Mismatch in Law School[1]

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Abstract
An important criticism of race-based admissions preferences is that they may hurt minority students who are thereby induced to attend selective schools. We use two comparisons to identify so-called "mismatch" effects in law schools, with consistent results. There is no evidence of mismatch effects on graduation or bar passage rates of black students above the bottom quintile of the entering credentials distribution. The data are consistent with mismatch effects for bottom-quintile black students but do not demonstrate the importance of these effects, as sample selection bias is a potentially important confounding factor in this range. There is no evidence from any comparison of mismatch effects on employment outcomes.

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Abstract

An important criticism of race-based admissions preferences is that they may hurt minority students who are thereby induced to attend selective schools. We use two comparisons to identify so-called "mismatch" effects in law schools, with consistent results. There is no evidence of mismatch effects on graduation or bar passage rates of black students above the bottom quintile of the entering credentials distribution. The data are consistent with mismatch effects for bottom-quintile black students but do not demonstrate the importance of these effects, as sample selection bias is a potentially important confounding factor in this range. There is no evidence from any comparison of mismatch effects on employment outcomes.

I. Introduction

Most selective colleges in the United States give admissions preferences to black applicants. Favored students may thereby attend schools from which white applicants with identical credentials would be rejected. Critics have argued that these preferences bring in students who are inadequately prepared for the selective, competitive schools to which they gain access, and that the purported beneficiaries would do better—learn more, be more likely to graduate, avoid psychological damage, etc.—if they were admitted only to schools more appropriate for their qualifications (Summers, 1970; Sowell, 1978; Thernstrom and Thernstrom, 1997).[2] This might be characterized as a negative peer effect, at least for some students: A student's outcomes will decline if the average qualifications of her classmates rise too high above her own (Loury and Garman, 1995).

Admissions preferences broaden black students' choice sets, so could only cause harm to these students if they make sub-optimal choices. Economists might rightfully be suspicious of the so-called "mismatch hypothesis" for this reason. There is evidence from a variety of contexts (see, e.g., Dynarski and Scott-Clayton, 2006, and Avery and Kane, 2005), however, that college choices are not always characterized by rational decisions under full information. Many features of the enrollment decision differ from those likely to promote optimal decisions: The enrollment decision is a one-shot game for the student, so there is no opportunity for learning from past mistakes; information about the causal effects of attending a selective school is not readily available; and students may misattribute the large differences in student outcomes between selective and unselective schools that arise from

[2] The Supreme Court has held repeatedly that affirmative action's relevant benefits are through its effects on diversity of the educational environment, suggesting that effects on white students' outcomes are the most important. See, e.g., *Regents of Univ. of Cal. v. Bakke* 438 U.S. 265 (1978), and *Grutter v. Bollinger,* 539 U.S. 306 (2003). Another important effect is on the white students who are displaced from selective schools by less-qualified minority applicants. Neither of these is our focus here. See Arcidiacono (2005) for a more comprehensive analysis and Holzer and Neumark (2000) for a review of the voluminous relevant literatures.

differences in student quality to the causal effects of the schools themselves. Selective schools have no incentive to correct this sort of mistake, while unselective schools have little credibility.

Unfortunately, there is very little useful empirical evidence (Holzer and Neumark, 2000). The key challenge to identification of mismatch effects is the measurement of counterfactual outcomes. What would have happened to black students admitted to highly-selective schools had they not received admissions preferences?

Differences between graduation rates of black and white students at the same college are sometimes presented as indications of mismatch (Thernstrom and Thernstrom, 1997; Herrnstein and Murray, 1994; D'Souza, 1991). As Kane (1998) notes, these groups have dramatically different entering credentials, and gaps in outcomes may simply reflect this.

A better, though imperfect counterfactual for a black student at a highly selective school is another black student, with the same entering credentials, at a less selective school. Bowen and Bok (1998) find that minority students at elite colleges experience better long-run outcomes than do similarly qualified minority students at less selective colleges, suggesting that concern about mismatch is misplaced. But students who are admitted to selective schools may be better qualified on unobserved dimensions than those who are not, so this strategy may yield upward-biased estimates of the true selectivity effect.[3]

A final strategy uses white students with the same observable qualifications, irrespective of school, as the counterfactual for black students. This leverages affirmative action preferences to generate potentially exogenous variation in selectivity: If access to selective schools hurts student outcomes, black students should obtain worse outcomes than

[3] Dale and Krueger (2002) compare pairs of students admitted to the same schools but making different matriculation choices. They find small positive effects of selectivity on employment outcomes, but do not focus specifically on students who might be mismatched (because, for example, their credentials are atypical of the schools they attend).

do otherwise-similar whites. Using an indirect version of this strategy, Sander (2004) concludes that mismatch effects are substantial in law schools.[4]

In this paper, we use each of the latter strategies—comparisons between students at selective and unselective schools and between black and white students—to evaluate the mismatch hypothesis as it applies to law school admissions. At the outset, it is important to emphasize the limited overlap between the distributions of admissions credentials of black and white law students: In our data on law school matriculants, the 95th percentile black student is at only the 54th percentile of the white distribution of an index of student entering credentials—discussed in greater detail below—and the 5th percentile white student is at the 61st percentile of the black distribution. There are similarly dramatic differences between the credentials of students attending selective and unselective schools. For each of our strategies, we first present estimates that rely on parametric specifications of the effect of entering credentials on outcomes, then use semiparametric methods that estimate the effect of interest without functional form assumptions.

With homogenous treatment effects of school selectivity, mismatch could arise only if selective schools were bad for all students. It is thus most useful to think of the mismatch hypothesis as a claim about the treatment effect specific to the particular group of black students who are induced to attend selective schools by affirmative action preferences. Our black-white comparison can be seen as the reduced form for an instrumental variables (IV) estimator of the selectivity effect that uses student race as an instrument. Under the appropriate identifying assumptions, the estimated selectivity effect is local to the population of interest. By contrast, our selective-unselective comparison is an OLS estimator. We

[4] 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. We compare Sander's analysis and our own in an Appendix, available from the authors.

demonstrate below that a weighted difference between the OLS estimates for white and black students can provide an alternative estimate of the local average treatment effect (LATE) for black "compliers."

We expect that the identifying assumptions for both approaches are violated. The likely bias is in opposite directions for the two estimators: The between-selectivity comparison (OLS) is most likely biased upward relative to the true selectivity effect – so most likely understates mismatch – while the between-race (IV) comparison is most likely downward-biased. The results should therefore bracket the true effect.

Our comparison between more- and less-selective schools offers no indication of negative effects of selective schools on education outcomes. Although school selectivity lowers class rank for both black and white students, this mechanical effect does not translate into later outcomes: Students attending more selective schools are more likely graduate from law school, are equally likely to pass the bar exam—a requirement to work as a lawyer—and earn higher post-graduation salaries.

Results of our between-race comparison are more mixed. Black students attend dramatically more selective schools than whites with similar entering credentials. They are also significantly less likely to graduate or pass the bar exam, but obtain better jobs. Black graduation and bar exam underperformance is concentrated at the bottom of the credentials distribution; among students in the upper four quintiles blacks perform as well or better than whites on every outcome except class rank.

Although three quarters of black students in our sample fall in the bottom quintile, the black-white comparison is potentially biased in this range by endogenous selection into law school: Poorly qualified white applicants are much more likely to be admitted nowhere than are poorly qualified black applicants. If unobservably better prepared students are more likely to be admitted, differential sample selection could generate mean performance differences between admitted white and black students even in the absence of mismatch effects.

We interpret our results as demonstrating that there are no mismatch effects on the graduation and bar passage rates of the most qualified black students: Any mismatch is restricted to the lowest-scoring students, few of whom attend the most selective law schools. For these students, the data are consistent either with mismatch or with differential sample selection, so do not support strong conclusions. There is no indication of mismatch effects on any black employment outcomes, though the presence of affirmative action in the job market means that these results are not directly informative about mismatch effects on academic achievement.

In the next section, we discuss the mismatch hypothesis in greater detail. Part III introduces our identification strategies and our empirical approach. Part IV describes our Bar Passage Study (BPS) data. Part V examines race effects on the selectivity of the school attended, and documents large selectivity effects on the degree to which students are mismatched. Part VI presents results estimates of the selectivity effect on post-law-school outcomes, and Part VII concludes.

II. Defining Mismatch

Mismatch is explicitly about *relative* outcomes from different sorts of schools, not about the pure effect of enrollment. Half of law school entrants from the bottom quintile of the admissions qualifications distribution do not pass the bar exam within 5.5 years of entering law school, and it is plausible that students in this range should pursue other careers at the outset. Many poorly-qualified black students would not be admitted anywhere without affirmative action, so its elimination might benefit them by displacing them to more appropriate pursuits. Evaluation of this would require data on students' alternatives if they do not attend law school, as an applicant may prefer even a low probability of becoming a lawyer to her other options.[5] Though this would be interesting, it is not the focus of our study.

The simplest model that would generate mismatch effects among schools is an unreasonable one in which selective schools have negative treatment effects on *all* students, or at least on all students who attend them. A more reasonable model of mismatch requires heterogeneous treatment effects. The selectivity effects of interest for evaluation of the mismatch hypothesis are those on black students who attend selective schools only because they have access to affirmative action preferences; in other words, on black affirmative action *compliers*.

It is plausible that selective schools have negative effects on the least-qualified students even while they have positive effects on students with better preparation. There is even reason to think that selectivity effects may be more negative for black students than for similarly-qualified white students. While an underprepared white student may be able to avoid notice, the visibility of race may make this impossible for black students. Observers with incomplete information about particular students' ability may form lower estimates for members of groups that benefit from admissions preferences (Murray, 1994; Steele, 1990; Sowell, 2004).[6] The resulting low expectations may be self-fulfilling for black students, as so-called "stereotype threat" has been claimed to worsen black performance in contexts where racial performance differences are salient (Steele and Aronson, 1998). Finally, the

[5] There are many careers open to law school graduates that do not involve the practice of law, so do not require the bar exam, and students who never practice may nevertheless benefit from attending law school.

[6] Justice Thomas writes in his *Grutter* dissent (539 U.S. 306, 373) that "The majority of blacks are admitted to the [University of Michigan] Law School because of discrimination, and because of this policy all are tarred as undeserving. This problem of stigma does not depend on determinacy as to whether those stigmatized are actually the 'beneficiaries' of racial discrimination."

Socratic method of instruction, widely used in law schools, may aggravate these effects by focusing classroom attention on black students as representatives of their race (Guinier et al., 1994).

We adopt a potential outcomes framework (Imbens and Angrist, 1994; Angrist, Imbens, and Rubin, 1996). Let $b_i=1$ indicate that student i is black and $b_i=0$ indicate that she is white, and let X_i be a vector of her observed admissions qualifications. For simplicity, suppose that law schools come in only two types, more- (s=1) and less-selective (s=0).[7] Let $y_i(s)$ be the outcome that student i would obtain if he or she attended a school of type s. The treatment effect on student i of attending a selective school is defined as $\tau_i \equiv y_i(1) - y_i(0)$. We observe only $y_i = (1\text{-}s_i)\, y_i(0) + s_i\, y_i(1) = y_i(0) + s_i\, \tau_i$, however, where s_i is the type of school that the student actually attends.

Let $s_i(b)$ be the selectivity of the school that student i would have attended had she been treated as if she were race b, so that $s_i = s_i(b_i)$ is the observed selectivity and $s_i(1\text{-}b_i)$ is the counterfactual.[8] Then the effect of affirmative action on the type of school that a black student attends is $s_i(1) - s_i(0)$.

Differences between $s_i(1)$ and $s_i(0)$ need not arise directly from admissions decisions alone, as the school attended reflects as well a series of student decisions about where to apply and, once accepted at several schools, to matriculate. We follow Angrist, Imbens and Rubin (1996) and refer to those students with $s_i(1) = s_i(0) = 0$ – who would attend unselective schools regardless of their access to preferences – as "never takers;" those with

[7] In fact, selectivity is approximately continuous, but only discrete measures are available in our data. We discuss below the implications of this for our analysis.

