changes in enrollment in the time period leading up to the policy changes. A way to test for this potential problem would be to add leads of the policy change variable to the regressions to investigate whether these leads can predict changes in the outcome variable (often referred to as a 'placebo' test in the literature). However, using the one-year lead of the policy change is not informative because some universities implemented the policy change in the year before they were forced to, making nearly every model fail the placebo test.[17] As a result, I run regressions with a dummy for whether a state will change its affirmative action policy in either two or three years in the future. Significant coefficients of this policy change lead would indicate that

[17] One way of dealing with these pre-policy change decreases in enrollment would be to switch the policy change year to, for example, 1996 in Texas (rather than 1997); this does not affect the main results of the paper. Another way of running these placebo tests would be to only include only Washington and California, the non early adopters. Results are similar to those discussed in this section.

changes in the racial composition of enrollment were associated with the introduction of the bans. Results are presented in Table 1.6. For blacks, none of the specifications are significant for any of the selectivity groups, so it appears unlikely that the bans were passed in response to changes in black enrollment (or something correlated with black enrollment). However, for Hispanics, some coefficients are significant and positive, although when using state or university time trends the coefficients lose significance. This could be interpreted as evidence that adding either state or university specific fixed time trends is sufficient in eliminating bias due to time trends in the data. However, it could also be evidence that states instituted bans in response to growing Hispanic enrollment in public universities.

Results are robust to changes in the functional form of the amount of time a ban has been in place (for example, adding a linear term in the number of years of exposure to an affirmative action ban) – allowing for more flexibility does not result in meaningful changes to results. Other changes in model specification that do not affect the coefficients are whether the state-specific economic conditions and accountability measures are included. Finally, re-running regressions while dropping one state at a time, results (not shown but available from author) remain similar.

1.5 Discussion

This paper investigates college enrollment and completion at four-year public universities in states which banned affirmative action. Estimates show that the falls in black and Hispanic enrollment were confined to the decile of most selective institutions. However, there is some evidence that fewer black and Hispanic students graduated from college following the bans, although the effect sizes are modest.

The results from the analysis suggest that affirmative action did succeed in raising the shares of black and Hispanic enrollment at the top institutions, but did not lead to more blacks enrolling in college. However, the effects of affirmative action – both at top tier schools and the university system generally – are small relative to the total population of minority students.

1.6 Acknowledgement

Material from Chapter 1 appears in the *Journal of Human Resources*, forthcoming Spring 2012.

1.7 Figures and Tables

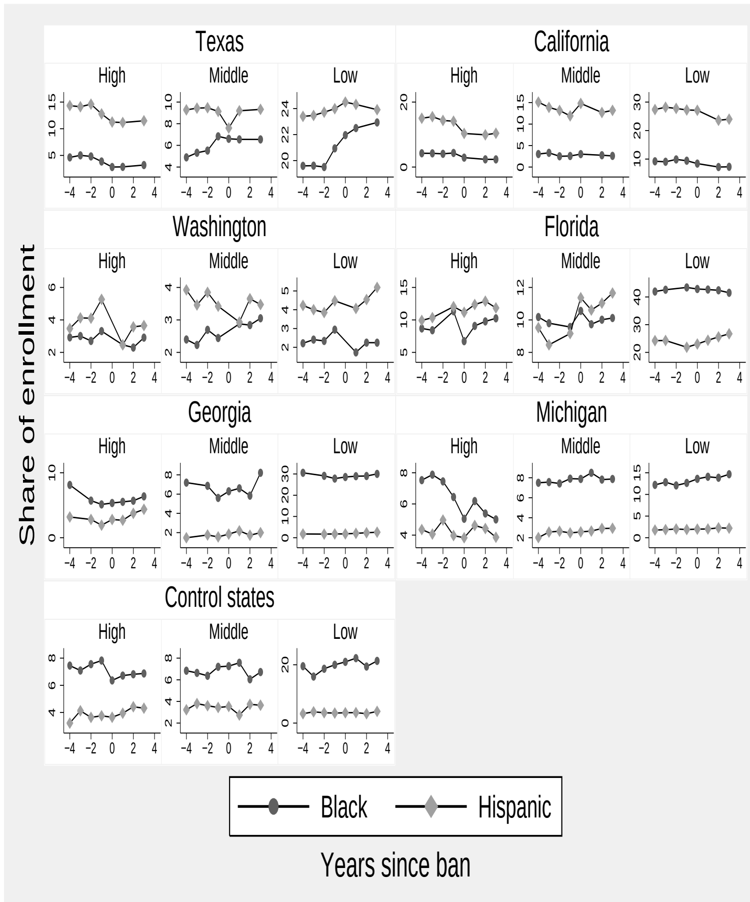


Figure 1.1a: Pooled average of enrollment share by state and university selectivity group for four years before and after an affirmative action ban, with year zero representing the first year of a ban. 'High' represents the group of most selective institutions, defined by SAT scores.

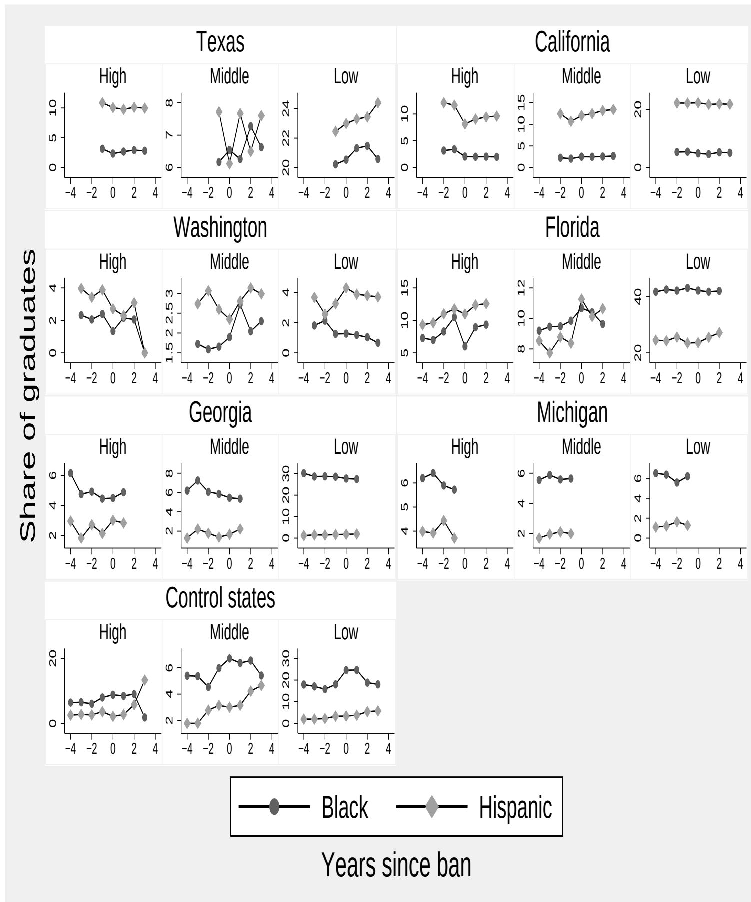


Figure 1.1b: Pooled average of graduate share by state and university selectivity group for four years before and after an affirmative action ban, with year zero representing the first year of a ban. 'High' represents the group of most selective institutions, defined by SAT scores.

Table 1.1: Summary statistics, IPEDS data

	All States	Ban states	Non ban states	Low Selectivity	Medium Selectivity	High Selectivity
Enrollment, all	1321.4	1847.6	1194.5	1240.5	2749.6	3835.3
	(1153.2)	(1453.9)	(1030.7)	(752.4)	(1654.8)	(1698.9)
Black enrollment pct	14.2	14.6	14.1	18.8	6.2	4.8
	(23.2)	(21.7)	(23.6)	(25.1)	(3.8)	(2.7)
Hispanic enrollment pct	5.9	13.8	4.0	16.5	6.5	10.3
	(11.0)	(17.3)	(7.7)	(20.1)	(5.3)	(5.8)
White enrollment pct	72.6	58.3	76.0	53.7	74.2	58.2
	(27.1)	(27.3)	(25.9)	(29.6)	(14.4)	(20.7)
Asian enrollment pct	4.7	10.1	3.4	8.0	9.6	23.1
	(8.8)	(13.2)	(6.8)	(11.1)	(12.0)	(19.1)
Unknown enrollment pct	1.8	3.2	1.5	3.3	2.8	3.9
	(3.5)	(4.0)	(3.2)	(4.4)	(3.2)	(3.2)
75 pctile SAT M	582	589	580	553	633	702
	(63)	(63)	(63)	(36)	(19)	(29)
Institutions	526	101	425	69	20	12

Source: 1990-2009 pooled IPEDS, public universities. See text.

Table 1.2: Sample averages of enrollment share before and after bans by state and selectivity

	Blacks			Hispanics		
	High	Medium	Low	High	Medium	Low
Texas						
Pre-ban	4.6	5.9	20	14	9.4	24
	(.59)	(3.6)	(29)	(1.3)	(1.8)	(27)
Post-ban	2.8	6.6	22	11	8.4	24
	(.13)	(5.2)	(30)	(1.9)	(1.1)	(27)
California						
Pre-ban	4.2	2.8	9.5	15	13	28
	(2.5)	(1.3)	(5.9)	(3.6)	(4.4)	(12)
Post-ban	2.6	2.9	7.8	10	14	25
	(1.3)	(1.3)	(5.7)	(1.6)	(4.9)	(12)
Florida						
Pre-ban	9.9	9.7	43	11	8.8	23
	(2.1)	(2.2)	(44)	(1.2)	(2.2)	(27)
Post-ban	8.5	10	43	12	11	24
	(1.6)	(2.1)	(43)	(.88)	(2)	)27)
Michigan						
Pre-ban	7.3	7.6	13	4.3	2.6	1.9
	(2.6)	(3.2)	(9.5)	(1.8)	(.81)	(.68)
Post-ban	5.5	8.1	14	4.3	2.7	2.1
	(2)	(2.7)	(12)	(1.5)	(.63)	(.75)
Adjacent state control group						
Pre-ban	7.5	6.8	18	3.8	3.6	3.6
	(3.4)	(5.8)	(25)	(2.6)	(4.2)	(7.5)
Post-ban	6.6	7	21	4	3.3	3.5
	(2.9)	(5.9)	(26)	(2.5)	(3.9)	(6.8)

Source: 1990-2008 pooled IPEDS, public universities. Averages taken over the three years before and after the passage of an affirmative action ban. Selectivity groups defined by SAT scores of incoming students (see text). Institutions weighted by total enrollment in 1996.

Table 1.3: Effect of ban on black enrollment share under various specifications

Selectivity group	Year FEs (1)	State trends (2)	Uni trends (3)	Uni sq trends (4)	Years restricted (5)	Adj States (6)	Adj St trend (7)
All	-0.03	-0.38*	-0.38	-0.33	-0.45	-0.45	-0.36*
	(0.34)	(0.19)	(0.19)	(0.26)	(0.27)	(0.30)	(0.17)
Institutions	526	526	526	526	526	265	265
R-squared	.031	.11	.53	.65	.022	.045	.089
High selectivity	-1.01**	-1.65***	-1.65***	-1.49***	-1.52***	-1.08*	-1.69***
	(0.32)	(0.25)	(0.25)	(0.35)	(0.40)	(0.43)	(0.25)
Institutions	46	46	46	46	46	19	19
R-squared	.1	.35	.44	.52	.18	.18	.53
Medium selectivity	0.61	0.42*	0.42*	0.36*	0.68***	0.61	0.41
	(0.34)	(0.17)	(0.18)	(0.15)	(0.17)	(0.46)	(0.26)
Institutions	116	116	116	116	116	60	60
R-squared	.035	.28	.43	.61	.047	.044	.2
Low selectivity	0.19	-0.17	-0.18	-0.18	-0.48	-0.69	-0.11
	(0.61)	(0.49)	(0.50)	(0.60)	(0.79)	(0.57)	(0.35)
Institutions	364	364	364	364	364	186	186
R-squared	.089	.19	.57	.67	.049	.14	.2

Source: 1990-2009 pooled IPEDS, public universities. Selectivity groups defined according to reported 75th percentile SAT math score (see text). Controls include average state income, unemployment, share of adults with college and high school degrees, whether a state adopted school accountability and whether a state adopted a top x% plan (see text). Universities weighted by total enrollment in 1996. Standard errors robust to clustering at the state level.
*Significant at 10% **5% ***1%

Table 1.4: Effect of ban on black and Hispanic enrollment and graduate shares

	Enroll public (1)	Graduate public (2)	Enroll private (3)	Enroll 2 year (4)
Panel 1: Blacks				
All	-0.38*	-0.62***	-0.52	0.01
	(0.19)	(0.16)	(0.28)	(0.13)
Institutions	526	520	1029	983
High selectivity	-1.65***	-1.24**	0.17	
	(0.25)	(0.44)	(0.49)	
Institutions	46	46	65	
Medium selectivity	0.42*	0.37	-0.69	
	(0.17)	(0.24)	(0.61)	
Institutions	116	116	74	
Low selectivity	-0.17	-0.54	-0.65*	
	(0.49)	(0.35)	(0.28)	
Institutions	364	358	890	
Panel 2: Hispanics				
All	-1.36*	-0.59	-0.72***	-0.32
	(0.65)	(0.36)	(0.20)	(0.38)
Institutions	526	520	1029	983
High selectivity	-2.87***	-1.81**	-1.33	
	(0.55)	(0.59)	(1.00)	
Institutions	46	46	65	
Medium selectivity	-0.54	0.61	-0.52	
	(0.38)	(0.64)	(0.44)	
Institutions	116	116	74	
Low selectivity	-1.00	-0.03	-0.66**	
	(1.02)	(0.20)	(0.22)	
Institutions	364	358	890	
Panel 3: Log total				
All-Ban	-0.014	0.024**		
	(0.02)	(0.01)		

Source: 1990-2009 pooled IPEDS, public universities.

Table 1.5: White and Asian enrollment and graduate shares

	Enr pub (1)	Grad pub (2)
Panel 1: Whites		
All	0.91*	-0.49
	(0.38)	(1.56)
Institutions	526	520
High selectivity	3.16**	-0.33
	(0.95)	(3.02)
Institutions	46	46
Medium selectivity	-0.48	-2.72
	(0.58)	(2.20)
Institutions	116	116
Low selectivity	0.61	0.28
	(0.90)	(0.66)
Institutions	309	306
Panel 2: Asians		
All	-0.23	0.11
	(0.16)	(0.33)
Institutions	526	520
High selectivity	-0.15	0.78
	(0.41)	(0.86)
Institutions	46	46
Medium selectivity	-0.32	-0.77*
	(0.25)	(0.34)
Institutions	116	116
Low selectivity	-0.29*	0.03
	(0.14)	(0.10)
Institutions	309	306

Source: 1990-2009 pooled IPEDS.

Table 1.6: 'Effect' of lead of ban on enrollment share

Selectivity group	Year FEs (1)	State trends (2)	Uni trends (3)	Uni sq trends (4)	Years restricted (5)	Adj States (6)
Blacks						
All	0.040	-0.167	-0.165	-0.133	0.029	-0.292
	(0.20)	(0.15)	(0.16)	(0.13)	(0.18)	(0.14)
High selectivity	0.500	-0.007	-0.011	0.041	-0.180	0.382
	(0.35)	(0.24)	(0.24)	(0.25)	(0.20)	(0.35)
Medium selectivity	0.035	-0.121	-0.123	-0.065	-0.010	-0.027
	(0.23)	(0.19)	(0.20)	(0.16)	(0.18)	(0.26)
Low selectivity	-0.017	-0.202	-0.205	-0.290	0.059	-0.508
	(0.36)	(0.31)	(0.32)	(0.25)	(0.36)	(0.29)
Hispanics						
All	0.955**	0.366	0.364	0.462*	0.763*	0.997*
	(0.35)	(0.23)	(0.23)	(0.22)	(0.29)	(0.35)
High selectivity	0.487	0.076	0.079	0.445	0.688*	0.861*
	(0.32)	(0.25)	(0.25)	(0.25)	(0.30)	(0.33)
Medium selectivity	0.726***	0.094	0.092	0.357	0.442	0.730***
	(0.17)	(0.17)	(0.18)	(0.19)	(0.26)	(0.18)
Low selectivity	1.447	0.829	0.825	0.579	0.559	1.416
	(0.84)	(0.48)	(0.50)	(0.42)	(0.29)	(0.82)

Source: 1990-2009 pooled IPEDS, public universities. Reported coefficients measure the average 'effect' of the second and third leads of the policy change. Selectivity groups defined according to reported 75th percentile SAT math score (see text).

1.8 Data Appendix

Institutional data is taken from the Integrated Postsecondary Education Data System (IPEDS), a survey of higher education institutions conducted by the National Center for Education Statistics (NCES). Data was accessed through the Data Center portion of the NCES web page. Every institution participating in a federal student aid program (including Pell grants and federal student loans) must report data on enrollment, degrees awarded, tuition and many other variables. My main focuses are fall enrollment of full-time, first-time students and the number of graduates from each enrolling class. The sample consists of enrollment data from 1990-2009, with the exception of 1999, which is not posted on the IPEDS web page. I include four-year public universities that report data for each year in the sample.[18] To construct a measure of institution selectivity, I use two reports from 2007: the 75th percentile math SAT score and 75th percentile ACT score. First, I divide schools into both SAT and ACT deciles. Schools in the top SAT group are then assigned to the group of most selective institutions. Universities in the top decile of ACT scores that do not report SAT scores are also assigned to the top selectivity group. The process is repeated for the second and third deciles of test scores, which make up the second selectivity group used in the paper. The final sample consists of a panel of 526 institutions.

1.8.1 Statewide School Accountability

A concurrent factor which could influence educational outcomes was the introduction of state school accountability systems, which have been found (for example, Hanushek and Raymond [2005]) to boost student achievement. These could be especially problematic if there was a disproportionate effect on minority students. To control for the introduction of these systems, I add dummies for whether an individual's (or institution's) state introduced consequential school accountability, as shown in Table 2 of Miller and Zhang [2006]. Following Hanushek and Raymond [2005], I

[18] About 80 percent of the institutions that reported data in 1990 meet this criteria.

define a consequential school accountability system to be one where school test performance is publicly disseminated and there are consequences for the results of these reports. I also add dummies for whether a state requires the passage of an exam to graduate from high school (taken from the Education Commission of the States web page).

1.8.2 Top $x\%$ Programs

Three of the affirmative action banning states enacted a "top $x\%$" rule after the ban. As mentioned earlier, Long [2004a] and Long [2007] show that a top $x\%$ rule fails to replace affirmative action in maintaining the share of minority students at top universities. Thus if minorities are affected by the elimination of race-based preferences, the estimation model would still show a net effect, even without controlling for top $x\%$ policy changes. The inclusion of these terms does not have a large impact on the estimates.

Chapter 2

Were Minority Students Discouraged From Applying to University of California Campuses After the Affirmative Action Ban?

[with Kate Antonovics] This paper uses student-level data to investigate how the college application behavior of underrepresented minorities (URMs) changed in response to the 1998 end of affirmative action in admissions at the University of California (UC). We show that all URMs experienced drops in probability of admission to at least one UC campus. However, the relative decline in URM SAT score-sending rates – our proxy for application – was small and concentrated at Berkeley and UCLA among URMs who experienced the largest relative drop in their predicted probability of admission. In addition, we find evidence of a shift towards less selective UCs rather than out of the UC system. Overall, our paper highlights the stability of URM application behavior in the face of substantial declines in their admission rates. Our findings suggest that efforts to increase diversity in the UC system should be targeted at increasing URMs' level of academic preparation before they apply for college rather than at increasing application rates.

2.1 Introduction

Affirmative action in college admissions continues to be a central issue in American higher education. Although the debate is often focused on abstract principles (for example, should past inequities be corrected by differential treatment?), several empirical questions are prominent in the discussion. For example, it is often argued that the abolition of racial preferences sends a message of institutional hostility to potential minority students, discouraging applications and further lowering the already depressed college graduation rates of blacks and Hispanics.[1] The University of Texas is currently defending its reintroduction of explicit racial preferences partly on this rationale.[2] Although the University of California and the University of Michigan – both governed by statewide bans on the use of racial preferences – eventually increased black enrollment levels over pre-ban levels, both institutions have been quite concerned about ways in which the bans affect their perceived receptivity to minorities.

In this paper, we explore the effect of California's statewide ban on racial preferences (also known as Prop 209) on the application rates of underrepresented minorities (URMs) to each of the eight campuses that made up University of California system at the time the ban went into effect. The simultaneous removal of racial preferences from the different UC campuses (which varied widely in their admissions selectivity and demographics) and the extensive publicity that accompanied this shift provide an excellent setting in which to study the effect of affirmative action bans on the application behavior of minority students. Using administrative data from the University of California on every first-year fall semester applicant to the University of California, we document the effect of the end of racial preferences in admission on

[1] "[UC Regent William] Bagley said [...] that many qualified minority students do not apply to the UC system because of some students' perception of its 'anti-affirmative action' stance" (2000, April 6). Minority admissions rise in U. of California. *Harvard U.*

[2] Fisher v. University of Texas (2011): "UT was keenly aware that by sending a message that people of all stripes can succeed at UT, the University would attract promising applicants from once-insulated communities, over time narrowing the credentials gap between minority and non-minority applicants. After Hopwood [Texas' affirmative action ban], such applicants were dissuaded from applying to UT."

