Do Affirmative Action Bans Lower Minority College Enrollment and Attainment?

Evidence from Statewide Bans

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ABSTRACT

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, despite evidence that fewer blacks and Hispanics graduated from college following a ban, the effects on graduation rates are very noisy.

I. 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. 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.

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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 (2010) 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 underrepresentation (defined by comparing enrollment to the statewide racial makeup of the relevant age group) of five percentage points.[1] Additionally, he finds that top-*x* percent programs (which guarantee graduating seniors in the top *x* percent of their class, where *x* varies by state, admission to a state university) offset some of the decline in minority students at some institutions. Like this paper, Hinrichs (2011) uses institutional data to analyze 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. Although Hinrichs (2011) 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. Although 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. Because 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 that 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

1. 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, although Bowen and Bok (1998) show that attending a more selective college improves other outcomes, such as the likelihood of (not getting a) divorce.

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 who did graduate tended to do so from less prestigious universities.

II. Data set

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.[2] Two measures 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 data set are lack of information on postcollege outcomes and on specific individuals, making an examination of the mechanisms of the effects difficult.[3]

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.

III. 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

$$\text{Enrollment share}_{jist} = \beta_0 + \beta_1 \cdot X_{st} + \theta \cdot \text{ban}_{st} + \gamma_i + \gamma_t + \varepsilon_{ist} \tag{1}$$

$$\text{Graduate share}_{jist} = \beta_0 + \beta_1 \cdot X_{st} + \theta \cdot \text{ban}_{st} + \gamma_i + \gamma_t + \varepsilon_{ist} \tag{2}$$

where 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

2. For a more complete discussion of the data used, see the Data Appendix.

3. 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 data set 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.

effects.[4] 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.[5] 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 that affect the outcome variables, such as time-varying statewide conditions. The vector X_{st} is designed to control for two factors that 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 toward 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 older than 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* percent program had been instituted.[6] The sources for these variables are detailed more thoroughly in the Appendix.

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.[7] 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 pretreatment period (if available) likely would not change the groupings.[8] 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

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

5. 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 precollege behavior (which presumably should have a time-varying component as students are given more time to adjust effort).

6. None of the results in this paper are sensitive to including these controls.

7. I use test scores, rather than admissions rates, because admissions rates are not necessarily informative about selectivity; see Hoxby (2009).

8. 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.

the institutions below the 70th percentile of SAT scores, and as a result combine these institutions into one group. A small number of institutions reported neither SAT nor ACT scores. Because 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 because it was the first year Texan universities conducted admissions after the conclusion of the *Hopwood* case. For Florida, I use 2001 because it was the first year affirmative action was banned, even though the top 20 percent 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 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.

A. Summary Statistics

Institutional summary statistics for four-year, public universities are presented in Table 1. Numbers for enrollment and graduates are institutional averages for the prepolicy change period: the three years before a ban in affected states and 1994–96 for nonban states.

The first three columns show the entire sample of 526 institutions in all states, followed by institutions in ban states and nonban 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.

B. Graphical Representation of the Effect of the Bans

A first look at enrollment over time in affected states is shown in Figure 1, which shows enrollment shares of blacks and Hispanics aggregated by state and selectivity group in a time window around each state's ban year.[9] The figure shows clear drops