[8] We have in mind the hypothetical experiment of making a single black student ineligible for preferences – treating her as white – while preserving the general admissions structure. An across-the-board elimination of affirmative action would likely increase $s_i=s_i(0)$ for at least some white students who are currently displaced from selective schools by affirmative action beneficiaries; it might also alter students' preference rankings across schools and/or change the environments that schools offer. All are peripheral to our investigation of the mismatch hypothesis, which in our reading is a claim about the *individual* effect of selectivity (Justice Thomas' statement, quoted in footnote 6, notwithstanding).

$s_i(1) = s_i(0) = 1$ as "always takers;" those with $s_i(1) = 1$ and $s_i(0) = 0$ – who are induced to attend selective schools by the availability of admissions preferences – as "compliers;" and those with $s_i(1) = 0$ and $s_i(0) = 1$ as "defiers." We denote the fraction of race-b, qualifications-X students in each group as $p^{nt}(b, X)$, $p^{at}(b, X)$, $p^{c}(b, X)$, and $p^{d}(b, X)$, respectively, and define $T^g(b, X)$ as the local average treatment effect (LATE)—the conditional mean of τ_i—in group g. We occasionally omit the dependence on X for notational simplicity, writing $p^g(b)$ and $T^g(b)$, though all of our analyses condition on observed admissions credentials; we discuss aggregation across X below.

The mismatch hypothesis is that selectivity has a net negative effect on the black students who are affected by the availability of preferences, or $T^c(1) \equiv E[\tau_i \mid b_i=1, s_i(1)> s_i(0)] < 0$.

III. Identification and Estimation

A. Two Comparisons

As noted earlier, we compare students of the same race and admissions qualifications at more- and less-selective schools and students of different races with the same qualifications irrespective of the school attended. We begin by discussing the assumptions under which each comparison provides consistent estimates of the average selectivity effect for a group of interest. The "pure" framework makes transparent the direction and magnitude of the biases produced by likely violations of these assumptions. We argue that the selective-unselective comparison most likely overstates the effect of selective schools (i.e. understates mismatch), while the black-white comparison is more likely biased against selective schools (so overstates mismatch).

1. *Within-race, between-school comparisons*

Write the mean of y conditional on race, qualifications, and selectivity as

(1) $Y(b, X, s) \equiv E[y_i \mid b_i = b, X_i = X, s_i = s]$

$= E[y_i(0) \mid b_i = b, X_i = X, s_i = s] + s\, E[\tau_i \mid b_i = b, X_i = X, s_i = s].$

and the selective-unselective difference in means, conditional on (b, X), as

(2) $D_s(b, X) \equiv Y(b, X, 1) - Y(b, X, 0)$

$= (E[y(0) \mid b, X, s = 1] - E[y(0) \mid b, X, s = 0]) + E[\tau \mid b, X, s = 1].$

The exclusion restriction under which this identifies an interesting parameter is that students seen at selective schools would have obtained the same outcomes, had they instead attended unselective schools, as did the students who in fact attended those schools:

Assumption (i). $E[y(0) \mid b, X, s = 1] = E[y(0) \mid b, X, s = 0]$.

$D_s(b, X)$ then identifies the average effect of selectivity on the treated:

(3) $D_s(b, X) = E[\tau \mid b, X, s = 1]$.[9]

Assumption (i) is likely violated: Law school admissions offices can observe student qualifications that are not reported in our data sets but that may be correlated with potential outcomes. If $y_i(0)$ is positively correlated with s_i conditional on b_i and X_i, $D_s(b, X) > E[\tau \mid b, X, s = 1]$ and we would overstate the effect of treatment on the treated. Law school admissions probabilities depend heavily on variables – LSAT scores and undergraduate grades – that we can include in X, so we expect that the difference is relatively small.[10] If so, estimates of the selectivity effect obtained from this comparison will provide a reasonably tight upper bound for the true selectivity effect.

[9] Note that this is an average of mean treatment effects for several groups (always-takers and compliers when b=1, and always-takers and defiers when b=0), weighted by the size of each. We discuss below how $D_s(1, X)$ and $D_s(0, X)$ can be combined to obtain an estimate of $T^c(1, X)$.

[10] A supplemental analysis, available from the authors, demonstrates that LSAT scores and undergraduate GPAs are several times better at predicting admission to law school than are SAT scores and high school GPAs for college admissions, even at the most selective colleges.

2. Between-race comparisons

Our second comparison is between black and white students with the same credentials, regardless of the school attended. Define $Y(b, X) \equiv E[y_i \mid b_i = b, X_i = X]$. We begin with two assumptions that enable us to identify the complier share. Suppressing the conditioning of each on X,

<u>Assumption (ii).</u> $p^d(0) = 0$.

<u>Assumption (iii).</u> $p^{at}(0) = p^{at}(1)$.

(ii) is the usual "monotonicity" assumption, and says simply that there are no white students at selective schools who would have attended unselective schools had they been eligible for preferences. With this, we can estimate $p^{at}(0)$ as the mean of s_i among whites; by (iii), this estimates $p^{at}(1)$ as well. The mean of s_i among blacks estimates $p^{at}(1)+p^c(1)$, so $p^c(1)$ can be obtained as the black-white difference in s.

The two remaining assumptions relate to potential outcomes:

<u>Assumption (iv).</u> $E[y_i(0) \mid b_i = 1] = E[y_i(0) \mid b_i = 0]$,

<u>Assumption (v).</u> $T^{at}(1) = T^{at}(0)$,

These state that the instrument is excludable: Black and white students would achieve the same average outcomes if all attended unselective schools, and those black and white students who are "always takers" –who would attend selective schools with or without admissions preferences – receive the same average benefits from selectivity.

Continuing to suppress dependence on X, the average outcome for race-b students is $Y(b) = E[\, y_i \mid b_i = b]$. Let $D_b \equiv Y(1) - Y(0)$ be the black-white difference. Then

$$(4)\qquad D_b = (E[y_i(0) \mid b_i = 1] - E[y_i(0) \mid b_i = 0]) + (E[\, s_i\, \tau_i \mid b_i = 1] - E[\, s_i\, \tau_i \mid b_i = 0])$$

$$= (E[y_i(0) \mid b_i = 1] - E[y_i(0) \mid b_i = 0])$$

$$+\ p^{at}(1)T^{at}(1) + p^c(1)*T^c(1) - p^{at}(0)T^{at}(0) - p^d(0)T^d(0).$$

Under assumptions (ii) through (v), $p^d(0)T^d(0) = 0$; $(E[y_i(0) \mid b_i = 1] - E[y_i(0) \mid b_i = 0]) = 0$; and $p^{at}(1)T^{at}(1) - p^{at}(0)T^{at}(0) = 0$. We are thus left with $D_b = p^c(1)T^c(1)$, the LATE for affirmative action compliers multiplied by the complier share.

It is also worth considering what can be obtained under lesser assumptions. Assumptions (ii) and (iii) are relatively innocuous.[11] Using just these, we can write

$$D_b = p^c(1)*T^c(1) + p^{at}(0)(T^{at}(1) - T^{at}(0)) + (E[y_i(0) \mid b_i = 1] - E[y_i(0) \mid b_i = 0]). \tag{5}$$

We focus first on the final term of (5). Research on the prediction of college grades (e.g. Rothstein, 2004; Young, 2001) generally indicates that white college students outperform black students with the same admissions credentials at the same colleges. If the same pattern holds in law schools—as is indicated by Wightman (2000), Wightman and Muller (1990), Anthony and Liu (2003), and Powers (1977)—$E[y_i(0) \mid b_i = 1] - E[y_i(0) \mid b_i = 0] < 0$. The sign of $T^{at}(1) - T^{at}(0)$ is less clear, but probably also negative: If black students indeed underperform white students with the same credentials and if τ_i tends to increase with $y_i(0)$, it follows that $E[\tau \mid b=1, X] < E[\tau \mid b=0, X]$, and it seems plausible that this would hold as well among always-takers. We should therefore expect that $D_b < p^c(1)*T^c(1)$, so $D_b/p^c(1)$ understates the true local selectivity effect and thus overstates mismatch.

There is another important source of bias at the lowest X values. Potential law students who did not actually attend are not in our sample. We show below that low-X applicants are quite likely to receive zero admissions offers, particularly when they are white. Those low-X students who are admitted to some law school likely have better unobserved

[11] Under statistical discrimination, affirmative action policies may reduce the signaling value of a selective school diploma for black students who could be admitted even without preferences. This suggests the possibility that (ii) is violated: Such a student might obtain a larger τ if there are no preferences than if preferences are given to some students. This may lead her to turn down admission to a selective school in the latter case although she would not do so in the former. It seems likely, nevertheless, that $p^d(0, X)/p^c(1, X)$ is small; this implies that any bias from neglecting defiers is small.

qualifications and higher $y_i(0)$s than do those who are rejected everywhere. If so, $E[y_i(0) \mid b_i, X_i=X]$ is higher in the sample than in the population. Because the selection rate is so much higher for blacks than for whites, this should hold particularly for whites, leading to downward bias in $E[y_i(0) \mid b_i=1, X_i=X] - E[y_i(0) \mid b_i=0, X_i=X]$ in the sample relative to the population. We do not attempt to correct our estimates for this. Instead, we present estimates of mismatch both including and excluding the lowest X values, where differential sample selection is most severe. The latter specifications estimate an effect local to the most qualified black compliers. Discussions of mismatch frequently cite the competitive pressure at the most selective schools—which enroll very few students of any race from the lowest reaches of the X distribution—as a source of the effect, so we might expect mismatch effects to be important even for relatively highly qualified students.

As we discuss below, we have only an imperfect proxy for the selectivity of the school attended. An important advantage of between-race comparison is that we can estimate the net effect of affirmative action on mean black outcomes—$p^c(1,X)T^c(1,X)$ or its integral over the black X distribution—without any reference to a selectivity measure.

3. Difference-in-differences

Although it seems natural to combine the two approaches, differencing both across type of school and between race, this does not identify the LATE of interest. Recall (2):

(6) $$D_s(b, X) = (E[y(0) \mid b, X, s = 1] - E[y(0) \mid b, X, s = 0]) + E[\tau \mid b, X, s = 1].$$

Once again suppressing X, the difference-in-differences estimator is

(7) $$\begin{aligned} D_s(1) - D_s(0) = {} & [(E[y_i(0) \mid b_i=1, s_i = 1] - E[y_i(0) \mid b_i=1, s_i = 0]) \\ & - (E[y_i(0) \mid b_i=0, s_i = 1] - E[y_i(0) \mid b_i=0, s_i = 0])] \\ & + E[\tau_i \mid b_i=1, s_i = 1] - E[\tau_i \mid b_i=0, s_i = 1]. \end{aligned}$$

The exclusion restriction that makes the terms involving $y_i(0)$ disappear is weaker than assumption (i). With assumption (ii) through (v), we can write the remaining portion as

$$\text{(8)} \qquad \mathrm{E}[\tau_i | b_i = 1, s_i = 1] - \mathrm{E}[\tau_i | b_i = 0, s_i = 1] = \frac{p^c(1)}{p^c(1) + p^{at}(1)}\left(T^c(1) - T^{at}(1)\right).$$

That is, even with strong assumptions difference-in-differences identifies only the *difference* between the LATEs for black compliers and always-takers.

There is, however, a quasi-difference-in-differences approach that allows us to use the across-selectivity comparison to estimate the LATE for black compliers. Under assumptions (i) through (v),

$$\text{(9)} \qquad D_s(1) = \frac{p^{at}(1)}{p^c(1) + p^{at}(1)} T^{at}(1) + \frac{p^c(1)}{p^c(1) + p^{at}(1)} T^c(1) \text{ and}$$

$$\text{(10)} \qquad D_s(0) = T^{at}(0) = T^{at}(1).$$

We can therefore estimate $T^c(1)$ as a quasi-difference:

$$\text{(11)} \qquad T^c(1) = D_s(1) + \frac{p^{at}(1)}{p^c(1)}\left(D_s(1) - D_s(0)\right).$$

We present estimates of $T^c(1)$ from this quasi-differenced approach in Section VI.D.