URMs' relative chances of admission to each of the eight UC campuses, and highlight how these changes depended on students' academic credentials. Then, using data from the College Board, we relate these changes in the probability of admission to changes in SAT score-sending behavior (our proxy for application behavior) to schools both inside and outside the UC System.[3]

Like us, Long (2004) also examines the effect of affirmative action bans on SAT score-sending behavior and finds that changes in the probability of admission predict changes in score-sending. The paper argues that the affirmative action bans in California and Texas caused URMs to send scores to lower quality colleges and non-URMs to send scores to higher quality colleges. A weakness of Long's work, however, is that he uses data from the National Education Longitudinal Study (NELS) to generate a student's predicted probability of admission to different schools. However, the NELS reports admissions outcomes only for students' top two college choices, which are likely to reflect students' beliefs about their chances of admission. In contrast, we have data on every campus to which applicants to the UC system applied. Thus, we are able to more precisely gauge the change in the likelihood that students with different academic credentials were admitted to the various UC campuses. A second limitation of Long's analysis is that, rather than having data on score-sending to specific institutions, Long's data divide universities into quintiles based on academic quality. In contrast, our data on SAT score-sending contain institution names, allowing us to look at the change in score-sending rates to each UC campus. This turns out to be important because effects of the ban on affirmative action varied considerably by campus.

Card and Krueger [2005] also investigate the effect of California's ban on the use of racial preferences on the SAT score-sending behavior of URMs.[4] They find no evidence that minority students with SAT scores above 1150 or GPA above A

[3]Card and Krueger [2005], discussed below, also use SAT score-sending rates as a proxy for application rates. In their paper, they investigate how well score-sending rates predict application rates by comparing the number of score-senders with the number of applicants in race-SAT-campus-year bins. They show that the number of score-senders in a bin is highly correlated with the number of applicants. Since we also have data on race, SAT scores, applications, and score-sending, we were able to conduct our own parallel analysis; our findings are similar, although the correlation decreases as the number of bins increases.

[4]Their study also examines Texas' ban on affirmative action.

reduced the rate at which they sent scores to at least one of the three most selective UC schools (Berkeley, UCLA and UC San Diego). One drawback of their analysis, however, is that Card and Krueger did not have access to detailed information on how the end of affirmative action affected URMs' chances of admission. Thus, while their paper provides strong evidence that highly qualified students were not discouraged from applying to the most selective UC campuses after the end of affirmative action, it is able to present only a partial picture of the changes brought about by the end of affirmative action. A major contribution of this paper is to link the relative changes in URMs' predicted probability of admission at each of the eight UC campuses to their changes in score-sending.

In addition to Long [2004b] and Card and Krueger [2005], several other papers examine the effect of bans on affirmative action in college admission on other aspects of the college admission process. Backes [2012] and Hinrichs [2012] find that the enrollment shares of URMs at selective universities fall in the wake of affirmative action bans, but find little evidence of a change in the URM share of the total college-going population. Arcidiacono et al. [2011] examine the effect of Prop 209 on the share of URMs enrolled in different types of colleges and URM graduation rates, and find evidence that URM graduation rates (the fraction of enrolled students who graduate) increased at the UC after Prop 209. Finally, Antonovics and Sander [2011] examine the likelihood that URMs enroll in UC schools conditional on being accepted and find evidence that URM enrollment rates increased after the end of affirmative action.

Our analysis reveals three important findings. First, we show that all URMs, regardless of their academic credentials, were affected by the ban on affirmative action. Relative to whites, even the most highly qualified URMs experienced a substantial fall in their chances of admission to the most selective UC schools (i.e. Berkeley and UCLA), while less qualified URMs became relatively less likely to gain admission to the least selective UC schools (i.e. UC Santa Cruz and UC Riverside). Second, the relative decline in URM score-sending rates after the end of affirmative action was small and concentrated at Berkeley and UCLA among URMs who experienced the largest relative drop in their predicted probability of admission. Third, for the group of URMs with the largest relative drop in their score-sending rates to Berkeley and UCLA, we find a relative increase in the number scores URMs sent to less selective

UCs, though the magnitude of this shift appears to have been small. Overall, our paper highlights the stability of URM application behavior in the face of substantial declines in their admission rates.

This paper proceeds as follows. Section 2 discusses institutional background in California and the details of the affirmative action ban. Section 3 presents our theoretical model, and Section 4 our empirical specification. Section 5 discusses our data sets. Section 6 shows results for both admissions rates and score-sending. Finally, Section 7 concludes.

2.2 Background on the Affirmative Action Ban

The University of California is one of the largest public university systems in the world, and with several campuses that do well in world rankings, it is unique in its combination of quality and scale. In the mid-1990s – the period this paper focuses upon – the UC system matriculated about 24,000 freshmen per year across eight campuses.[5] Since California graduates more than 10 times that many students from high school each year, admission into the UC system has long been quite competitive.

Since at least the 1970s, the University has been concerned about increasing racial diversity on the campuses, but the system's structure made those concerns play out in an unusual way. Under the California Master Plan for Higher Education, which was adopted in the early 1960s and has been only slightly modified since, the University of California is supposed to guarantee admission at some campus to all California high school graduates who are among the top eighth in their statewide cohort (as measured by a combination of grades and test scores). The Cal State system is supposed to provide a similar guarantee to the top third, and community colleges enroll other students who meet basic thresholds of college qualifications. The top eighth rule put a significant constraint, long before the ban on affirmative action, on UC's ability to admit large numbers of black and Hispanic students with low academic credentials. Although the university instituted special admissions to qualify more minorities for the UC-eligible pool, in the years before the affirmative action ban

[5]There are 10 UC campuses, but one of them (San Francisco) enrolls only graduate students and another (Merced) opened its doors in 2005.

both blacks and Hispanics were underrepresented among UC freshmen – compared to the pool of high school graduates – by half or more. Thus, in 1996 blacks made up just over 4 percent of UC freshmen, while they made up over 8 percent of Stanford's freshmen.

If the University of California as a whole was constrained in admitting URMs even before the ban on affirmative action, the individual campuses were fairly unconstrained in competing for the modest number of URMs who made it into the eligible pool. The ironic result was that around half of all the blacks and Hispanics who matriculated at UC schools in the mid-1990s entered the two most elite and competitive campuses, Berkeley and UCLA. Blacks, for example, made up about 8 percent of the new freshmen at Berkeley, but only 2 percent of the new freshmen at UC Irvine, a less elite campus.

The first threat to affirmative action in California was in July 1995, when the Board of Regents of the University of California passed a resolution (SP1) which stipulated that UCs would discontinue considering race in admissions by the beginning of 1997. In November 1996, Proposition 209 (Prop 209), which banned the use of racial preferences in university admissions, was passed by voters, leading the UC to delay the implementation of SP1. Prop 209 underwent legal challenges until the Supreme Court denied further appeals in November 1997. Thus, the incoming class of 1998 was the first to be admitted without the use of affirmative action.

It is important to recognize that in an effort to minimize the effects of Prop 209 on minority enrollment, UC campuses increased minority outreach efforts.[6] However, these efforts were widely viewed as ineffective, at least initially.[7] Part of the reason for lack of effective programs was that in the immediate aftermath of Prop 209, there were concerns about whether race-specific outreach (as opposed to, for example, targeting low income areas) was permitted after Prop 209. In addition, outreach

[6]For example, "In an attempt to improve minority access to UC without the help of affirmative action, the university's investment in kindergarten-through-12th-grade outreach has rocketed from about $60 million in 1995 to $180 million last year and a planned $250 million this year" (2000, January 21). UC Regents Urged to Step Up Minority Outreach at Schools. *The San Francisco Chronicle.*

[7]"[In the last five years] minority admissions have dropped significantly and outreach expenditures have almost quadrupled to nearly $300 million with minimal results" (2000, June 8). Effects of minority outreach may take time for U. California system. *The Daily Bruin.*

programs focusing on elementary and middle schools would not have an effect until those students reached college age.

To the extent that increased outreach had an effect, our measured effects of Prop 209 will be the net effect of the ban and the change in outreach efforts.

2.3 Theoretical Effects of the Ban

In this section, we present a simple model to help understand the theoretical effects of banning affirmative action on the application behavior of minority students. To do so, we use Long's 2005 model of application behavior. In his model, students decide how many applications to send to different types of schools, where the type of school is defined by its level of selectivity and where students are indifferent between attending schools of the same type. When each application has a fixed cost α, the expected indirect utility of applying to college is given by

$$E\left[V\left(N_1, ..., N_J\right)\right] = E(S) - \alpha N, \tag{2.1}$$

where N_j is the number of applications to campuses of type j, S is the indirect utility gained from the campus attended, and N is the number of applications sent. Like Long, we focus on the case in which there exist three alternatives. That is, we assume students choose whether to apply to type 1 colleges, type 2 colleges or some non-selective alternative (type 3). The indirect utilities associated with each of these choices is V_1, V_2, and V_3, respectively, where $V_1 > V_2 > V_3$. The indirect utility of attending a given college captures the value students place on attending that college and could reflect factors such as peer ability, expected labor market outcomes, and the racial composition of the student body.

Assuming independence of admissions decisions among campuses and equal rejection rates, π_j, among campuses of type j, a student's expected indirect utility of attending college is given by

$$E(S) = \left(1 - \pi_1^{N_1}\right) V_1 + \pi_1^{N_1}\left(1 - \pi_2^{N_2}\right) V_2 + \pi_1^{N_1}\pi_2^{N_2} V_3. \tag{2.2}$$

Students then choose N_1 and N_2 to maximize their expected indirect utility of applying to college, resulting in

$$N_1^* = \frac{\ln\left(\frac{\alpha[1-(\ln\pi_1/\ln\pi_2)]}{\ln\pi_1(V_2-V_1)}\right)}{\ln\pi_1}. \tag{2.3}$$

To understand how Prop 209 could have affected URM application behavior, note that the end of racial preferences increased the probability of rejection for most URMs at most campuses (we discuss these patterns in greater detail below). In addition, the effect of an increase in the rejection rate, π_1, has an ambiguous effect on the number of applications sent to type 1 schools. That is, the sign on $\frac{\partial N_1^*}{\partial \pi_1}$ is ambiguous. An increase in the rejection rate could cause students to apply to additional schools in this group if V_1 is much greater than V_2 (in an effort to reduce the likelihood of being pushed out of the top tier), or to reduce their number of applications if the probability of acceptance becomes too low relative to the cost of applying. This same ambiguity holds for $\frac{\partial N_2^*}{\partial \pi_2}$. Thus, perhaps unexpectedly, an increase in URMs' rejection rates at the UC could have increased the optimal number of applications URMs sent to UC schools.

Of course, Prop 209 is also likely to have changed the value that URMs placed on attending UC schools. In our model, $\frac{\partial N_1^*}{\partial V_1} > 0$, thus there is a positive relationship between the value students place on attending type 1 schools and the optimal number of applications to send to these schools. It is unclear, however, whether Prop 209 increased or decreased the value URMs placed on attending UC schools. Opponents of Prop 209, for example, have argued that Prop 209 may have increased the perception of institutional hostility towards URMs at UC schools and that URMs would not be interested in attending schools with so few same-race peers. On the other hand, the knowledge that URMs no longer received preferential treatment in UC admissions could have increased the signalling value of a UC degree, as employers would know that URM graduates were as qualified as non-URMs. In addition, it is possible that increased recruitment measures worked to increase V_1.

The fact that the signs of $\frac{\partial N_1}{\partial \pi_1}$ and $\triangle V_1$ are both ambiguous is important because it means that it is impossible to determine whether observed changes in application patterns arose because Prop 209 changed URMs' chances of being admitted to UC schools or because it changed the value that URMs placed on attending UC schools. Nonetheless, it is important to understand the extent to which URM application rates fell in the wake of Prop 209 and how those changes were related to changes in URMs' predicted probability of admission to the various UC campuses.

While informative, the above model does not allow us to determine how URMs' application behavior would change if URMs' probability of admission fell differentially across the different UC campuses. As we show below, it is generally the case that Prop 209 had a larger effect on admission rates at more selective UC schools than at less selective UC schools, though this pattern varies somewhat depending on URMs' academic credentials. Thus, Prop 209 may not only have altered the total number of applications sent to UC schools, but also the rate at which URMs applied to individual campuses. That is, we might expect Prop 209 to shift URMs' applications away from more selective UCs (where admissions rates fell considerably) and toward less selective UCs (where admission rates were relatively stable). In our empirical analysis, we focus on application rates to individual campuses, but we also examine the total number of applications sent to UC schools.

2.4 Empirical Specification

We begin by using administrative data from the UC Office of the President (UCOP) to document how the end of affirmative action affected admission rates for URMs relative to other racial groups. We then use data from the College Board to relate these changes in admission rates to changes in SAT score-sending patterns.

Throughout the paper, we employ a simple differences-in-differences (DD) specification, which compares the minority-white gap after the implementation of Prop 209 to the gap before the implementation of Prop 209. For admissions, a basic equation for individual i applying to campus c in year t can be written as:

$$\begin{aligned}\text{Admitted}_{ict} = \alpha + \delta \cdot X_i + \beta_0 \cdot \text{URM}_i + \beta_1 \cdot \text{Ban}_t \\ + \theta \cdot \text{URM}_i \cdot \text{Ban}_t + \epsilon_{ict}\end{aligned} \tag{2.4}$$

In this specification, Ban is equal to one for the 1998-2000 cohorts and equal to zero for the 1995-1997 cohorts. For each campus, the regression sample is restricted to those who applied to the campus. The coefficient of interest θ represents the effect of the ban on the URM-white admission gap. The estimate is consistent if the error terms ϵ are uncorrelated with the introduction of the ban. The X_i term consists of controls for an applicant's SAT, GPA, parental income, and parental education; we use robust standard errors in all regressions.[8] For SAT score-sending, we use the same specification above, but use Sent_{ict}, the probability that individual i sends SAT scores to campus c in year t as the left-hand side variable.

As discussed below, the administrative data we use to examine the change URMs' relative probability of admission after the implementation of Prop 209 allow us to identify only the three-year window in which a student applied (1995-1997 vs. 1998-2000). By design, the break in the time periods corresponds to the implementation of Prop 209. Nonetheless, one drawback of our data is that it does not allow us to include time trends in our analysis. On the other hand, the fact that we are grouping our data by the three-year period before and the three-year period after the implementation of Prop 209 alleviates concerns about the impact of serial correlation in our outcome variable on our ability to make valid statistical inference.[9]

2.5 Data

We draw on two datasets for our analysis: a dataset obtained from the University of California Office of the President (UCOP) and a dataset produced by the College Board (CB) and provided to us, with the College Board's permission, by David Card.[10] The two datasets each contain individual-level data on hundreds of thousands of students, cover periods both before and after the implementation of Prop 209 in California, and fill in an important weakness of the other.

[8] In response to the bans, universities may have changed the weight placed on demographic characteristics. An alternative specification would be to include all interactions of URM, Ban, and X_i, and then compare average predicted values for URMs relative to whites before and after Prop 209. Performing this procedure results in the estimation of similar effects as simply letting all effects load onto the URM·Ban coeffcient as in Equation 2.4, so for the remainder of the paper we use the simpler specification.

[9] See Bertrand et al. [2004].

[10] This dataset was used by David Card and Alan Krueger in the 2005 paper we build upon here.

2.5.1 UCOP

Our UCOP data include all applicants who applied for freshman admission to any of UC's eight undergraduate campuses from 1995 through 2000.[11] The data contain individual-level information on students' race, adjusted high school GPA, SAT scores, parental income, and parental education. In addition, the data report the campuses to which each student applied, the campuses that accepted the applicant, and the campus at which the student enrolled, if any.[12]

Despite its strengths, the UCOP data has some significant drawbacks, most of which are related to its release as a publicly accessible dataset. In an effort to protect student privacy, UCOP collapsed many important descriptive categories.[13] Thus, for example, SAT scores and high school grades are reported in categories rather than discrete units and year of application and enrollment is grouped into three-year cohorts (1995-1997 and 1998-2000). As mentioned above, by design, the second-period cohort begins the year the ban on racial preferences was implemented. Race is also collapsed, from 10 categories into four: white, Asian, URM and other/unknown. The URM category includes American Indians, blacks, Chicanos and Latinos; these are the primary groups who received preferential treatment based on race before Prop 209. The other/unknown category includes both students who indicate that their race falls outside the categories used by the university, as well as students who choose not to reveal their race (a group that grew substantially after Prop 209 went into effect). In our empirical analysis, we compare admissions rates of URMs with the combined set of white and other/unknown, but our results are not sensitive to the choice of dropping the other/unknown group or including them with whites.[14] In addition, throughout the paper, we use whites rather than Asians as our primary control group because the application patterns of Asians differ substantially from those of both

[11] The eight UC campuses are Berkeley, Los Angeles, San Diego, Santa Barbara, Davis, Irvine, Santa Cruz and Riverside.

[12] Additional information about this rich, publicly available dataset can be found in Antonovics and Sander [2011].

[13] Public institutions releasing individual-level data are generally cautioned to suppress or combine data cells containing fewer than five observations (where cells are represented by combinations of information that can be established from public sources other than the database). UCOP generally combined data cells containing fewer than one hundred observations – a very cautious stance.

[14] The magnitude of the change in admissions probability for URMs is so large that it shows up no matter how the excluded group is defined.

whites and URMs (Asians apply at very high rates to UC schools).

2.5.2 College Board

The College Board data include every California and Texas high school student who took the SAT I as part of the cohorts that graduated from high school between 1995 and 2000, inclusive.[15] It also includes all blacks and Hispanics who took the SAT elsewhere in the United States as part of the 1995-2000 cohorts, and a 25 percent sample of all other SAT-takers who were part of those cohorts, but were neither black nor Hispanic nor residents of California or Texas. Demographic information comes from a background questionnaire that students fill out when they apply to take the SAT I. Students report their high school grades, their parents' income and education level, their race or ethnicity, their high school coursework and activities, and their academic and career aspirations. The College Board adds to these data information on student performance on SAT I and SAT II exams.

Students tell the College Board to which schools they would like their SAT scores sent, and although the UC has a centralized admission process, if students wish to apply to more than one UC campus, they must send their SAT scores to each of those campuses. During the registration process for the SAT (before students know their scores), students are allowed to send their scores to up to four schools for free. If students elect to send scores to more than four schools or if they choose to send scores after the registration process is over, then there is an additional fee for each score sent (currently, the fee is $10.50 per score sent). Although the fee to send additional scores is seemingly small, work by Pallais [2009] suggests that students' score-sending behavior is sensitive to small changes in price. Thus, students are likely to focus their score-sending on schools to which they are likely to apply. Indeed, as mentioned above, like Card and Krueger [2005], we find a strong correlation between SAT score-sending rates and application rates for various race-SAT-year bins.[16] Thus, we use the set of schools to which students send their scores as a proxy for student interest

[15] In other words, a student taking the SAT in May 1994 as a high school junior would be part of a cohort that generally finished high school in 1995 and was reported by College Board as part of the 1995 cohort.

[16] Results available upon request.

in a particular college and whether the student will eventually apply. In our data, the College Board reports up to 30 schools to which students send their SAT scores, though only 10 percent of students send to 10 or more schools. In order to capture the population of students interested in attending college, in our main analysis we restrict our sample to Californians who send their scores to at least one school.

Race information in the College Board data is more detailed than that in the UCOP data, with race being divided into the categories American Indian, Asian, black, Hispanic, white, other, and missing. To maintain consistency with the UCOP data, we condense the race categories into the following three groups: URM, Asian, and white (which, as with in UCOP data, includes the categories other and missing). As mentioned above, we do not group together Asians and whites in our control group because the score-sending patterns of Asians differ substantially from those of whites and URMs.

College Board Data and the Timing of Prop 209

Defining the pre and post periods in the UCOP data is straightforward, since by construction there are only two time periods which fall on either side of the change in the admissions regime. Defining the pre and post period in the College Board data, however, is more complicated since the timing of when students took the SAT and sent out their SAT scores will affect whether their observed score-sending choices should best be thought of as occurring before or after the end of the use of racial preferences. Table 3.1 shows a timeline of important events in the passage of Prop 209. Because Prop 209 begin receiving media attention in the summer of 1996 and then was passed by voters in November 1996, it is difficult to know whether students in the 1997 cohort, who likely took the SAT between Spring 1996 and Fall 1996, should be counted in the pre or post Prop 209 period.[17] Thus, we drop all observations from the 1997 cohort. Whether or not 1997 is included does not change our main conclusions – most of the effects on score-sending are concentrated in 1999 and 2000 – but including the 1997 cohort muddies the interpretation of our estimates.

