Mismatch in Law School

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Abstract
According to the "mismatch" hypothesis, affirmative action preferences in admissions induce minority students to attend selective schools where they are unable to compete with their more qualified white classmates. We implement two tests of mismatch using data on law students. Students attending more selective law schools earn substantially lower grades than similarly-qualified students at less selective schools, but are no less likely to graduate or pass the bar exam, and obtain better jobs at higher salaries. We also compare black students to whites. In the upper four quintiles of the LSAT-undergraduate GPA distribution, blacks and whites graduate and pass the bar exam at similar rates, though blacks attend more selective schools and earn lower grades; blacks also obtain better post-law-school jobs. In the bottom quintile, black bar passage rates are lower. However, this cannot confidently be attributed to mismatch, as many more whites than blacks are unable to gain admission to law school, introducing the potential for sample selection bias.

I. Introduction

Since the late 1960s, many colleges and universities have exercised affirmative action in their admissions decisions, giving preferences to black applicants over white applicants with similar entering credentials. Critics have argued that these preferences *hurt* black students by admitting them to schools for which they are unprepared, and that the purported

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beneficiaries would do better—would learn more, would be more likely to graduate, would avoid psychological damage, etc.—if they were required to attend schools more appropriate for their qualifications (Summers 1970; Sowell 1978; Thernstrom and Thernstrom 1997). This so-called "mismatch" might be characterized as a negative peer effect: A student's outcomes will decline if the average qualifications of her classmates rise too high above her own (Loury and Garman 1995).

Evidence regarding the mismatch hypothesis has been extremely limited (Holzer and Neumark, 2000). Substantial differences between black and white college completion rates are sometimes presented as indications of mismatch (Thernstrom and Thernstrom 1997; Herrnstein and Murray 1994; D'Souza 1991), though these may simply reflect large black-white gaps in entering students' qualifications (Kane 1998). In an influential study, Bowen and Bok (1998) find that minority students at elite colleges, when compared to similarly qualified students at somewhat less selective colleges, experience better long-run outcomes. The elite college minority students in Bowen and Bok's sample have qualifications well below their white classmates, so one would expect them to have done poorly if mismatch effects were important. More recently, however, Sander (2004) analyzes differences between the outcomes of black and white law students, and concludes that mismatch effects are substantial.[1]

In this paper, we use the same data as that used in Sander's (2004) study to examine the mismatch hypothesis. The Bar Passage Survey (BPS), conducted by the Law School

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[1] Sander reaches this conclusion by examining the difference between the law school grades of similarly-qualified black and white students, a difference he attributes to affirmative action. He then estimates a model for bar exam passage rates with grades and school selectivity as independent variables; he finds that the grade effect in this latter model, when multiplied by the race effect from the former, is large enough to swamp the positive effect of school selectivity on bar passage. Sander's analysis and conclusions have been the subject of vehement criticism (Ayres and Brooks 2005; Chambers et al. 2005; Dauber 2005; Wilkins 2005; Ho 2005), to which Sander (2005a; 2005b) responded.

Admissions Council, is a rich longitudinal data set with information about admissions qualifications, law school selectivity, and a variety of outcomes for a majority of the students who entered accredited law schools in the Fall of 1991. In contrast to Sander's relatively indirect analysis, which relies on the questionable assumption that mismatch is the only factor that could produce a gap between the law school GPAs of similarly-qualified white and black students, we adopt more direct estimation strategies that do not rely on the consideration of law school GPA as a mediating factor.

The key challenge in estimating mismatch is the measurement of counterfactual outcomes. Namely, what would have happened to black students admitted to selective schools had they not received admissions preferences? We study two comparison groups, implementing each comparison with reweighting techniques (DiNardo, Fortin, and Lemieux, 1996; Hahn, 1998; Hirano, Imbens, and Ridder, 2003) that do not require us to parameterize the relationship between admissions qualifications and outcomes.

We begin with a comparison between students attending selective law schools and a matched (on race and admissions qualifications) sample of students at non-selective schools. Although this comparison may yield upward-biased estimates of the selective school effect—students who wrote strong application essays, for example, may have been more likely both to attend selective schools and to pass the bar exam than students who are observably similar in our data but who wrote weaker essays—its simplicity and direct relationship with the hypothesis in question make it a natural starting place.

Second, we compare black students with a matched (on observed admissions qualifications) sample of white students. This approach adopts Sander's (2004) assumption that observable credentials summarize the gap in potential outcomes between black and white entering students. If this assumption is incorrect, it may yield biased estimates as well.[2] However, the bias is likely in the opposite direction to that in our first analysis: In a variety of contexts, black students have been found to underperform white students with the same credentials in the same environments, and any such effects will be labeled as mismatch by our analysis. Thus, we expect that the two estimates should bracket the true mismatch effect.

At the outset, it is important to emphasize the limited overlap between the distributions of admissions credentials of white and black law students: Using Sander's (2004) index of LSAT scores and undergraduate grade point averages, the white-black gap is 1.6 standard deviations at the mean; the 95th percentile black student is at only the 53rd percentile of the white distribution; and the 5th percentile white student is at the 60th percentile of the black distribution. The large racial gap warrants against a simple between-group comparison of outcomes. Simply controlling linearly for LSAT scores and undergraduate grades reduces the black-white gap in bar passage rates by 40%, and when we allow for a nonlinear relationship we eliminate more than half of the gap.

We find no evidence of black underperformance in the upper four quintiles of the admissions index distribution. In the bottom quintile, however, there is a 14 percentage point gap in bar passage rates that cannot be explained by differences in entering credentials. Evidence for mismatch effects on bar passage is thus concentrated at the bottom of the qualifications distribution. Although three quarters of blacks in our sample fall in the bottom quintile, inference about mismatch in this range is hazardous: Only a small fraction of applicants with such poor credentials are admitted to law school, and this fraction is

[2] Our assumptions are somewhat weaker than Sander's: He requires that race be uncorrelated with either latent GPAs or latent bar passage probabilities conditional on admissions credentials, but we can dispense with assumptions on GPAs for our analysis of bar passage.

notably smaller for whites than for blacks. If better prepared students are more likely to be admitted—as would occur if lower-ranked law schools evaluated applications for indications of preparedness that we cannot observe—this could generate differences between the mean performance of admitted white and black students even in the absence of mismatch effects.

We interpret our results as demonstrating convincingly that there are no mismatch effects on the bar passage rates of the most qualified black students. For students in the bottom quintile of the qualifications distribution, the data are consistent either with mismatch or with differential selection; the possibility of the latter prevents strong conclusions about the causal effects of affirmative action policies on bar passage rates. By contrast, when we turn to employment outcomes, we find large, positive effects of attending a selective school and of being black throughout the credentials distribution.

Part II describes aspects of legal education that make it an attractive arena in which to study mismatch. Part III provides a more formal discussion of the mismatch hypothesis. We define the term, distinguishing it from other common conceptions, and identify various formulations of the hypothesis. Part IV discusses our identification strategies and our empirical approach. Part V describes the Bar Passage Study (BPS) data, with particular attention paid to limitations that constrain our analyses. Parts VI and VII presents results, and Part VIII concludes.

II. Why Law Schools?

While mismatch might appear anywhere that admissions preferences are granted, there are reasons to believe that it is particularly likely in legal education. First, while colleges can always lower grading and graduation standards to mask mismatch effects (Mansfield 2001), the bar exam—the same for all prospective lawyers in a state, graded without regard to the race of a taker or the school that she attended, and outside of law schools' direct control—makes such manipulation more difficult.[3] The most elite law schools are renowned for doing relatively little to help their students prepare for this exam, while lower-ranked, less-selective schools devote more of their curricula to preparation (Edwards 1992; White 1993). Thus, attendance at very selective schools seems particularly likely to reduce success rates among students at risk of failing the bar exam.

Second, the legal job market relies heavily on students' grades, particularly those earned in the first year of law school when courses are typically exam-based and graded on strict curves. Students' grades are important qualifications for access to the stepping stones (law review membership and good summer internships) to many prestigious post-law-school jobs. Even in the absence of mismatch effects on students' actual achievement, a given student at more selective schools can be expected to rank lower within her class, which, unless employers take this into account, could adversely affect her employment outcomes.

Legal education is also analytically appealing. In other contexts, one is forced to rely on post-graduation salaries. These may be uninformative about student achievement, both because salaries of recent graduates are imperfectly correlated with the desirability of their jobs—some prestigious jobs, i.e. those in government agencies, have low current salaries but promise higher long-run income—and because employers may themselves practice affirmative action in hiring. By contrast, bar exam passage provides an objective indicator of law students' success at an early point in their careers.

Moreover, admissions decisions at the most selective law schools are extremely dependent on applicants' easily-observed numerical qualifications. A probit model for

[3] The exam varies across states, but nationally only 83% of students pass the exam on the first try (authors' calculations from National Conference of Bar Examiners, 1995).

admission decisions at 77 highly-ranked law schools yields LSAT and undergraduate GPA coefficients of 0.13 and 1.42, respectively, indicating that a single extra LSAT point (1/7 standard deviation) or a 0.1 increase in GPA (on the standard four point scale) raises the admission probability for the most marginal applicants by more than five percentage points.[4] Thus, while observational mismatch studies must always confront the possibility that school selectivity is endogenous to student qualifications, this is less of a concern in law schools than in undergraduate education.[5]

III. Defining Mismatch

The "mismatch" hypothesis is that some students would obtain better outcomes—e.g. higher grades, higher bar passage rates, and better jobs at higher salaries—if they attended less-selective schools than if they attended more-selective schools, and that the minority students admitted to selective law schools via affirmative action preferences fall into this group. Thus, were affirmative action eliminated, the students who would be displaced into less selective schools would see their outcomes improve.