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

Table 1
Summary Statistics

		By State		By Selectivity		
	All States	Ban States	Nonban States	Low Selectivity	Medium Selectivity	High Selectivity
Enrollment, all	1321	1848	1195	1241	2750	3835
	(1153)	(1454)	(1031)	(752)	(1655)	(1699)
Black enrollment	14.2	14.6	14.1	18.8	6.2	4.8
percent	(23.2)	(21.7)	(23.6)	(25.1)	(3.8)	(2.7)
Hispanic enrollment	5.9	13.8	4.0	16.5	6.5	10.3
percent	(11.0)	(17.3)	(7.7)	(20.1)	(5.3)	(5.8)
White enrollment	72.6	58.3	76.0	53.7	74.2	58.2
percent	(27.1)	(27.3)	(25.9)	(29.6)	(14.4)	(20.7)
Asian enrollment	4.7	10.1	3.4	8.0	9.6	23.1
percent	(8.8)	(13.2)	(6.8)	(11.1)	(12.0)	(19.1)
Unknown	1.8	3.2	1.5	3.3	2.8	3.9
enrollment	(3.5)	(4.0)	(3.2)	(4.4)	(3.2)	(3.2)
percent						
Graduates, all	685	1035	601	476	1743	3070
	(856)	(1189)	(732)	(366)	(1239)	(1407)
Black graduates	12.5	13.0	12.4	16.7	5.5	3.8
percent	(23.4)	(22.4)	(23.7)	(26.2)	(4.2)	(2.4)
Hispanic graduates	5.2	12.3	3.5	14.9	5.9	8.5
percent	(10.5)	(17.0)	(7.3)	(19.8)	(4.9)	(4.6)
White graduates	73.2	58.4	76.7	53.8	74.0	58.4
percent	(27.5)	(28.2)	(26.2)	(30.4)	(16.1)	(22.2)
Asian graduates	4.7	10.0	3.4	8.0	9.2	22.5
percent	(8.9)	(13.3)	(6.9)	(11.5)	(12.0)	(18.7)

Unknown graduates	2.2	3.7	1.8	3.8	2.9	4.3
percent	(4.6)	(4.8)	(4.5)	(5.3)	(3.3)	(3.8)
75th percentile SAT	582	589	580	553	633	702
Math	(63)	(63)	(63)	(36)	(19)	(29)
Income (in dollars)	16,710	17,986	16,402			
	(1951)	(1503)	(1923)			
School accountability	0.20	0.57	0.11			
	(0.37)	(0.43)	(0.29)			
Share over high	0.87	0.86	0.88			
school	(0.04)	(0.04)	(0.04)			
Share over college	0.25	0.25	0.25			
	(0.04)	(0.02)	(0.05)			
Unemployment	0.04	0.04	0.04			
	(0.01)	(0.01)	(0.01)			
Number of institutions	526	101	425	69	20	12

Notes: 1990–2009 pooled IPEDS, public universities. Affected universities are those located in Texas, California, Washington, Florida, Georgia, and Michigan. Selectivity groups defined according to reported 75th percentile SAT math score (see text). Selectivity group averages reported for institutions in ban states only. Additional summary statistics available from author.

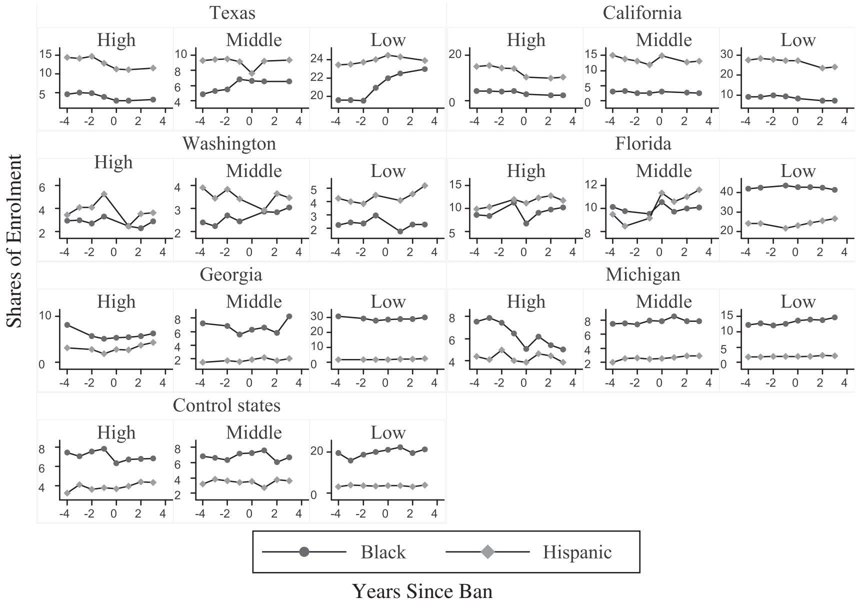

Figure 1
Enrollment Share by State and Selectivity Group

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.[10] For these states, it appears there may have been a decrease in black enrollment at the most selective institutions following the bans.