[17]Searching the LexisNexis article database gives the first mention of Prop 209 in July (1996, July 22). UC Regent Blasts Special Graduations: Connerly questions ceremonies sponsored by some ethnic groups. *The San Francisco Chronicle.*

We include the 1998 cohort with the treatment (post-Prop 209) group since their SAT score-sending decisions were likely to have been made after passage of Prop 209, and while most students in the 1998 cohort are likely to have taken the SAT (and made their score-sending decisions) during the period in which Prop 209 was still being challenged in court, a sampling of news articles from this time suggests that it was believed Prop 209 would eventually be upheld.

2.6 Results

We first document changes in admissions probability for URMs relative to whites, and then show how score-sending patterns changed after Prop 209.

2.6.1 Changes in the Probability of Admission (UCOP Data)

Summary statistics for UC applicants are shown in Table 3.2. Relative to whites, minority applicants tend to have lower SAT scores, lower GPAs, and are more likely to come from disadvantaged backgrounds. Our UCOP data do not provide continuous measures of parental income and education, so rather than showing sample means for each of the 10 intervals of parental income, we collapse these intervals into the three categories shown in Table 3.2. For education, we use a dummy variable for whether an applicant has a college-educated parent.[18] The definitions of these categories have little impact on the predicted effects of Prop 209.

To better understand the predictors of admission to UC campuses, Table 2.3 shows basic difference-in-difference regression coefficients for each campus and the UC system as a whole. Going from left to right, the campuses are ordered from most selective – defined by the average math SAT score of incoming freshman – to least selective. Relative to whites, URMs were significantly less likely to gain admission to the top schools after Prop 209. At Berkeley and UCLA, the likelihood of admission fell by 22 and 18 percentage points, respectively, for URMs compared to whites. Given that about 50 percent of URM applicants were accepted to Berkeley and UCLA before

[18]Categorizing education in this way is natural since many schools give an admissions preference to first-generation college students.

Prop 209 (authors' calculation), these declines are substantial. Relative to Berkeley and UCLA, there is a somewhat smaller fall in the relative admission rates of URMs at UCSD and UC Davis. Then, at the the remaining four campuses, the effects of Prop 209 on URMs' relative probability of admission is smaller still. Thus, the relative fall in URMs' chances of admission was the largest at the most selective UC schools.

Interestingly, at the top six campuses, the coefficient on Ban*URM is smaller in magnitude than the coefficient on URM, suggesting that even though Prop 209 significantly reduced the admissions advantage enjoyed by minority applicants, it did not eliminate it entirely. Of course, this continued admissions advantage may have been indirect. For example, the campuses may have given admissions advantages to students with characteristics that are correlated with race but that are not available in our data (such as whether the student is from a single-parent family). Indeed, the fact that parental income and parental education are negatively correlated with the likelihood of admission suggests that admissions preferences were given to students from disadvantaged backgrounds. Finally, the coefficient on URM tends to be the largest at the most selective schools, suggesting that before Prop 209 the extent of racial preferences was the highest at the most selective UC schools.

While Table 2.3 suggests that having higher test scores and a higher GPA is positively related to the likelihood of admission, the relationship between the *change* in URMs' relative likelihood of admission after Prop 209 is likely to be non-monotonically related to their academic credentials. Students with sufficiently high or low academic credentials would have been unaffected by the end of racial preferences if they would have been accepted (high credentials) or rejected (low credentials) under either regime. To better understand which students bore the brunt of the change in admissions regime, we use their index score, which is a weighted average of high school GPA (40 percent) and SAT scores (30 percent each for Math and Verbal). The index score variable was created for us by the UC Office of the President, where the weights were determined by regressing students' first-year UC GPA on high school GPA and SAT scores. Thus, by construction, the index score is designed to predict students' performance in college. From our standpoint, a primary reason for using the index score is that it provides a nice summary of students' academic credentials (as measured by GPA and test scores). In addition, the index is calculated from students' *actual* high school GPA and *actual* SAT scores (rather than the discretized versions of those variables available in the rest of the data). Thus, it is the only continuous measure of academic credentials available to us.

To characterize the effect of Prop 209 at various points of the academic credential distribution, we divide URM applicants to the UC into percentiles based on their index score. We then categorize non-URMs into corresponding bins based on their index scores. Thus, for example, we can compare URM applicants at a given index percentile with non-URM applicants whose index falls in a similar range. For a summary of average SAT and GPA within each percentile, see Figure 2.1a. Note that since whites tend to have higher GPA and SAT scores, there are relatively more whites at the higher end of the URM distribution. Figure 2.1b shows the share of white applicants who fall into each decile of the URM distribution; about 30% of white applicants have index scores that would place them in the top decile of the minority distribution.

An overview of how Prop 209 affected the admissions rates of applicants at various points of the credential distribution is shown in Figure 2.2a, which plots average admissions rates to Berkeley (conditional on applying) for URMs and non-URMs at each index percentile. Prior to Prop 209, URMs at nearly every point of the credential distribution were more likely than whites to gain admission to Berkeley, and this advantage was the largest for applicants in the middle of the distribution. After Prop 209, much of the gap in admission rates between URMs and whites was closed, although conditional on index, URMs remained more likely to be admitted.

To get a sense of how Prop 209 affected URMs' relative chances of admission at different points of the credential distribution, Figure 2.3 plots the coefficient on Ban*URM (using the same basic specification outlined in Equation 4) at each percentile of the URM credential distribution for each of the eight UC campuses. The figure is generated using a three-step process. First, coefficient estimates are obtained for each percentile of the index distribution. Second, the set of point estimates is smoothed using local polynomial smoothing. Finally, standard errors are obtained using a bootstrap method. The resulting plot shows the change in admissions probability for URMs relative to whites at different percentiles of the academic credential distribution.

Figure 2.3 indicates that even for students at the top of the URM credential distribution, there was a large and statistically significant drop in URMs' relative chances of admission to Berkeley, though at both Berkeley and UCLA the largest relative drop in URMs' chances of admission occurred for those in the middle of the URM credential distribution. At UC San Diego and UC Davis, the largest relative drop in URMs' likelihood of admission occurred for students in the 30th-60th percentiles of the URM credential distribution. At UC San Diego, for example, students at the 60th percentile of the URM credential distribution saw their predicted probability of admission fall by close to 50 percentage points (relative to a pre Prop 209 admission probability of about 60 percent). At the bottom four campuses, URMs in the lowest deciles experienced a significant relative drop in their chances of admission. At UC Santa Cruz, for example, URMs in the bottom 10 percent of the credential distribution experienced a 20 percentage point drop in their probability of admission relative to similarly qualified whites. Thus, due to the wide range of selectivity of the various UC campuses, all URMs, regardless of their academic credentials, were affected in some way by Prop 209. URMs at the top of the credential distribution saw their relative chances of admission fall at the most selective UCs, while those at the bottom of the credential distribution saw their relative chances of admission fall at the least selective UCs. In addition, the fact that different groups of students were affected differently at each campus suggests that URMs may have responded to Prop 209 by shifting applications away from some schools and towards others. The next section investigates these changes in score-sending patterns.

2.6.2 Changes in SAT Score-Sending (College Board Data)

We now turn to the effect of the ban on affirmative action on the SAT score-sending behavior of URMs using data from the College Board. As with the UCOP data, we begin by reviewing summary statistics, showing predictors of score-sending, and finally breaking students into credential groups.

Summary statistics by race and time period are shown in Table 2.4 and show similar patterns as the UCOP data. The main difference is that in the College Board dataset, we are able to differentiate between race, so we show blacks and Hispanics separately.

Table 2.4 helps explain why we do not group Asians with whites as part of our control group; their overall score-sending rates are much higher than those of other groups. In addition, in the 1990s, there was a general increase in Asian interest in UC campuses which began before Prop 209 and continued afterwards (the starkest example is at UC Irvine). Plotting score-sending rates shows a steady upward climb in the Asian score-sending rate, rather than a break from trend around the time of Prop 209, and this pattern holds after controlling for student-level characteristics.[19] As a result, we drop Asians from the remainder of our score-sending analysis. When Asians are added to the excluded category, there is little change in the estimates for the most selective schools, where Asian score-sending rates were relatively stable. However, at schools such as Irvine, where Asian score-sending increased dramatically, URM score-sending estimates become smaller.

Since students can their SAT scores to up to four schools for free, one possible explanation for the stability of URM score-sending rates after Prop 209 is that most students send scores to exactly four schools. Of course, even if this is true, students can still change the set of schools to which they send scores, and our data allow us to capture this. In addition, as Table 2.4 shows, approximately 50 percent of SAT takers sent their SAT scores to more than four schools both before and after Prop 209.

Table 2.5 shows the estimation results of our basic difference-in-difference specification. We see that after the ban on affirmative action, blacks and Hispanics experienced the largest relative declines in score-sending rates at the most selective UC campuses, and the relative declines in score-sending rates were generally smaller at the other UC campuses, with the exception of UC Santa Barbara where the relative decline in score-sending rates for blacks and Hispanics was comparatively large. At UC Riverside the score-sending rates of blacks and Hispanics actually increased by 2 percentage points relative to whites. This increase likely reflects the fact that URMs

[19]As a formal check, we run a 'placebo' test, comparing score-sending in 1995 to those in 1996, both pre policy change years. Non-significant coefficients would be evidence of similar trends before the passage of Prop 209. While coefficients for URMs are generally not significant, many campuses have significant coefficients for Asians, further evidence of pre-existing differential trends between Asians and whites. Results available from authors.

responded to the end of racial preferences by shifting their applications towards the less selective UCs. We discuss this phenomenon in greater detail below.

2.6.3 Magnitude of Findings

The declines in score-sending rates for blacks and Hispanics reported in Table 2.5 are small relative to the drop in their admissions rates reported in Table 3. At Berkeley, for example, the admission rate of blacks fell by 22 percentage points relative to whites, while their relative score-sending rate fell by only 3 percentage points.[20] Of course, this may just reflect a small elasticity of score-sending rates with respect to admission rates, but the relationship between score-sending rates and admissions rates in the cross section does not support this. For example, a 22 percentage point fall in URMs' chances of admission to UC Berkeley is roughly equivalent to moving from the 60th to the 36th percentile of the URM credential distribution prior to Prop 209 (see Figure 2.2a). Given that the gap in URM score-sending rates between these two percentiles is 9 percentage points (see Figure 2.2b), the 3 percentage point fall in the score-sending rates of URMs relative to whites at Berkeley after Prop 209 is comparatively small.

Interestingly, the relative decline in score-sending rates to Berkeley and UCLA is also small relative to URMs' initial higher tendency to send their SAT scores to those schools. For example, prior to Prop 209, the black score-sending rate to Berkeley was 18 percentage points higher than that of whites, and after Prop 209 this gap fell by only 3 percentage points, suggesting that Prop 209 only closed 17 percent of the gap in score-sending rates. In contrast, Prop 209 greatly reduced the gap in admissions rates. Table 2.3 shows that, prior to Prop 209, URMs were 38 percentage points more likely to be admitted to Berkeley than were whites with similar observable characteristics. After Prop 209 this gap fell by 22 percentage points, suggesting Prop 209 closed almost 58 percent of the gap in admissions rates.

Another way to gauge the size of the effect of Prop 209 on score-sending rates is to assume that the change in URMs' score-sending rate equals the change in their application rate and then determine the fall in the number of URMs who would have

[20]This implies a 15 percent drop in URMs' score-sending rate and a 45 percent fall in their admission rate.

been admitted due to this change in application behavior. A rough estimate of the yearly change in the number of students admitted to Berkeley due to changes in application behavior is given by:

$$\begin{aligned}\triangle\text{Admitted URMs} &= \triangle\text{URM Application Rate} * \text{Number of URM Applicants} \\ &\quad * \text{URM Admission Rate} \\ &= -.024 * 4604 * 0.24 \\ &= -27\end{aligned}$$

In the above calculation, all of the values are taken at their post Prop 209 levels. It is important to recognize that this calculation is likely to overstate the fall in the number of URMs who were admitted into Berkeley because, as we discuss below, the fall in score-sending rates after Prop 209 occurred primarily for those who experienced the largest drop in their probability of admission, suggesting that the post Prop 209 admission rate for this group is likely to be lower than the average post Prop 209 admission rate for URMs who actually applied.

Nonetheless, we can compare the above number to the yearly fall in the number of admitted URMs due to the fall in the admission rate after Prop 209:

$$\begin{aligned}\triangle\text{Admitted URMs} &= \triangle\text{URM admission rate} * \text{Number of URM applicants} \\ &= -.22 * 4604 \\ &= -1013\end{aligned}$$

To obtain the change in eventual enrollment, we then multiply the numbers obtained above by the enrollment rate (the fraction of admitted students who eventually enroll), which was approximately 0.41 for URMs in the post period. Doing so implies that 11 fewer URMs went to Berkeley because of changes in application behavior and 415 fewer URMs went to Berkeley because of changes in the admissions rate. These estimates ignore potential selection effects (for example, those who were no longer admitted may have been more likely to enroll, conditional on acceptance), but regardless of how the calculation is performed, it appears that Prop 209's effect on URM enrollment came largely through admissions probabilities rather than application rates.

2.6.4 Effects by Academic Credentials

As we saw in Figure 2.3, the magnitude of the relative change in admissions rates for URMs at the different UC campuses depended very much on their academic credentials. Thus, it seems likely that the relative change in score-sending rates for URMs would also depend on academic credentials, so we repeat our score-sending analysis for the index percentiles used in Figure 2.3. For example, students in the 90th percentile are those with index scores that would place them in at the 90th percentile of URM applicants to the UC system. Since the response of blacks and Hispanics shown in Table 2.5 is similar, we combine them into one URM group. This simplifies our results and has the added benefit of easing comparison with our estimates of the changes in admission rates obtained from the UCOP data where blacks and Hispanics could not be separately identified. In general, the effects of Prop 209 are similar for blacks and Hispanics, but any changes that did occur were more pronounced for blacks.

Average score-sending by index percentile is shown in Figure 2.2b. As with admissions probability, there was a large gap in score-sending rates between URMs and whites before Prop 209. However, Prop 209 appears to have closed a much smaller share of the score-sending gap than the admissions gap.

To get a sense of how Prop 209 affected URMs' relative score-sending rates at different points of the credential distribution, Figure 4 plots the coefficient on Ban*URM (using the basic specification outlined in Equation 4) at each percentile of the URM credential distribution for each of the 8 UC campuses. Consistent with the findings in Table 2.5, Figure 4 suggests that Prop 209 may have led URMs to shift their applications away from the most selective UC schools (like Berkeley) towards the less selective UC schools (like Riverside). In addition, at Berkeley and UCLA (the only two schools at which we find a statistically significant drop in URMs' relative score-sending rates for a large part of the credential distribution), the fall in score-sending rates is the largest for URMs who experienced the largest relative drop in admission rates – those in the middle of the credential distribution. Nonetheless, even for this group, the fall in score-sending rates is small, never exceeding 3 percentage points. Interestingly, we also see that even though URMs in the top 20 percent of

URM credential distribution experienced a large relative drop in their probability of admission to Berkeley (about 30 percentage points), there is no statistically significant change in the rate at which these URMs sent scores to Berkeley; a similar pattern emerges for UCLA as well. Overall, Figure 4 suggests that Prop 209 had the largest impact on URMs who experienced the largest drop in their chances of admission to UCLA and Berkeley and that URMs in this group responded by increasing the rate at which they sent scores to other UC schools, but the magnitude of these changes is small.

Focussing on Berkeley and UCLA, Figure 2.5 shows that URMs in the 20th through 60th percentiles of the academic credential distribution experienced the largest relative drop in score-sending rates to these schools. We next investigate further the nature of the changes in score-sending patterns for URMs with academic credentials in this range. To simplify the remainder of our analysis, we combine students into three groups: the top 40 percent (whose score-sending rates to Berkeley and UCLA were relatively stable), the middle 40 percent (where the relative decline in URMs' score-sending rates to Berkeley and UCLA was concentrated), and the bottom 20 percent.

The top panel of Table 2.6 shows how the average number of SAT scores sent to Berkeley and UCLA changed for URMs relative to whites after the ban on affirmative action. For the middle 40 percent of the academic credential distribution, we find a statistically significant drop in the number of scores sent to Berkeley and UCLA, though the size of the point estimate is extremely small and suggests that URMs reduced the number of scores send to Berkeley and UCLA by only 0.037 (so that for every 100 scores sent before Prop 209, only 96.3 were sent after Prop 209). We also find evidence of a small relative drop in the number of scores sent to Berkeley and UCLA for URMs in the bottom 20 percent of the academic credential distribution, but we find no relative change in the number of scores for those in the top 40 percent.

The number of scores sent to the other six UCs are also examined in the top panel of Table 2.6. Interestingly, for URMs in the middle of credential distribution, we find that the drop in the number of scores sent to Berkeley and UCLA is offset by a similarly sized increase in the number of scores sent to the other UC campuses. For the higher and lower credential groups, there is no statistically significant change in the number of applications to lower-tiered UC schools. We also find that there was no relative change in the average number of applications URMs sent to the UC system as a whole.

A direct implication of the fact that students in the middle 40 percent sent fewer applications to Berkeley and UCLA and more applications to other UCs is that the average quality of the set of UC schools to which URMs sent their scores must have declined. To examine this, the bottom panel of Table 2.6 uses the average SAT score of incoming freshmen at the schools to which students sent their SAT scores as the outcome variable (for example, 1344 at Berkeley and 1111 at Riverside). This allows us to gauge the extent to which there was a fall in the average quality of the schools to which URMs sent their SAT scores after the end of affirmative action. Not surprisingly, the middle credential group sent scores to less selective schools, but the magnitude of the change is extremely small; the average combined math and verbal SAT score at the UC schools to which URMs sent scores fell by only 5 points. In addition, there is some evidence that students in the top 40 percent sent their SAT scores to lower quality UC schools, although again the magnitude of this effect is extremely small (less than a 2 point drop in combined math and verbal SAT scores).[21]

We test whether the basic patterns revealed in Table 2.6 by our score sending data from the College Board – that URMs in the middle of the credential distribution shifted applications away from the top tier towards the remaining UCs – also appear in our application data from the UCOP. This helps reassure us that changes in application behavior are reflected in score-sending patterns. Note, however, that while Table 6 focusses on Californians who sent their SAT scores to at least one school, our analysis of the UCOP data focusses on Californians who applied to at least one UC school (because those are the only individuals in the UCOP data). Results are shown in Table 2.7. While the magnitude of the findings is somewhat larger for our UCOP data than our College Board data, the basic patterns hold: relative to simi-

[21] Another avenue by which students gain entrance into UCs is by initially attending another college and then transferring; this would not be captured by our score-sending data. StatFinder releases data on the number of transfer students by year and race (available at statfinder.ucop.edu). A visual inspection of a plot of transfer students by race over time reveals no obvious trend break after Prop 209, with the possible exception of an increase for whites at UCLA.

larly qualified whites, URMs in the middle of the credential distribution had a small but statistically significant drop in the number of applications sent to Berkeley and UCLA, and a statistically significant increase in the number of applications sent to the remaining UCs. Consistent with the College Board data, the UCOP data also show that the most highly qualified URM applicants did not send fewer applications (relative to whites) to Berkeley and UCLA after Prop 209. Like the College Board data, Table 7 also shows a small drop (less than 7 points) in the average combined SAT score of freshman at the schools to which URMs applied. Unlike the College Board data, however, the UCOP data show that for the most highly qualified applicants, there was a small relative increase in the number of applications URMs sent to the remaining UCs and in the number of applications URMs sent to UC schools in general. Nonetheless, on balance, the changes in application patterns in the UCOP data tell a similar story to the changes in score-sending patterns in the College Board data.

2.6.5 Other Schools

This section explores whether there were changes in score-sending patterns to the most popular UC alternatives.

Selective non-UCs

To assess whether URMs changed their score-sending patterns to schools outside the UC, we first identify the set of schools to which URMs most frequently send their SAT scores in 1995 (prior to the ban on affirmative action).[22] We begin by examining the number of scores sent to the 10 schools to which the most highly qualified 80 percent of URMs sent the most scores in 1995.[23] While it also would be possible to examine the number of scores sent to the 10 schools to which the most highly qualified 20 percent or 40 percent of URMs sent their scores, there is considerable overlap between the most popular schools in these groups.

[22] Alternatively, we could have counted the number of scores sent to non-UC schools, but this number is difficult to interpret since it would include scores sent to scholarship and other organizations.

[23] These are (in order of most frequent receivers to least) USC, Stanford, Loyola Marymount, University of San Diego, Pepperdine, Santa Clara, Harvard, University of San Francisco, Arizona State, and Occidental College.