B. Methods

The development thus far conditions on a vector of observable admissions qualifications, X. We take two approaches to this. First, we estimate regression models that assume a constant treatment effect and a specific functional form for the relationship between $y_i(0)$ and X. Second, we use kernel regression techniques to allow for a non-parametric relationship between outcomes and a unidimensional index formed from the X variables.[12] These yield estimates of the differences of interest as functions of the index. To obtain estimates of unconditional average effects, we reweight the data so that the index distribution in the comparison group matches that in the treatment group, then compare means in the reweighted data (DiNardo, Fortin, and Lemieux, 1996; Barsky et al., 2002).

1. Regression estimates

For the selective-unselective comparison, we estimate

(12) $y_{ib} = Z_{ib}\,\beta_b + s_{ib}\,\tau_b + \varepsilon_{ib}$,

where Z_{ib} contains the elements of a second-order polynomial in the elements of X. For the black-white comparison, we use pooled data from both races to estimate:

(13) $y_i = Z_i\,\beta + b_i\,\theta + \varepsilon_i$.

These approaches require strong regularity assumptions. Both require, for example, that τ_i not vary with X_i. If these assumptions are satisfied, however, $\hat{\tau}_b$ estimates the effect of the treatment on the treated and $\hat{\theta}$ estimates the average treatment effect among compliers multiplied by the black compliance rate.

2. Nonparametric estimates

Regression estimates are biased if $E[y_i(0) \mid X_i]$ cannot be absorbed by the terms in Z, and in any case are uninformative about any variation of selectivity effects with X. We use kernel-weighted locally linear regressions to obtain nonparametric estimates of $E[y_i \mid Z_i, b_i, s_i]$ (for the between-schools comparison) and $E[y_i \mid Z_i, b_i]$ (for the between-race comparison), where Z is now our univariate admissions index.[13] Given these estimates, it is straightforward to estimate the treatment effects:

[12] The decision to focus on an index of X rather than on each of the X elements individually—we use two, the LSAT score and the undergraduate GPA—is made for ease of graphical presentation. We present specification checks below that drop this single-index assumption, with essentially identical results.

[13] We use an Epanechnikov kernel and a rule-of-thumb approximation to the optimal bandwidth (Fan and Gijbels, 1996, p. 111).

(14) $E[\tau \mid Z, b, s=1] = E[y \mid Z, b, s=1] - E[y \mid Z, b, s=0], = D_s(b, X)$

or, in the case of the between-race comparison, the product of the treatment effects with the compliance rate:

(15) $T^c(1, Z)*p^c(1, Z) = E[y \mid Z, b=1] - E[y \mid Z, b=0] = D_b(X)$.

We form confidence intervals for the nonparametric estimates by bootstrap resampling, drawing 500 samples (with replacement) from the original data, re-estimating the treatment effect functions in each sample, and taking pointwise quantiles of the bootstrap distributions. That is, our estimated 90% confidence interval for the mean difference between groups for students with $Z=Z^0$ ranges from the 5th to the 95th percentile of the Z^0 estimate across bootstrap samples.

3. Reweighting estimates

To convert these estimates to average treatment effects for a population of interest, we need to integrate over the appropriate Z distribution. We adopt the re-weighting approach used by Barsky et al. (2002; see also DiNardo, Fortin, and Lemieux, 1996; Hahn, 1998; and Hirano, Imbens, and Ridder, 2003): If $\varphi(Z, 1)$ and $\varphi(Z, 0)$ are the densities of Z in the treatment and comparison groups, we reweight comparison observations by $\varphi(Z, 1)/\varphi(Z, 0)$.[14] In the re-weighted data, the Z distribution is the same in the comparison as in the treatment group, and the simple difference in (reweighted) means estimates the average treatment effect for the treatment group.[15]

[14] Both densities are estimated via a kernel density estimator with an Epanechnikov kernel and using Fan and Gijbel's (1996, p. 47) "normal reference bandwidth," $h=2.34\hat{\sigma} n^{-1/5}$, using a common $\hat{\sigma}$ for both subsamples. We restrict the sample to Z values in the common support of the treatment and comparison groups, defining this to establish a reasonable upper bound on the reweighting factors.

[15] This is equivalent to a propensity score (Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002) estimator in which the propensity score is a nonparametric function of Z. It is also a non-parametric analogue to the Oaxaca-Blinder decomposition (Oaxaca and Ransom, 1999) for linear models, dividing the between-group difference in outcomes into a portion due to differences in Z distributions (means in the linear case) and a remainder conditional on Z (DiNardo, Fortin, and Lemieux, 1996; Barsky et al., 2002).

For the selective-unselective comparison, "treatment" is selectivity, so the difference in re-weighted means—essentially a matching estimator—then estimates the average selectivity effect on students attending selective schools (Angrist 1998): $E[\tau | b, s=1] = \int E[\tau | Z, b, s=1] \varphi(Z,1) dZ$. For the black-white comparison, blacks are the "treated" group, so we estimate the average black effect on black students. Note that under the assumptions outlined above, the LATE for black compliers is

$$
\begin{aligned}
\mathrm{E}[\tau_i \mid b_i = 1, \text{i is a complier}] &= \int \mathrm{T}^c(1,Z)\phi(Z \mid b=1, \mathit{complier})dZ \\
&= \frac{\int \mathrm{T}^c(1,Z)\Pr\{\mathit{complier} \mid b=1, Z\}\phi(Z \mid b=1)dZ}{\int \Pr\{\mathit{complier} \mid b=1, Z\}\phi(Z \mid b=1)dZ} \\
&= \frac{\int D_b(Z)\phi(Z \mid b=1)dZ}{\int p^c(1,Z)\phi(Z \mid b=1)dZ}
\end{aligned}
\tag{16}
$$

The ratio of the average of $D_b(Z)$ over the black distribution to the similarly-averaged complier share thus yields the LATE of interest. We again use bootstrap resampling to account for sampling variation in both the reweighting factors and the reweighted means.

IV. Data

Our data come from the Law School Admission Council's (LSAC) Bar Passage Study (BPS; Wightman 1998, 1999), which attempted to survey all students entering accredited law schools in the fall of 1991. Survey responses were matched with administrative records on admissions qualifications and academic progress, and with information from state bar associations about bar passage outcomes through July 1996.

The BPS contains information on over 27,000 students, about 62% of the 1991 cohort.[16] We focus on the 24,051 black and white students with valid data on entering

[16] Most non-response was individual: 163 of 172 accredited law schools participated in the study. The estimated response rates for blacks and whites were 59% and 61%, respectively (Wightman 1999). We have found no indication that non-response differed systematically by entering credentials.

credentials, of whom 7.6% are black. Students were followed through law school and administrative data on bar exam outcomes were collected from state bar associations. A subset of BPS respondents was given a follow-up survey in late 1994, four to six months after students' scheduled graduation, with questions about employment and salary. As non-graduates had low response rates to the follow-up survey, we limit our analyses of employment outcomes to graduates. The sample size for these analyses is 3,174. A more detailed discussion of the sample and variable construction is in the Data Appendix.

Summary statistics for the BPS sample are reported in the first two columns of Table 1. The remaining columns present means by race (columns 3-4) and by race and selectivity (5-8), trimming the samples as described below to ensure overlap in the credentials distributions of the black and white or selective and unselective samples.

The BPS masks the identity of the law schools that students attend, grouping them into six "clusters" of schools that are similar along dimensions like size, cost, selectivity, tuition level, and minority representation. 24% of respondents are in two clusters that contain the most selective schools, one third in the so-called "Elite" cluster, the most selective, and the rest in the "Public Ivy" cluster (Wightman 1993). We treat these as highly selective, though our results are robust to using just the first. Table 1 indicates very little difference between whites and blacks in the fraction attending schools in either cluster.

The third panel of Table 1 reports statistics for our two X variables, the LSAT score and the undergraduate grade point average (UGPA). LSAT scores ranged from 10 to 48 in 1991, with mean 36.8 and standard deviation 5.5 among BPS respondents. The undergraduate GPA, computed from student transcripts, ranges from 0 to 4.0. The BPS contains no information about the college attended, so GPAs are not adjusted for the difficulty of the undergraduate curriculum.[17] The black-white gaps in LSAT scores and UGPAs in our trimmed sample are about 1.2 and 0.75 standard deviations, respectively, and are somewhat larger in the full sample.

As mentioned above, our nonparametric analyses use an index of these two admissions variables. We first standardize each to range from 0 to 1000, then average them with weight 0.4 on the GPA and 0.6 on the LSAT.[18] The black-white gap in this index is 1.3 standard deviations (in the trimmed sample). We present most of our results in terms of admissions index percentile scores. Average percentile scores for blacks and whites are 16.8 and 50.6, respectively. Within each race, students at selective schools have much better credentials than students at less-selective schools.

Figure 1A displays the admissions index density among black and white students in the BPS, and Figure 1B displays the cumulative distribution of percentile scores for each group. Three quarters of black matriculants are in the bottom quintile of the index distribution. There are few whites with index scores like those seen in the bottom quintile of the black sample, and few blacks with scores in the top decile of the white distribution. Applying our reweighting strategy to the full data set would therefore mean giving very large weights to the few observations on low-scoring whites. To minimize this, we trim the sample at the points indicated by the vertical lines in Figures 1A and 1B. Once the sample is so restricted, the maximum reweighting factor needed to make the white index distribution

[17] Surprisingly, most law school admissions offices do not seem to take account of the undergraduate institution in their evaluation of undergraduate GPAs (Hagle, undated).

[18] These weights are taken from Sander (2004), who describes them as a close approximation to those used in law school admissions. A probit model for attendance at the most selective cluster of law schools yields nearly identical weights, as does a model predicting law school GPAs within clusters. None of our results are sensitive to the substitution of indices based on weights from either of these models, and we also present results below that use the full bivariate distribution of LSATs and undergraduate GPAs.

resemble that for blacks is 30, and the 95th percentile is 3.5. Columns 3 and 4 of Table 1 report means for the trimmed sample.[19]

The final panel of Table 1 reports summary statistics for several outcome measures. The first two rows describe academic performance during the first year of law school, when curricula are typically standardized and grades issued on strict curves.[20] The first row reports the grade point average at the end of that year; in the BPS, this is standardized within law schools. The second row converts this to a class rank measure, under the assumption that GPAs are normally distributed within each law school. The average black is at the 24th percentile of her class and the average white at the 53rd percentile.

We next consider measures of attainment. The third row reports the fraction (91%) of students who graduate from law school. Our second indication of attainment is passage of the bar exam, which is required in order to practice law.[21] As exams are blind-graded, passage is not directly dependent on race or school quality. We use two measures of the bar exam outcome that differ in the treatment of the 7% of graduates who do not attempt the bar exam. As some law school graduates choose not to practice law—so need not take the exam—it is not clear whether a non-taker should be counted as a failure. Our first measure does so, while our second omits graduates who did not attempt the exam.[22] In each case,

[19] We perform a similar trimming exercise for our selective-unselective comparison, and report trimmed means in columns 5-8 of Table 1. In the trimmed sample, the maximum weight needed to make the unselective sample resemble that at selective schools is 13, and the 95th percentile is 3.

[20] First year grades are available for nearly all students, even those who drop out after their first year, and are important determinants of access to prestigious internships and post-graduation clerkships. Analyses of cumulative grades yield similar results in any case.

[21] The exam's difficulty varies across states, and graduates need only take the exam in the state where they hope to practice. Our measures capture the student's ability to pass the exam in a state where she would be willing to practice. To the extent that our treatment and comparison groups differ in their willingness to practice in states with easy exams, however, differences in bar exam outcomes may not perfectly reflect differences in achievement.

[22] There is some misclassification in the latter, as 14 states (accounting for about 4.4% of students who pass the bar) do not report failed attempts at the exam. A back-of-the-envelope calculation suggests that about 3% of the graduates that we count as non-takers in fact failed the exam in one of these states.

we count any passage—regardless of the number of attempts—by July 1996 as a positive outcome.[23]

Finally, we consider three employment outcomes from the post-graduation survey: The first is an indicator for full time employment. The second attempts to measure the quality of the job: We (subjectively) classify certain prestigious jobs—clerkships, professorships, large law firms, etc.—as "good" jobs. Finally, for full time workers we examine the annual salary, reported in Table 1 in levels and logs. Each measure is unavailable for dropouts, so our analyses of employment outcomes condition on graduation.