Figure 1 underscores the importance of accounting for time trends, because some states have steady preexisting trends in minority enrollment. A simple difference-in-difference regression comparing highly selective universities in Florida to those in nonban states would suggest that the affirmative action ban increased the share of Hispanic students enrolling, because average enrollment was higher in Florida after the ban and roughly constant in nonban 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 2 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

10. 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.

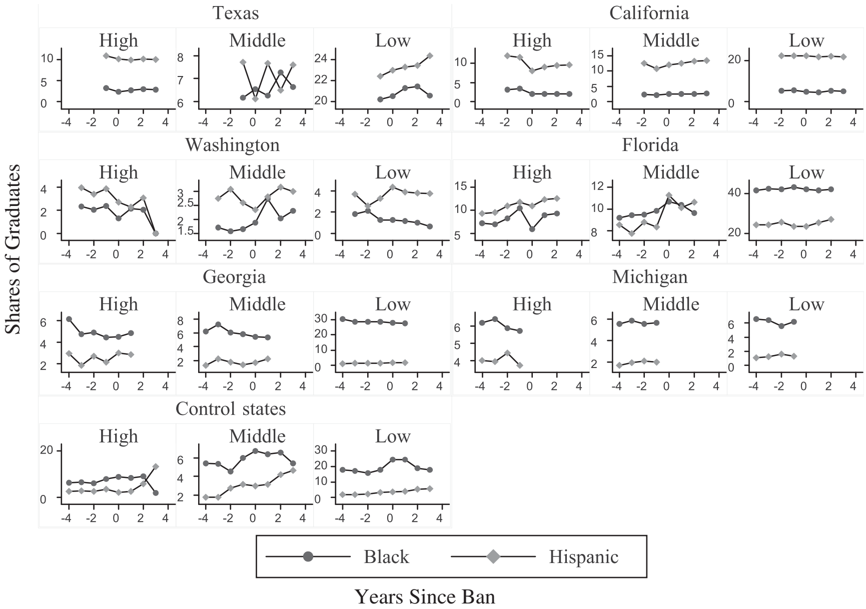

Figure 2
Graduate Share by State and Selectivity Group

graduates were black or Hispanic.[11] Figure 2 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 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—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 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 that 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.

11. 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.

Table 2
Sample Averages of Enrollment Share by State and Selectivity

	Blacks			Hispanics		
	High	Medium	Low	High	Medium	Low
Texas						
Preban	4.6	5.9	20	14	9.4	24
	(0.59)	(3.6)	(29)	(1.3)	(1.8)	(27)
Postban	2.8	6.6	22	11	8.4	24
	(0.13)	(5.2)	(30)	(1.9)	(1.1)	(27)
California						
Preban	4.2	2.8	9.5	15	13	28
	(2.5)	(1.3)	(5.9)	(3.6)	(4.4)	(12)
Postban	2.6	2.9	7.8	10	14	25
	(1.3)	(1.3)	(5.7)	(1.6)	(4.9)	(12)
Washington						
Preban	3	2.5	2.6	4.5	3.6	4.1
	(0.3)	(0.49)	(1.1)	(0.66)	(0.46)	(0.83)
Postban	2.4	2.9	2	3	3.3	4.3
	(.12)	(1)	(0.63)	(0.78)	(0.55)	(0.68)
Florida						
Preban	9.9	9.7	43	11	8.8	23
	(2.1)	(2.2)	(44)	(1.2)	(2.2)	(27)
Postban	8.5	10	43	12	11	24
	(1.6)	(2.1)	(43)	(0.88)	(2)	(27)
Georgia						
Preban	5.4	6.2	28	2.4	1.7	1.8
	(.42)	(2.9)	(24)	(0.62)	(0.45)	(1.1)
Postban	5.5	6.2	29	3.1	1.9	2.1
	(0.17)	(3.7)	(25)	(0.58)	(0.32)	(1.1)
Michigan						
Preban	7.3	7.6	13	4.3	2.6	1.9
	(2.6)	(3.2)	(9.5)	(1.8)	(.81)	(.68)
Postban	5.5	8.1	14	4.3	2.7	2.1
	(2)	(2.7)	(12)	(1.5)	(0.63)	(0.75)
Adjacent state control group						
Preban	7.5	6.8	18	3.8	3.6	3.6
	(3.4)	(5.8)	(25)	(2.6)	(4.2)	(7.5)
Postban	6.6	7	21	4	3.3	3.5
	(2.9)	(5.9)	(26)	(2.5)	(3.9)	(6.8)