Results for the number of scores sent to this set of schools are shown in Table 2.8. For these regressions, we show blacks and Hispanics separately since their results are quite different. Blacks in the middle 40 percent and bottom 20 percent credential groups had marginally significant increases in the number of scores sent to these schools, while all Hispanics reduced the number of scores sent to these schools. To further investigate, we show residual plots shown in Figure 2.6. It appears that Hispanics may have experienced a downward trend in scores sent to these selective schools that pre-dated Prop 209.[24]

Less Selective non-UCs

We also examine the number of scores sent to the 23 campuses in the California State University (CSU) system, since these schools form the overwhelming majority of non-UC schools to which low-credential URMs (those in the bottom 20 percent of the academic credential distribution) sent SAT scores. It is worth noting that the CSU system was also affected by the statewide ban on affirmative action. Interestingly, however, at the time the ban went into effect (1998) virtually none of the CSU campuses were selective; 21 out of the 23 were able to admit every qualified student. Of the two schools that did have selective admissions, Cal Poly San Luis Obispo and Sonoma State, the only discernible break in enrollment numbers for URMs occurred at Cal Poly San Luis Obispo, suggesting that the ban on affirmative action had very little *de facto* effect on the CSU system.[25] For the CSU system, Table 2.8 reports that there is no statistically significant relative change in the number of scores sent by URMs to schools in the CSU system, except for a small increase for URMs in the bottom 20 percent of the credential distribution.

2.7 Conclusion

Our analysis of admission rates and SAT score-sending patterns reveals a number of interesting findings. First, all URMs, regardless of their academic credentials,

[24] While it would be possible to add race-specific time trends to these regressions, the limited number of years causes standard errors to be large.

[25] The CSU system has become much more selective over time, so the ban on affirmative action is likely to bite at many schools in the CSU system now.

experienced a substantial relative drop in their chances of admission to at least one UC school. Second, the relative decline in URM score-sending rates after the end of affirmative action was concentrated at Berkeley and UCLA among students who experienced the largest drops in their predicted probability of admission to those schools, but the fall in score-sending rates was still small relative to the drop in URMs' predicted probability of admission. Third, for the group of students for whom URMs' relative score-sending rates to Berkeley and UCLA fell the most, we find evidence that, relative to whites, URMs increased the number of scores they sent to less selective UCs. This had the effect of lowering the average quality of the set of UC schools to which URMs sent their SAT scores (relative to whites), though the magnitude of this drop was extremely small.

An important issue in the debate surrounding Prop 209 (and bans on affirmative action more generally) is whether the end of the use of racial preferences lowered the value URMs placed on attending UC schools. For example, URMs may have become less interested in applying to UC schools after the ban because they feared (justifiably) that they would have fewer same-race peers if they were admitted. As discussed above, our theoretical model suggests that it is difficult to determine whether the changes in SAT score-sending rates documented above arose because of changes in URMs' predicted probability of admission or because of changes in the value URMs placed on attending UC schools. Nonetheless, taken at face value, our results are not broadly consistent with the idea that Prop 209 dramatically lowered URMs' interest in attending UC schools.

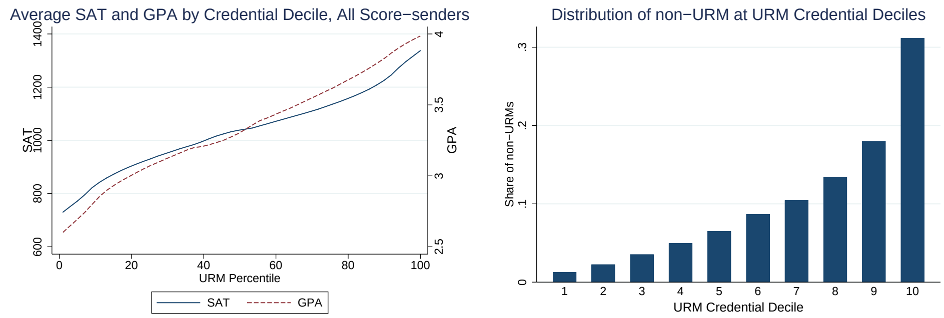
In particular, while our model suggests that a fall in URMs' chances of admission could lead to an increase in the rate at which they sent their SAT scores to UC schools, admissions rates and score-sending rates are generally positively associated, and consistent with this, we find that the largest relative drops in URMs' score-sending rates occurred among URMs who experienced the largest relative drops in their predicted probability of admission. In light of this, the fact that URMs' score-sending rates declined by so little despite the enormous drop in their predicted probability of admission is hard to reconcile with the possibility that Prop 209 severely lowered the value URMs placed on attending UC schools.

In terms of evaluating why the total enrollment numbers of URMs fell dramatically at schools like Berkeley and UCLA after the end of racial preferences, the results of this paper suggest that this fall was brought about by the direct effect of the drop in URMs' chances of admission and not because URMs were discouraged from applying. Minority interest in the UC appears to have remained high. Conditional on observables, URMs were more likely than whites to apply to UC schools both before and after the ban on affirmative action. This suggests that efforts to increase diversity in the UC system should be targeted at increasing URMs' level of academic preparation before they apply for college.

2.8 Acknowledgement

This paper was co-authored with Kate Antonovics and has been submitted for publication. We thank the University of California Office of the President, the College Board, and David Card for assistance in obtaining our data. In addition, we are grateful to Prashant Bharadwaj, Julie Cullen, Gordon Dahl, Karthik Muralidharan, Joel Sobel, UC San Diego seminar participants, and especially Richard Sander for helpful comments and discussion.

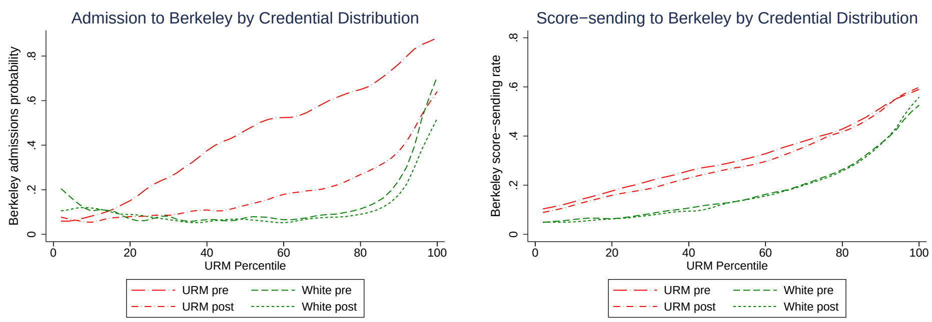
2.9 Figures and Tables



(a) Description of Deciles

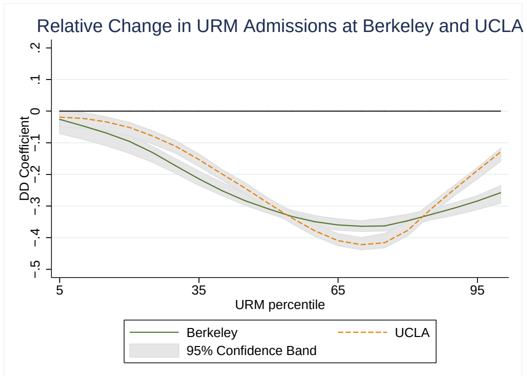
(b) Relative Distributions of URMs and non-URMs

Figure 2.1: Index generated by weighted average of SAT (30% each for verbal and math) and GPA (40%). URM Percentile axis consists of percentile of distribution of URM UC applicants.

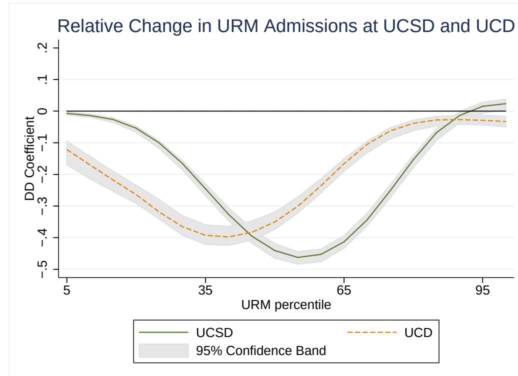


(a) Average Admissions to Berkeley (b) Average Score-sending to Berkeley

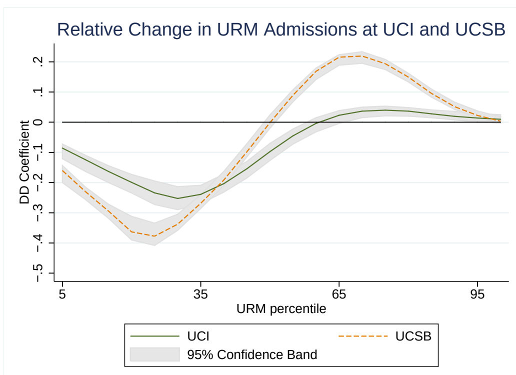
Figure 2.2: Index generated by weighted average of SAT (30% each for verbal and math) and GPA (40%). URM Percentile axis consists of percentile of distribution of URM UC applicants.



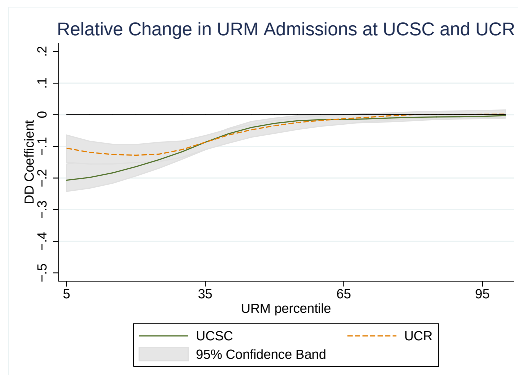
(a) Top tier



(b) Second tier

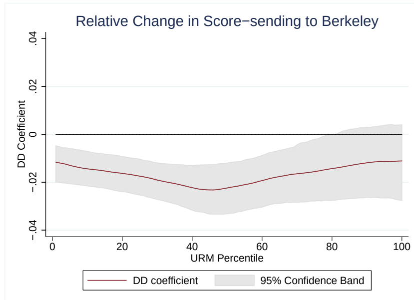


(c) Middle tier

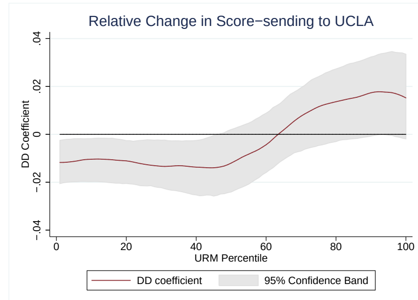


(d) Low tier

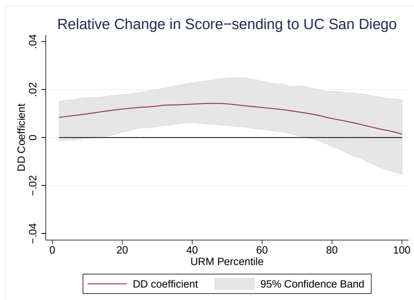
Figure 2.3: The dependent variable is a binary indicator of whether the student was accepted to a given school conditional on applying to that school. Each point shows OLS coefficient estimate on the interaction between post Prop 209 and URM for a decile of the academic credential distribution of URM UC applicants. Each estimate also includes the controls used in Table 3.2: SAT math and verbal scores, high school GPA, and parental income and education. The excluded race category includes white and other/unknown. Sample includes 1995-1997 (pre Prop 209) and 1998-2000 (post Prop 209).



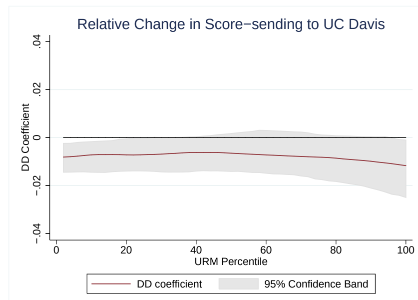
(a) Berkeley



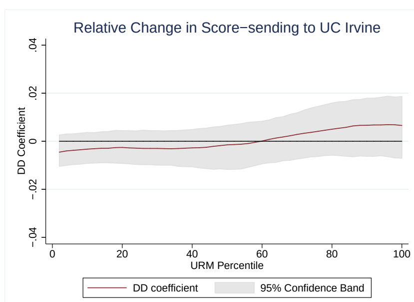
(b) UCLA



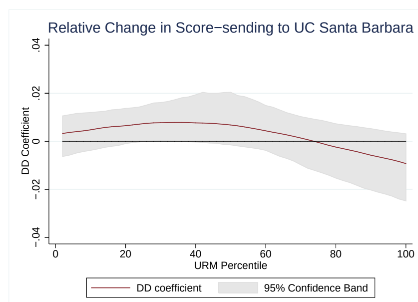
(c) UCSD



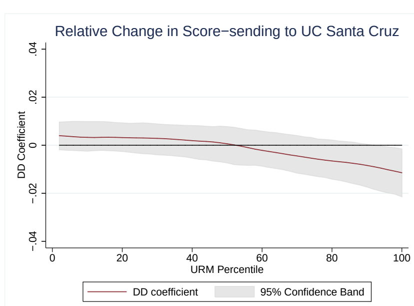
(d) UCD



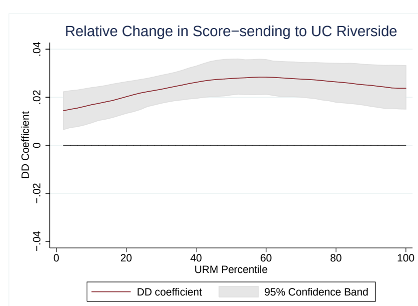
(e) UCI



(f) UCSB



(g) UCSC



(h) UCR

Figure 2.4: Relative Change in URM Score Sending. See notes for Figure 2.5.

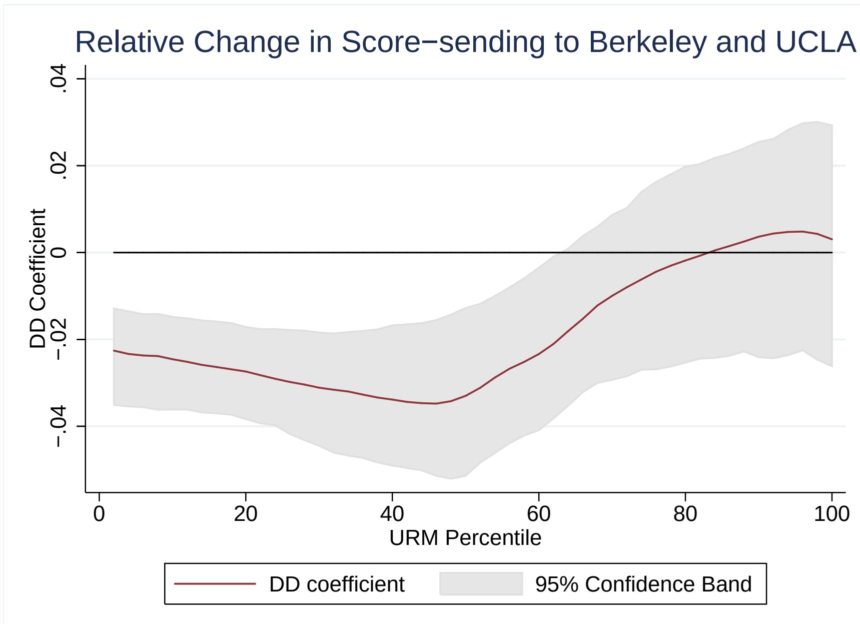


Figure 2.5: The dependent variable is a binary indicator of whether the student sent their SAT scores. Each point shows OLS coefficient estimate on the interaction between post Prop 209 and URM for a percentile of the academic credential distribution of URM UC applicants. Each estimate also includes the controls used in Table 3.2: SAT math and verbal scores, high school GPA, and parental income and education. The excluded race category includes white and other/unknown. Sample includes 1995-1996 (pre Prop 209) and 1998-2000 (post Prop 209). Plots smoothed with local polynomial regressions; bootstrapped standard errors displayed.

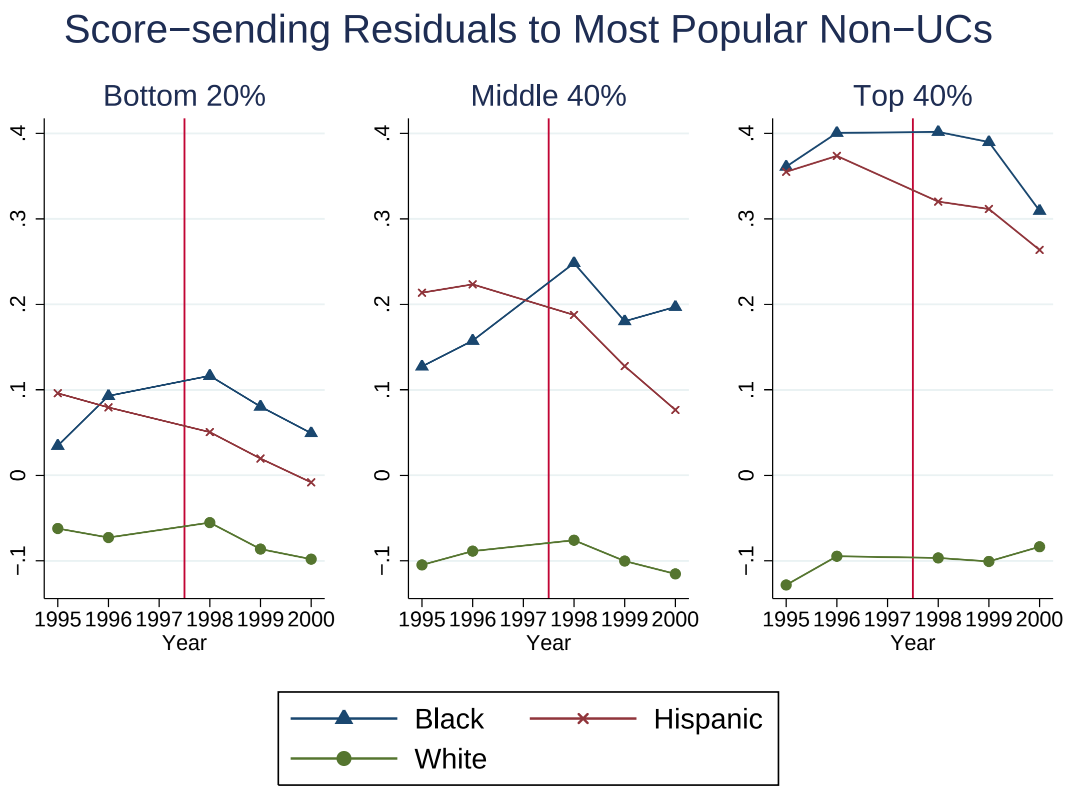


Figure 2.6: Score-sending residuals to most frequent 10 outside options of top 80 percent of URM applicants (see text for list of schools). Residuals obtained from regressing number of scores sent on the demographic characteristics shown in Table 3.2: SAT math and verbal scores, high school GPA, and parental income and education. Students divided into groups based on academic credentials of URM UC applicants.

Table 2.1: Proposition 209 Timeline

Date	Event
July 1995	SP1 (was never implemented)
Spring 1996	1997 cohort begins taking SAT
July 1996	First mention of Prop 209 in media
Summer 1996	Prop 209 reported as being likely to pass
November 1996	Prop 209 passed by voters
Fall 1996	End of observation period for 1997 cohort
Spring-Fall 1997	1998 cohort takes SAT
November 1997	Supreme Court denies appeals, Prop 209 becomes law
Fall 1998	First affected cohort (1998) enrolls

Notes: See text for description.

Table 2.2: UCOP Summary Statistics

	All		White		URM		Asian	
	Before	After	Before	After	Before	After	Before	After
SAT Math	597	602	608	612	528	534	622	625
	(94)	(93)	(82)	(82)	(93)	(93)	(90)	(91)
SAT Verbal	569	572	597	597	525	527	558	562
	(97)	(96)	(85)	(86)	(93)	(93)	(102)	(100)
Adj HS GPA	3.6	3.6	3.6	3.7	3.4	3.5	3.7	3.7
	(.5)	(.49)	(.5)	(.49)	(.48)	(.49)	(.5)	(.49)
Parent Income <40,000	.33	.29	.19	.17	.54	.49	.39	.36
Parent Income 40,000-99,999	.43	.42	.48	.44	.37	.38	.42	.4
Parent Income >100,000	.24	.29	.33	.39	.098	.13	.19	.24
Parent Educ: At Least 4 Year College	.66	.65	.79	.77	.37	.36	.65	.64
Total UC applications	2.9	3.2	2.6	2.9	2.7	3	3.3	3.6
	(1.4)	(1.5)	(1.4)	(1.5)	(1.2)	(1.3)	(1.6)	(1.6)
Observations	136766	149305	63258	70448	26694	27707	46814	51150

Notes: Standard deviations in parentheses. Before includes years 1995-1997; after includes 1998-2000. White includes white and other/unknown, while URM includes blacks, Hispanics, and American Indians.