As the employment measures are available for only a small fraction of dropouts, our analyses of these outcomes condition on graduation. Even beyond this, the employment analyses differ from those of academic outcomes, as employers of young lawyers may themselves practice affirmative action. Black-white differences in job market outcomes cannot be interpreted, therefore, as reflections of differences in academic achievement in law school. On the other hand, if law firms are competitive profit-maximizers, a black salary premium would mean that black lawyers have higher marginal revenue products than do white lawyers with similar entering credentials. A school that hopes to maximize its graduates' productivity should then cater to firm preferences by itself practicing affirmative action. We discuss the implications of our analyses of employment outcomes in the conclusion.

Black students' GPAs and graduation and bar passage rates are notably worse than those of white students, and students at unselective schools do worse than those at selective schools. The same is true for selective-unselective differences in employment outcomes, but black-white differences in these outcomes are smaller and do not take a consistent sign.

[23] 97% of students who pass by July 1996—two years after the scheduled graduation date—do so by July 1995, so the BPS' truncated record of exam outcomes likely misses only a few passages.

V. Measuring Preferences

Even the least selective law schools have competitive admissions, and 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 2 shows this admissions rate as a function of race and LSAT-undergraduate GPA cells (Barnes and Carr, 1992). White students whose credentials would have placed them in the bottom quarter of the BPS distribution were more likely to be rejected than to be accepted. Black admission rates were above one half everywhere above the fifth percentile, and were double or more those of similarly-qualified whites through the lower part of the distribution.

The BPS sample of matriculants excludes students who failed to gain admission to law school. If these students were worse in unobserved ways than those who were accepted, then comparisons of low-scoring black and white BPS respondents are likely biased against blacks by the greater selectivity of the white sample. A formal selection correction is not feasible, so we evaluate the impact of sample selectivity by reporting analyses that exclude the lowest-scoring students, among whom differential selectivity appears most problematic. Figure 2 suggests that limiting the analysis to students with above-median scores would eliminate the differential selection, but this would leave us with too few black observations (see Figure 1B). As a compromise, we select the 20th percentile as the limiting point for these analyses. Estimates from the limited sample are, of course, local to the most qualified quartile of black matriculants.

Among students who are admitted, preferences should provide access to more selective schools. Figure 3 displays the fractions of white and black students in the BPS data who attend schools in the two highly selective clusters, as functions of the admissions index percentile. Throughout the index distribution, black students are much more likely to attend schools in these clusters than are white students.[24]

The estimates in Figures 2 and 3 reflect a series of student decisions—about where to apply in each figure, and in Figure 3 about where to enroll—in addition to the direct effects of admissions preferences. A student who applied only to highly selective schools and was admitted to none would be counted as a non-admit in Figure 2, even if she might have been admitted to a "safety" school. Similarly, Figure 3 does not distinguish between students who attended less selective schools because they could not gain admission to more selective schools and those who chose not to apply to or matriculate at the latter. Krueger, Rothstein, and Turner (2005) show that black college applicants tend to apply to more selective schools than do whites with the same credentials. If this holds in law school as well, Figure 2 understates the direct effect of preferences on admissions decisions, while Figure 3 may overstate them.

For our purposes, it is not necessary to distinguish between the direct effect and indirect effects that operate through application and matriculation choices. The group of interest for evaluation of mismatch is the set of black compliers who attend selective schools when preferences are available but would attend unselective schools if they were not, regardless of the proximate causes of these decisions. Our key assumption is that black and white students would matriculate at selective schools at the same rates (conditional on observable characteristics) if they were subject to the same admissions rules. If so, the difference in profiles between the two panels of Figure 3 is the complier share, $p^c(1, X)$. The left panel of Figure 4 displays this difference, along with a 90% confidence interval. It indicates that black students near the middle of the BPS credentials distribution are around

[24] As noted earlier, our nonparametric analyses of the selective-unselective comparison use samples trimmed to exclude observations outside the common support, indicated by vertical lines in Figure 3.

forty percentage points more likely to attend highly selective schools than are whites with the same credentials. The difference is largest for the highest-scoring students and lowest at the bottom of the distribution.

Black-white differences in selectivity are probably understated, particularly among students at the bottom of the admissions index distribution. Our binary selectivity measure ignores variation in selectivity among s=0 schools. While few students of either race from the bottom quintile enroll at highly selective schools, there nevertheless are probably differences in the selectivity of the schools attended by black and white students in this range. We do not have a good measure of school selectivity outside of the top tiers, but we can use latent variable models to recover a measure of between-race differences in a more continuous underlying selectivity. Specifically, we suppose that latent selectivity, s^*, is normally distributed conditional on race and credentials, and that s=1 iff $s^*>0$. Then the mean of s^* for (b, Z) students can be estimated as $\sigma\Phi^{-1}(E[s \mid b, Z])$, where $\sigma^2 = Var(s^* \mid b, X)$ and Φ is the normal CDF, and the conditional black-white difference in s^* is $\sigma(\Phi^{-1}(E[s \mid b=1, Z]) - \Phi^{-1}(E[s \mid b=0, Z]))$. As the scaling of s^* is arbitrary, we choose σ so that $Var(s^* \mid b=0) = 1$. The right panel of Figure 4 shows the estimated black-white difference in latent selectivity. This is much more stable, with a black advantage around 0.75 – 1 through most of the distribution.

Table 2 presents estimates of the overall complier share, averaged across the index distribution. The first row shows the black coefficients from linear probability models for attendance at a school in one of the two highly selective clusters. When we do not control for entering credentials (col. 1), we find no black effect on selectivity. When we add quadratic controls for (LSAT, UGPA), in column 2, the black effect turns large and positive, indicating that blacks are nineteen percentage points more likely to attend selective schools than whites with similar credentials. We repeat this specification in column 3 using the trimmed sample where black and white index distributions both have positive support, with similar results. In column 4, we take a more flexible approach to absorbing the effects of entering credentials. Here, the estimated complier share is the black-white difference in mean selectivity in trimmed data that have been reweighted to make the admissions index distribution of whites match that of blacks. The estimated black effect on selectivity in this specification is 17 percentage points.

The second row of the table reports marginal effects from probit models, evaluated at the black mean. These are somewhat smaller than the linear probability model estimates. The third row reports the average black effect on underlying latent selectivity, as in Figure 4B, that is implied by the probit model or the nonparametric analysis. Black students attend schools that are a bit less than one standard deviation more selective, though the estimate from the reweighted data is slightly smaller.

Columns 5-8 repeat these specifications on the subsample of students with admissions indices outside the bottom quintile. As noted above, concerns about differential selection into law school matriculation are less severe in this subsample. The limitation tends to increase the black effect on binary selectivity but has much smaller effects on the latent selectivity estimates. As yet another sensitivity check, the second panel of Table 2 repeats the analysis using attendance at only the most selective, "elite" cluster as a proxy for selectivity. Effects on the probability of attendance at this cluster are naturally smaller, but the estimated black effects on latent selectivity are slightly larger than in the basic specification.

It remains to be shown that selectivity causes greater mismatch between students and their peers, holding entering credentials constant. To examine this, we look to students' first-year class ranks. For any given student, a more competitive environment should lead to a lower rank. Figure 5 presents mean rank in class (scaled so that the top rank is 1 and the bottom 0) as a function of race, school type, and the admissions index. More qualified students have higher ranks than those with lower index scores, and students at less selective schools higher ranks than similarly-qualified students at more selective schools. White students also achieve notably higher ranks than blacks.

Table 3 presents several estimates of selectivity effects on rank. The first two rows show OLS estimates of the effects of attending a school in one of the highly-selective clusters, separately for whites and for blacks. Control variables and sample definitions correspond to those in Table 2, though columns 3 and 7 of that table are omitted here. Attending selective-cluster schools appears to lower rank by about 0.06 for whites, and by twice that for blacks. Effects on blacks in the top four quintiles are even larger.

The second panel presents the black-white comparison for class rank. When differences in entering credentials are controlled, blacks have ranks about 0.19 lower than whites (or 0.23 in the upper four quintiles). The second row in this panel presents IV estimates of the effect of moving from the "unselective" to the "selective" category, using race as the instrument. These estimates indicate impossibly large negative selectivity effects, suggesting that a shift to highly selective schools reduces rank by around twice the possible range. The most plausible explanation is that the black-white difference in the selectivity of the school attended is larger than is captured by our binary proxy. The final row shows IV estimates of the effect of a one standard deviation increase in latent selectivity, a more reasonable -0.28 (-0.24 in the upper quintiles).

Regardless of how we control for qualifications, then, students attending more selective schools attain lower ranks than those attending less selective schools, and black students attain lower ranks than white students.

VI. Results

There are evidently large differences in the selectivity of the schools that black and white students attend, and each of our comparisons indicates that selectivity leads to greater mismatch. If mismatch lowers outcomes for marginal students, both selective-unselective and black-white comparisons should show negative effects on "real" outcomes.

A. Regression estimates

We begin with regression estimates of our two comparisons for each of our six outcome measures. The first comparison is in Table 4, which reports coefficients on a "selectivity" dummy in OLS regressions with controls for a quadratic in (LSAT, UGPA). For white students, in the first row, the selectivity effect is positive and significant on five of the six outcomes, with an insignificant negative effect only for full-time employment. The second row of the table shows estimates for black students. The estimated selectivity effects are positive and significant for graduation and salaries; all others are small and insignificantly different from zero. The final rows show p values for the hypotheses that the white and black effects are equal or are both zero. In no case can we reject equality (though we come close for full-time employment), though we reject the zero effects in four of six cases.

The first row of Table 5 presents analogous estimates for the black-white comparison. Here we see that black students have significantly worse graduation and bar passage results than similarly-qualified whites, with the bar passage coefficient a large -9%. Since black students attend more selective schools than whites with the same credentials, these estimates are consistent with negative selectivity effects. By contrast, the black effects on employment outcomes are positive, in two cases large and significant.

The second set of estimates in Table 5 exclude the bottom quintile of the admissions index distribution, where sample selection bias is potentially most severe. All of the point estimates are notably more positive in the subsample, and there is no indication of a negative black effect on any outcome.

B. Graphical analysis of bar passage

The models in Tables 4 and 5 include fairly sparse parameterizations of the relationship between students' entering qualifications and their eventual outcomes. If these are mis-specified, the estimated effects are biased in unknown directions. The next step in our analysis is to loosen the restriction to allow for arbitrarily non-linear effects on bar passage rates. For ease of graphical presentation, we impose a single index restriction and present estimates that are nonparametric in our admissions index. We focus on our first bar passage measure, which treats non-takers as failures.

Figure 6 presents estimates of bar passage rates as a function of the admissions index percentile score, separately by race and selectivity. Passage rates are increasing in the index for both races, although more steeply for blacks than for whites. Within race, there is little difference between the estimates for students at selective and at unselective schools; if anything, the former series is above the latter. By contrast, there are sizable black-white differences, concentrated at the bottom of the distribution.

Figure 7 shows the selective-unselective difference in passage rates, separately for each race. Because there is only one series in each panel here, we can add 90% confidence intervals to each series. Differences hover around zero for each race, and are significant only at the very lower tail of the black distribution.

Figure 8 presents the black-white difference. At the very bottom of the index distribution, white passage rates exceed those of blacks by about 15 percentage points. This gap shrinks as we move up the distribution,however, and is insignificantly different from zero at all points above the 40th percentile.