Notes: 1990–2009 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.

IV. 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 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 nonban state sample to only states adjacent to a banning state. Reported coefficients are expressed in percentage terms: a coefficient of one represents a one 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.[12] The specifications that 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 because the sample is smaller.[13]

The next row restricts the sample to the decile of the most selective universities. 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.[14] Finally, there is no evidence that the remaining low selectivity institutions experienced a change in black enrollment following the bans.

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

13. The restricted years include 1995–2001, designed to capture a period in which many states changed their affirmative action laws.

14. To compare effects across selectivity groups, coefficients should be weighted by the total number of students in each group. Using the Table 1 to perform a rough calculation yields about 46,000 (12 schools * 3,835 students/school) students enrolling in the high group and 55,000 (20*2,750) students enrolling in the medium group each year in affected states.

Table 3
Effect of Ban on Black Enrollment Share Under Various Specifications

	Year dummies (1)	State trends (2)	University trends (3)	University squared trends (4)	Years restricted (5)	Adjacent states (6)	Adjacent and trend (7)
All institutions	−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)
Number	526	526	526	526	526	265	265
R-squared	0.031	0.11	0.53	0.65	0.022	0.045	0.089
High-selectivity institutions	−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)
Number	46	46	46	46	46	19	19
R-squared	0.1	0.35	0.44	0.52	0.18	0.18	0.53
Medium-selectivity institutions	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)
Number	116	116	116	116	116	60	60
R-squared	0.035	0.28	0.43	0.61	0.047	0.044	0.2
Low-selectivity institutions	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)
Number	364	364	364	364	364	186	186
R-squared	0.089	0.19	0.57	0.67	0.049	0.14	0.2

Notes: 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* percent plan (see text). Universities weighted by total enrollment in 1996. Standard errors robust to clustering at the state level. In addition to the controls listed above, all models include year dummies, while Column 2 adds state-specific linear time trends, Column 3 university-specific linear time trends, Column 4 university-specific quadratic time trends, Column 5 a restricted time horizon, Column 6 a restricted group of control states, and Column 7 a restricted group of states with state-specific linear time trends (see text). * Significant at 10 percent, ** 5 percent, *** 1 percent.

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, nonsignificant decreases in the bottom 70 percent of universities. Because 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 4 shows a summary of regressions using the specification used in Table 3, Column 2 for various samples. Thus, the first panel of Column 1 is identical to Table 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 percent level. Furthermore, the enrollment drops at the top selectivity group was larger for Hispanics than for blacks—about 2.9 percentage points. There were no significant changes in the second tier and nonsignificant, 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 0.38/11.7 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 postpolicy change mean of 11.3 percent (author's calculation), this is a change of about 20 percent. Note that in absolute terms the number of Hispanics at the top institutions fell by more than for blacks; however, because 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.[15] 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).

Because 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 4. Coefficients reported are from regressions with log total enrollment as the outcome variable. The coefficients are consistently small and insignificant: the policy change appears not to affect total enrollment at the average institution or at the top tier.