Table 2.3: Probability of Admission Difference-in-Difference

	Berkeley	UCLA	UC SD	UC D	UC Irvine	UC SB	UC SC	UC R
URM	0.42***	0.37***	0.27***	0.28***	0.09***	0.14***	0.07***	0.02**
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Asian	-0.00	0.02***	0.03***	-0.04***	-0.03***	0.01***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
URM*Ban	-0.30***	-0.25***	-0.20***	-0.21***	-0.05***	-0.02***	-0.06***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Asian*Ban	0.01	0.01**	0.00	0.02***	-0.00	-0.00	0.00	-0.00
	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
R2	0.27	0.38	0.50	0.36	0.40	0.39	0.18	0.17
Obs	133589	152268	134536	101649	97102	112949	65976	63985

Notes: Robust standard errors in parentheses. The dependent variable is a binary indicator of whether the student was accepted to a given school conditional on applying to that school. The excluded race category includes white and other/unknown. SAT scores are divided by 100 for similar scaling to the other controls. The sample includes data from 1995-1997 (pre Prop 209) and 1998-2000 (post Prop 209).

$***$ $p<0.01$, $**$ $p<0.05$, $*$ $p<0.1$

Table 2.4: College Board Summary Statistics

	All		White		Black		Hispanic		Asian	
	Before	After	Before	After	Before	After	Before	After	Before	After
SAT Math	520	527	543	553	428	436	456	459	554	560
	(114)	(116)	(102)	(102)	(100)	(103)	(102)	(103)	(117)	(120)
SAT Verbal	502	507	538	543	437	441	450	450	490	498
	(115)	(115)	(102)	(102)	(103)	(104)	(105)	(104)	(123)	(121)
Adj HS GPA	3.3	3.3	3.4	3.4	2.9	3	3.1	3.2	3.4	3.4
	(.63)	(.61)	(.6)	(.58)	(.62)	(.62)	(.62)	(.62)	(.61)	(.59)
Parent Income <40,000	.45	.39	.28	.22	.62	.57	.68	.63	.54	.49
Parent Income 40,000-99,999	.4	.39	.5	.47	.31	.32	.26	.28	.35	.35
Parent Income > 100k	.11	.14	.17	.22	.038	.055	.033	.044	.082	.1
Parent Educ: At Least 4 Year College	.57	.56	.7	.7	.47	.47	.28	.26	.59	.57
Number of Schools to Which Sent Scores	5.6	5.7	5.6	5.6	5.2	5.5	5.1	5.2	6.3	6.2
	(2.8)	(2.9)	(2.9)	(3)	(2.6)	(2.8)	(2.3)	(2.4)	(2.9)	(2.9)
Sent to UC	.67	.68	.62	.65	.52	.52	.64	.64	.83	.84
Number of UCs to Which Sent Scores	1.8	2	1.5	1.7	1.1	1.2	1.5	1.5	2.7	3
	(1.8)	(1.9)	(1.6)	(1.8)	(1.4)	(1.5)	(1.5)	(1.6)	(2)	(2)
Sent to More Than 4 Schools	.54	.55	.52	.54	.46	.5	.45	.47	.66	.67
Observations	196,405	314,308	95,645	152,306	14,006	20,907	38,749	63,276	45,624	74,966

Notes: Standard deviations in parentheses. Sample includes Californians who sent their SAT scores to at least one school. Before includes years 1995-1996; after includes 1998-2000. White category includes white and other/unknown.

Table 2.5: Score-sending Difference-in-difference

	Berkeley	UCLA	UC SD	UC D	UC Irvine	UC SB	UC SC	UC R
Black	0.17***	0.18***	-0.00	0.01**	0.01***	-0.09***	-0.05***	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Hispanic	0.12***	0.17***	0.05***	0.00	0.06***	0.00	0.01***	0.04***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ban*Black	-0.02***	-0.01	0.00	-0.01*	-0.01	0.00	-0.00	0.02***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Ban*Hispanic	-0.01**	-0.00	-0.00	-0.02***	-0.00	-0.01	-0.01*	0.01***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
R2	0.14	0.10	0.09	0.07	0.02	0.06	0.02	0.01
Observations	384889	384889	384889	384889	384889	384889	384889	384889

Notes: Robust standard errors in parentheses. The dependent variable is a binary indicator of whether the student was sent a score to a given school. The excluded race category includes white and other/unknown, while URM includes blacks and Hispanics. SAT scores are divided by 100 for similar scaling to the other controls. Sample includes Californians who sent a score to at least one school and includes data from 1995-1996 (pre Prop 209) and 1998-2000 (post Prop 209).

$***$ p<0.01, $**$ p<0.05, $*$ p<0.1

Table 2.6: Score-sending Difference-in-difference Coefficients by Academic Credentials

	Bottom 20%	Middle 40%	Top 40%
Number of scores to Berkeley and UCLA			
URM	0.184***	0.347***	0.342***
	(0.005)	(0.007)	(0.009)
Ban*URM	-0.026***	-0.053***	-0.012
	(0.007)	(0.009)	(0.011)
Number of scores to UC other than BER/UCLA			
URM	0.132***	0.183***	-0.019
	(0.009)	(0.012)	(0.015)
Ban*URM	-0.023	0.014	-0.003
	(0.012)	(0.016)	(0.019)
Number of UC scores			
URM	0.316***	0.530***	0.323***
	(0.012)	(0.016)	(0.019)
Ban*URM	-0.048**	-0.039	-0.016
	(0.015)	(0.020)	(0.024)
UC senders: average SAT of UCs sent to			
URM	11.827***	16.331***	13.837***
	(1.042)	(0.732)	(0.617)
Ban*URM	-0.578	-5.322***	-2.378**
	(1.371)	(0.938)	(0.764)

Notes: Robust standard errors in parentheses. Each set of estimates shows OLS coefficient estimate on the URM constant term and interaction between post Prop 209 and URM. Each estimate also includes the controls used in Table 3.2: SAT math and verbal scores, high school GPA, and parental income and education. The excluded race category includes white and other/unknown, while URM includes blacks and Hispanics. Sample includes Californians who sent a score to at least one school and includes data from 1995-1996 (pre Prop 209) and 1998-2000 (post Prop 209). Groups are created by using the academic credential distribution of URM UC applicants. Bottom panel restricted to students who sent to at least one UC.

$***$ p<0.01, $**$ p<0.05, $*$ p<0.1

Table 2.7: Application Difference-in-difference Coefficients by Academic Credentials

	Bottom 20%	Middle 40%	Top 40%
UC applicants: applications to Berkeley and UCLA			
URM	0.25***	0.27***	0.09***
	(0.01)	(0.01)	(0.01)
Post*URM	-0.06**	-0.07***	0.01
	(0.02)	(0.01)	(0.01)
UC applicants: applications to UC other than BER/UCLA			
URM	-0.24***	-0.34***	-0.30***
	(0.03)	(0.02)	(0.01)
Post*URM	0.07	0.11***	0.04*
	(0.04)	(0.02)	(0.02)
UC applicants: number of UC scores			
URM	0.01	-0.07***	-0.21***
	(0.03)	(0.02)	(0.02)
Post*URM	0.01	0.04	0.05*
	(0.04)	(0.03)	(0.02)
UC applicants: average SAT of UCs applied to			
URM	11.59***	14.10***	7.72***
	(1.12)	(0.57)	(0.47)
Post*URM	-5.80***	-6.63***	-2.80***
	(1.60)	(0.80)	(0.64)

Notes: Conditional on applying to at least 1 UC.
$*\!*\!*$ p<0.01, $*\!*$ p<0.05, $*$ p<0.1

Table 2.8: Score-sending to non-UCs, Difference-in-difference Coefficients

	Bottom 20%	Middle 40%	Top 40%
Number of scores sent to 10 most frequent receiving schools from top 80 percent of URMs (selective)			
Black	0.147***	0.251***	0.499***
	(0.010)	(0.016)	(0.026)
Hisp	0.167***	0.329***	0.477***
	(0.008)	(0.011)	(0.014)
Ban*Black	0.031*	0.083***	-0.025
	(0.013)	(0.021)	(0.033)
Ban*Hisp	-0.049***	-0.060***	-0.055**
	(0.011)	(0.014)	(0.018)
Number of scores sent to Cal State system			
URM	0.116***	-0.102***	-0.186***
	(0.014)	(0.013)	(0.011)
Ban*URM	0.050**	0.007	-0.000
	(0.019)	(0.017)	(0.014)

Notes: Robust standard errors in parentheses. Each set of estimates shows OLS coefficient estimate on the URM constant term and interaction between post Prop 209 and URM. Each estimate also includes the controls used in Table 3.2: SAT math and verbal scores, high school GPA, and parental income and education. The excluded race category includes white and other/unknown, while URM includes blacks and Hispanics. Sample includes Californians who sent a score to at least one school and includes data from 1995-1996 (pre Prop 209) and 1998-2000 (post Prop 209). Groups are created by using the academic credential distribution of URM UC applicants. List of schools may be found in text.

$***$ $p<0.01$, $**$ $p<0.05$, $*$ $p<0.1$

Chapter 3

Explicit Versus Implicit Racial Preferences and Their Effect on Student Quality

[with Kate Antonovics and Valerie Ramey] In November 1996, California voters passed Proposition 209, barring the University of California system from explicitly using race as a criterion in admissions. Using administrative data on every applicant to the University of California (UC) from 1995-2000, we present evidence that while UC campuses responded by cutting *explicit* racial preferences, they also changed the weight given to SAT scores, grades and family background characteristics to *implicitly* favor minorities, though the admissions rate of minorities remained far below its pre-Prop 209 level. Interestingly, we find that these changes had little effect on the quality of admitted students as measured by expected first-year college GPA. We also investigate the consequences of Prop 209 for human capital investment prior to college entry. We find some evidence that SAT scores and high school GPA responded to the changes in the admission rules at the UC schools, though the magnitude of the response was extremely small. Finally, we find no consistent evidence that Prop 209 affected the racial gap in achievement prior to college entry.

3.1 Introduction

In November 1996, California voters passed Proposition 209, barring the University of California system from explicitly using race as a criterion in admissions.[1] While bans on affirmative action like Prop 209 prohibit the explicit use of race in admission, they do not prevent colleges from promoting racial diversity indirectly. For example, even with a ban in place, colleges can still adopt admissions rules that implicitly favor minorities by placing more weight on student characteristics that are associated with being a minority. As pointed out by Fryer et al. [2008], those characteristics that best predict whether a student is a minority are necessarily different from those that best predict academic performance in college – otherwise affirmative action would not be needed in the first place. Thus, to the extent that colleges react to bans on affirmative action by changing their admissions formulas to place less weight on student characteristics that predict academic success, this could affect student quality both directly by changing the set of students who are admitted and indirectly by altering students' incentives to invest in human capital prior to college entry.

In light of this, our paper makes several contributions to the literature on affirmative action in higher education. First, using administrative data on every fall freshman applicant to each of the eight University of California campuses from 1995-2000, we document the extent to which each campus reacted to Prop 209 by changing its admissions rule to implicitly favor under-represented minorities (URMs), and we investigate how the new admissions rules affected the average quality of the pool of admitted students. To our knowledge, our paper is the first to systematically evaluate how observed changes in admissions rules following a ban on explicit racial preferences affected the pool of admitted students. Second, using data from the College Board (CB) and the National Assessment of Educational Progress (NAEP), we examine the

[1] The effects of Prop 209 and similar affirmative action bans in the late 1990s continue to be hotly debated. In February 2012, for example, the Supreme Court announced that it would hear Fisher v. University of Texas, a case involving the legality of race-conscious admissions at the University of Texas.

potential indirect effects of Prop 209 on student quality by focusing on human capital investment prior to college entry, and we highlight the weaknesses of previous research that has attempted to do this.

We find that the removal of explicit racial preferences dramatically lowered admissions rates for URMs relative to whites at selective UC campuses. This decline was partially offset by UC admissions officers decreasing the weight placed on applicant characteristics associated with being a non-URM, such as having college-educated parents and high SAT math scores. However, the resulting increase in URM admissions probability from reweighting was considerably smaller than the decline due to the removal of explicit racial preferences, leading to a net decline in URMs' admissions probabilities. The decrease in weight placed on SAT math coupled with an increase in weight placed on high school GPA at most UCs allowed for the preservation of quality of admitted students (as measured by expected first-year college GPA). In addition, we find evidence that, relative to students in the rest of the country, Californians performed better on the SAT verbal section and that eighth grade Californians scored lower on NAEP mathematics exams. Furthermore, we show evidence of a small increase in Californians' reported high school GPA. Taken together, these results are generally consistent with Californians responding to the new UC admissions rules. Finally, we find little evidence of a widening in the minority-white achievement gap within California.

The paper will proceed as follows. In Section 3.2 we discuss the related literature and the institutional details of California's affirmative action ban. Then in Section 3.3 we explore how the admissions rule changed at each of the eight UC campuses and document the extent to which the changes in the estimated admissions rule were able to restore minorities' admission rates to their pre-Prop 209 levels. Section 3.4 explores the short-term effects of the changes in the estimated admissions rule on the quality of the pool of admitted students. Section 3.5 then uses data from the College board and from the NAEP to examine the longer-term implications of Prop 209 on human capital investment prior to college entry. In Section 3.6, we discuss the usefulness of a number of other datasets we explored to determine the effect of Prop 209 on human capital investment prior to college entry, and Section 3.7 concludes.

3.2 Related Literature and Details of Ban

Here we consider the related literature on affirmative action in higher education, with a particular focus on papers that address the effect of affirmative action on student quality.[2]

As discussed above, UC schools may have reacted to Prop 209 by replacing explicit racial preferences with implicit racial preferences. Indeed, the theoretical literature on affirmative action in higher education draws a distinction between "color-sighted affirmative action", wherein there are explicit racial preferences in admissions, and "color-blind affirmative action", wherein colleges implicitly favor minorities by using admissions rules that favor students who possess characteristics that are positively correlated with being a member of a targeted racial group (see, for example, Fryer et al. [2008] and Ray and Sethi [2010]). Both forms of affirmative action stand in contrast to laissez-fair admission regimes in which race is not considered either explicitly or implicitly. Since Prop 209 only banned the explicit use of racial preferences, UC schools could still attempt to promote racial diversity through implicit racial preferences. Thus, Prop 209 most likely moved the UC from color-sighted to color-blind affirmative action (and one goal of this paper is to empirically establish this fact).

Building a model of college admissions, Chan and Eyster [2003] show that a move from color-sighted to color-blind affirmative action could decrease the average quality of admitted students (regardless of race) since color-blind affirmative action may lead admissions officers to partially ignore applicants' qualifications. Ray and Sethi [2010] additionally point out that color-blind affirmative action creates an incentive for admissions officers to adopt admissions policies that are non-monotone in the sense that, within each racial group, some students with lower scores are admitted while those with higher scores are rejected. In this case, average student quality will necessarily be lower under color-blind relative to color-sighted affirmative action.

Both Chan and Eyster [2003] and Ray and Sethi [2010] take students' human capital investment decisions as fixed so that all changes in average student quality

[2]See Holzer and Neumark [2000] for a comprehensive review of the theoretical and empirical literature on affirmative action more generally.

operate through changes in the pool of admitted students. In contrast, building a model of college admissions with endogenous human capital investment, Fryer et al. [2008] show that color-blind affirmative action will alter students' incentives to invest in human capital. Thus, they note that relative to color-sighted affirmative action, color-blind affirmative action may lower student quality both in the short run (by altering the pool of admitted students) and in the long run (by lowering incentives to invest in human capital). This distinction between the short-run and long-run effects moving from color-sighted to color-blind affirmative action is central to our analysis of Prop 209. In addition, we emphasize that the potentially negative effects of color-blind affirmative action noted in the theoretical literature are not race-specific; the average quality of all students could be lower under color-blind than under color-sighted affirmative action. Finally, we note that the theoretical literature on affirmative action in higher education does not yield definitive predictions about how the racial *gap* in student quality will change with a move from color-sighted to color-blind affirmative action. Nonetheless, our paper explores this issue empirically.

Several other papers have empirically investigated the likely effects of banning explicit racial preferences on student quality. Using the College and Beyond data, Fryer et al. [2008] simulate the effects of moving from color-sighted to color-blind affirmative action on student quality, and find that the quality of the pool of admitted students (as measured by predicted class rank in college) is lower under color-blind relative to color-sighted affirmative action. Similarly, Epple et al. [2008] calibrate a general equilibrium model of college choice and college admissions that allows admissions officers to use color-blind affirmative action after a ban on color-sighted affirmative action, and find that bans on color-sighted affirmative action (like Prop 209) are inefficient in the sense that they lower the average SAT scores of admitted students, especially at highly selective colleges. Since neither Fryer et al. [2008] nor Epple et al. [2008], however, use data from before and after a ban on affirmative action, they cannot assess the actual observed effects of moving from color-sighted to color-blind affirmative action. Our paper builds upon their work by showing how the weights given to student characteristics in determining admissions in fact changed after Prop 209. Like us, Long and Tienda [2008] examine how the weights given to student characteristics changed at public universities in Texas after that state banned affirmative action. Their focus, however, is on assessing whether these changes in the admissions rule were able restore the share of minorities who were admitted rather than on the effect on student quality.[3]

Like us, Furstenberg [2010] uses data from the College Board to explore how the bans on affirmative action in California and Texas affected human capital investment prior to college entry.[4] Specifically, relative to states that did not ban affirmative action, Furstenburg finds that the black-white test score gap grew by 4% in California and 6% in Texas, and the Hispanic-white gap grew by about 8% in California. As we discuss in greater detail Section 3.5, however, we are unable to replicate Furstenburg's results. In addition, while Furstenburg's focus is on the racial *gap* in test scores, we also focus on how the SAT scores of Californians changed relative to the SAT scores of students in other states.[5] This is important, since, as mentioned above, a move from color-sighted to color-blind affirmative action is likely to impact *all* students via changes in the admissions rule. Additionally, unlike Furstenburg, we also use the College Board data to examine the effects of Prop 209 on reported high school GPA, which is a strong predictor of college admissions and was given increased weight by most UCs after Prop 209.

Caldwell [2010] uses data from the National Longitudinal Survey of Youth (NLSY) to show that blacks – especially those in 7th and 8th grade – scored lower on cognitive achievement tests following the bans on affirmative action in California and Texas. Unfortunately, as we discuss in Section 3.6, we doubt the appropriateness of using the NLSY to examine state-level policy changes like Prop 209. In addition, we note that some of Caldwell's analysis uses outcome variables that first appear in 1996, so he does not conduct a pre- and post- policy change comparison, but rather displays coefficients on the interaction between race and state. Caldwell attributes these coefficients to the policy change, but they may simply reflect differences between

[3]Table 4 of their paper indicates that the change in weights led to a small reduction in the SAT/ACT scores of admitted students, but the paper does not otherwise assess the quality of admitted students.

[4]The University of Texas stopped the explicit use of racial preferences starting with the freshman class of 1997.

[5]The implicit assumption here is that Prop 209 had little effect on the human capital investment decisions of those in other states. To the extent that it did, our approach will tend to understate the effects of Prop 209.

minority students in California and Texas relative to the rest of the country.

Evidence that students' human capital investment decisions respond to the incentives created by colleges admissions policies is also evident in Cortes and Friedson [2011] and Cullen et al. [2011], both of which find evidence of students moving to lower quality high schools after Texas introduced its top ten percent plan, which guaranteed admission to any public university in Texas for students who graduated in the top ten percent of their high school class.

Finally, we note that one strand of the literature on affirmative action examines whether affirmative action creates a mismatch between the quality of the average student and the quality of the average minority (see, for example, Sander [2004], Rothstein and Yoon [2008], and Arcidiacono et al. [2011]). The hypothesis is that aggressive affirmative action programs destine minority admits to be at the bottom of their incoming class in terms of academic credentials. As a result, the claim is that these students are likely to do poorly (relative to their white peers) in college, which in turn may adversely affect their later life outcomes. In this paper, we focus on student quality rather than on the extent of mismatch between a student's academic credentials and that of his or her peers.

3.2.1 Background on California's Affirmative Action Ban

The effort to remove racial preferences in California was an extended process spanning multiple years. To motivate our definition of the pre- and post- years in our empirical specifications, we summarize the key institutional details below.

The first threat to affirmative action in California was in July 1995, when the Board of Regents of the University of California passed a resolution (SP-1) stipulating that the UC system would discontinue considering race in admissions by the beginning of 1997. In November 1996, Proposition 209 (Prop 209), which banned the use of racial preferences in university admissions, was passed by voters, leading the UC to delay the implementation of SP-1.[6] Prop 209 underwent legal challenges until the Supreme Court denied further appeals in November 1997. Thus, the incoming class

[6]Searching the LexisNexis article database gives the first mention of Prop 209 in July 1996.

of 1998 was the first to be admitted without the use of affirmative action.[7]

It is important to recognize that in an effort to minimize the effects of Prop 209 on minority enrollment, UC campuses increased minority outreach efforts.[8] This could be one channel through which URMs and whites could be differentially affected by the ban on affirmative action. However, these efforts were widely viewed as ineffective, at least initially.[9] Part of the reason for lack of effective programs was that in the immediate aftermath of Prop 209, there were concerns about whether race-specific outreach (as opposed to, for example, targeting low income areas) was permitted after Prop 209. In addition, outreach programs focusing on elementary and middle schools would not have an effect until those students reached college age. To the extent that increased outreach had an effect, our measured effects of Prop 209 represent the net effect of the ban and the change in outreach efforts.