C. Reweighted estimates

We use the reweighting strategy discussed above to obtain estimates of average selective-unselective and black-white differences in bar passage rates and other outcomes. For the selective-unselective comparison, we estimate the density of index scores separately in each race-selectivity group, using the sample trimmed to ensure common support within each race, then reweight the observations at unselective schools to reproduce the index distribution seen among students of the same race at selective schools. Mean differences in outcomes between the selective sample and the reweighted unselective sample—estimating the average effect of the selectivity treatment on the treated—are reported in the first two rows of Table 6, first for whites and then for blacks. As in Table 4, the results indicate positive and significant effects of selectivity for whites for five of six outcomes. For the bar passage measure, depicted in Figure 7, the estimate is +2.1%, reflecting the positive effect depicted in the Figure at high scores that account for a large portion of the white selective-school distribution. Effects are noisier but generally similar for blacks, and again we can reject equality of effects for only one of six outcomes.

The third row of Table 6 shows estimates of the black-white difference in outcomes, after reweighting white observations to have the same index distribution as among blacks. Even after reweighting, blacks have much lower bar passage rates than whites, by about 10.9 percentage points. The black effect is also negative for graduation (insignificant) and for bar passage conditional on taking the exam (significant), but is positive for the three employment outcomes (though significant for only one).

Figure 8 indicated that the black deficit in bar passage rates was most severe at the bottom of the distribution, where non-random selection into matriculation might create the largest biases in the black-white comparison. The second panel of Table 6 reports estimates from the subsample of students in the top four quintiles of the index distribution. Selective-unselective comparisons are generally similar to those seen in the full sample. By contrast, the black-white comparison yields quite different results: The negative estimates for graduation and bar passage shrink substantially, much more than can be explained by sampling variation, while the positive effects on employment outcomes grow by about half. Thus, neither of our strategies is consistent with the presence of large negative selectivity effects on students in the top four quintiles of the entering qualifications distribution, though the black-white comparison does indicate negative selectivity effects on students from the bottom quintile (at least for graduation and bar passage).

Table 7 presents several alternative estimates of the effects on bar passage. The first column repeats the estimates from Table 6. The second column loosens the single-index assumption imposed in our reweighting estimates so far, this time reweighting the data based on the bivariate density of LSAT scores and undergraduate GPAs. As bivariate densities require more data to estimate precisely than univariate densities, we are forced to trim the data more aggressively to ensure overlapping support. Perhaps as a consequence, all of the estimates are attenuated slightly from Column 1, but the basic pattern is similar. The third column returns to the single index model but adds controls for a set of observable characteristics that might differ between the comparison groups. These include a gender indicator; a quadratic in the student's age; indicators for students who took time off between college and law school; continuous measures of maternal and paternal education; and an indicator for whether any family members have previously attended law school.[25] This slightly reduces positive effects from the selective-unselective specification, but has little impact on the black-white comparisons. Finally, the fourth column repeats this specification on a sample that excludes part-time and evening students. This restriction has little effect.

D. LATE estimates

As noted earlier, both of our strategies can be used to generate estimates of local average treatment effects of selectivity for the black students who comply with race-based preferences. Table 8 presents these. Both the selective-unselective and the black-white comparisons indicate implausibly large negative effects of selectivity on class rank. The selective-unselective comparison yields imprecise but positive estimates for all other outcomes. By contrast, the black-white comparison indicates negative selectivity LATEs on graduation and bar passage and positive effects on employment outcomes, all quite large. The next two rows repeat these estimates for students in the top four quintiles. As noted earlier, this subsample yields notably larger selectivity effects.

The magnitude of the black-white estimates reflects the limited nature of our binary selectivity measure, which as discussed earlier causes us to understate the first-stage relationship between race and selectivity. The final rows of Table 8 replace this first stage with the one for latent continuous selectivity. The resulting estimates are more reasonable, indicating that attending a school that is one standard deviation more selective reduces the average black complier's grades by 1.1 standard deviations and her graduation and bar

[25] In principle, we could reweight the data to balance all of these characteristics between our comparison groups, although this would require a prohibitively large sample. Instead, balance only the admissions index distribution, then estimate a regression in the reweighted data of the bar passage rate on the selectivity or race indicator of interest, a quadratic in (LSAT, UGPA), and each of the new control variables.

passage probabilities by 6 and 17 percentage points, respectively. In the top four quintiles, the latter estimates fall to approximately zero, and employment effects are large and positive.

Figure 9 offers a graphical presentation of LATEs on bar passage from the black-white comparison, as functions of the admissions index. Consistent with the table, these are much more negative at the bottom of the index distribution, and are larger when we rely on our binary selectivity proxy than when we adjust to obtain estimates of the effects of latent selectivity.

VII. Conclusion

Experiments that randomly assign students to more- or less-selective schools are unlikely. Research and policy on mismatch effects must proceed from non-experimental analyses that are identified only via assumptions about counterfactual outcomes.

This paper has explored two exclusion restrictions, 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 Study data are well suited for non-experimental analyses.

We find little convincing support for claims that mismatch is an important consequence of affirmative action in law school admissions. We reject large mismatch effects on bar passage rates for any 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 but the potential for bias deriving from differential selection into law school counsels interpretive caution.

We find no evidence of mismatch effects on employment outcomes. Black students are much more likely to obtain good jobs than are similarly-qualified white students, and those outside of the bottom quintile of the credentials distribution obtain a 15% salary premium. As noted above, this "reverse mismatch" (Alon and Tienda, 2005) could reflect affirmative action on the part of employers rather than differences in academic success. A crucial question, which we cannot answer, is how firms' hiring patterns would change if law schools eliminated affirmative action: If high-paying firms would hire from less selective schools, if necessary, to obtain black lawyers, the black salary premium might persist. Thus, our analysis does not provide clear support for the claim that affirmative action in law school admissions *helps* black students. It does demonstrate, however, that there is little compelling evidence for negative effects, and that the evidence points against them for all but the least qualified students.

Appendix: Comparing our analysis with Sander (2004)

We find little evidence of negative mismatch effects on black students who rank above the 20th percentile of the entering credentials distribution. This contrasts sharply with Sander (2004), whose analysis of the same data set leads him to the conclusion that mismatch effects are large. This appendix attempts to account for the differing results, which are attributable to two differences in the analyses. First, Sander adopts a structured specification in which affirmative action affects law school tier, tier affects law school GPA, and tier and GPA determine graduation, bar passage, and other outcomes. Sander finds, as do we, large mismatch effects on law school GPA; in his specification these somewhat mechanically carry through to other outcomes. Second, although Sander also presents reduced-form analyses akin to that in our Figure 6, his presentation obscures the degree to which black-white differences are concentrated at the bottom of the credentials distribution. In fact, his reduced-form results are consistent with ours, showing substantial mismatch effects on graduation and bar passage in the bottom quintile of the index distribution but none or tiny effects outside that range.

We focus here on Sander's analysis of mismatch effects on dropout rates, and we follow Sander's variable and sample construction here despite minor differences from the definitions used in the main text (explained in the Data Appendix). We also follow Sander in treating school cluster as a cardinal variable, ranging from 1 to 6 (with 5 and 6 our "highly selective" group), and we include only linear controls for undergraduate GPA and LSAT scores. The basic differences between the approaches come through even in these very simple specifications.

Appendix Table A demonstrates the differences between the "structured" and reduced-form approaches. Parts III and V of Sander's paper demonstrate, respectively, that black students attend substantially more selective schools than do white students with the same entering credentials and that blacks obtain lower GPAs in law school. Columns 1 and 2 of Appendix Table A present estimates of specifications for law school tier and first year GPA that, while somewhat different than those that Sander presents, yield similar estimates.

Holding constant LSAT and GPA, there is nearly a full cluster difference between the schools that black and white students attend. The between-race difference equals the effect of more than 9 LSAT points (on a 10-48 scale) or 1.2 undergraduate GPA points.[26] Not surprisingly, this affects black students' law school GPAs, particularly when they are standardized within schools as in the BPS: Black GPAs are 0.7 standard deviations below those of similarly-credentialed whites, an effect as large as that produced by 18.4 LSAT points or 2.5 undergraduate GPA points.

Sander assumes that the black effect in each of these models estimates the causal effect of affirmative action. When he turns to analyze graduation outcomes, he estimates a logistic regression for dropping out that excludes students' entering credentials but includes the tier and law school GPA. Affirmative action effects are assumed to come via the previously-estimated black effects on these variables. Sander includes a black dummy in his dropout model, but interprets it as a specification test: If affirmative action is the only source of black-white differences and if it operates solely through tier and GPA, the black coefficient in Sander's specification should be zero.

Column 3 of Table A presents Sander's estimates.[27] Higher GPAs and higher tiers are both associated with lower dropout rates. Conditional on these, black students drop out at marginally significantly *lower* rates than whites. Sander notes that the GPA coefficient is much larger than the tier coefficient, and interprets this as evidence that mismatch effects of affirmative action on GPAs dominate the positive effects on tier, yielding a negative net impact on graduation rates. Although he does not formalize this calculation, it seems clear how one might do so: The net impact of affirmative action in Sander's methodology equals the product of the GPA effect from column 3 with the black effect from column 2, plus the product of the tier effect from column 3 with the black effect from column 1. This is presented in the bottom row of the table: Affirmative action appears to increase the index of the logistic model for dropout by 0.69 points, corresponding to a 5.9 percentage point effect on dropout rates for students with average black characteristics.

Our analysis of black-white comparisons proceeds from the idea that any mismatch effects should be apparent as a black effect in a reduced-form dropout model that conditions only on entering credentials. Column 4 of Table A presents an analysis of this form. The black coefficient is indeed positive and highly significant: It implies that for a student with the mean black characteristics, being black leads to dropout rates 2.8 percentage points higher.[28] This is less than one half of the net affirmative action effect in Sander's analysis. The difference is driven by the law school GPA: While being black has large negative effects on law school GPA (presumably driven by its effects on tier), it appears to be misleading to assume that these effects will carry through to dropout rates as strongly as would be implied by the within-race effect of GPA on dropping out that appears in Column 3.

We found in the main text that the reduced-form black effect is concentrated in the bottom quintile of the entering credentials distribution, with a very small black-white difference conditional on credentials in the upper four quintiles. Column 5 of Table A presents a final specification that estimates separate black effects in the bottom quintile and

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[26] The LSAT and GPA weights are quite different from those used to compute the admissions index, but this derives from the odd specification for the dependent variable. When a more sensible variable—e.g. an indicator for attending a highly selective school—is used the weights are quite close to those used in the index.

[27] Sander reports "standardized coefficients," where we report un-transformed logistic coefficients. Standardization aside, the estimates in our Column 3 are identical to those that Sander reports in his Table 5.6.

[28] This is quite similar to the 2.1 percentage point marginal effect from a more flexible model reported in Table 2.

top four quintiles. As in our main analysis, the black effect appears entirely in the bottom quintile, and in the top four quintiles blacks are slightly (and insignificantly) less likely to drop out than are whites.

Sander also presents reduced-form estimates, in the form of tables that compare black and white outcomes within ranges of his entering credentials index. Columns 1 and 2 of Appendix Table B reproduce Sander's Table 5.7. We supplement his table in two ways: We compute and report the black-white difference in each row and its p-value (Columns 3 and 4), and we report the frequency distribution of BPS respondents across ranges (Columns 5-7). Sander describes his results as follows:

> At the most elite schools (the schools attended by the one-eighth of black students with index scores above 700), the advantages of low institutional attrition entirely offset lower grades. But across most of the range of index scores, black attrition rates are substantially higher than white rates, simply because racial preferences advance students into schools where they will get low grades. (p. 441)

Our table reveals two interesting codas to Sander's description. First, it shows that the black-white differences are not significantly different from zero at a 5% level in *any* of Sander's ranges (although they are jointly significant). Second, two-thirds of BPS respondents (and three-quarters of whites) are in Sander's top range, with index scores above 700; another sixth are in the second, 640-700 range. These bins have substantially smaller black-white differences in non-completion than do the lower bins, which together include only 17% of BPS respondents. This again matches our earlier conclusion that black underperformance appears primarily in the bottom quintile of the credentials distribution.

Data Appendix

In this Appendix we provide a bit more detail about the idiosyncrasies of the BPS data, focusing on several decisions made by the LSAC in its aggressive protection of respondents' privacy that limit the value of the data. Programs and data are available from the authors upon request.