15. 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.

Table 4
Effect of Ban on Black and Hispanic Enrollment and Graduate Shares

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

Note: see notes from Table 3.

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 (dis-

Table 5
Effect of Ban on White and Asian Enrollment and Graduate Shares

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

Note: see notes from Table 3.

cussed 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 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—more than three 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.

A. Graduation

To see how the effects on enrollment shares translate 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 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 0.37 percentage point increase. Finally, the least selective institutions show a nonsignificant decrease in graduate shares.

For Hispanics, Column 2 shows a nonsignificant 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, because 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 because 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, with little evidence of change in the overall number of blacks enrolling in college 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.

B. Private Institutions, Two-Year Institutions, Out-of-State Students, and Nonrace 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 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 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, nonsignificant decrease in enrollment share.

Another factor that 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. 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 occurred 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 would provide 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. However, the evidence presented about blacks and Hispanics would still be valid.

C. 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 &apos+acebo’ 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.[16] 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 changes in the racial composition of enrollment were associated with the introduction of the bans. Results are presented in Table 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, rerunning regressions although dropping one state at a time, results (not shown but available from author) remain similar.

16. One way of dealing with these prepolicy 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 nonearly adopters. Results are similar to those discussed in this section.

Table 6
"Effect" of Lead of Ban on Enrollment Share

Selectivity group	Year dummies (1)	State trends (2)	University trends (3)	University squared trends (4)	Years restricted (5)	Adjacent states (6)
Panel 1: Blacks						
All	0.04	−0.17	−0.17	−0.13	0.03	−0.30
	(0.20)	(0.15)	(0.16)	(0.13)	(0.18)	(0.14)
High-selectivity	0.50	−0.01	−0.01	0.04	−0.18	0.38
	(0.35)	(0.24)	(0.24)	(0.25)	(0.20)	(0.35)
Medium-selectivity	0.04	−0.12	−0.12	−0.07	−0.01	−0.03
	(0.23)	(0.19)	(0.20)	(0.16)	(0.18)	(0.26)
Low-selectivity	−0.02	−0.20	−0.21	−0.29	0.06	−0.51
	(0.36)	(0.31)	(0.32)	(0.25)	(0.36)	(0.29)
Panel 2: Hispanics						
All	0.96**	0.37	0.36	0.46*	0.76*	1.00*
	(0.35)	(0.23)	(0.23)	(0.22)	(0.29)	(0.35)
High-selectivity	0.49	0.08	0.08	0.45	0.69*	0.86*
	(0.32)	(0.25)	(0.25)	(0.25)	(0.30)	(0.33)
Medium-selectivity	0.73***	0.09	0.09	0.36	0.44	0.73***
	(0.17)	(0.17)	(0.18)	(0.19)	(0.26)	(0.18)
Low-selectivity	1.45	0.83	0.83	0.58	0.56	1.42
	(0.84)	(0.48)	(0.50)	(0.42)	(0.29)	(0.82)

Notes: 1990–2009 pooled IPEDS, public universities. Reported coefficients measure the average "effect" of the second and third leads of the policy change. See notes from Table 3.

V. Discussion

This paper investigates college enrollment and completion at four-year public universities in states that 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.

Appendix

Data

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.[17] To construct a measure of institution selectivity, I use two reports from 2007: the 75th percentile math SAT score and75th 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.

Statewide School Accountability

A concurrent factor that 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 insti-

17. About 80 percent of the institutions that reported data in 1990 meet these criteria.

tution's state introduced consequential school accountability, as shown in Table 2 of Miller and Zhang (2006). Following Hanushek and Raymond (2005), I 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).

Top-x Percent Programs

Three of the affirmative action banning states enacted a top-*x* percent rule after the ban. As mentioned earlier, Long (2004) and Long (2007) show that a top-*x* percent 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* percent policy changes. The inclusion of these terms does not have a large impact on the estimates.

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