In 2001, the UC implemented Eligibility in a Local Context (ELC), guaranteeing admission to the top four percent of students in each California high school (conditional on completing specified coursework). The new policy was designed to attract students from high schools which did not typically send many students to UCs, giving the UC a way to potentially increase URM enrollment. Since this policy implemented a new admissions rule for UC campuses, we do not use any data later than 2000 in an effort to isolate the effects wrought by Prop 209.

Finally, we note that the state of California implemented changes to the public K-12 schools near the implementation of Prop 209. The 1999 passage of the Public Schools Accountability Act created a system to measure the performance of students, schools, and subgroups within schools. Beginning in 2000, schools were eligible for rewards if all ethnic subgroups within schools either scored above a certain threshold or met targets for test score growth. Since non-URMs were more likely to already be above the threshold, this measure possibly created an incentive for schools to increase

[7] For a complete summary of the events of the ban, see Table 3.1.

[8] For example, "In an attempt to improve minority access to UC without the help of affirmative action, the university's investment in kindergarten-through-12th-grade outreach has rocketed from about \$60 million in 1995 to \$180 million last year and a planned \$250 million this year" (2000, January 21). UC Regents Urged to Step Up Minority Outreach at Schools. *The San Francisco Chronicle.*

[9] "[In the last five years] minority admissions have dropped significantly and outreach expenditures have almost quadrupled to nearly \$300 million with minimal results" (2000, June 8). Effects of minority outreach may take time for U. California system. *The Daily Bruin.*

the resources devoted to minority students at the expense of high performing whites and Asians. In addition, schools with low test scores could opt into an intervention program designed for schools not meeting growth targets. To the extent that low performing schools had higher concentrations of minorities, the program could also affect test score gaps in California. As a robustness check, we remove observations from 2000 in our College Board data and find no substantial impact on our main results.[10]

3.3 Changes in the Admissions Rule

We begin by investigating how each of the eight UC campuses changed its admissions rule in response to Prop 209. To do so, we use administrative data on every fall freshman applicant to the UC from 1995-2000.[11] The data contain individual-level information on each student's race, adjusted high school GPA, SAT scores, parental income, and parental education. In addition, the data report the campuses to which each student applied, the campuses that accepted the applicant, and the campus at which the student enrolled, if any.[12] Since these data were provided by the University of California Office of the President, we refer to them as the UCOP data.

In an effort to protect student privacy, the UC Office of the President collapsed many important variables into descriptive categories before releasing the data. Thus, for example, SAT scores and high school grades are reported in bins rather than as numeric values. To facilitate the comparison of our results across the different datasets we employ, we assign the midpoint of these bins to be each student's test score (or grade) and then standardize so each is mean zero with a standard deviation of one. Year of application and enrollment is also grouped into three-year cohorts (1995-1997 and 1998-2000). By design, the second-period cohort begins the year the ban on racial preferences was implemented. Race is also collapsed into four categories: white,

[10]We are unable to remove 2000 from the UCOP data since 1998-2000 are collapsed into a single year category. In the NAEP, 2000 is the only year in the post period.

[11]The eight UC campuses are Berkeley, Los Angeles, San Diego, Santa Barbara, Davis, Irvine, Santa Cruz and Riverside.

[12]Additional information about this publicly available dataset can be found in Antonovics and Sander [2011].

Asian, URM and other/unknown. The URM category includes Native Americans, blacks, Chicanos and Latinos, which are the primary groups that received preferential treatment based on race before Prop 209. The other/unknown category includes both students who indicate that their race falls outside the categories used by the university, as well as students who choose not to reveal their race (a group that grew substantially after Prop 209 went into effect). In our empirical analysis, we compare admissions rates of URMs with the combined set of whites, Asians and other/unknown. Our primary reason for grouping students in the other/unknown category with Whites and Asians is that the average characteristics of students in the other/unknown group are very close to the average characteristics of Whites and Asians. Nonetheless, our results are not sensitive to dropping the other/unknown group.

Table 3.2 presents basic summary statistics of the UCOP data used in our analysis. As might be expected, relative to non-URMs, URMs who applied to the UC have lower average SAT scores, lower average high school GPAs and come from families with lower parental income and education. The bottom panel of Table 3.2 also presents the admission rates for URMs and non-URMs at each of the eight UC campuses before and after Prop 209. As the table shows, there was a substantial drop in URMs relative chances of admission after Prop 209, especially at the more selective UC schools.

In order to investigate the effect of Prop 209 on the admissions rule used by each campus, we begin by estimating the following equation using OLS:

$$A_{ijt} = \delta_{0j}\text{URM}_i + \delta_{1j}X_i + \delta_{02j}\text{URM}_i\text{Post}_t + \delta_{12j}X_i\text{Post}_t + \epsilon_{ijt}$$

where A_{ijt} is an indicator for whether student i is admitted to school j in time t (conditional on application), X_i includes a set of student-level characteristics used in determining admissions and a constant term, and Post_t is an indicator that takes on the value of one after Prop 209 went into effect. Conversations with admissions officers at the UC indicate that during this time period campuses generally assigned points (or weights) to different dimensions of a student's application. In addition, prior to Prop 209 race was only used to set different admissions thresholds for different groups. Thus, in the above specification, we allow race to enter linearly (rather than being interacted with X_i). In this framework, δ_{02j} captures the change in the explicit racial preference at school j after Prop 209, and δ_{12j} captures the change in the weights on the various dimensions of student characteristics.

Of course, campuses have much richer information about students than we do. For example, we have no information on the quality of student essays, an additional factor in admissions. Thus, we cannot estimate the precise admissions rule. Nonetheless, we can determine whether student characteristics that are correlated with, for example, SAT math scores (including SAT math scores themselves) became more or less important in predicting admission after the implementation of Prop 209.

Columns (2)-(9) of Table 3.3 show the results of this exercise for each campus, where the campuses are ordered from most to least selective as determined by the average SAT math score of admitted students. Row 1 indicates the extent of the admissions advantage given to URMs prior to Prop 209. As the table shows, this advantage tended to be greatest at the more selective UC schools. At Berkeley, for example, on average URMs were 42.5 percentage points more likely to be admitted than equally qualified non-URMs. According to Row 2, much of this admissions preference disappeared after Prop 209. Interestingly, however, we note that it did not disappear entirely (the coefficients in Row 2 are smaller in magnitude than those in Row 1). This could arise because, despite Prop 209, admissions officers continued (either consciously or unconsciously) to give preferential treatment to URMs in admissions. Alternatively, if there are factors that are positively correlated both with the probability of admission and with being a URM, but that we do not observe in our data, then the coefficient on URM would remain positive even if campuses did not explicitly consider race in admissions. Nonetheless, it is clear that after Prop 209 there was a sizable drop in the likelihood that URMs relative to non-URMs were admitted to the various UC campuses.

We also see that students are more likely to be admitted if they have high SAT scores and good grades in high school. At UC Berkeley, for example, a one standard deviation increase in a student's SAT math score is associated with a 9.7 percentage point increase in their probability of admission, which is large relative to the baseline admission rate of about 30 percent. Table 3.3 also indicates that an admission preference is given to students with disadvantaged backgrounds; the coefficients on parental income and parental education are both negative.

How might schools change their admissions formula to implicitly favor URMs after Prop 209? To answer this, Column (1) of Table 3.3 shows the coefficients from a regression of an indicator for whether an applicant was a URM on SAT scores, high school GPA and family background characteristics. As expected, SAT math scores and high school GPA are negatively associated with being a URM, though the association is much stronger for SAT math scores than high school GPA. Interestingly, controlling for other factors, high SAT verbal scores appear to be *positively* associated with being a URM. In the context of the UC, this positive association arises because of the large number of Asian applicants with low SAT verbal scores. If Asians are dropped from the regression in Column (1), the coefficient on SAT verbal scores becomes negative and statistically significant. Finally, high parental education and income also both negatively predict being a URM. Thus, if UC campuses wanted to implicitly favor URMs, one might expect them to place less weight on SAT math scores and to increase the preference given to students with low parental income and education. On the other hand, it isn't obvious how campuses should change the weight on SAT verbal scores and high school GPA. It is worth noting, however, that even though a strong high school GPA negatively predicts being a URM, this relationship is weak. At the same time, for students who ultimately enroll in one of the UC schools, the association between high school GPA and first-year college GPA is very high (at least as high as the relationship between SAT scores and first-year college GPA).[13] Thus, if UC campuses wanted to increase the likelihood of admitting URMs while still maintaining high academic standards, they might decrease the weight placed on SAT scores while increasing the weight placed on high school GPA.

Consistent with the idea that UC campuses responded to Prop 209 by changing their admissions rules to implicitly favor URMs, we see that the coefficient on Post*SAT Math is negative at each of the eight UC campuses, suggesting that SAT math scores became less important in determining admission after Prop 209. Similarly, we see that parental income and parental education became more important (negative) predictors of admission, which is consistent with the idea that the campuses increased the admissions advantage given to students from disadvantaged backgrounds. As discussed above, it isn't clear how campuses interested in promoting

[13]Details available upon request.

racial diversity would change the weight given to SAT verbal scores and high school GPA. Indeed, at several of the campuses, SAT verbal scores appear to have become less important predictors of admission, though at UCSD, UCI and UCSB, it appears that more weight was given to SAT verbal scores after Prop 209. Similarly, although high school GPA became more important in predicting admission at most schools, the importance of high school GPA fell at both UCLA and UCSD.

As further evidence that UC schools reacted to Prop 209 by changing their admission formulas to implicitly favors URMs, Table 3.4 shows the extent to which URMs' predicted relative chances of admission increased because of the changes in the importance of academic achievement and family background characteristics in predicting admissions. In particular, Table 3.4 uses the coefficient estimates in Table 3.3 to simulate the probability of admission for URM and non-URM applicants to each campus in the absence of explicit racial preferences (that is, setting URM=0 for everyone) and only allowing the weights on SAT scores, high school GPA, parental income and parental education to change over time. Doing so isolates the effect of the change in the weights given to these factors on students' likelihood of admission.

As Table 3.4 shows, eliminating explicit racial preferences substantially lowered URMs' relative chances of admission. That is, the predicted admission rates for URMs in Table 3.4 are far below the actual admission rates for URMs documented in Table 3.2. However, Table 3.4 also shows that the changes in the weights given to students' academic performance and family background characteristics implicitly worked to increase URMs' relative chances of admission. At Berkeley, for example, the changes in the importance of SAT scores, high school GPA and family background characteristics led non-URMs' probability of admission to fall from 33 percent to 27.6 percent, while URMs' probability of admission increased from 10.7 percent to 12.1 percent. The bottom row of Table 3.4 shows the magnitude of this relative increase at each of the eight campuses. Interestingly, the magnitude of the relative increase in URMs' predicted probability of admission is larger at the most selective UC schools, where the extent of explicit racial preferences was the largest even before Prop 209. At several of the less selective UCs we even see a decrease in URMs' relative chances of admission, though the magnitude is very small. Recall that these simulated admission probabilities are calculated identically for URMs and non-URMs (all applicants are treated as if they are white). Thus, any changes in the admission rate of URMs and non-URMs over time is driven purely by racial differences in the distribution of student characteristics and the change in the predictive power of those characteristics in determining admissions after Prop 209.

On one hand, the increases in URMs' relative chances of admission shown in Table 3.4 are small relative to the fall in URMs' probability of admission due to the end of explicit racial preferences. On the other hand, the fall in URMs predicted probability of admission would have been larger had the association between the student characteristics and the likelihood of admission not changed after Prop 209.

3.4 The Effect on the Pool of Admitted Students

Any change in a college's admission rule necessarily affects the set of admitted students. Thus, given the apparent changes in UC schools' admissions rules after Prop 209, an obvious concern is the extent to which these changes affected the average quality of admitted students. Answering this question is complicated by the fact that in addition to the changes brought about by Prop 209, UC schools became more selective over time due to a general increase in the number of applicants. To illustrate, Table 3.2 shows that the probability of admission fell even for non-URMs after Prop 209 at every campus except UC Riverside. Since changes in the degree of selectivity can also affect student quality, it is necessary to hold selectivity constant when trying assess the effect of the weights given to different elements of a student's record in determining admission.

With this in mind, we use the results from Table 3.3 to simulate how both the changes to the admissions rule and changes to the admissions rate affected the pool of admitted students. To insure that changes to the pool of predicted admits are not driven by changes in the pool of applicants before and after Prop 209, we conduct these simulations for the set of students who applied in the post-Prop 209 period. From this group, we first identify the set of non-URMs who are predicted to have been admitted to Berkeley according to the estimated pre-Prop 209 admission rule at the pre-Prop 209 admission rate. Column (1) of Table 3.5 presents the average characteristics of this group. Column (2) then presents the average characteristics of non-URMs who are predicted to have been admitted according to the estimated pre-Prop 209 admission rule but at the post-Prop 209 admission rate. Thus, moving from Column (1) to Column (2) isolates the effect of the general increase in selectivity (holding constant the admissions rule) on student quality. Column (3) then calculates the expected characteristics of non-URMs who are predicted to have been admitted to Berkeley according to the estimated post-Prop 209 admission rule at the post-Prop 209 admission rate. Thus, moving from Column (2) to Column (3) isolates the effect of the change in the estimated admission rule (holding constant the admission rate) on student quality. Columns (4)-(6) present the analogous results for URMs.

Given the general increase in selectivity at UC Berkeley as we move from Column (1) to (2), we naturally see an increase in average SAT scores and high school GPA of students predicted to have been admitted, though the changes are generally small. There is also a slight increase in average parental income, though we see almost no change in parental education. As a way to summarize these changes, we use data on students who enrolled at UC Berkeley to estimate the relationship between first-year college GPA and student characteristics. In particular, we regress first-year college GPA on SAT scores, high school GPA, parental income and parental education for any student who enrolled in Berkeley between 1995-2000.[14] Doing so allows us to derive a measure of expected first-year college GPA for students predicted to be admitted under the different admission scenarios in each column.[15] Note that since we do not allow the relationship between first-year college GPA and student characteristics to vary either over time or by race, any difference in predicted first-year college GPA across the columns of Table 3.5 is driven by changes in the average characteristics of students who are predicted to be admitted in each column. As the table shows, there is a slight increase in expected GPA when moving from Column

[14]Estimates available upon request.

[15]Given standard concerns about selection, caution should be used in interpreting these estimates of first-year college GPA since we are using the relationship between student characteristics and first-year college GPA for those who enrolled to generate predictions about expected first-year college GPA for those who are predicted to have been admitted. Nonetheless, this measure of expected GPA (which just can be thought of as a weighted average SAT scores, high school GPA and family background characteristics) is a useful way to summarize the combined effect of the changes in average student characteristics across the different columns in Table 3.5.

(1) to (2). In addition, the UCOP data also contain information on intended major, and we also see a slight increase in the fraction of students who state that they intend to major in science.

Given that the end of explicit racial preferences led URMs' chances of admission to fall considerably relative to whites after Prop 209, we naturally see larger changes in measured student quality between Columns (4) and (5) than between Columns (1) and (2). For example, the expected combined SAT score of URMs predicted to be admitted increases by 71 points, which is about half of the pre-Prop 209 racial gap in SAT scores. Given the fairly large shifts in expected student characteristics between Columns (4) and (5), we naturally also see the expected first-year college GPA of those predicted to be admitted increases by .12 (from 3.04 to 3.16), which again represents about half of the racial gap in expected first-year college GPA prior to Prop 209. Thus, the dramatic fall in the admission rate of URMs relative to non-URMs appears to have led to a substantial increase in the quality of admitted URMs relative to non-URMs.

Recall that the difference between Columns (2) and (3) (and between Columns (5) and (6)) isolates the effect of the change in the estimated admissions rule on student quality, holding the admissions rate fixed. Moving from Column (2) to (3), we see a drop in average SAT scores, and this fall is large enough to wipe out the increase in SAT scores between Columns (1) and (2) that was brought about by the general increase in selectivity over time. On the other hand, we see an increase in the average high school GPA of those predicted to be admitted. This increase is to be expected given that Table 3.3 suggests that Berkeley placed more weight on high school GPA in determining admissions after Prop 209. Interestingly, in terms of expected first-year college GPA, the increase in average GPA appears to compensate for the fall in average SAT scores so that expected first-year college GPA barely changes between Columns (2) and (3). Comparing the results for URMs in Columns (5) and (6) tells a similar story; average SAT scores fall, average high school GPA increases and expected first-year college GPA remains unaffected. We conduct a similar analysis for each of the remaining UC campuses and obtain qualitatively similar results.[16]

[16]For the sake of brevity, we do not include the results for the other UC campuses, but they are

Thus, although the changes in the estimated admissions rule (holding constant the admissions rate) affected student quality in the sense that admitted students tended to have lower SAT scores and higher high school GPAs after Prop 209, the expected first-year college GPA of admitted students remained constant. As a result, overall student quality appears to have been unaffected, at least in the short run. Of course, this claim assumes that the best overall measure of student quality is expected first-year college GPA. If colleges care about some other dimension of student quality that is more closely related to SAT scores than high school GPA, then Prop 209 could have lowered average student quality. In addition, as we discuss below, the changes in the weights given to SAT scores and high school GPA also may have affected students' incentives to invest in human capital prior to college entry. We investigate this possibility in the next section.

3.5 The Effect on Human Capital Investment

In the above section, we showed evidence that UC campuses reacted to Prop 209 in part by changing the weights given to test scores, high school GPA and family background characteristics. Even though the direct effect of these estimated changes in the admissions rule had little effect on student quality (as measured by predicted first-year college GPA), the ban may have had indirect effect on student quality via changes in human capital investment. In particular, given the apparent reduced emphasis on SAT math scores relative to SAT verbal scores and high school GPA, we might expect to see a shift in human capital investment away from investments that increase SAT math scores and towards investments that increase both SAT verbal scores and high school GPA.

To test for the effects of Prop 209 on human capital investment, we make use of data from the College Board, which contain information on both SAT scores and self-reported GPA. In addition, we utilize data from the National Assessment of Educational Progress (NAEP) on a representative sample of eighth graders. For the time frame we examine, the NAEP contains both math test scores and time spent

available upon request.

doing homework. Both data sets allow us to investigate whether there were shifts in human capital investment consistent with the changes in the admissions rules at the UC after Prop 209. To ease the comparison of our results across datasets and outcome measures, we normalize test scores and GPA to be mean zero with standard deviation 1.

We measure the effects of the policy change in two ways. First, we explore the reaction of Californians relative to the rest of the country. Second, we document how the gap between whites and URMs changed in California relative to the rest of the country. Each of the two measures is important. Since Californians of all races were affected by the change in admissions policy for UC campuses, they may be thought of as one treated group. Comparing Californians to students in other states reveals the extent to which Prop 209 changed investment incentives for all Californians. On the other hand, affirmative action policies are generally thought of as a way to address the gap between white and minority students. Viewed in this way, it is natural to ask how the removal of explicit racial preferences affected the racial gap in student quality.

3.5.1 Empirical Strategy

Consider the following basic empirical model:

$$\begin{aligned}
\text{Outcome}_{ist} = {} & \beta_0 \text{CA}_s + \beta_1 \text{Post}_{st} + \beta_2 \text{Black}_i \\
& + \beta_{01} \text{CA}_s \text{Post}_{st} + \beta_{02} \text{CA}_s \text{Black}_i + \beta_{12} \text{Post}_{st} \text{Black}_i \\
& + \beta_{012} \text{CA}_s \text{Post}_{st} \text{Black}_i \\
& + \beta X_i + \epsilon_{ist}.
\end{aligned}$$

Following the discussion above, there are two coefficients of interest. First, to the extent that Californians had a common response to Prop 209, it would be captured by β_{01}, which represents the change in the dependent variable for Californians relative to the rest of the country. Second, β_{012} represents the change in the black-white test score gap in California relative to the rest of the country after Prop 209. Since affirmative action policy is largely driven by achievement gaps between minority and white students, it is important to understand how Prop 209 affected these gaps.

In practice, we estimate the above model using OLS and expand the our estimating equation to include the full set of interactions for Hispanics and Asians. We also experiment with adding state-specific time trends, which, as discussed below, turn out to be important for our analysis. We use the sampling weights provided by each dataset, cluster our standard errors at the state level, and for the NAEP, we additionally correct our standard errors for sampling scheme (details below). The X term includes available demographic controls, which differ by dataset, and a constant term. We drop observations from Louisiana, Mississippi, Texas, and Washington, which were affected by their own affirmative action policy changes during our sample period.

Asians, who constitute a large portion of the college-going population in California, are not considered URMs for the purposes of admissions to the UC. Although we report the coefficients that capture the change in the Asian-white gap in California relative to the rest of the country, we focus our discussion on the black-white gap and the Hispanic-white gap since blacks and Hispanics were the intended beneficiaries of affirmative action policy at the UC and since the outcomes for Asians and whites are generally similar.[17]

We begin by presenting the details of our analysis of the College Board data and then move on to discussing our results from the NAEP.