The most important limitation is that the BPS data do not report the actual law school that each respondent attends. Instead, law schools are grouped into six "clusters," and only the cluster is reported. Moreover, the LSAC has masked the composition of each cluster. Wightman (1993) describes the cluster analysis—which attempted to group schools along dimensions like size, cost, selectivity, tuition level, and minority representation—that was used to create the clusters. There appears to be substantial overlap in the selectivity of the various clusters, though two (labeled "Elite" and "Public Ivy" in the BPS data) are clearly more selective that the other four. We use these two as our proxy for "highly selective" schools, though we acknowledge that this classification involves some measurement error (Sander, 2005b).

A second data limitation relates to the undergraduate GPA. The BPS provides no information about the undergraduate college, so it is impossible to adjust the undergraduate GPA for the difficulty of the curriculum or to convert in into something resembling a measure of class rank.

Finally, as noted in the text the bar exam measures are limited in two ways. First, though states vary in the difficulty of their bar exams, the BPS does not report which state's exam a student took. As the state is likely endogenous in any case, it isn't clear how such information could be used. We assume that students who have difficulty passing the exam attempt it in the easiest state where they would be willing to live, so that an observed failure indicates that the student cannot practice law anywhere where she would be willing to.

A second limitation to the bar exam measure is that 14 states report successful attempts at the exam but not failures. Some students who are not observed to attempt the exam, then, in fact failed it in one of these states. As these states represent only a small fraction (about 4%—Wightman 1998) of observed passages, we expect that the number of misclassifications is small. Misclassification rates for the outcome of the first attempt are likely higher, so we focus on a measure of whether students ever pass the exam. As noted in the text, we use one measure that counts (observed) non-attempters as failures and another that excludes those non-attempters who graduated law school.[29]

Admissions Index

As noted in the text, much of our analysis uses a single index of the LSAT score and the undergraduate GPA. After converting these two variables to range from 0 to 1—the GPA theoretically ranges from 0 to 4 and the LSAT ranged from 10 to 48 in 1991—we average these with 0.4 weight on the GPA:[30]

$$Index_i = 400 * \frac{UGPA_i}{4} + 600 * \frac{LSAT_i - 10}{48-10}.$$

These weights are those used by Sander (2004) We have explored two alternative weights. The first uses fitted values from a probit model for attending a school in the highly selective clusters, including a black indicator as an additional control. The second uses fitted values from an OLS regression for the first year GPA, with a black indicator and cluster dummies as additional controls. Each of these correlates above 0.999 with Sander's index, and results are not sensitive to the use of either.[31]

Outcome Measures

We describe here the construction of our seven outcome measures.

- 1st year class rank. As noted in the text, the law school GPA variables in the BPS are standardized (to mean zero and standard deviation 1) within each law school. We convert the first year GPA measure to a class rank, assuming that GPAs are normally distributed within each school. Thus, rank = Φ(standardized GPA); it ranges from 0 to 1.
- Graduation status. This is set to 1 for students who graduated law school, and to 0 for students who stopped out of law school or who had not graduated by the close of the study. It is set to missing for students who withdrew from the study, either because they transferred to a non-participating school or because they were no longer comfortable participating.
- Bar passage. This is set to 1 for students who were observed to pass the exam, and to 0 for all others, regardless of graduation or study participation status. (Note that

[29] Non-graduates are counted as failures in both measures, as there is no interpretation in which these represent positive outcomes.

[30] 21 students have reported undergraduate GPAs of 4.1 and 5 have 4.2s. These may come from schools that give extra points for honors courses. We do not censor the data, so there are 6 observations with index values above 1000. Note also that the LSAT has since converted to a different scoring scale.

[31] Note that the models in columns 1 and 2 of Appendix Table 1 assign somewhat greater weight to the UGPA than does our index. The odd specifications in these models—chosen to conform to those used by Sander (2004)—account for this. In any case, indices generated using these models are also highly (>0.99) correlated with our index.

bar exam outcomes were collected for students who withdrew from the study, though about half are not observed to take the exam.)

- Bar passage if attempt. This is identical to the previous variable, except that students with graduation status equal to 1 who were never observed to attempt the exam are excluded.
- Full time employment. This and the remaining outcomes are available only for students sampled for the follow-up survey (see below).
- "Good" job. This is set to 1 for graduates whose "current work setting" is a judicial clerkship (14.0%), academic (0.8%), a prosecutor's office (4.4%), a public defender's office (2.3%), or a large private law firm (20.0%). It is set to zero if the current work setting is a medium (10.6%) or small (19.1%) firm, a solo practice (3.1%), a legislative office (0.6%), a government agency (8.3%), a public interest group (2.5%), a business or financial institution (7.7%), "other" law-related work (3.8%), or other non-law related work (2.8%). This classification is of course arbitrary; we have explored other classifications with similar results. Blacks are notably overrepresented (controlling for their admissions indices) in academia, prosecutors' offices, large private firms, legislative offices, government agencies, and public interest groups; they are substantially underrepresented in mid-size and small firms and solo practices.
- Ln(salary). Salaries are reported in 8 bins. We assign each observation to the middle of the bin (using $10,000 as the middle of the "<$20,000" bin and $90,000 as the middle of the ">$80,000" bin), then compute the log. Individuals who are not employed full time are excluded.

Follow up survey

Our employment measures are taken from a follow-up survey conducted approximately six months after students' scheduled graduations. This follow-up was administered to a probability sample of students in the full BPS survey, but unfortunately the data do not report the exact sampling probabilities. We construct approximate probability weights to reproduce the race-cluster distribution in the full sample. With these weights, which are used in all of our employment analyses, white students at schools with above-average black shares are overrepresented within each cluster.

Response rates to the follow-up survey were less than perfect. Students who did not graduate from law school responded at very low rates, and are excluded from our analyses of employment outcomes. Among graduates, the response rate was 75% overall but only 65% for blacks, though this difference does not appear to be attributable to differences in entering credentials.

Control variables

Columns 3 and 4 of Table 7 add a vector of additional control variables. These are age at law school entry and its square; an indicator for students who graduated from college before 1991; linear measures of maternal and paternal education, in years; and an indicator for whether a parent, grandparent, spouse, or partner has attended law school.

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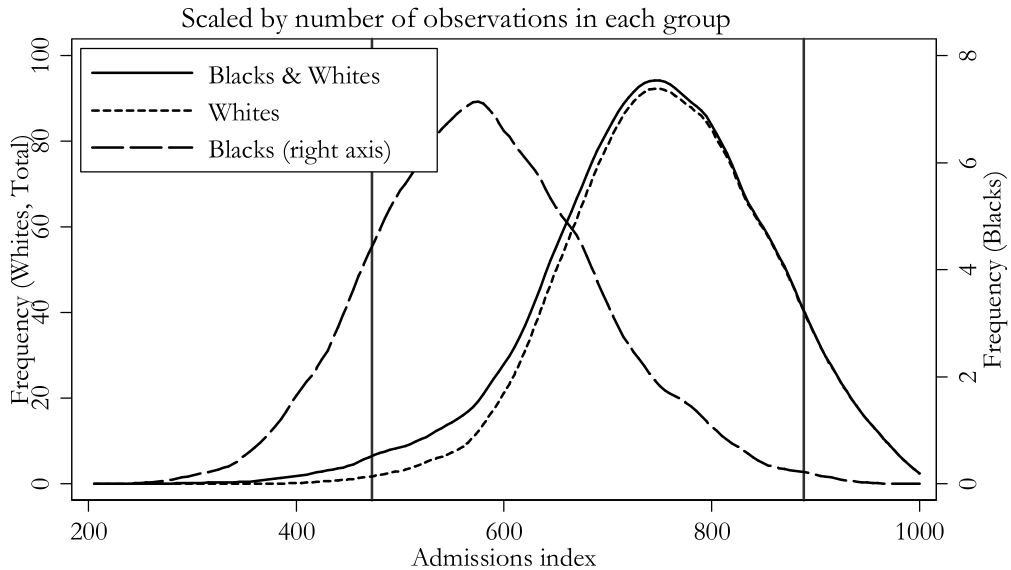
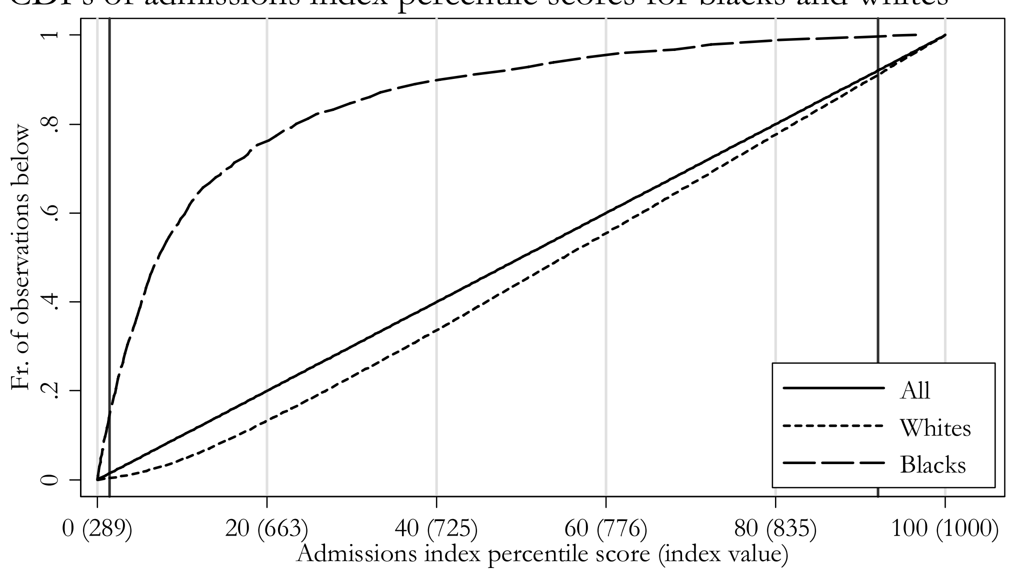
Figure 1A.
Density of admissions index among black and white BPS respondents



Note: Admissions index is 400*UGPA + 600*LSAT, after standardizing each to range from 0 to 1. Vertical lines indicate trimming points for analyses that are limited to the region of common support.

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



Note: Figure displays CDFs of the percentile scores--which by construction are uniformly distributed in the full sample--for whites and blacks separately. Vertical lines indicate trimming points for analyses that are limited to the region of common support.