It is important to identify two related claims that are not, in our understanding, what is commonly meant by mismatch. First, it might be that *all* students would be better off

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[4] LSAC (1992) reports the number of applications and admissions by LSAT-GPA cell for most law schools for the 1991-2 admissions cycle, including 77 of the schools ranked in the U.S. News and World Report (2005) top 100. The coefficients reported above come from a grouped probit model that includes the cell midpoint LSAT and GPA as well as school fixed effects. The "most marginal applicants" are those at the steepest part of the probit curve, with baseline admissions probabilities of 0.5; as applicants with average qualifications have admissions probabilities of 0.11, the corresponding effects for these students are about 2.5 percentage points.

[5] A computation similar to that above using the coefficients from Kane's (1998, Table 12-2) model for undergraduate admissions at the most selective quintile of colleges indicates that a 1/7 standard deviation increase in SAT scores and a 0.1 point increase in high school GPA raise admissions probabilities of applicants with average characteristics by only 0.7 and 1.5 percentage points, respectively. Kane's model includes controls for race, family income and parental education, which ours does not, and is estimated on microdata.

attending less-selective law schools. If selective law schools have constant, negative treatment effects, elimination of affirmative action would improve minority students' outcomes but would reduce those of white students with no impact on the grand mean.[6] In this case, the ideal policy might be to eliminate the selective law schools, or to alter their curricula to more closely resemble those seen at less competitive schools. We consider the "universal mismatch" hypothesis to be implausible and therefore uninteresting. Consideration of this hypothesis, however, suggests that an appropriate analytical framework for mismatch must allow for heterogeneous treatment effects of attending selective law schools.

A second related hypothesis contends that some applicants are so poorly prepared for law school that they would be better off pursuing other careers. Only a bit over half of law school entrants from the bottom decile of the admissions qualifications distribution pass the bar exam within five and a half years of entering law school, and it is plausible that students in this range should not have been admitted in the first place.[7] To the extent that affirmative action leads some of the least qualified black applicants to be admitted to law school who would otherwise be rejected everywhere, then, its elimination might benefit some of these applicants by compelling them to seek other careers before investing considerable time and expense in unsuccessful attempts to become lawyers.

A priori, it is impossible to know what bar passage rate is high enough to rationalize law school attendance over non-law options. A serious evaluation requires data on students' alternatives if they do not attend law school, which we do not have. We focus on mismatch

[6] This assumes constant treatment effects on mean outcomes. With constant treatment effects on the index in a probit or logit model for a dichotomous outcome, elimination of affirmative action could raise or lower mean outcomes, but any such effect would be small.

[7] It is also plausible that students who never practice law nevertheless benefit from attending law school. When we analyze employment outcomes, below, we include students who failed or did not take the bar exam.

among law schools, which can be plausibly measured by examining bar passage rates and other indicators of law school success, rather than on mismatch *between* law school and other career choices, which would require much more detailed information about life outcomes.

Finally, it is important to note that naïve comparisons that do not take careful account of ability differences among students may be extremely misleading. Students attending less selective law schools achieve much worse outcomes, on average, than do those attending more selective schools, and black students' outcomes are generally inferior to those of whites. These gaps cannot easily be attributed to mismatch effects, however, as each of these comparisons is between groups that are substantially different in their entering credentials (Kane 1998). An important advantage of our analytical approach is that we do not rely on assumptions about the particular parametric form of the relationship between entering credentials and student outcomes.

Notation

We adopt a potential outcomes framework. For simplicity, suppose that law schools come in only two types, selective (s=1) and non-selective (s=0). Let p_i^s be the outcome that student i would obtain if he or she attended a school of type s. Of course, for any individual student only $p_i = s_i p_i^1 + (1-s_i)p_i^0$ can ever be observed. Let b_i be a race indicator (with $b_i=1$ indicating a black student), and let X_i be a vector of observable admissions qualifications.

One can formulate three versions of the mismatch hypothesis. In its weakest form, it might simply state that there are some students who would fare better attending a non-selective school than a selective school:

<u>Mismatch version 1</u>: For some i, $p_i^1 < p_i^0$.

This, of course, is untestable without further structure. A slightly stronger version of the hypothesis might state that there are some categories of students, identifiable ex ante, for whom this is true:

<u>Mismatch verson 2</u>: For some (b, X), E[p_i^1-p_i^0 | b_i=b, X_i=X]<0.

A final version of the hypothesis states that the average black student who is brought into selective schools via affirmative action policies is mismatched. Let $s_i^{noAA} \in \{0, 1\}$ be the selectivity of the law school that the student would attend were there no racial preferences used in admissions.

<u>Mismatch version 3</u>: E[p_i^1-p_i^0 | b_i=1, s_i>s_i^{noAA}]<0.

This is the claim that seems to motivate most discussions of mismatch.[8]

IV. Observable Implications and Methods

The impact of affirmative action on black students with characteristics X can be written as the product of admissions preferences and the mismatch effect, plus a term reflecting the extent to which affirmative action beneficiaries are selected for the benefit that they will obtain from attending a selective school:

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\begin{aligned}
(1)\qquad & E\left[\left(p_i^1 - p_i^0\right) * \left(s_i - s_i^{noAA}\right) \middle| b_i = 1, X_i = X\right] \\
& = M(1, X)\delta(1, X) + \operatorname{cov}\left(\left(p_i^1 - p_i^0\right), \left(s_i - s_i^{noAA}\right) \middle| b_i = 1, X_i = X\right),
\end{aligned}
$$

where M(b, X)=E[p_i^1-p_i^0 | b_i=b, X_i=X] is the average latent mismatch among (b, X) students and δ(b, X)=E[s_i-s_i^{noAA} | b_i=b, X_i=X] is the impact of affirmative action on these students' distribution across schools.

[8] An example is Sander's (2004) claim that affirmative action reduces the number of black lawyers. In our notation, the effect of affirmative action on black students who would attend law school in any case is E[(p_i^1-p_i^0)*(s_i – s_i^{noAA}) | b_i=1]. It seems reasonable to suppose that $s_i \geq s_i^{noAA}$ for black students. Thus, this effect reduces to that stated in the hypothesis. Sander's claim also requires that any black students who would not attend law school at all in the absence of affirmative action pass the bar exam at sufficiently low rates that they are outweighed by those who fail the exam due to affirmative action-induced mismatch.

To evaluate the intermediate version of the mismatch hypothesis, we require estimates of $M(b, X)$ throughout the (b, X) distribution. For the strong version of the hypothesis, we also require estimates of $\delta(b, X)$ and either assumptions or evidence about the covariance of admissions decisions with individual mismatch. For much of our analysis, we impose an unconfoundedness (Rosenbaum and Rubin 1983; Rubin 1978) assumption: Admissions offices at selective schools—which presumably seek either to identify students with a large p_i^1 (high likelihood of success at selective schools) or a large p_i^1-p_i^0 (big benefit from attending selective schools)—observe no more about students' qualifications than do we, and student matriculation decisions are similarly ignorable. Thus, $(p_i^0, p_i^1) \perp (s_i, s_i^{noAA}) \mid b_i, X_i$. With this assumption, the (latent) individual mismatch is the same for students who attend selective schools as for observationally identical students who attend unselective schools.

δ is straightforward to estimate. As is common in the literature (see, e.g., Krueger, Rothstein, and Turner 2005), we approximate a race-blind admissions rule by that observed among white students, and we further assume that black application and matriculation decisions would mirror those of white students with similar qualifications in the absence of affirmative action. This implies that $\delta(0, X)=0$ and $\delta(1, X)=E[s_i \mid b_i=1, X_i=X] - E[s_i \mid b_i=0, X_i=X]$.[9]

The mismatch function, $M(b, X)$, is the estimand of prime interest. It is helpful to decompose it into components deriving from the subsets of (b, X) students attending selective and unselective law schools:

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[9] In practice, a race-blind admissions rule will likely produce $E[s_i^{noAA} \mid X_i=X]$ intermediate between $E[s_i \mid b_i=1, X_i=X]$ and $E[s_i \mid b_i=0, X_i=X]$, so our expression for $\delta(1, X)$ overstates the impact of moving to race-blind admissions on black enrollment rates. However, blacks make up only 8% of whites and blacks in law school, so the approximation $\delta(0, X)=0$ is likely reasonably accurate and the overstatement small.

$$M(b,X) = E[s \mid b,X]\left(\bar{p}(1,b,X) - \bar{p}^c(1,b,X)\right) \\ + \left(1 - E[s \mid b,X]\right)\left(\bar{p}^c(0,b,X) - \bar{p}(0,b,X)\right). \tag{2}$$

In this expression, $\bar{p}(s,b,X) = E\left[p_i^s \mid s_i = s, b_i = b, X_i = X\right]$ is the average outcome among of (b, X) students attending type-s schools, while $\bar{p}^c(s,b,X) = E\left[p_i^{1-s} \mid s_i = s, b_i = b, X_i = X\right]$ is the average outcome that these students would have obtained had they attended schools of the other type. Actual outcomes are readily observable; counterfactual outcomes are not.

We adopt two strategies for identifying the counterfactual. We expect that the first is biased against the mismatch hypothesis and the second in favor. Our two estimates thus should bracket the true effect.

A. Within-race comparisons between more- and less-selective schools

Our first strategy for identifying the counterfactual is quite simple. We use students observed attending less selective schools to estimate the counterfactual for students of the same race and observed qualifications at more selective schools. The difference between the observed outcomes of students at selective schools and those of students with the same (b, X) at less selective schools is:

$$\begin{aligned} D(b,X) &= E[p_i \mid s_i = 1, b, X] - E[p_i \mid s_i = 0, b, X] \\ &= E[p_i^1 \mid s_i = 1, b, X] - E[p_i^0 \mid s_i = 0, b, X] \\ &= E[p_i^1 - p_i^0 \mid s_i = 1, b, X] + \left(E[p_i^0 \mid s_i = 1, b, X] - E[p_i^0 \mid s_i = 0, b, X]\right) \\ &= \left(\bar{p}(1,b,X) - \bar{p}^c(1,b,X)\right) + \left(\bar{p}^c(1,b,X) - \bar{p}(0,b,X)\right) \end{aligned} \tag{3}$$

The first term in the final line of (3) is the average selective-school effect for students attending selective schools, while the second is the difference in potential unselective-school outcomes between students observed attending selective and unselective schools.