3.5.2 College Board (SAT Scores and High School GPA)

The College Board data consist of SAT test takers who are expected to graduate from high school between 1994 and 2000. Our sample consists of all black and Hispanic test takers nationwide, all Californian test takers, and a 20 percent random sample of the rest of the country. The College Board includes a range of descriptive variables that are generated when students fill out the Student Descriptive Questionnaire before taking the exam. These include race, gender, parental characteristics, college aspirations, high school GPA, and many other variables.

Defining the period before and after the ban on racial preferences in the College Board data is complicated by the timing of when students took the SAT. Unfortu-

[17] And adding Asians to the excluded group in the DDD regressions gives similar results.

nately, our data do not reveal the date on which students took the SAT, but it is likely that students in a given graduation cohort took the SAT between the spring and fall of the preceding year. With this in mind, Table 3.1 shows the timing of important events related to the passage of Prop 209 along with the timing of when various graduation cohorts are likely to have taken the SAT. As the table indicates, the first major threat to affirmative action at the UC came in July 1995 when the Regents of California passed SP-1, which committed the UC system to an eventual ban on the use of racial preference in admissions. It was not until over two years later, however, in November 1997, when Prop 209 officially became law that the UC stopped the use of explicit racial preferences. Thus, students who took the SAT between July 1995 and November 1997 did so during a time of considerable uncertainty about the future of explicit racial preferences. To ensure a clean before and after comparison, we drop observations in the 1996-1998 cohorts. In general, including 1996-1998 pushes any estimated effects towards zero (which is not surprising due to uncertainty about whether Prop 209 would eventually pass). In other words, to the extent we find effects of the preference ban, they are concentrated in 1999 and 2000.

One advantage of using the College Board sample is that nearly all of the SAT takers are interested in going to college, so they should be the ones most readily affected by the affirmative action bans. On the other hand, a potential problem with using the College Board is that students decide whether or not to take the SAT, which could lead to sample selection bias. Indeed, Dickson [2006] finds that removal of affirmative action in Texas led to a decline in the share of minority students taking either the ACT or SAT. However, using actual enrollment data from public universities in states which banned affirmative action, neither Backes [2012] nor Hinrichs [2012] find any evidence that fewer URMs enrolled in college. In addition, Furstenberg (2010) shows that the demographic characteristics of SAT takers are generally uncorrelated with the introduction of the bans in California and Texas.

Basic summary statistics for our College Board sample are displayed in Table 3.6. Consistent with the UCOP data, blacks and Hispanics tend to score lower on the SAT, have lower GPAs, and have parents with lower levels of education. Not surprisingly, a high fraction of SAT test takers report hoping to obtain at least a BA: about 80 percent. Of the remainder, nearly all report their degree goal as “Other” or

"Unknown". The small remaining share are those who hope to obtain certificates or associates degrees.

As a first pass at gauging the effects of Prop 209 on SAT scores and high school GPA, we plot normalized average SAT scores by race and year in Figure 3.1. Panel (a) shows normalized SAT scores for Californians and the rest of the US. Although whites tend to score higher than URMs, the gap appears to be stable over time. Panel (b) shows normalized GPA. The patterns in the figure underscore the importance of controlling for state-specific time trends: there was a gradual rise in GPA over time that begins before the implementation of the preference ban. By including state-specific time trends, we measure the deviation from these trends that accompanied the preference bans, rather than attributing pre-existing trends to the policy change.

Table 3.7 shows our coefficient estimates. For each outcome variable, we present two sets of results: with and without state-specific linear time trends. Figure 3.1 shows that, for GPA especially, there was a strong upward trend over time in both California and the US as a whole. Failing to account for this time trend could bias results. For example, if California's rate of increase were slower than the rest of the country (which was indeed the case), our estimating equation would attribute to Prop 209 a decrease in Californian GPA which was actually the result of a flatter trend. On the other hand, it is unclear how much weight to place on time trends estimated from four years of data, so we provide both sets of results and advise caution when interpreting estimates.

For SAT scores, the positive and statistically significant CA*Post coefficient in both specifications provides evidence that, relative to their peers in other states, Californians performed better on the SAT after Prop 209. When looking at math and verbal scores separately, we see that the increase in Californian's SAT scores was driven by an increase in SAT verbal scores, and there is no evidence that SAT math scores increased in California relative to other states once state-specific linear time trends are included in the model. These changes are consistent with students reacting to the reweighting of applicant characteristics by the UC system, which appears to have increased the importance of SAT verbal scores relative to SAT math scores in determining admissions. Table 3.7 also shows that the gap in test scores between Hispanics and whites fell in California relative to the rest of the country, and there appears to have been no change in the black-white test score gap in California relative to the rest of the country. Finally, we note that although a number of the estimated coefficients are statistically significant (we have large sample sizes), the magnitude of our estimated coefficients are small. None of the normalized estimates is larger than 6 percent of a standard deviation. In addition, the estimated changes in the racial test score gaps are a small relative to the overall racial test score gap. For example, the 5 percent of a standard deviation increase in the Hispanic-white SAT test score gap in column (2) only represents about 7 percent of the Hispanic-white SAT test score gap in California.

The final column in Table 3.7 shows results with normalized self-reported GPA as the outcome variable. The sign of the coefficient on CA*Post depends on whether state-specific time trends are included. When the time trends are included, the point estimate is 0.03 of a standard deviation and is statistically significant. Taken at face value, this suggests that Californians report having higher GPAs after Prop 209 relative to the rest of the country. As with the relative change in SAT verbal scores, this is consistent with the apparent changes in the admissions rules at the UC brought about by Prop 209, where high school GPA became a more important predictor of admissions in the post period. However, we note that this result is sensitive to whether time trends are included, and is small in magnitude.

In addition, regardless of whether time trends are included, the gap in self-reported GPA between Hispanic and whites and between blacks and whites declined substantially in California relative to the rest of the country after Prop 209, even though the increased importance of GPA in predicting admissions affected whites along with URMs. The 0.05 standard deviation relative increase for Hispanics represents 14 percent of the GPA gap between Hispanics and whites in California, and the 0.08 standard deviation relative increase for blacks represents about 12 percent of the GPA gap between blacks and whites in California.

We note that in contrast to our results, Furstenberg finds a widening of the black-white SAT gap in California following Prop 209. There are two important differences between his College Board sample and ours. First, his data consist of a 30% sample of SAT takers, while we have obtained a more comprehensive dataset containing all Californian test takers, all black and Hispanic test takers nationwide, and a 25% sample of the remaining non-Californian whites. As a result, our estimates are more precise. Furthermore, to the extent that the College Board is prone to errors when generating random samples, it would affect Furstenberg's results more readily than ours, since our data include the entire population of minority and Californian test takers. Second, Furstenberg's College Board sample only includes the 1996-2000 cohorts; his paper compares the 1996-1997 cohorts to the 1998-2000 cohorts. However, interpreting results from 1996-1998 is difficult since, as discussed above, the UC Regents first announced their intention to end their use of racial preferences in July 1995. Thus, it's possible that students began responding to the policy change long long before 1998, his first post-policy change year. In contrast to Furstenburg, our main results compare the 1994 and 1995 cohorts to the 1999 and 2000 cohorts to ensure a clean before and after comparison. However, even when using our dataset with Furstenberg's sample years, definition of the policy change year, estimating equation, and set of controls (obtained by direct correspondence with Furstenberg), we are still unable to replicate his finding of an increase in the black-white gap in California.[18]

In addition to altering the incentives to invest in test scores and GPA, the removal of racial preferences could also affect the decision of whether to attend college. On the SAT Questionnaire, students are asked to indicate what type of degree they hope to obtain. However, using the College Board to elicit college preferences is problematic since the sample is pre-selected to include only SAT test takers, so our results capture changes in college aspirations only among those who have enough interest in college to sit for the SAT. We conducted an analysis of stated degree goal preferences and failed to find an effect. This is not surprising due to the nature of the data and should not necessarily be taken as evidence that minority students were not discouraged from attending college after Prop 209.

In summary, the College Board results provide some evidence that Californian students reacted to the changes in UC admissions policy by investing more in SAT verbal (relative to math) scores and GPA. However, these effects are sensitive to specification and small in magnitude. At the same time, there is no consistent

[18]Furstenberg no longer has access to the version of the data used in his paper, so we were unable to explore further.

evidence that Prop 209 adversely affected the racial gaps in human capital investments. In California relative to the US, the Hispanic-white SAT gap increased and the Hispanic-white and black-white GPA gaps decreased.

3.5.3 NAEP (8th Grade Math Test Scores and Homework)

We supplement our College Board analysis by examining math test scores of a younger group of students by using the eighth grade math sample of the National Assessment of Educational Progress (NAEP), administered by the National Center for Education Statistics.[19] Beginning in 2002, states wishing to receive a Title I grant must participate in the NAEP; however, state assessments have been given since 1990 on a voluntary basis, when 37 states, including California, participated. According to the NAEP administrators, the twin goals of the survey are to compare student achievement across states and to track changes in achievement over time, both of which are useful for this paper.[20]

There are two advantages to supplementing our College Board data with the NAEP. First, the NAEP is designed to be representative of the statewide population of eighth-grade students enrolled in public schools, so it does not suffer from the same selection issues of the College Board, which is nearly entirely comprised of students interested in attending college. Second, to the extent that Prop 209 altered incentives to invest in human capital, the effects should be apparent at earlier ages, allowing us to test whether there are patterns apparent in multiple datasets. However, the NAEP suffers from some serious drawbacks. First, background information is relatively sparse: for example, parental education but not income is available. Second, smaller sample sizes and nonrandom sampling (discussed below) result in estimates that are considerably more imprecise than those from the College Board data. Third, only a limited number of years are available, including only one assessment (2000) after the policy change.

Our sample consists of Grade 8 state mathematics results from 1990, 1992, 1996, and 2000. Since only public schools were sampled in 1990, we restrict our

[19]The NAEP Reading and Writing portions for Grade 8 each began in 1998.
[20]For further details, see http://nces.ed.gov/nationsreportcard/faq.asp.

sample to students enrolled in public schools. In addition, to ensure a consistent group of control states, we include only states that were sampled in each of the four years.[21]

The sampling scheme for the NAEP involves selecting approximately 100 schools per state, followed by 30 students per school. Since this does not constitute a simple random sample – the sample consists of clusters of students within a school – standard errors generated by statistical packages are generally too small. To generate proper standard errors, a set of replicate weights generated by jackknife repeated replication is provided and can be used to used to estimate the sampling error. There is also additional sampling variation due to the testing procedure. The battery of questions contains over 100 items, making it impractical to have each student answer each item. Thus, each student answers a subset of questions; the answers are then used generate a set of five plausible test score values. When appropriate, we inflate standard errors to correct for the fact that test scores represent an imputed value.[22]

Summary statistics by race are shown in Table 3.8. As with the other datasets, whites tend to have higher test scores than do URMs and have more educated parents.[23]

While sitting for the test, students are asked a number of background questions, including demographic and family information. Also included is a question about time spent on homework; students indicate which interval best describes their study habits.[24] In contrast to test scores and GPA, time spent studying represents a

[21]The authors were also able to obtain Grade 4 NAEP Reading exams from 1992 and 1998 and Grade 4 NAEP Mathematics exams from 1992, 1996, and 2000. We did not find any significant effects on test scores or time spent studying (for Californians relative to the rest of the country or for minority-white gaps within Californian), but estimates are imprecise due to having fewer years of data.

[22]We use Kevin Macdonald's PV procedure, which inflates the standard errors by a measure of the variability of the five imputed values. See http://ideas.repec.org/c/boc/bocode/s456951.html for more information about the module and http://nces.ed.gov/nationsreportcard/tdw/analysis/est_pv_individual.asp for a description of plausible values.

[23]Using either mother or father education separately results in a large number of missing values, so we use the composite provided in the dataset.

[24]In 1990 and 1992, these intervals were 'Don't usually have', 'have but don't do', '1/2 hour or less', '1 hour', '2 hours', and 'more than 2 hours'. 1996 and 2000, the latter two response options were condensed into 'more than 1 hour'. To maintain consistency across years, we create the following categories: 'no homework', '1/2 hour or less', '1 hour', and 'more than 1 hour'. The share of students

point-in-time measure of human capital formation.[25]

Table 3.8 shows the share of students reporting that they do any homework and the share who report doing more than one hour per week (the highest response category). We also create an imputed measure of homework (using the midpoint of each time interval as our measure of the time spent doing homework) for use only in our summary statistics. Despite the fact that whites score much higher on the NAEP math test than do blacks and Hispanics, there is very little variation by race in the amount of time spent doing homework. In addition, while Californians have lower NAEP math test scores than the rest of the country, they report spending more time on homework.

As an initial exploration of the NAEP data, we plot mean test scores and homework time by race and year in Figure 3.2. Panel (a) shows the results from the Grade 8 mathematics assessment. It appears that the average Californian performed worse in 2000 relative to the rest of the country, but the fall may have begun before the policy change. As with the College Board test scores, it is difficult to detect a pattern in the URM-white test score gap in California. Panel (b) shows trends in imputed homework time. For the US, there appears to be a steady fall in time spent studying from 1990 to 1996. A general decline in time spent studying would be consistent with findings in other settings (e.g. Babcock and Marks [2011], who find declines in time spent studying in college). In 2000, there was an increase in average time spent studying by both Californians and the rest of the country.

Our main NAEP results are presented in Table 3.9. The first column shows our estimates for normalized NAEP math scores, comparing 1990, 1992, and 1996 (pre) to 2000 (post).[26] The coefficient on CA*Post indicates that relative to the rest of the country NAEP math test scores fell in California relative to the rest of the country after Prop 209 by about 1/10 of a standard deviation. In addition, there's little

reporting '2 hours' and 'more than 2 hours' in 1990 and 1992 is very similar to the share reporting 'more than 1 hour' in 1996 and 2000. However, we cannot rule out the change in response options affecting our estimates.

[25]Questions where respondents are asked to recall time spent studying likely suffer from considerable measurement error. A better measure of time spent studying would be time use diaries; however, to our knowledge, there are no time use datasets during the relevant time period. We discuss our efforts to find other datasets below.

[26]Removing observations from 1996 yields similar results. Results available from authors.

evidence of a change in the black-white or Hispanic-white test score gaps, although standard errors are much larger than the College Board dataset due to stratified sampling, imputation of the dependent variable, and considerably smaller sample size. The second column adds linear state time trends to the previous regression with little impact on the results.

The third column of Table 3.9 shows the coefficients from an ordered probit with the categorical homework variable as the outcome.[27] The positive and significant CA*Post coefficient suggests that Californians were more likely to enter higher response values in 2000 relative to the rest of the country. To get a sense of the magnitudes reported in Column 3, Column 4 uses the estimates from Column 3 to generate the predicted change in probability of entering the highest response value (‘more than 1 hour per week’) for a discrete change in explanatory variable from zero to one. According to the estimates, Californians were seven percentage points more likely to report doing more than one hour of homework per week. Relative to a mean value of 34 percent, this is a substantial increase.

The next two columns repeat the above estimates with the addition of state-specific linear time trends. Once these trends are added, the increase in time spent studying by Californians is much smaller and no longer significant. Given the patterns shown in Figure 3.2, that the addition of time trends shrinks Californian estimates should not be surprising. It appears that non-Californians experienced a decline in time spent studying which pre-dated Prop 209. Generating estimation results without controlling for this decline attributes to the policy change what was likely simply a nationwide decline in study time.[28] Once these controls are added, Californians are estimated to be two percentage points more likely to enter the highest response value, although this effect is not statistically significant.[29] Finally, none of the specifications show a statistically significant change in the gap between the amount of time whites and URMs spend studying, although the standard errors are large.

Once time trends are accounted for, the only significant change we find in the

[27]The possible outcomes are ‘no homework’, ‘1/2 hour or less’, ‘1 hour’, and ‘more than 1 hour’.

[28]The same pattern – an increase for Californians which disappears once time trends are added – is evident in the Grade 4 mathematics assessment, although it contains only three years of data.

[29]Estimating a probit model for whether students were more likely to spend more than one hour studying yields similar results.

NAEP data is a widening of the gap between the math scores of Californians and non-Californians. There is no evidence that Californians changed their time spent studying after Prop 209, or that the gap between minority and white test scores or time spent studying was affected.

3.6 Evidence from Other Datasets

The College Board and NAEP are not the only datasets that provide measures of student quality. As a guide to future research, we briefly discuss below why several of the most obvious alternative sources of data are ill-suited to exploring the effect of banning affirmative action on student quality.

3.6.1 National Longitudinal Survey of Youth

As mentioned above, Caldwell [2010] uses the National Longitudinal Survey of Youth (the NLSY) to argue that Prop 209 increased the racial gap in academic achievement prior to college. We obtained the same restricted version of the NLSY used by Caldwell and were able to replicate his qualitative findings. However, using the NLSY to make inferences about state-specific trends is problematic, as that is not the intended design of the survey. From the NLS FAQ,[30]

> *The National Longitudinal Surveys are designed to represent specific birth cohorts at the national level. The surveys cannot provide representative estimates for States [...] NLS data files with geographic variables are available on a restricted basis for authorized researchers to use, but the permitted uses do not include producing estimates for States.*

Thus, there is no reason to believe that estimates of the effects of Prop 209 generated from the NLSY are representative of the general population.

Exacerbating this issue is the fact that the NLSY has relatively small sample sizes. For example, one of Caldwell's strongest results suggests that the PIAT math scores of 13 and 14 year olds fell considerably for blacks in California relative to blacks in the rest of the country after Prop 209. As it turns out, however, there are only 62

[30] http://www.bls.gov/nls/nlsfaqs.htm#anch14

blacks in this age range in California during the time period Caldwell investigates, and only 17 of these are from the period after Prop 209. Given this, we have additional reason to doubt the generalizability of Caldwell's results.

3.6.2 Time Use Diaries

Perhaps the best measures of human capital investment come from time-use diaries, which record the actual time invested in human capital accumulation. The American Time Use Survey (ATUS), however, did not begin until 2003, which is well after Prop 209 went into effect. In principal, one could use the ATUS to explore time use patterns in states which banned affirmative action after 2003 (for example, Michigan ended racial preferences in university admissions in 2004), but none of the states that banned affirmative action after 2003 have large enough minority populations to make such a study reliable.

The Child Development Supplement of the Panel Study of Income Dynamics (PSID) also collected time use diaries of youths in 1997 and 2002/2003. However, it has the same problems as the NLSY: it has relatively small sample sizes, and it is not designed to be representative at the state level. In addition, time diaries are only available in 1997 and 2002/2003, both of which are after the passage of Prop 209.

3.7 Summary

Much of the popular debate surrounding affirmative action in higher education focusses on issues of fairness. Nonetheless, also important to the debate over affirmative action is the extent to which banning affirmative action (in the form of explicit racial preferences) may affect student quality. Indeed, a number of scholars have pointed out that since policies such as Prop 209 give colleges and universities an incentive to place a greater weight on non-academic factors in determining admissions, they could lower student quality both by lowering the quality of students who are admitted and by weakening students' incentives to invest in their academic qualifications prior to college entry.

In this paper, we provide evidence that UC schools responded to Prop 209 in two ways. First, they drastically reduced the explicit admissions advantage given to URMs prior to Prop 209. Second, they appear to have decreased the weight placed on SAT math scores and increased the weight given to high school GPA, SAT verbal scores (at least at several campuses) and family background characteristics in determining admissions.

We then explore the direct effects of these changes in the admissions rule on the quality of students admitted to UC schools. Holding constant the changes in the admissions rate for each racial group, we find that the changes in the weights given to students' academic and family background characteristics had little effect on the quality of admitted students (as measured by expected first-year college GPA). In particular, for both URMs and non-URMs, while the new admissions rules lowered the average SAT scores of students predicted to be admitted, they also increased the average high school GPA of students in this group. Since both SAT scores and high school GPA positively predict first-year college GPA, the net effect on expected first-year GPA was negligible.