Figure 2.
Fraction of applicants admitted to at least one school, by race and index percentile (normed to BPS distribution)

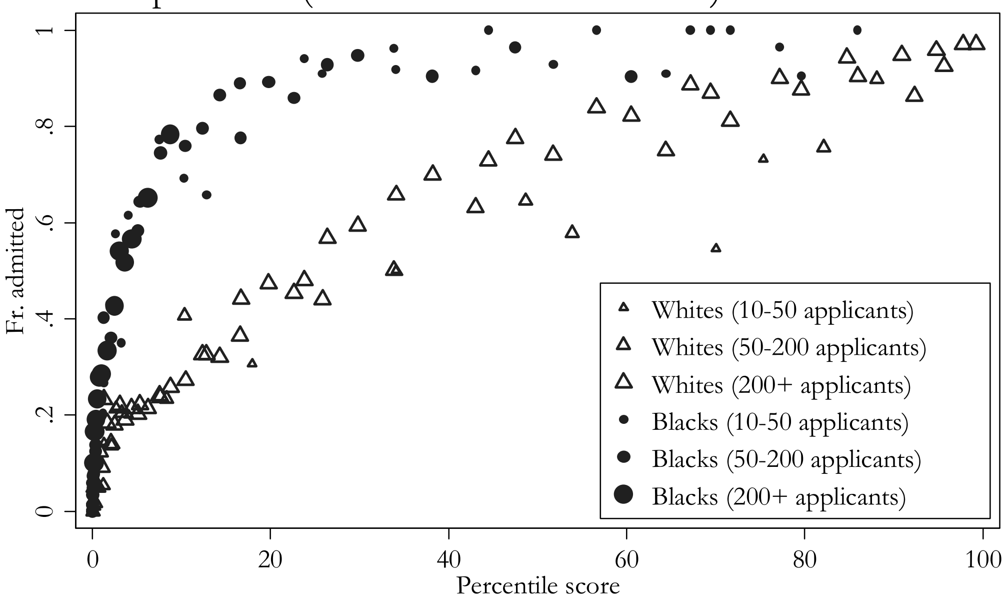
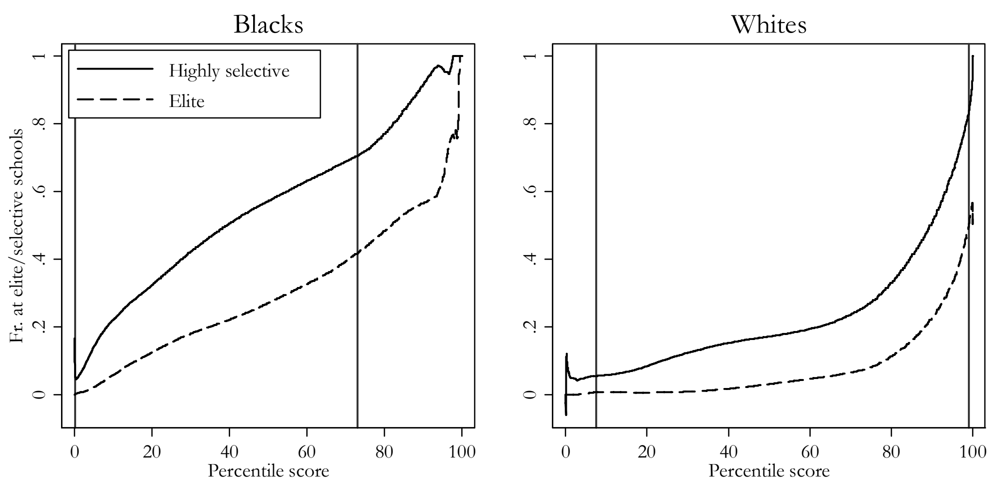
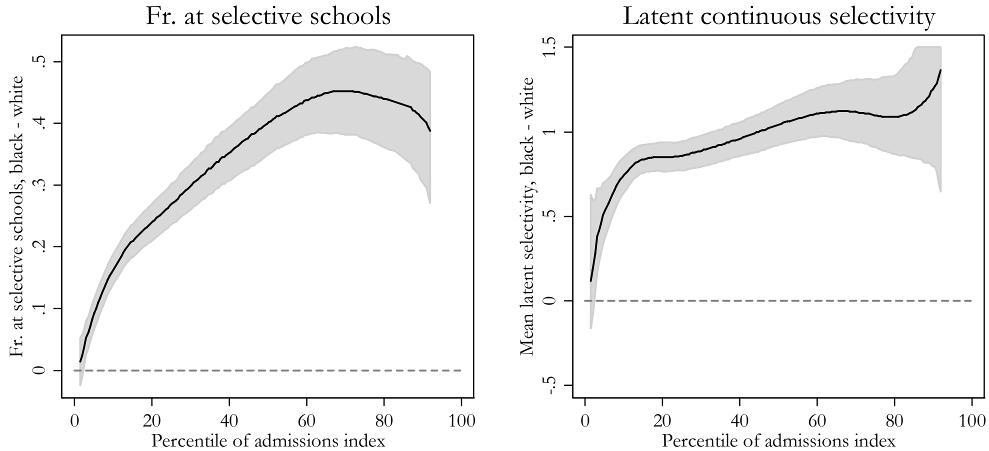


Figure 3.
Fraction of blacks and whites at elite and highly selective law schools, by index percentile



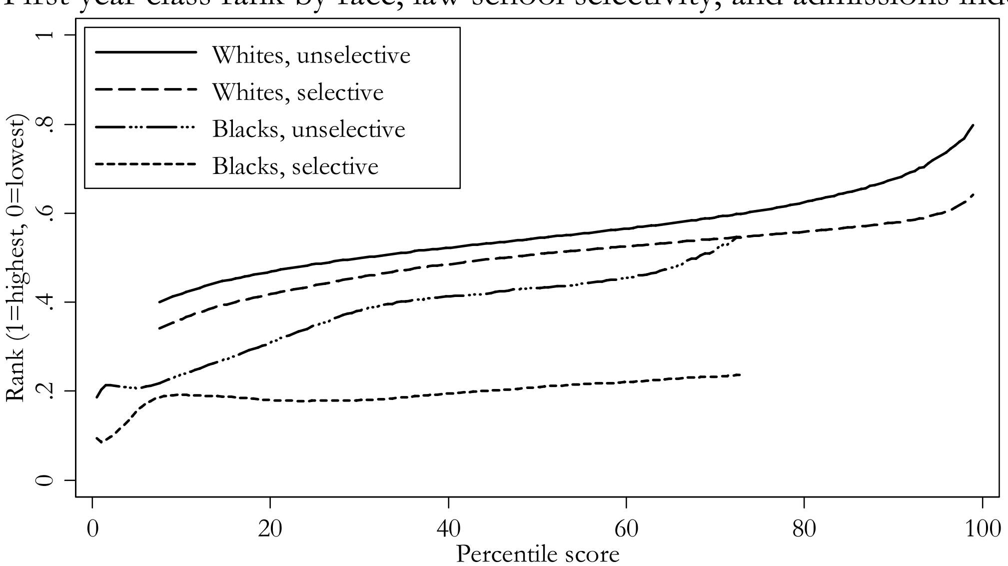
Note: Fractions are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Vertical lines indicate trimming points for analyses that are limited to the region of common support between the selective and unselective subsamples.

Figure 4.
Black-white difference in selectivity of school attended, by index percentile



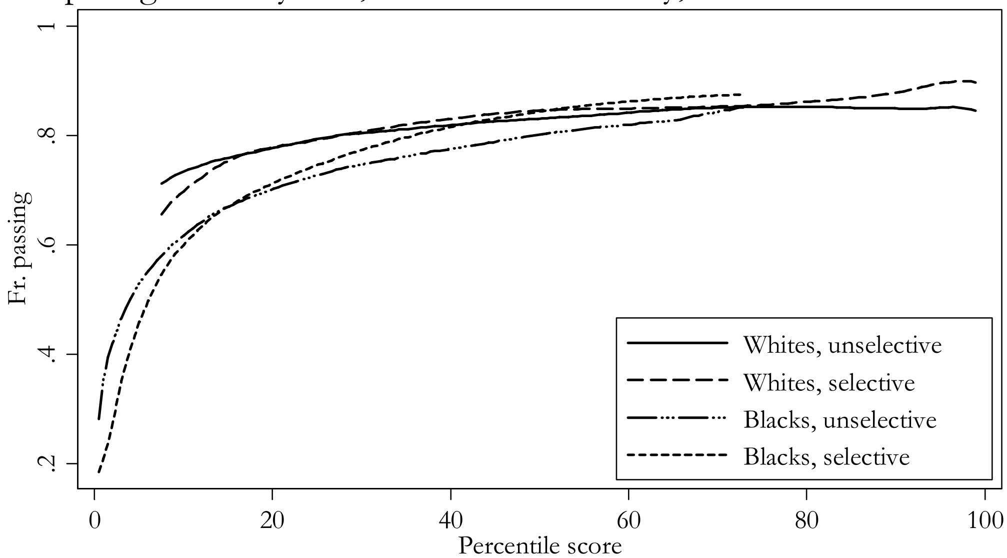
Note: Fractions are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for the subsample of common support. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing 500 bootstrap samples from the underlying data and re-estimating the differences in these samples. In the right panel, the CI is censored from above at +1.5.

Figure 5.
First year class rank by race, law school selectivity, and admissions index



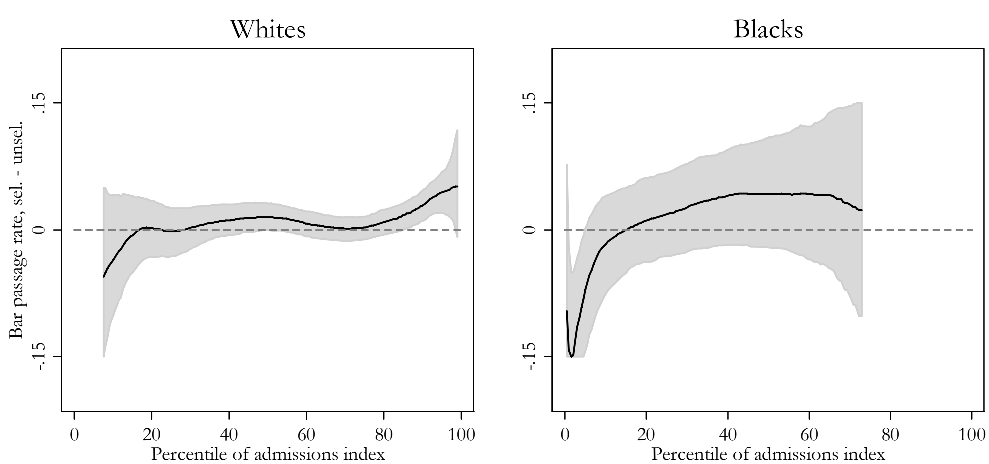
Note: Ranks are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for percentile points within the trimmed samples used for selective-nonselective comparisons.

Figure 6.
Bar passage rates by race, law school selectivity, and admissions index



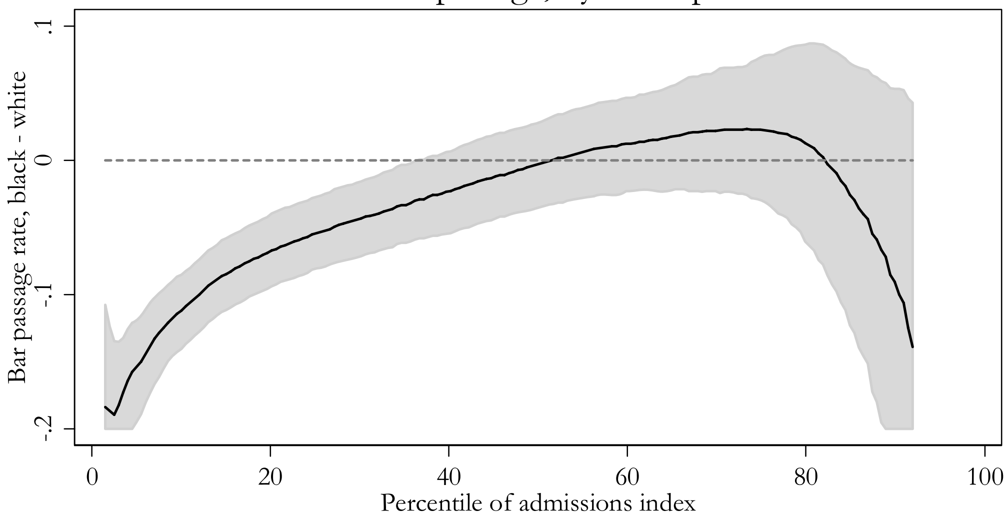
Note: Rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for percentile points within the trimmed samples used for selective-nonselective comparisons.

Figure 7.
Selective-unselective difference in bar passage, by race and index percentile



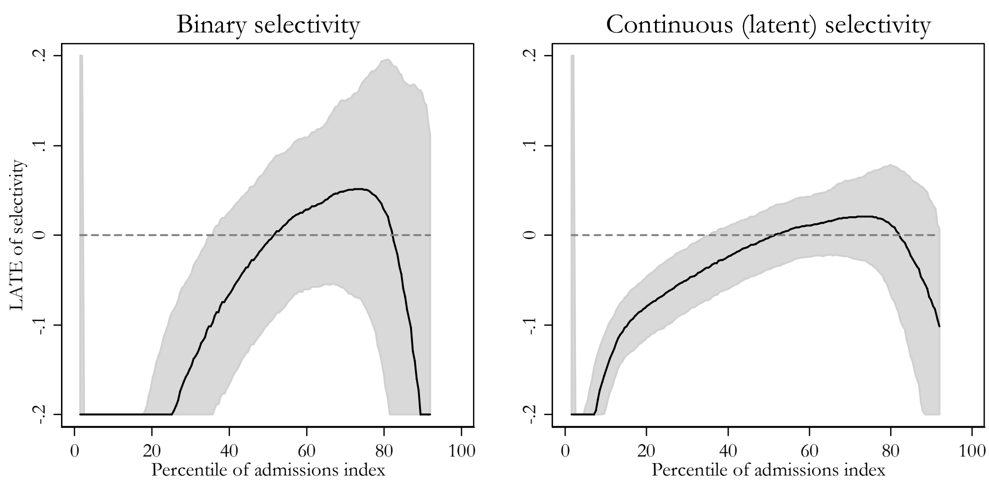
Note: Passage rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for the subsample of common support, computed separately for each race. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing bootstrap samples from the underlying data and re-estimating the differences in these samples. Estimates and CIs are censored at +/- 0.15.