Interpretation of D(b, X) as informative about M(b, X) relies heavily on the assumption that the caliber of the school attended is unrelated to unobserved determinants of bar passage rates. Formally, assume that $E\left[p_i^j \mid s,b,X\right] = E\left[p_i^j \mid b,X\right]$ for j=0, 1.[10] This implies that $\bar{p}^c\left(s,b,X\right) = \bar{p}\left(1-s,b,X\right)$ for each s, b, and X, and that

$D\left(b,X\right) = \bar{p}\left(1,b,X\right) - \bar{p}^c\left(1,b,X\right) = M\left(b,X\right)$.

Our assumptions are violated if, for example, law school admissions offices can observe student qualifications that are not reported in our data sets, and if these qualifications are correlated with potential bar passage probabilities. Because law school applicants submit essays, recommendation letters, transcripts, and other indicators of student ability that are not available for our analysis, our assumptions are almost certainly incorrect, and $E\left[p_i^j \mid s_i = 1,b,X\right] - E\left[p_i^j \mid s_i = 0,b,X\right] > 0$. This makes the final term in (3) positive, so D(b, X) is upward-biased relative to M(b, X). However, given the evidence cited above that law school admissions probabilities depend heavily on the numerical qualifications that we do observe, we expect that the error is relatively small. If so, estimates of the mismatch effect obtained from this comparison will provide a reasonably tight upper bound for the true effect.

B. Comparisons between whites and blacks

Our second strategy compares the outcomes of black students to those of white students with similar admissions qualifications. Write the average outcome among (b, X) students as

$$\begin{aligned} E[p_i \mid b_i=b, X_i=X] &= E[p_i^0 + s_i(p_i^1 - p_i^0) \mid b_i=b, X_i=X] \\ &= E[p_i^0 \mid b_i=b, X_i=X] + \mathrm{cov}[s_i, (p_i^1 - p_i^0) \mid b_i=b, X_i=X] \end{aligned}$$

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[10] Note that this rules out the standard Roy model of self selection, where s_i is increasing in $(p_i^1 - p_i^0)$.

(4) $$+ E[s_i \mid b_i=b, X_i=X]*E[p_i^1 - p_i^0 \mid b_i=b, X_i=X] = \bar{p}_b^0(X) + C_b(X) + \bar{s}_b(X) * M(b,X)$$

where the notation $\overline{Z}_b(X)$ indicates $E[Z_i \mid b_i=b, X_i=X]$ and $C_b(X)=\mathrm{cov}[s_i, (p_i^1-p_i^0) \mid b_i=b, X_i=X]$.

The white-black gap in mean outcomes among students with qualifications X is then

(5) $$\begin{aligned} &E[p_i \mid b_i = 0, X_i = X] - E[p_i \mid b_i = 1, X_i = X] = \\ &= \left(\bar{p}_0^0(X) - \bar{p}_1^0(X)\right) + \left(C_0(X) - C_1(X)\right) + \left(\bar{s}_0(X) * M(0,X) - \bar{s}_1(X) * M(1,X)\right) \\ &= \Delta p^0(X) + \Delta C(X) + \Delta\bar{s}(X) * M(1,X) - \bar{s}_0(X)\left(M(0,X) - M(1,X)\right) \end{aligned}$$

where $\Delta f(X)$ indicates $f_0(X)-f_1(X)$. The first term in the final line of (5) captures the black-white difference in outcomes if everyone attended unselective schools. The second term captures the differential between whites and blacks in the covariance between selectivity and the benefit (or cost) of attending a selective school, and is positive if selective schools do a better job of identifying white students who will benefit from attending a selective school than they do of identifying black students who will so benefit. The final term is the difference between the selective school effect on white students and that on black students, multiplied by the white enrollment rate at selective schools. We approximate each of these terms as zero:

(6) $$\Delta p^0(X) = \Delta C(X) = M(0,X) - M(1,X) = 0 .$$

In this case, the white-black gap in outcomes reduces to

(7) $$\Delta p(X) = \Delta\bar{s}(X) * M(1,X),$$

the average selective school effect on black students, weighted by the difference between black and white enrollment rates at selective schools. If the average black student with characteristics X would be better off attending unselective schools, $M(1, X)<0$ and $\Delta p(X)<0$.

It is worth emphasizing the key assumption implicit in our approach: Black students' outcomes would mirror those of white students with the same admissions credentials if the two groups were similarly distributed across schools. This assumption is almost certainly violated in ways that lead us to overstate the degree of mismatch. Formally, each of the three terms in (6) are likely negative. There is substantial evidence from undergraduate admissions that black students underperform white students with the same entering credentials, even when they attend the same colleges (see, e.g., Rothstein, 2004; Young, 2001). A similar pattern appears to hold in law school (Wightman 2000; Wightman and Muller 1990; Anthony and Liu 2003; Powers 1977). Thus, it seems likely that $\Delta p_0(X)<0$. The sign of $\Delta C(X)$ is less obvious, but given the reduced admissions standards in terms of observables for black applicants, admissions offices may exert more effort to identify unobserved predictors of success at selective colleges for black than for white applicants, which could produce $\Delta C(X)<0$. Finally, black underperformance suggests that a difficult law school curriculum may overmatch black students more than it does whites with similar observable characteristics, producing $M(0, X) - M(1, X)>0$ and $-\bar{s}_0(X)(M(0,X)-M(1,X))<0$. Although we expect that such biases are relatively small, we view $\Delta p(X)$ as a lower bound for the true mismatch effect $M(1,X)\Delta\bar{s}(X)$.

There is another important source of bias in our estimates of mismatch at the lowest X values. Our comparisons are between black and white students who are observed attending law schools; those students who are "at risk" for attending law school but do not actually attend are not included in our samples. Below, we discuss evidence that the probability that a low-X student will have the opportunity to attend *any* law school is substantially less than one. There is almost certainly selection on unobservables here: Those low-X students who are admitted to some law school likely have better unobserved qualifications than are those who are rejected everywhere. If so, our sample estimates of $\bar{p}_b^0(X)$ are upward-biased relative to what would be seen if outcome measures were available for all race-b students with qualifications X.

The sample selection is notably more severe for white students than for blacks with the same entering credentials, as would be expected if affirmative action operates even at the margin of admission to the least-selective law schools. If there is any selection on unobservables in these admissions decisions, the resulting bias in $\bar{p}_0^0(X)$ is larger than that in $\bar{p}_1^0(X)$. Our estimates of $\Delta\bar{p}(X)$ are therefore upward-biased, particularly at low X values where the differential selection is most extreme. Because we do not have good information about the selection process, we do not attempt to correct our estimates for this. Instead, we present estimates of mismatch both including and excluding the lowest X values. When we exclude the lowest-X portion of our sample, we obtain estimates of the effect of affirmative action on the most qualified black students.

C. Methods

All of our analyses allow for an arbitrarily nonlinear relationship between an index of entering credentials (LSAT scores and undergraduate grade point averages) and potential outcomes. We accomplish this via a form of matching, re-weighting data from a comparison group so that the index distribution within that group matches that in the group of interest (DiNardo, Fortin, and Lemieux, 1996; Hahn, 1998; Hirano, Imbens, and Ridder, 2003).

Specifically, let $f_{b,s}(X)$ be the probability density of the admissions index among race-b students at type-s schools, and let $f_b(X)$ be the density of the index among all race-b students. Our first strategy involves comparisons between race-b students attending selective and unselective schools. We estimate the selective school effect on index-X students as the difference between the kernel mean outcomes of s=1 and s=0 students of race b. The average selective school effect on race-b students is the mean of this across the race-b index distribution, and is mechanically equal to the selective-non-selective difference in weighted means where the weighting factors are $f_b(X)/f_{b,s}(X)$.

Our second strategy uses white students to construct the counterfactual for black students. We present kernel mean outcomes as functions of admissions credentials for whites and blacks separately. These estimate $\bar{p}_0(X)$ and $\bar{p}_1(X)$, respectively; the difference between them estimates $\Delta\bar{p}(X)$. To compute the overall effect of affirmative action on black students' bar passage rates, we average $\Delta\bar{p}(X)$ across X values, with weights equal to the density of X among black students. Again, we implement this by reweighting, comparing the actual black mean outcome to the white weighted mean, using weights $f_1(X)/f_0(X)$ to obtain a white X distribution which matches that seen among black students. Our approach amounts to a decomposition of the black-white gap in overall bar passage rates into a component due to differences in X distributions and a remainder that we attribute to mismatch (see, e.g., DiNardo, Fortin, and Lemieux 1996 and Firpo, Fortin, and Lemieux 2005). An important advantage of this approach is that we do not require a perfect measure of the extent of admissions preferences enjoyed by black students; as we discuss below, we have only limited information about the selectivity of the school that students in our data attend.

We estimate all densities with a kernel density estimator, using an Epanechnikov kernel. There is sampling error in our density estimates that carries over into the reweighting factors. We use bootstrap resampling to estimate the distribution of our weighted means and of the differences among them, reestimating the densities, weighting factors, and weighted means in each bootstrap sample.

V. Data

Our data come from the Bar Passage Survey (BPS; Wightman 1998, 1999), conducted by the Law School Admissions Council (LSAC). The LSAC attempted to survey all students entering ABA-approved law schools in the fall of 1991. Survey responses were matched with administrative information from LSAC databases on students' undergraduate grade point averages (UGPAs) and Law School Admissions Test (LSAT) scores, and with information about law school and bar passage outcomes up until July 1996.