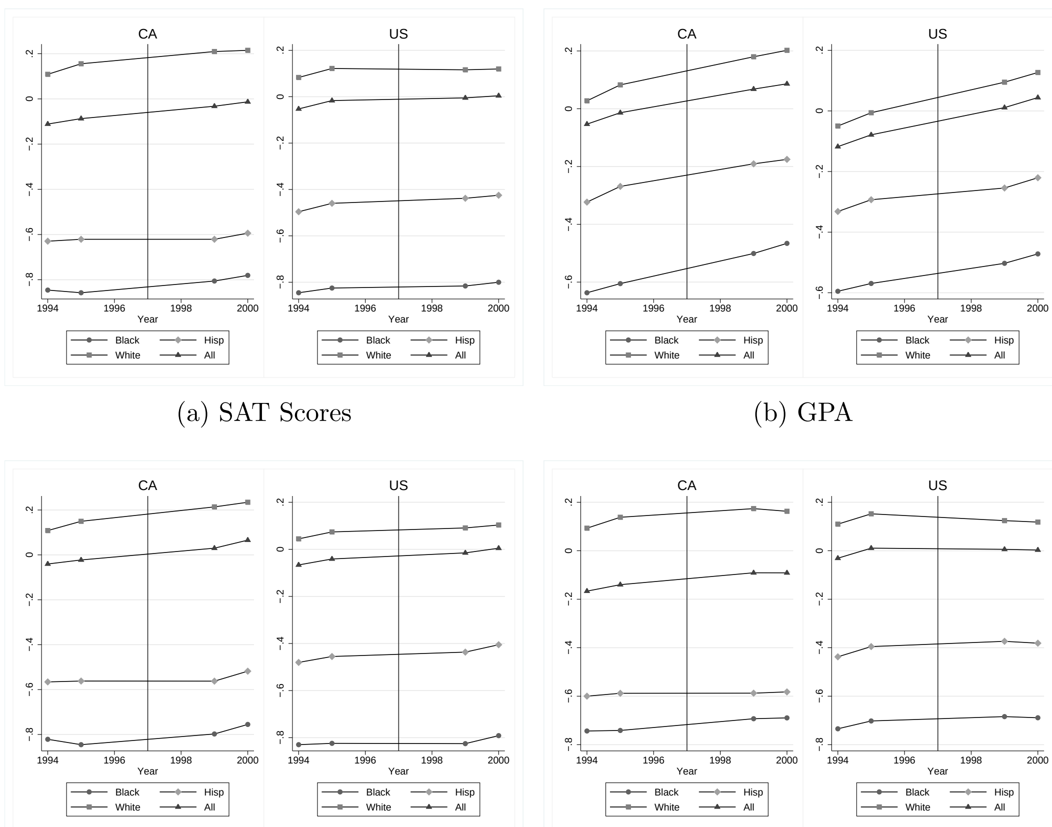
In terms of human capital investment, our results are broadly consistent with our findings regarding the changes in admissions rules: relative to the rest of the country, high school GPA and SAT verbal scores went up in California while 8th grade math test scores declined–though we note that our results are sometimes sensitive to whether we include state-specific time trends in our analysis. On one hand, the magnitude of our findings is quite small. On the other hand, our post period only goes until 2000, and the cumulative long-run effects on human capital investment could be much larger than suggested by the magnitude of our point estimates.

Either way, assessing the effects of Prop 209 (both direct and indirect) on student quality is unclear. To the extent that student quality is related to math test scores, Prop 209 may have lowered student quality, but to the extent that student quality is related to high school GPA and verbal test scores, Prop 209 may have had the opposite effect. Finally, we find no consistent evidence that Prop 209 affected the racial gap in human capital investment prior to college entry.

3.8 Acknowledgement

This paper was co-authored with Kate Antonovics and Valerie Ramey and is being prepared for submission for publication.

3.9 Figures and Tables



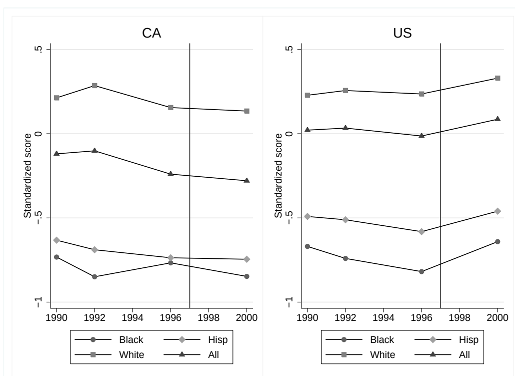
(a) SAT Scores

(b) GPA

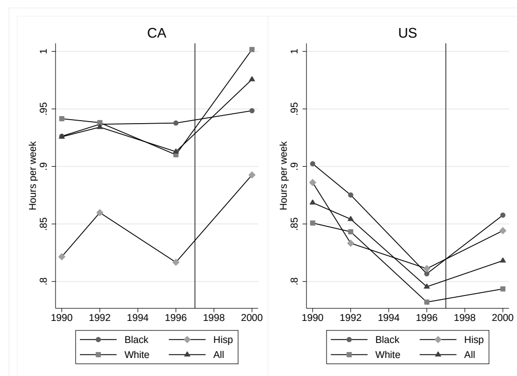
(c) SAT Math Scores

(d) SAT Verbal Scores

Figure 3.1: College Board outcomes. Each panel shows the mean of the normalized outcome (SAT scores or GPA) by race and year. US panel excludes California, Louisiana, Mississippi, Texas, and Washington.



(a) NAEP Math Grade 8 scores



(b) NAEP Grade 8 HW.

Figure 3.2: NAEP outcomes. Panel A shows the mean of normalized NAEP math test scores by race and year. The US panel excludes California, Louisiana, Mississippi, Texas, and Washington.

Table 3.1: Proposition 209 Timeline

Date	Event
Spring 1995	1996 graduation cohort begins taking SAT
July 1995	Regents of UC pass SP-1
Fall 1995	1996 graduation cohort finishes taking SAT
Jan - March 1996	1996 NAEP sampled
July 1996	First mention of Prop 209 in media
November 1996	Prop 209 passed by voters
Spring-Fall 1997	1998 cohort takes SAT
November 1997	Supreme Court declines to review case, Prop 209 law
Fall 1998	First affected cohort (1998) enrolls

Notes: See text for description.

Table 3.2: UCOP Summary Statistics

	All		Non-URM		URM	
	Before	After	Before	After	Before	After
SAT Math	597	602	614	617	528	534
	(94)	(93)	(86)	(86)	(93)	(93)
SAT Verbal	569	572	580	583	525	527
	(97)	(96)	(95)	(94)	(93)	(93)
HS GPA	3.6	3.6	3.7	3.7	3.4	3.5
	(.5)	(.49)	(.5)	(.49)	(.48)	(.49)
Parental Income/10,000	6.1	6.5	6.5	6.9	4.5	4.9
	(3.3)	(3.4)	(3.3)	(3.3)	(3)	(3.2)
Parent At Least College	.66	.65	.73	.72	.37	.36
	(.47)	(.48)	(.44)	(.45)	(.48)	(.48)
Obs	136766	149305	110072	121598	26694	27707
Admitted to Berkeley	.36	.27	.32	.28	.52	.25
Admitted to UCLA	.4	.31	.38	.32	.47	.25
Admitted to UCSD	.59	.42	.59	.44	.58	.32
Admitted to UCD	.74	.66	.72	.67	.85	.62
Admitted to UCI	.72	.61	.73	.63	.68	.53
Admitted to UCSB	.78	.54	.78	.54	.78	.52
Admitted to UCSC	.84	.81	.84	.82	.84	.76
Admitted to UCR	.84	.87	.85	.88	.81	.82

Notes: Standard deviations in parentheses. Before includes years 1995-1997; after includes 1998-2000. Non-URM includes White, Asian and other/unknown. URM includes blacks, Hispanics, and Native Americans.

Table 3.3: Predictors of URM and Admission to Each UC Campuses

	URM (1)	Berkeley (2)	UCLA (3)	UCSD (4)	UCD (5)	UCI (6)	UCSB (7)	UCSC (8)	UCR (9)
URM		.42***	.36***	.24***	.30***	.11***	.13***	.07***	.03***
		(.00)	(.00)	(.00)	(.01)	(.01)	(.00)	(.01)	(.01)
Post*URM		-.31***	-.26***	-.18***	-.23***	-.05***	-.02***	-.06***	-.03***
		(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
SAT Math	-.11***	.10***	.09***	.11***	.09***	.06***	.09***	.03***	.03***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Post*SAT Math		-.05***	-.03***	-.01***	-.02***	-.01***	.01***	.00	-.00
		(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
SAT Verbal	.02***	.14***	.16***	.10***	.09***	.08***	.09***	.06***	.06***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Post*SAT V		-.04***	-.03***	.01***	-.00	.04***	.01**	-.00	-.01***
		(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
HS GPA	-.01***	.14***	.19***	.27***	.18***	.21***	.15***	.11***	.12***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Post*HS GPA		.02***	-.01***	-.02***	.04***	.04***	.08***	.04***	.00
		(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Parent Coll	-.15***	-.03***	-.03***	-.03***	-.02***	-.00	-.03***	-.02***	-.03***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.01)	(.00)
Post*Par Coll		-.00	-.02***	-.03***	-.03***	-.00	-.02***	-.01	.01*
		(.01)	(.00)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Parental Inc	-.01***	-.00***	-.01***	-.01***	-.00***	-.00**	-.01***	-.00**	-.01***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Post*Par Inc		-.00***	-.00***	-.01***	-.01***	.00***	-.01***	.00*	.01***
		(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)
Post		-.01	-.03***	-.06***	.03***	-.12***	-.20***	-.02***	-.02***
		(.01)	(.00)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Constant	.34***	.24***	.37***	.57***	.74***	.78***	.93***	.91***	.96***
	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.00)	(.01)	(.01)

Notes: Column 1 shows the coefficients from a linear regression of an indicator for whether an applicant to the UC was a URM on academic achievement and family background characteristics. The remaining columns show the coefficients from a linear regression of an indicator for whether an applicant to a given campus was admitted to that campus on academic achievement and family background characteristics. The data span the years 1995-2000, and the post-Prop 209 period spans 1998-2000.

Table 3.4: Predicted Probability of Admission (Without Racial Preferences)

	Berkeley	UCLA	UCSD	UCD	UCI	UCSB	UCSC	UCR
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Non-URMs Pre	0.33***	0.39***	0.59***	0.72***	0.73***	0.80***	0.85***	0.85***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Non-URMs Post	0.28***	0.31***	0.44***	0.67***	0.62***	0.51***	0.81***	0.88***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
URMs Pre	0.11***	0.14***	0.36***	0.56***	0.58***	0.68***	0.78***	0.80***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
URMs Post	0.12***	0.12***	0.25***	0.55***	0.45***	0.39***	0.74***	0.82***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Relative URM Inc.	0.07	0.06	0.03	0.03	-0.02	-0.01	-0.01	-0.01
SE	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Notes: Row 1 shows the predicted probability that non-URM applicants to each campus would have been admitted prior to Prop 209, Row 2 shows the predicted probability that non-URM applicants to each campus would have been admitted post Prop 209, Row 3 shows the predicted probability that URM applicants would have been admitted prior to Prop 209 in the absence of racial preferences (i.e. with the coefficient on URM and post*URM in the previous table set equal to zero), and Row 4 shows the predicted probability that URMs would have been admitted post Prop 209 in the absence of racial preferences. The final row show the relative increase in URMs predicted probability of admission after Prop 209 in the absence of racial preferences (i.e. (Row 4-Row 3)-(Row 2 -Row 1)). All predictions are based on the estimates in the previous table. Standard errors in parentheses.

Table 3.5: Characteristics of Applicants Expected to be Admitted Under Different Admission Regimes, Berkeley

	Non-URM			URM		
Admission Rule	Pre	Pre	Post	Pre	Pre	Post
Admission Rate	Pre	Post	Post	Pre	Post	Post
	(1)	(2)	(3)	(4)	(5)	(6)
SAT Math	702	706	699	623	656	644
	(34.7)	(30.9)	(38.4)	(66.1)	(54.6)	(62.4)
SAT Verbal	690	693	683	616	654	640
	(40.8)	(38.5)	(45)	(68.8)	(54.5)	(64)
High School GPA	4.16	4.18	4.24	3.94	4.09	4.16
	(.187)	(.163)	(.056)	(.353)	(.25)	(.185)
Parent At Least College	.83	.823	.795	.496	.555	.498
	(.376)	(.382)	(.403)	(.5)	(.497)	(.5)
Parental Income/10,000	7.39	7.49	7.15	5.72	6.06	5.61
	(3.18)	(3.19)	(3.28)	(3.27)	(3.26)	(3.23)
Pred. First-Year College GPA	3.26	3.27	3.28	3.04	3.16	3.16
	(.092)	(.0848)	(.0796)	(.165)	(.119)	(.11)
Intended Science Major	.429	.444	.44	.343	.376	.386
	(.495)	(.497)	(.496)	(.475)	(.484)	(.487)
Observations	19737	16925	16966	5683	2808	2815

Notes: Considers only students who applied after Prop 209. "Admission Rule" refers to the weights given to academic achievement and family background characteristics. "Admission Rate" refers to the rate at which students were admitted. Column (1), for example, shows the expected characteristics of students who would have been admitted according to the weights given the academic achievement and family background according to the pre-Prop 209 admission rule and the pre-Prop 209 admission rate.

Table 3.6: College Board Summary Statistics

	All	Black	Hisp.	White	Asian	All	Black	Hisp.	White	Asian	All	Black	Hisp.	White	Asian
SAT	1018	858	919	1048	1047	1008	858	897	1063	1032	1020	858	934	1046	1058
	(203)	(180)	(194)	(190)	(224)	(210)	(188)	(188)	(188)	(221)	(201)	(179)	(196)	(190)	(225)
SAT Math	511	424	460	524	554	513	426	452	535	548	511	424	466	523	558
	(110)	(97)	(104)	(104)	(120)	(114)	(101)	(101)	(102)	(119)	(110)	(96)	(105)	(104)	(120)
SAT Verbal	507	435	459	524	494	494	433	445	528	484	510	435	469	524	500
	(108)	(99)	(106)	(102)	(127)	(114)	(103)	(103)	(103)	(123)	(107)	(99)	(107)	(101)	(128)
Normalized SAT	0	-.79	-.49	.15	.14	-.052	-.79	-.6	.22	.067	.0091	-.79	-.41	.14	.2
	(1)	(.89)	(.96)	(.94)	(1.1)	(1)	(.93)	(.93)	(.93)	(1.1)	(.99)	(.88)	(.97)	(.94)	(1.1)
Normalized SAT M	0	-.79	-.46	.12	.39	.022	-.77	-.53	.22	.33	-.0037	-.79	-.41	.1	.42
	(1)	(.88)	(.94)	(.94)	(1.1)	(1)	(.92)	(.92)	(.93)	(1.1)	(.99)	(.87)	(.95)	(.94)	(1.1)
Normalized SAT V	0	-.67	-.45	.16	-.13	-.12	-.69	-.58	.19	-.22	.021	-.67	-.36	.15	-.065
	(1)	(.91)	(.98)	(.94)	(1.2)	(1.1)	(.95)	(.95)	(.95)	(1.1)	(.99)	(.91)	(.99)	(.94)	(1.2)
GPA	3.2	2.9	3.1	3.3	3.4	3.2	2.9	3.1	3.3	3.4	3.2	2.9	3	3.2	3.4
	(.66)	(.66)	(.65)	(.65)	(.64)	(.64)	(.63)	(.63)	(.61)	(.62)	(.67)	(.66)	(.67)	(.65)	(.65)
Normalized GPA	0	-.5	-.23	.072	.24	.047	-.53	-.21	.15	.24	-.0083	-.5	-.24	.063	.24
	(1)	(1)	(.99)	(.98)	(.96)	(.96)	(.95)	(.95)	(.92)	(.94)	(1)	(1)	(1)	(.98)	(.98)
Male	.46	.41	.43	.46	.48	.45	.43	.42	.46	.48	.46	.41	.44	.46	.49
Income	5.5	3.6	3.8	6	4.8	5.2	3.9	3.6	6.4	4.5	5.5	3.6	4	5.9	5
	(3.3)	(2.7)	(2.9)	(3.2)	(3.4)	(3.5)	(2.9)	(2.7)	(3.4)	(3.3)	(3.2)	(2.6)	(3)	(3.2)	(3.4)
Mom educ	15	14	12	15	14	14	14	11	15	14	15	14	13	15	14
	(3.4)	(3.1)	(4.8)	(3)	(4.4)	(4.3)	(3.1)	(5.3)	(2.9)	(4.5)	(3.2)	(3.1)	(4.2)	(3)	(4.3)
Dad educ	15	14	12	15	15	14	14	11	16	14	15	14	13	15	15
	(3.6)	(3.4)	(5.1)	(3.2)	(4.4)	(4.5)	(3.3)	(5.5)	(3.1)	(4.4)	(3.4)	(3.4)	(4.6)	(3.2)	(4.3)

Notes: Standard deviations in parentheses.

Table 3.7: Changes in SAT Scores and GPA, College Board Data

	SAT		SAT M		SAT V		GPA	
CA	0.01	0.01	0.04	0.04	-0.03	-0.02	0.07**	0.09**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.04)	(0.04)
CA*Post	0.03***	0.03***	0.02***	0.01	0.03***	0.06***	-0.03***	0.03**
	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
CA*Post*Hisp	-0.04***	-0.05***	-0.03***	-0.03**	-0.05***	-0.06***	0.07**	0.05*
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)
CA*Post*Black	0.01	-0.01	0.03**	0.00	-0.01	-0.03	0.07***	0.08***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
CA*Post*Asian	-0.02*	-0.03	-0.03***	-0.03	-0.01	-0.02	-0.01	-0.04
	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.04)
State time trends		x		x		x		x

Each column shows the coefficient estimates from a regression of the outcome variable listed at the top of each column on an indictor for whether the student was from California, an indictor for whether they took the SAT in the post period, and an indicator for the student's race, along with the full set of interactions between these variables. Additional controls for gender, family income, and parental education are also included in the regressions. Standard errors clustered at state level. All outcome variables normalized to have mean zero and standard deviation one. Years include 1994-1995 (pre- period) and 1999-2000 (post- period). Included time trends are linear state-specific time trends. The excluded racial group includes white, other, and unknown.

Table 3.8: NAEP Summary Statistics

	All					California					Rest of Country				
	All	Black	Hisp.	White	Asian	All	Black	Hisp.	White	Asian	All	Black	Hisp.	White	Asian
NAEP math score	-0.00	-0.80	-0.63	0.26	0.36	-0.13	-0.86	-0.68	0.22	0.40	0.04	-0.79	-0.54	0.27	0.31
	(1.00)	(0.88)	(0.93)	(0.88)	(1.08)	(1.04)	(0.94)	(0.92)	(0.89)	(1.05)	(0.99)	(0.87)	(0.93)	(0.88)	(1.10)
Male	0.51	0.53	0.53	0.50	0.49	0.51	0.50	0.53	0.50	0.48	0.51	0.53	0.52	0.50	0.51
Parental Educ	14.01	13.89	12.75	14.19	14.72	14.00	14.41	12.58	14.61	14.85	14.01	13.82	13.04	14.10	14.57
	(2.07)	(2.01)	(2.28)	(1.98)	(1.91)	(2.15)	(1.82)	(2.25)	(1.76)	(1.84)	(2.04)	(2.03)	(2.29)	(2.00)	(1.97)
Hrs HW / week	0.86	0.87	0.85	0.84	1.10	0.94	0.94	0.85	0.95	1.16	0.83	0.86	0.84	0.82	1.04
	(0.53)	(0.54)	(0.54)	(0.52)	(0.49)	(0.53)	(0.54)	(0.54)	(0.51)	(0.47)	(0.53)	(0.54)	(0.55)	(0.52)	(0.51)
At least 1hr HW	0.27	0.30	0.27	0.24	0.51	0.34	0.36	0.27	0.33	0.56	0.25	0.29	0.28	0.23	0.44
Do any HW	0.89	0.89	0.86	0.89	0.95	0.91	0.90	0.87	0.92	0.96	0.88	0.89	0.86	0.88	0.93
Observations	198342	30381	11656	142080	8584	7357	611	2030	3771	734	190985	29770	9626	138309	7850

Notes: Standard deviations in parentheses. Rest of country excludes California, Louisiana, Mississippi, Texas, and Washington. Sample includes 1990, 1992, 1996 (pre- period) and 2000 (post- period).

Table 3.9: NAEP Grade 8 State Mathematics Assessments

	Math score		Homework (ordered probit)			
			Coeff.	Marg effect	Coeff.	Marg effect
CA	-0.08***	-0.08***	0.20***	0.07***	0.12***	0.04***
	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)	(0.01)
CA*Post	-0.12***	-0.11*	0.21***	0.07***	0.06	0.02
	(0.04)	(0.06)	(0.05)	(0.02)	(0.07)	(0.02)
CA*Post*Hispanic	0.05	0.05	-0.07	-0.02	-0.07	-0.02
	(0.08)	(0.08)	(0.10)	(0.03)	(0.10)	(0.03)
CA*Post*Black	0.00	-0.00	-0.17	-0.05	-0.17	-0.05
	(0.10)	(0.10)	(0.15)	(0.04)	(0.15)	(0.04)
CA*Post*Asian	0.02	0.02	-0.11	-0.03	-0.10	-0.03
	(0.13)	(0.13)	(0.15)	(0.04)	(0.15)	(0.04)
State time trends		x			x	x

Each column shows the coefficient estimates from a regression of the outcome variable listed at the top of each column on an indictor for whether the student was from California, an indictor for whether they took the NAEP in the post period, and an indicator for the student's race, along with the full set of interactions between these variables. Additional controls for gender and parental education are also included in the regressions. Years include 1990, 1992, 1996 (pre- period) and 2000 (post- period). Test scores normalized to have mean zero and standard deviation one. Included time trends are linear state-specific time trends. White students comprise the excluded racial group. Marginal effect shows the predicted increase in probability of doing more than one hour of homework (the highest response category) for a discrete change in explanatory variable from zero to one, evaluated at the mean values of the controls.

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