163 out of 172 accredited law schools agreed to participate in the BPS (Wightman 1999). Over 27,000 students from these schools responded to the survey and gave consent for the merging of their administrative records. The resulting data set contains information on about 62% of entering law school students from the relevant cohort. We discard all students who are neither black nor white, leaving an analysis sample of 24,479 students, of whom 7.6% are black. A subset of BPS respondents was chosen to receive follow-up questionnaires in their second and third years of law school.[11] Our sample for analyses of items from the third follow-up (which asks about law school GPA and about post-law-school employment plans) is 2,928.[12]

The BPS data are exceedingly rich, providing a reasonable approximation to a full cohort of law school students. However, our analysis is limited by a few decisions made by LSAC in its aggressive protection of respondents' privacy. Most importantly, the BPS data

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[11] Sampling probabilities for the follow-up survey depend on race, school cluster (see below), and school racial composition. Unfortunately, the BPS data contain no information about the latter, so do not permit construction of perfect inverse probability weights. The codebook recommends weighting by race, which would not yield a representative sample. We divide the data into race-school cluster cells, and weight the subsample data by the ratio of the size of the relevant cell in the full data set to that in the subsample. With these weights, the subsample is representative of black students but overrepresents white students from schools with a high fraction black within each cluster.

[12] The response rate for this follow-up survey, administered only to students who participated in the original study, was 66% overall and 54% for blacks.

do not report the actual law school that each respondent attended. Instead, law schools are grouped into six clusters. Clusters are intended to group "similar" schools along dimensions like size, cost, selectivity, tuition level, and minority representation. In practice, it is difficult to rank the clusters perfectly by selectivity, though two of the six clusters (which are labeled "Elite" and "Public Ivy" in the BPS data, though neither label is quite accurate; see Wightman, 1993) are clearly more selective than the others. In our analyses that compare schools of different selectivity levels, we characterize these two clusters as "selective" and the remaining clusters as "non-selective."

A second limitation of the BPS data relates to the GPA variables. Law schools conventionally adjust undergraduate GPAs to account for differences in grading standards between institutions. BPS respondents, however, report unadjusted UGPAs, and the BPS contains no information about the undergraduate institution. As a result, our measures of admissions qualifications are imperfectly related to those actually used in admissions. Oddly, the BPS measure of law school grade point average (LGPA) is limited in the opposite way: Only LGPAs standardized within law schools are reported. This severely limits the value of analyses that take LGPA as the outcome variable, though for completeness we present results for this variable. It is perhaps best to think of the LGPA variable as a measure of class rank rather than of absolute performance.

Finally, the variables characterizing students' outcomes on the bar exam are limited in two ways. First, state bar exams vary substantially in difficulty, but we do not observe in which state students took the exam, much less in which states the student considered taking it. Second, in 14 states where the bar association did not participate in the BPS data collection, successful attempts at the exam are observed—these are posted on public lists—but failed attempts are not. Estimates of the passage rates of students attempting the bar are thus upward-biased to an unknown extent. To avoid this bias, we ignore all information about failed attempts, treating as our primary outcome an indicator for whether the student was ever observed to pass the bar exam in any state during the 2.5 years for which data were collected. "Failures" on this outcome can thus represent students who chose careers that did not require admission to the bar, and who therefore did not take the exam, as well as those who took the exam and failed.[13]

Summary statistics for black and white students in the BPS data are reported in Table 1. The first several rows describe admissions credentials. LSAT scores in 1991 ranged from 11 to 48, with a mean of 33.3 and a standard deviation of about 7 among applicants. Mean scores of matriculants are somewhat higher, 36.8. Note the large gap between black and white students in the BPS, over 1.5 standard deviations in LSAT scores and somewhat smaller in UGPAs.[14] Black and white median LSAT scores are 28.5 and 37, respectively; 72% of blacks in the BPS have LSAT scores below the overall 20th percentile. We focus our analysis on an admissions index, defined as

(8) $$Index_i = 400 * \frac{UGPA_i}{4} + 600 * \frac{LSAT_i - 11}{48 - 11}.$$ [15]

The black-white gap in this index is nearly 1.7 standard deviations. We present most of our results in terms of percentile scores, computed from the admissions index using its distribution within the BPS sample. The average black BPS respondent is at the 15th percentile of the pooled distribution, and the average white is at the 54th percentile. 76

[13] A bit less than 16% of study participants (7% of those observed to graduate from law school) were not matched to any bar exam data, indicating either that they did not take the exam at all or that they failed the exam in a non-participating state.

[14] Among all 1991 law school applicants, this gap is 1.33 standard deviations, though the standard deviation is larger for this group; the absolute gap is slightly bigger among applicants than among BPS respondents. All statistics about applicants are computed from Table 3 of Wightman (1997).

[15] This index is taken from Sander (2004), who describes it as a close approximation to the weights used in law school admissions. A probit model for attendance at the most selective cluster of law schools yields nearly identical weights.

percent of blacks but only 13 percent of whites are below the pooled 20th percentile. Figure 1A displays the density of admissions index scores among black and among white students in the BPS, while Figure 1B displays cumulative distribution functions of the percentile scores for each group.

The second group of rows in Table 1 reports the distribution of students across the six "clusters" of law schools. We number clusters by their ranking in observed mean index scores, though other selectivity rankings are similar. The clusters have mean indices of 845, 780, 760, 706, 654, and 569, respectively. Figure 2 displays the fraction of white and of black students at each admissions index percentile who are observed attending law schools in cluster 1 (labeled "elite" in the BPS documentation) and cluster 2 ("public ivy"). These figures are consistent with large affirmative action preferences: Probit models for cluster 1 attendance or for attendance in either 1 or 2 that include the admissions index and race as explanatory variables indicate that black students have enrollment probabilities resembling those of white students with index scores nearly 170 points (1.5 standard deviations) higher. Of course, these figures incorporate the effects of any black-white differences in application behavior, admissions credentials—black students may have attended lower-quality colleges than whites with similar observable credentials—or matriculation decisions, as well as that of admissions preferences.

The final rows of Table 1 present summary statistics for our outcome measures. The first of these is the indicator for eventual bar passage, discussed above. Two outcomes that predate bar-taking are an indicator for whether the student graduated from law school and the law school GPA, LGPA, which as mentioned above is best thought of as a long-tailed measure of class rank. We also examine two employment-related outcomes that are available only for the subsample of BPS respondents targeted for the follow-up surveys. The first is an indicator for whether the "current work setting" as of 3.5 years after entering law school is a judicial clerkship (9.8% of respondents), an academic job (0.2%), a prosecutor's office (3.1%), a public defender's office (1.6%), or a large law firm (14.0%), which we take to be positive employment outcomes for lawyers.[16] The second is the annual salary.[17] Table 1 indicates that black students' average outcomes are significantly worse than those of whites on four of the five measures.

A. Selection into law school

An issue that will become important later in the analysis is the possibility of selectivity in which law school applicants ultimately matriculate. Figure 3 shows the fraction of white and black applicants for the 1991 entering cohort who appear in the BPS sample. The representation rate is notably lower for applicants with relatively poor credentials. Moreover, black applicants are substantially more likely to appear in the BPS data than are whites with similar credentials. The racial gap is particularly large in the bottom third of the credentials distribution, where black representation rates are roughly double those of whites.

This gap appears primarily to reflect differences in admissions policies. Only 56% of the 92,648 applicants from the BPS cohort were admitted to any law school (Barnes and Carr 1992; see also Wightman, 1997). Figure 4 shows the fraction of applicants who were admitted to at least one school, by entering credentials and race.[18] White students whose credentials would have placed them in the bottom third of the BPS distribution were more

[16] This variable is set to zero for lawyers in mid-size or smaller firms (20.8%), solo practice (2.1%), government (5.3%), public interest (1.8%), and several other categories. It is also set to zero for respondents reporting non-law-related work (6.0%), part-time employment (7.8%) or non-employment (22.0%).

[17] Salaries are reported in 8 bins (less than $20,000, $20-$30,000, …, $70-$80,000, and greater than $80,000). We assign each respondent to the midpoint of the relevant bin, using $15,000 for the lowest bin and $100,000 for the highest; respondents who are not employed full-time are excluded from the salary analyses.

[18] Barnes and Carr (1992) report the number of 1990-1 applicants in each of 90 LSAT-undergraduate GPA cells, separately by race, and the number of those who were admitted to at least one school. We assign to each cell the mean admission index percentile of BPS respondents in that cell.

likely to be rejected than to be accepted. Black admission rates were well above one half at all points above the fifth percentile, and are double or more those of similarly-qualified whites through much of the lower part of the distribution.

These differential admission rates suggest that comparisons between low-scoring black and white law students may be subject to sample selection bias. It is somewhat plausible that the distribution of latent outcomes is similar for white and black applicants with identical LSAT scores and undergraduate GPAs. But admissions offices have more information than this, and it seems likely that students who are admitted have better essays, recommendation letters, or other qualitative qualifications than do observably (to us) identical students who are rejected from all the schools to which they apply. If admissions offices distinguish among students on the basis of measures that are not reported in the BPS but are correlated with potential outcomes, and if these offices set a lower threshold for black than for white applicants—a necessary consequence if affirmative action is practiced by the schools to which marginal applicants apply—then white-black comparisons using the BPS data are biased upward by the differential sample selection apparent in Figures 3 and 4.

On the basis of Figure 3, it seems reasonable to assume that comparisons between races will not be unduly biased by selection at index values above about the 30^{th} percentile of the index distribution. Restricting our analysis in this way would come at a substantial cost, as this would exclude 85% of black students in the BPS sample. We choose a compromise threshold that risks limited selection bias in order to enlarge the sample size; estimates below use the upper 80% of the admissions index distribution.

B. Simple regression estimates

Table 2 presents simple regression versions of our two comparisons, first between students attending selective and unselective schools (columns A and B) and second between blacks and whites (columns C and D). Each specification controls for quadratics in LSATs and UGPAs and for an interaction between the LSAT score and the UGPA. The first columns indicate that students attending selective schools—proxied by the "elite" and "public ivy" clusters—do better than similarly-qualified students at less selective schools on all outcomes but law school grades, though the selective school effect is generally insignificant in the smaller black sample (and the point estimate is very slightly negative for bar passage in this sample). Column C indicates that black students graduate and pass the bar at lower rates than do whites with similar qualifications, and earn much lower GPAs in law school, though they are also more likely to get good jobs and they earn higher salaries. Column D restricts the sample to students with admissions qualifications above the bottom quintile. When this is done, the negative black effect on graduation and bar passage disappears, indicating that black underperformance is concentrated at the bottom of the qualifications distribution.

The models in Table 2 all include relatively sparse parameterizations of the relationship between students' entering qualifications and their eventual outcomes. If this is mis-specified, the selective school and black student effects are biased in an unknown direction. The next two sections present reweighting analyses that are robust to arbitrarily nonlinear relationships between the admissions index and outcomes. In Section VI, we explore the selective/unselective comparison, and in Section VII we turn to the black-white comparison.

VI. Results: Within-race comparisons across clusters

To implement our across-cluster comparisons, we treat clusters 1 and 2 as "selective" law schools and the remaining clusters as "unselective," acknowledging the substantial measurement error involved in this classification (Sander 2005b). Figure 5A displays the fraction of students passing the bar by race, admissions index percentile, and selectivity of school, while Figure 5B displays the selective-unselective gap by race and admissions index percentile.[19] These gaps are generally close to zero, and are positive—indicating that selective schools produce *higher* bar passage rates than unselective schools—at many points.

Integrating over the within-race distributions, we estimate that the black bar passage rate would be 3.9 percentage points lower if all blacks attended selective schools than if all attended unselective schools, though this estimate is insignificantly different from zero. For whites the corresponding figure is 1.1 percentage points higher, also insignificant. Table 3 presents this result in tabular form. Without accounting for differences in admissions qualifications, blacks attending selective schools graduate at a rate 15.6 percentage points higher than do blacks attending unselective schools. When the two subsamples are reweighted to have the same distribution of the admissions index as does the overall black sample, however, we obtain the -3.9 figure cited above. Thus, more than 100% of the raw selective-unselective gap in black bar passage rates can be attributed to differences in entering credentials.[20]

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[19] Graphs are trimmed at the 3rd and 97th percentile of the within-race or pooled distributions, whichever is more restrictive. Thus, for blacks the graphs are shown for percentile scores between 3 and 69; for whites, between 8 and 97.

[20] The choice of a "base" distribution for reweighting is analogous to the choice of a base group in a Oaxaca-Blinder decomposition (Oaxaca and Ransom, 1999). Our decision to use the overall admissions index distribution is akin to use of the grand mean of the X vector as the base for a Oaxaca-Blinder decomposition.

The remaining rows of Table 3 present identical analyses of other outcomes: Law school graduation, mean law school GPA, quality of the student's first job, and mean starting salary. The estimated selective-school effect is positive and significant for whites for three of the four outcomes. For blacks, only one estimate is positive and significant, and the "good job" coefficient is negative (but insignificant). Unsurprisingly, selective schools have large negative effects on LGPAs (class ranks) for both races, but these do not seem to carry over to the more cardinal outcomes.

Another way to express the results is as estimates of the impact of affirmative action on black students' outcomes. Figure 2 shows black and white students' probabilities of attending "selective" law schools at each admissions index percentile. Elimination of affirmative action can be approximated as forcing black students to the observed white profile. Table 4 presents estimates of the impact of this on black outcomes, predicated on the continued assumption that selectivity is uncorrelated with latent outcomes conditional on race and the admissions index. We estimate that the elimination of affirmative action would worsen black students' outcomes along every dimension but LGPA, in two cases significantly. The distinction between this result and the sometimes-negative effect of selective schools (on, e.g., bar passage) that we estimate in Table 3 arises from the concentration of affirmative action preferences at points in the index distribution where blacks experience positive selective-school effects.

VII.   Results: Comparisons between whites and blacks

Our second comparison is between white and black law school students with the same entering credentials, regardless of cluster. This comparison takes advantage of the fact that, due to affirmative action, blacks are likely to have the opportunity to attend more selective law schools than are whites with similar entering credentials, even if we cannot always observe selectivity.

Figure 6A shows average bar passage rates for whites and for blacks in the BPS data, and Figure 6B shows the white-black gap at each percentile of the admissions index distribution. Bar passage rates are an increasing function of the admissions index. In the upper four fifths of the index distribution, black students generally perform about as well as whites with similar credentials and sometimes outperform them, but in the lower quintile, black students substantially underperform whites. As indicated by Figure 1B, about three quarters of black law school students are in the bottom quintile of the overall distribution, so when we average the gaps depicted in Figure 6B weighting by the number of black students at each percentile the portion of the white-black gap in bar passage rates that cannot be attributed to differences in observables is positive and significant.

This is presented in the left-hand columns of Table 5, in the second row for bar passage: Over half of the raw gaps in bar passage and law school graduation are attributable to differences in the entering credentials of black and white students, with the remaining half appearing among students with the same credentials. A smaller portion of the LGPA (class rank) gap is attributable to observables, consistent with a larger mismatch effect on class rank than on other outcomes. When we turn our attention to employment outcomes, the raw black-white gaps are insignificantly different from zero, but the reweighted data indicate that blacks are more likely to obtain "good" jobs and earn higher salaries than are whites with similar credentials. This perhaps reflects affirmative action in hiring on top of that in admissions, but it certainly offers no evidence that mismatch is hurting black students' employment prospects.

As noted earlier, white-black comparisons may be biased by endogenous selection into the BPS sample at the lower end of the qualifications distribution. The three right-most columns of Table 5 present estimates of the white-black gap among students in the upper four-fifths of the qualifications distribution. Keep in mind that only a quarter of black students fall into this category. Nevertheless, the implied mismatch effect is small and not too imprecise: On average, black students in this subgroup pass the bar at rates only 1.9 (s.e. 2.0) percentage points lower than do similarly-qualified white students. The only indication of mismatch in this subsample is in the LGPA, where the mismatch effect appears even larger than it did in the full sample. This suggests that the sample restriction has not eliminated black-white gaps in admissions prospects, but that affirmative action does not appear to have the impacts on black students' outcomes—at least among relatively high scorers—that would be implied by the mismatch hypothesis.

The black-white analysis—which is likely biased toward overstating mismatch—thus indicates that affirmative action has zero or a small negative effect on the bar passage rates of black students in the top four quintiles. In the bottom quintile, the estimated negative effects are substantially larger, consistent either with large mismatch effects or with selection biases deriving from the omission from the sample of many white applicants not admitted to any law school.

To convert our estimates of $\Delta\bar{p}(X)$ into estimates of the mismatch function M(1, X), we divide by the affirmative action effect on attendance at selective schools. Because selectivity is in reality continuous rather than dichotomous, any such computation is necessarily a crude approximation. Nevertheless, the BPS' top two clusters are a reasonable proxy for selective schools. Figure 2 showed the black-white gap in enrollment at these schools, which we take as an estimate of $-\Delta\bar{s}(X)$. Dividing the gap in bar passage rates from Figure 6B by (the negative of) this affirmative action effect on admission yields our estimate of the mismatch function, M(1, X). This is graphed in Figure 7. As might be expected from the previous results, this mismatch function is large and negative in the bottom quartile of the qualifications distribution but is close to zero elsewhere.

The point estimates of M(1, X) are clearly incorrect, as they indicate that selective schools depress graduation rates of the least qualified students by more than 300 percentage points. This reflects limitations of our proxy for preferences, $\Delta\bar{s}(X)$. This proxy is particularly poor at the bottom of the X distribution, where few students are on the margin of admission to the most selective schools but there may still be dramatic affirmative action preferences at less selective schools. As an alternative, the dashed line in Figure 7 presents estimates that use the log-odds ratio,

$$\text{(9)} \qquad \ln\left(\frac{E[s_i \mid b_i = 1, X_i = X]}{1 - E[s_i \mid b_i = 1, X_i = X]} * \frac{1 - E[s_i \mid b_i = 0, X_i = X]}{E[s_i \mid b_i = 0, X_i = X]}\right),$$

in place of $\Delta\bar{s}(X)$ in the M(1, X) computation.[21] This has a similar pattern to the earlier estimates and a much more reasonable scale. The indicated effect of a one standard deviation increase in selectivity is now a sensible -30 percentage points at the very lowest X values. Above the 20^{th} percentile of the index distribution, the mismatch effect is generally smaller than five percentage points in absolute value and is as often positive as negative.

VIII. Conclusion

Absent a randomized experiment in which students are assigned to attend more or less selective colleges without regard to their entering credentials, we will never have

[21] This would be appropriate if true selectivity had an extreme value distribution conditional on (b, X), and if our selectivity proxy merely indicated whether true selectivity passed some threshold. In this case, M(b, X) is the the effect of a one standard deviation increase in true selectivity

completely convincing estimates of the causal effect of selective schools on students' outcomes.[22] As such an experiment is quite unlikely to be undertaken, researchers and policymakers must proceed on the basis of non-experimental analyses. These analyses can identify mismatch effects only via assumptions about counterfactual outcomes.

This paper has explored two sorts of assumptions, with different likely biases, using data on law students' graduation rates, bar exam passage rates, and early career employment outcomes. There are many analytical and substantive reasons to study mismatch in law schools, and the Bar Passage Survey data are nearly ideal for non-experimental analyses.

As in previous studies of mismatch at the undergraduate level (see, e.g., Loury and Garman 1995 and Kane 1998), we find that the data do not support claims—from, e.g., Sander (2004), using the same data and purporting to rely on a similar identifying assumption—that mismatch is an important consequence of affirmative action in law school admissions. We reject large mismatch effects on bar passage rates for all but the least qualified law school students. For students in the bottom quintile of the entering credentials distribution, the data are consistent with sizable mismatch effects on black bar passage rates, though the possibility of sample selection bias counsels against treating this as strong evidence.

We do find decisive evidence of mismatch effects throughout the entering credential distribution on academic performance in law school. Perhaps because law school curricula are poorly aligned with the bar exam, these effects do not carry over into bar passage rates. When we turn our attention to employment outcomes, we find that black students are much more likely to obtain good jobs than similarly-qualified white students, and that they obtain a 10-20% salary premium. Though this could reflect affirmative action in the legal job market

[22] Though see Dale and Krueger (2002) for quasi-experimental estimates.

as much as "reverse mismatch" (Alon and Tienda, forthcoming), it is not consistent with the claim that affirmative action in law school admissions damages the prospects of black applicants.

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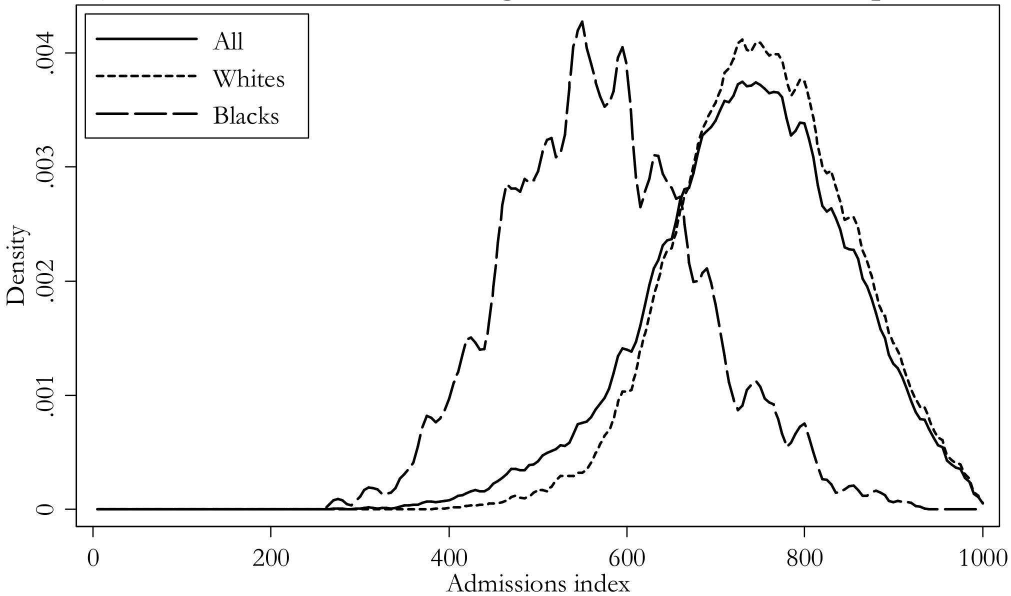
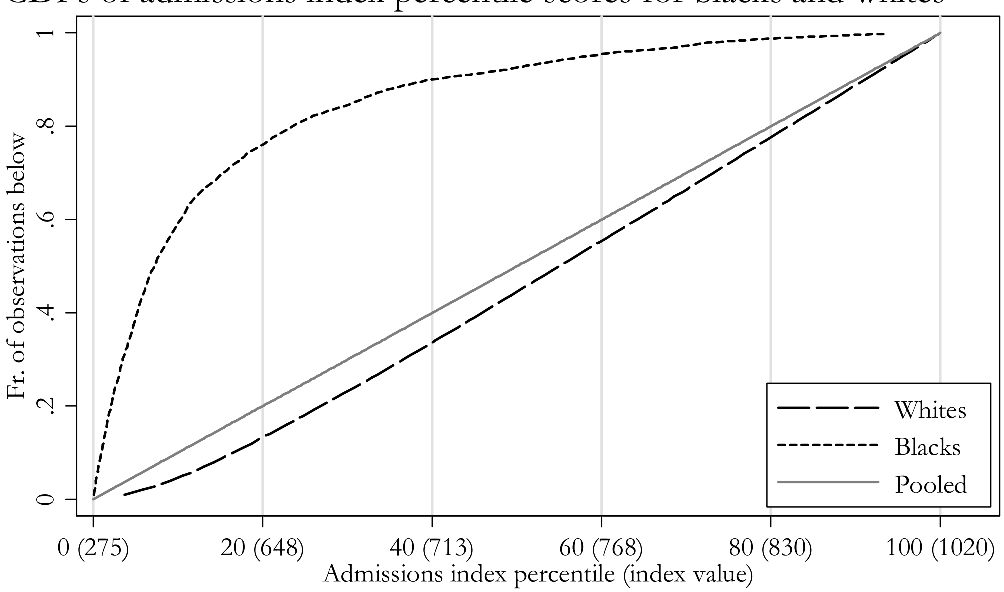
Figure 1A.
Density of admissions index among black and white BPS respondents



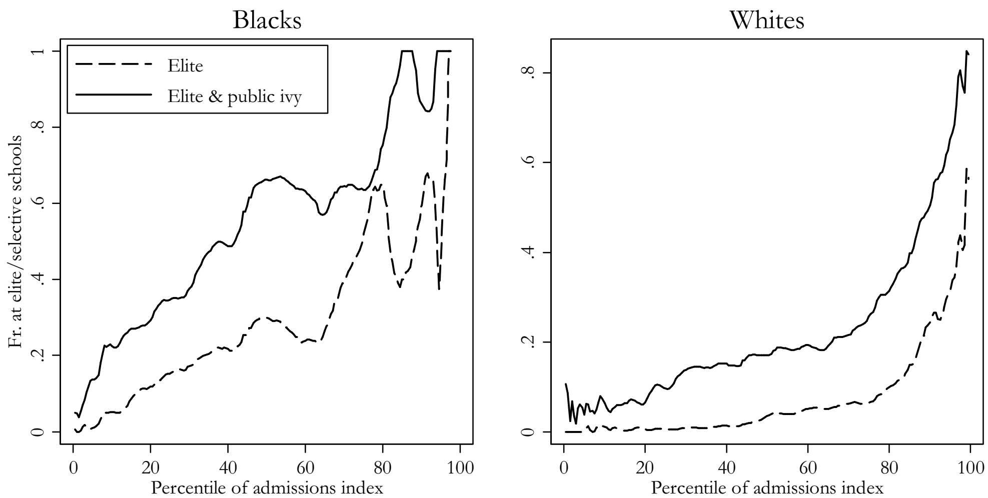
Note: Admissions index is 1000*((0.4*ugpa/4)+(0.6*(lsat-11)/(48-11))).

Figure 1B.
CDFs of admissions index percentile scores for blacks and whites



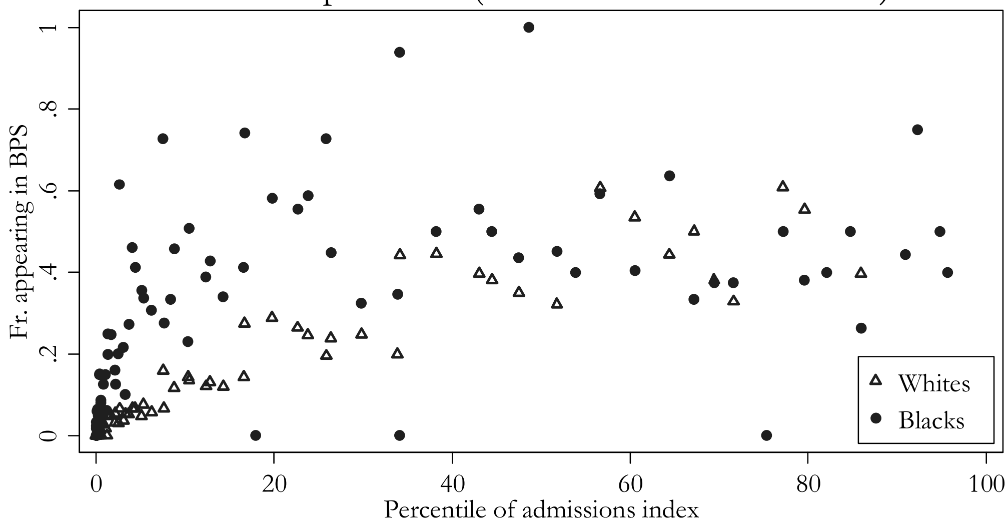
Note: Figure displays CDFs of the percentile scores--which are uniformly distributed in the pooled sample--for whites and blacks separately.

Figure 2.
Fraction of blacks and whites at "elite" and "public ivy" law schools, by percentile of admissions index



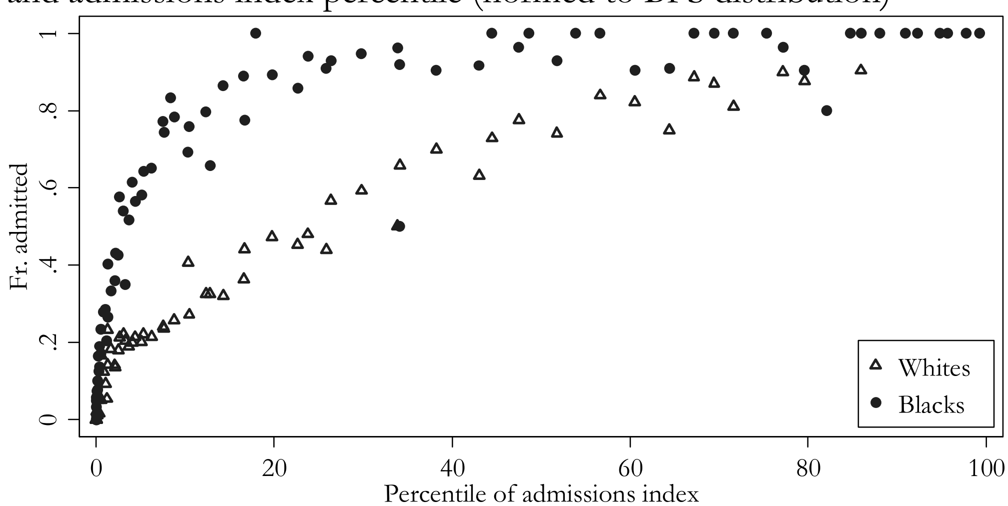
Note: Fractions are smoothed using a kernel mean smoother (with an Epanechnikov kernel and bandwidths of 4 points for whites and 8 points for blacks) applied to the underlying admissions index.

Figure 3.
Fraction of applicants appearing in BPS sample, by race and admissions index percentile (normed to BPS distribution)



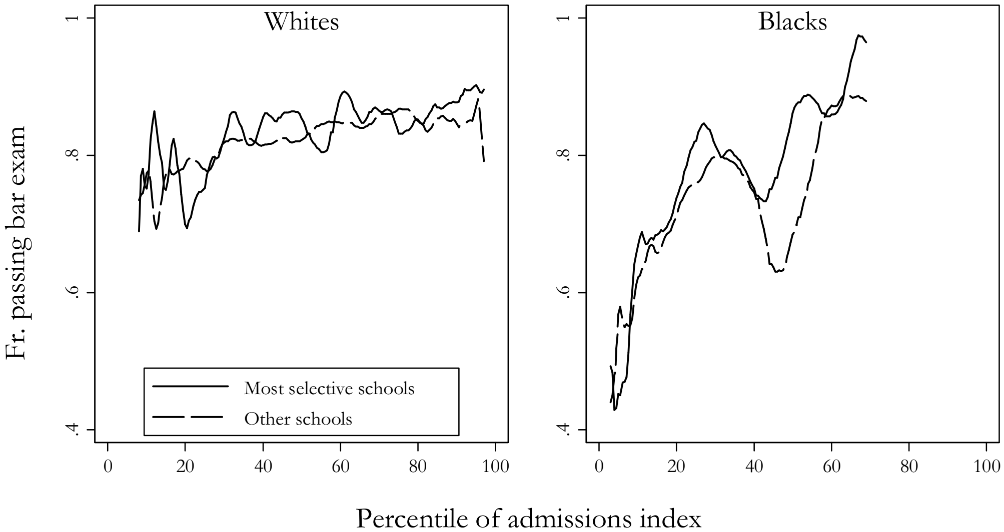
Each marker represents an LSAT-UGPA cell, and is assigned the mean admissions index percentile among BPS respondents in that cell. The clustering of cells at lower percentiles reflects the higher index distribution among matriculants (in the BPS) than among applicants.

Figure 4.
Admission rate of applicants, by race and admissions index percentile (normed to BPS distribution)



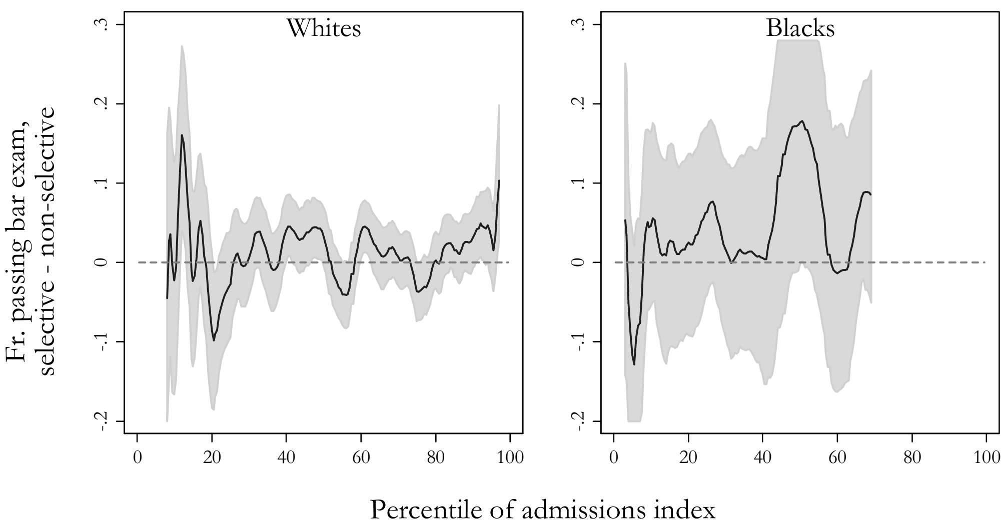
Notes: "Admission rate" is the fraction of applicants who were admitted to at least one school Each marker represents an LSAT-UGPA cell, and is assigned the mean admissions index percentile among BPS respondents in that cell. The clustering of cells at lower percentiles reflects the higher index distribution among matriculants (in the BPS) than among applicants.

Figure 5A.
Bar passage rates by race, law school selectivity, and admissions index



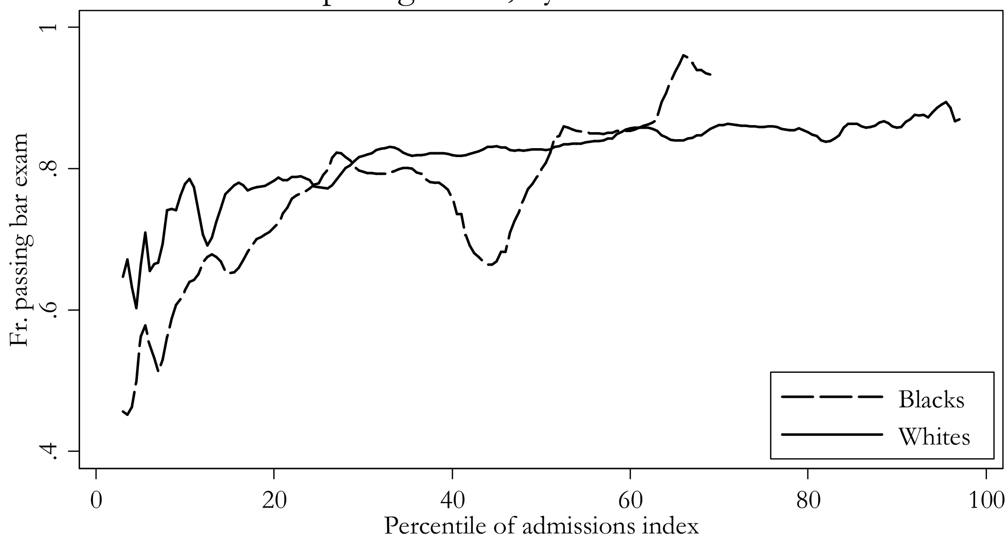
Note: Passage rates are smoothed using a kernel mean smoother (with an Epanechnikov kernel and bandwidths of 5 points for whites and 10 points for blacks) applied to the underlying admissions index. Estimates are not shown for percentile scores below the third percentile (overall or within race, whichever is greater) or above the 97th percentile.

Figure 5B.
Selective vs. non-selective differences in bar passage rates, by race and admissions index



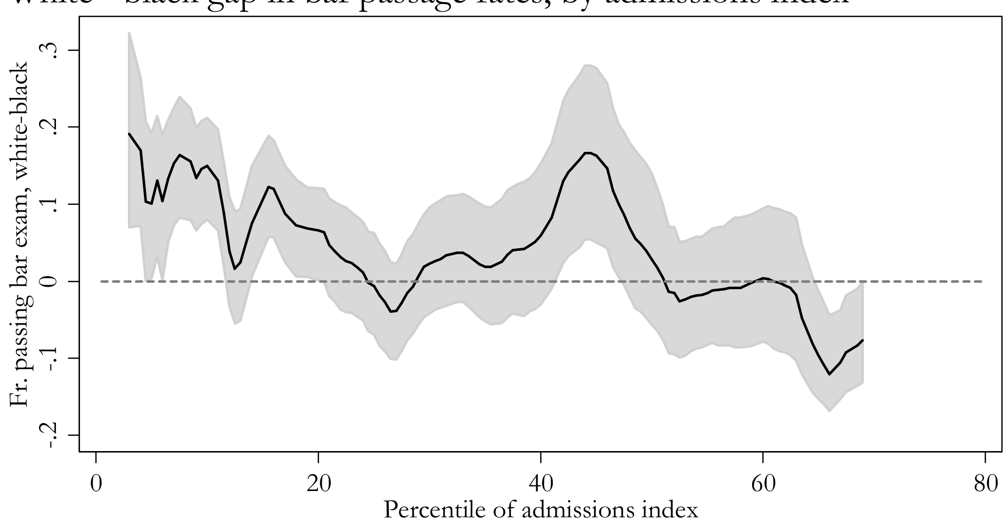
Note: Estimates are not shown for percentile scores below the third percentile (overall or within race, whichever is greater) or above the 97th percentile. 90% pointwise confidence intervals, indicated by shaded areas, are computed by drawing 500 bootstrap samples from the underlying data and re-estimating the gaps in these samples.

Figure 6A.
White and black bar passage rates, by admissions index



Note: Passage rates are smoothed using a kernel mean smoother (with an Epanechnikov kernel and bandwidths of 4 points for whites and 8 points for blacks) applied to the underlying admissions index. Estimates are shown for all values between the pooled 3 and 97 percentiles, except that black estimates are not shown above the black 97th percentile.

Figure 6B.
White - black gap in bar passage rates, by admissions index



Note: Passage rates are smoothed using a kernel mean smoother (with an Epanechnikov kernel and bandwidths of 4 index points for whites and 8 index points for blacks). Estimates are shown for all values above the pooled 3rd percentile and below the black 97th percentile. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing 500 bootstrap samples from the underlying data and re-estimating the gap in these samples.

Figure 7.
Estimates of mismatch effect on bar passage
using two measures of affirmative action preferences

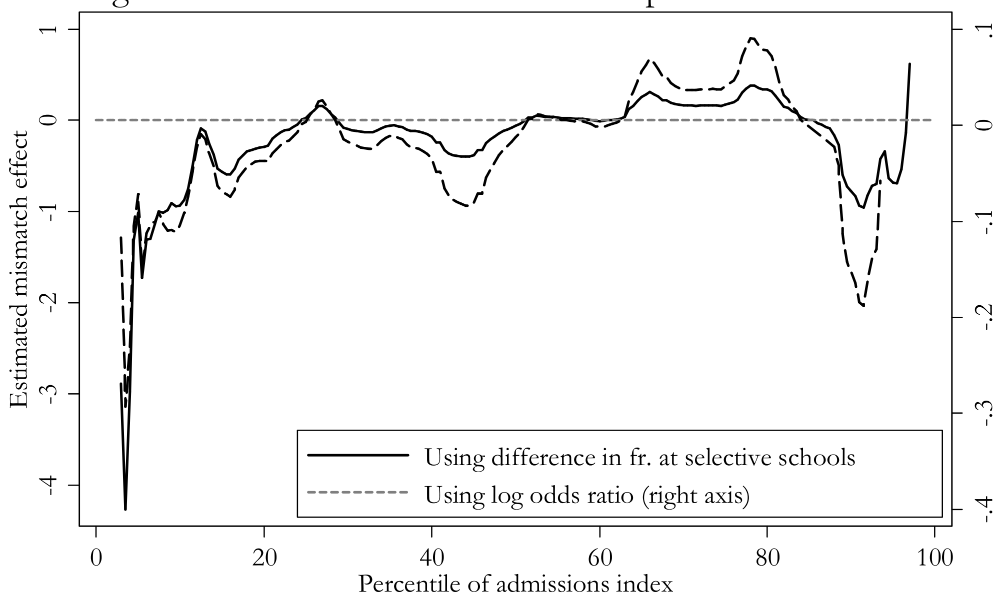


Table 1. Summary statistics for BPS sample

	Blacks & Whites		Blacks		Whites		Black-White	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Diff.	S.E.
	(A)	(B)	(C)	(D)	(E)	(F)	(G)	(H)
N	24,484		1,874		22,610			
Admissions credentials								
LSAT	36.8	5.6	28.7	6.0	37.4	5.0	-8.7	(0.1)
UGPA	3.23	0.42	2.87	0.43	3.26	0.40	-0.40	(0.01)
Admissions index	741	108	574	107	755	96	-180	(3)
Admissions index %ile	51.3	28.5	14.7	18.5	54.3	27.1	-39.6	(0.5)
Law school cluster								
1 ("Elite")	8.0%		8.0%		8.0%		0.0%	(0.7)
2 ("Public Ivy")	16.3%		14.9%		16.4%		-1.5%	(0.9)
3 ("2nd Tier Public")	27.8%		28.9%		27.7%		1.2%	(1.1)
4 ("2nd Tier Private")	36.3%		24.4%		37.3%		-13.0%	(1.0)
5 ("3rd Tier")	8.9%		5.9%		9.2%		-3.3%	(0.6)
6 ("Minority")	2.7%		18.0%		1.4%		16.5%	(0.9)
Outcomes								
Ever observed to pass bar exam?	80.5%		56.7%		82.5%		-25.8%	(1.2)
Graduated from law school?	90.2%		79.3%		91.1%		-11.8%	(1.0)
LGPA (stdized within school)	0.06	0.98	-1.02	0.91	0.13	0.94	-1.15	(0.03)
"Good" job	28.5%		29.6%		28.4%		1.1%	(1.9)
Annual salary ($1,000s)	$40.6	$18.1	$38.3	$18.9	$40.8	$18.1	-$2.4	(0.9)

Table 2. Simple regression estimates

	Approach 1: Selective vs. unselective		Approach 2: Black vs. white	
	Blacks	Whites	Full sample	Top 80%
	(A)	(B)	(C)	(D)
Panel A: Dependent variable is eventual bar passage (probit marginal effects)				
Selective	-0.006	**0.015**		
	(0.032)	(0.006)		
Black			**-0.073**	-0.015
			(0.012)	(0.018)
Panel B: Dependent variable is law school graduation (probit marginal effects)				
Selective	**0.053**	**0.032**		
	(0.024)	(0.004)		
Black			**-0.021**	0.007
			(0.008)	(0.012)
Panel C: Dependent variable is law school GPA (OLS coefficients)				
Selective	**-0.550**	**-0.196**		
	(0.061)	(0.017)		
Black			**-0.738**	**-0.829**
			(0.030)	(0.048)
Panel D: Dependent variable is indicator for a good job (probit marginal effects)				
Selective	0.030	**0.052**		
	(0.042)	(0.024)		
Black			**0.245**	**0.307**
			(0.028)	(0.037)
Panel E: Dependent variable is annual salary, in $1,000s (OLS coefficients)				
Selective	**9.8**	**6.3**		
	(1.9)	(1.1)		
Black			**6.3**	**7.6**
			(1.1)	(1.5)

Notes: All models include quadratics in LSAT and the undergraduate GPA and a linear LSAT-UGPA interaction.

Table 3. Analyses of selective/non-selective school differences

	Blacks			Whites		
	Non-selective	Selective	Difference	Non-selective	Selective	Difference
	(A)	(B)	(C)	(D)	(E)	(F)
N, full sample	1,408	414		16,782	5,417	
N, follow-up sample	639	217		1,623	707	
Percent ever observed passing bar exam						
Raw data	53.7%	69.3%	**15.6%**	81.8%	86.0%	**4.3%**
	(1.4)	(2.3)	(2.6)	(0.3)	(0.5)	(0.6)
Reweighted	56.6%	52.6%	-3.9%	82.3%	83.4%	1.1%
	(1.4)	(2.9)	(3.0)	(0.3)	(0.7)	(0.7)
Percent graduating from law school						
Raw data	76.8%	89.1%	**12.3%**	90.2%	94.9%	**4.6%**
	(1.1)	(1.5)	(1.9)	(0.2)	(0.3)	(0.4)
Reweighted	78.7%	82.6%	4.0%	90.6%	93.6%	**3.0%**
	(1.1)	(2.6)	(2.8)	(0.2)	(0.4)	(0.5)
LGPA						
Raw data	-0.94	-1.26	**-0.31**	0.13	0.16	**0.04**
	(0.03)	(0.05)	(0.06)	(0.01)	(0.01)	(0.02)
Reweighted	-0.87	-1.44	**-0.57**	0.21	0.04	**-0.17**
	(0.03)	(0.05)	(0.06)	(0.01)	(0.02)	(0.02)
"Good" job (follow-up subsample)						
Raw data	26.2%	41.2%	**15.0%**	23.5%	44.0%	**20.5%**
	(1.8)	(3.4)	(3.8)	(1.1)	(2.0)	(2.3)
Reweighted	29.4%	28.7%	-0.7%	25.7%	31.8%	**6.1%**
	(2.0)	(3.6)	(4.1)	(1.3)	(2.1)	(2.5)
Salary (follow-up subsample, in $1,000s)						
Raw data	34.9	47.3	**12.4**	37.6	49.7	**12.1**
	(0.8)	(1.7)	(1.9)	(0.5)	(0.9)	(1.0)
Reweighted	36.1	44.6	**8.4**	38.7	45.1	**6.5**
	(1.0)	(2.5)	(2.7)	(0.5)	(1.1)	(1.2)

Notes: "Selective" schools are those in the "elite" and "public ivy" clusters of the BPS; "non-selective" schools are all others. "Reweighted" estimates use data reweighted so that both the selective and non-selective school subsamples have the same admissions index distributions as the full sample (within-race). Samples exclude index points at which the density in one subsample is more than 200 times that in the other subsample. Standard errors in parentheses, estimated by bootstrap with 500 replications. Bold differences are significant at the 5% level.

Table 4. Implied effect of affirmative action on black bar passage from selective/non-selective comparison.

	Observed mean outcome among blacks	Mean outcome if blacks were distributed across clusters like whites	Implied effect of affirmative action
	(A)	**(B)**	**(C)**
Bar passage	57.7%	57.2%	+0.51%
	(1.2)	(1.3)	(0.52)
Law school graduation	80.0%	79.1%	**+0.87%**
	(0.9)	(1.0)	(0.39)
LGPA	-1.01	-0.92	**-0.095**
	(0.03)	(0.03)	(0.013)
"Good" job (follow-up subsample)	29.9%	29.5%	+0.40%
	(1.7)	(1.9)	(0.86)
Salary (follow-up subsample, in $1,000s)	38.3	36.8	**+1.48**
	(0.8)	(0.9)	(0.48)

Table 5: Analyses of white-black differences

	Full sample			Top 80%		
	Whites	**Blacks**	**B-W Difference**	**Whites**	**Blacks**	**B-W Difference**
	(A)	**(B)**	**(C)**	**(D)**	**(E)**	**(F)**
Percent ever observed passing bar exam						
Raw data	82.4%	58.1%	**-24.3%**	83.9%	80.0%	**-3.9%**
	(0.3)	(1.2)	(1.2)	(0.3)	(2.0)	(2.0)
Reweighted	69.3%	58.1%	**-11.2%**	81.9%	80.0%	-1.9%
	(1.2)	(1.2)	(1.7)	(0.4)	(2.0)	(2.0)
Percent graduating from law school						
Raw data	91.1%	80.2%	**-10.9%**	91.8%	91.4%	-0.5%
	(0.2)	(1.0)	(1.0)	(0.2)	(1.3)	(1.4)
Reweighted	84.7%	80.2%	**-4.6%**	90.6%	91.4%	0.8%
	(1.0)	(1.0)	(1.4)	(0.3)	(1.3)	(1.4)
LGPA						
Raw data	0.12	-1.01	**-1.13**	0.18	-0.78	**-0.96**
	(0.01)	(0.03)	(0.03)	(0.01)	(0.05)	(0.05)
Reweighted	-0.32	-1.01	**-0.69**	0.05	-0.78	**-0.83**
	(0.02)	(0.03)	(0.03)	(0.01)	(0.05)	(0.05)
"Good" job (follow-up subsample)						
Raw data	27.2%	30.3%	3.0%	28.8%	48.8%	**19.9%**
	(1.1)	(1.7)	(2.0)	(1.1)	(3.4)	(3.6)
Reweighted	17.3%	30.3%	**13.0%**	21.0%	48.8%	**27.8%**
	(2.5)	(1.7)	(3.1)	(1.4)	(3.4)	(3.6)
Salary (follow-up subsample, in $1,000s)						
Raw data	40.2	38.4	-1.9	41.2	44.8	**3.6**
	(0.5)	(0.8)	(0.9)	(0.5)	(1.5)	(1.6)
Reweighted	35.4	38.4	**2.9**	37.8	44.8	**7.0**
	(1.2)	(0.8)	(1.4)	(0.7)	(1.5)	(1.6)

Notes: "Reweighted" estimates use data in which white observations are reweighted to have the same admissions index distribution as black observations. Samples exclude index points at which the density in one subsample is more than 200 times that in the other subsample. Standard errors in parentheses, estimated by bootstrap with 500 replications. Bold differences are significant at the 5% level.