Figure 8.
Black-white difference in bar passage, by index percentile



Note: Passage rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for the subsample of common support. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing bootstrap samples from the underlying data and re-estimating the differences in these samples. CIs are censored at -0.2.

Figure 9.
Local average treatment effects of school selectivity from black-white comparison



See text for description of binary and latent selectivity. Estimates are shown only for the subsample of common support. Confidence intervals are indicated by shaded areas, and computed by drawing bootstrap samples from the underlying data and re-estimating the differences in these samples. Both point estimates and confidence intervals are censored at +/-0.2.

Table 1. Summary statistics

	Full sample		Race overlap subsample		Selectivity overlap subsamples			
					Blacks		Whites	
	Mean	S.D.	Blacks	Whites	Sel.	Unsel.	Sel.	Unsel.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N	24,051		1,574	20,170	386	1,378	5,171	16,243
Black	7.6%		100%	0%	100%	100%	0%	0%
Female	43.7%		60.1%	42.6%	59.8%	59.3%	43.6%	42.4%
Law school type								
Selective (top 2 clusters)	24%		25%	20%	100%	0%	100%	0%
Elite (top cluster)	8%		9%	5%	32%	0%	32%	0%
Admissions credentials								
LSAT	36.8	5.5	30.1	36.7	32.0	27.8	40.3	36.8
UGPA	3.23	0.42	2.91	3.23	3.01	2.82	3.41	3.23
Admissions index	747	105	608	745	649	563	819	746
Admissions index %ile	51.6	28.4	16.8	50.6	25.1	10.3	71.4	50.3
Outcomes								
1st year LGPA	0.06	0.98	-0.98	0.11	-1.19	-0.97	0.16	0.16
1st year class rank (est.)	0.52	0.29	0.24	0.53	0.18	0.24	0.55	0.55
Graduated from law school?	91%		84%	92%	90%	78%	95%	91%
Ever pass bar exam?	81%		62%	82%	68%	54%	86%	82%
Ever pass bar (if attempted)?	86%		66%	88%	74%	58%	92%	87%
Empl. full time (if grad.)	66%		65%	64%	73%	60%	69%	65%
"Good" job (if employed)	40%		44%	36%	50%	40%	57%	33%
Salary (if FT; $1,000s)	$39.8	$18.8	$38.4	$38.1	$47.3	$34.3	$49.0	$36.8
Log salary (if FT)	10.51	0.47	10.44	10.47	10.65	10.35	10.72	10.44

Notes: Columns 1 and 2 report statistics for the full sample. Columns 3 and 4 are computed over the subsample in which the white and black admissions index distributions overlap, and columns 5 and 6 (7 and 8) are computed over the subsample in which the black (white) selective and unselective distributions overlap. See text for details.

Table 2. Black-White differences in selectivity

	Full sample				Top 4 quintiles			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
In selective ("elite" and "public ivy") clusters								
Black coefficient (OLS)	-0.016	**0.189**	**0.205**	**0.172**	**0.231**	**0.343**	**0.342**	**0.335**
	(0.010)	(0.012)	(0.012)	(0.012)	(0.021)	(0.020)	(0.020)	(0.024)
Marginal effect at black mean	-0.016	**0.161**	**0.167**		**0.231**	**0.355**	**0.353**	
(probit)	(0.010)	(0.010)	(0.011)		(0.024)	(0.025)	(0.025)	
Black effect on latent	-0.051	**0.842**	**0.880**	**0.647**	**0.615**	**1.047**	**1.040**	**0.926**
selectivity	(0.034)	(0.045)	(0.046)	(0.059)	(0.061)	(0.063)	(0.063)	(0.060)
In "elite" cluster								
Black coefficient (OLS)	-0.001	**0.088**	**0.099**	**0.079**	**0.148**	**0.211**	**0.212**	**0.208**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.014)	(0.013)	(0.011)	(0.020)
Marginal effect at black mean	-0.001	**0.042**	**0.044**		**0.148**	**0.205**	**0.209**	
(probit)	(0.007)	(0.005)	(0.007)		(0.020)	(0.020)	(0.021)	
Black effect on latent	-0.007	**1.299**	**1.294**	**0.881**	**0.624**	**1.379**	**1.372**	**1.164**
selectivity	(0.044)	(0.066)	(0.067)	(0.087)	(0.067)	(0.075)	(0.075)	(0.073)
Quadratic in LSAT, UGPA	n	y	y	n	n	y	y	n
Restrict to common support	n	n	y	y	n	n	y	y
Nonparametric in index	n	n	n	y	n	n	n	y

Notes: Latent continuous selectivity is computed from the probit model under the assumption that selectivity is normally distributed with standard deviation 1 conditional on race and entering credentials; it is then scaled so that the unconditional standard deviation among whites is 1. The "common support" sample is discards observations with admissions indices that have extremely low density in the white or the black subsamples, as described in the text. "Nonparametric" estimates report the black-white difference in means in the common support sample, after reweighting white observations to have the same admissions index distribution as is observed among blacks.

Table 3. Selectivity effects on 1st year class rank from selective-unselective and black-white differences

	Full sample			Top four quintiles		
	Raw	Regression-adjusted	Reweighted	Raw	Regression-adjusted	Reweighted
	(1)	(2)	(3)	(4)	(5)	(6)
Selective-unselective difference						
Whites	0.008	**-0.060**	**-0.066**	-0.008	**-0.060**	**-0.063**
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Blacks	**-0.052**	**-0.116**	**-0.130**	**-0.177**	**-0.210**	**-0.207**
	(0.014)	(0.015)	(0.015)	(0.025)	(0.026)	(0.026)
Black-White difference						
Reduced form	**-0.312**	**-0.189**	**-0.183**	**-0.265**	**-0.226**	**-0.228**
	(0.007)	(0.008)	(0.009)	(0.014)	(0.013)	(0.012)
IV (selective school dummy)	26.845 (24.854)	**-0.991** (0.072)	**-1.062** (0.079)	**-1.145** (0.123)	**-0.657** (0.052)	**-0.681** (0.049)
IV (latent selectivity)			**-0.282** (0.027)			**-0.241** (0.017)

Notes: Columns 1 and 3 include no controls beyond the selective (rows 1-2) or black (row 3) indicator. Column 2 controls for a quadratic in (LSAT, UGPA). Column 3 reports the between-group difference in means in data trimmed to the common support and then reweighted so that the two groups have similar admissions index distributions.

Table 4. Regression estimates of selectivity effects on outcomes: Selective-unselective comparison

	Law school graduation	**Bar passage**		**FT emp.**	**Good job**	**Ln(salary)**
		Any	**If attempted**			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Whites						
Selective	**0.029**	**0.014**	**0.025**	-0.040	**0.105**	**0.153**
	(0.005)	(0.006)	(0.006)	(0.025)	(0.031)	(0.030)
N	22,083	22,215	20,864	2,306	1,532	1,501
R2	0.011	0.017	0.027	0.031	0.103	0.115
Blacks						
Selective	**0.049**	-0.007	-0.002	0.054	0.021	**0.227**
	(0.024)	(0.029)	(0.029)	(0.043)	(0.052)	(0.053)
N	1,809	1,836	1,705	838	537	528
R2	0.058	0.131	0.148	0.049	0.082	0.126
p, equal effects	0.42	0.47	0.36	0.06	0.17	0.22
p, both zero	0.00	0.09	0.00	0.13	0.00	0.00

Notes: Coefficients on a selective school indicator are reported. All specifications include controls for a quadratic in (LSAT, UGPA).

Table 5. Regression estimates of black-white difference in outcomes

	Law school graduation	Bar passage		FT emp.	Good job	Ln(salary)
		Any	If attempted			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Full sample						
Black	**-0.033**	**-0.091**	**-0.094**	0.051	**0.208**	**0.100**
	(0.008)	(0.011)	(0.010)	(0.038)	(0.046)	(0.045)
N	23,892	24,051	22,569	3,144	2,069	2,029
R2	0.025	0.058	0.082	0.027	0.091	0.098
Top four quintiles						
Black	0.006	-0.015	-0.027	**0.137**	**0.287**	**0.157**
	(0.013)	(0.018)	(0.015)	(0.063)	(0.075)	(0.071)
N	19,700	19,808	18,616	2,294	1,555	1,525
R2	0.007	0.007	0.013	0.036	0.092	0.090

Notes: Coefficients on a black indicator are reported. All specifications include controls for a quadratic in (LSAT, UGPA).

Table 6. Reweighted estimates of selective-unselective differences (avg. for students in selective schools, by race) and black-white differences (avg. for blacks)

	Law school graduation	**Bar passage**	**Bar passage (if attempted)**	**FT employment**	**Good job**	**Ln(salary)**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Full Sample						
Selective-Unselective Comparison						
Whites	**0.031**	**0.021**	**0.029**	-0.011	**0.165**	**0.193**
	(0.004)	(0.007)	(0.005)	(0.023)	(0.034)	(0.029)
Blacks	**0.048**	0.014	0.021	0.038	0.017	**0.225**
	(0.020)	(0.029)	(0.030)	(0.038)	(0.056)	(0.058)
p, equal effects	0.40	0.84	0.80	0.27	0.02	0.63
p, both zero	0.00	0.01	0.00	0.54	0.00	0.00
Black-White comparison						
	-0.038	**-0.109**	**-0.112**	0.019	**0.163**	0.062
	(0.013)	(0.016)	(0.016)	(0.037)	(0.047)	(0.054)
Top four quintiles						
Selective-Unselective Comparison						
Whites	**0.031**	**0.021**	**0.030**	-0.006	**0.166**	**0.195**
	(0.004)	(0.006)	(0.005)	(0.025)	(0.034)	(0.029)
Blacks	0.026	0.049	0.029	0.059	0.078	**0.209**
	(0.027)	(0.042)	(0.040)	(0.059)	(0.079)	(0.081)
p, equal effects	0.85	0.51	0.97	0.31	0.31	0.87
p, both zero	0.00	0.00	0.00	0.59	0.00	0.00
Black-White comparison						
	0.004	-0.018	-0.032	**0.132**	**0.278**	**0.147**
	(0.013)	(0.019)	(0.018)	(0.032)	(0.042)	(0.042)

Notes: All estimates are restricted to samples of continuous support, which vary across comparisons.

Table 7. Additional specification tests for bar passage effects

	Base specification	Reweighted based on joint (LSAT, UGPA) distribution	Reweighted based on index, with additional controls	
	All students	All students	All students	Regular students
	(1)	(2)	(3)	(4)
Full Sample				
Selective-Unselective Comparison				
Whites	**0.021**	**0.017**	0.010	0.014
	(0.007)	(0.007)	(0.007)	(0.007)
Blacks	0.014	0.006	-0.001	0.004
	(0.029)	(0.028)	(0.033)	(0.033)
p, equal effects	0.84	0.71	0.74	0.78
p, both zero	0.01	0.04	0.34	0.16
Black-White comparison				
	-0.109	**-0.091**	**-0.106**	**-0.108**
	(0.016)	(0.014)	(0.019)	(0.020)
Top four quintiles				
Selective-Unselective Comparison				
Whites	**0.021**	**0.017**	0.010	**0.015**
	(0.006)	(0.007)	(0.007)	(0.007)
Blacks	0.049	0.027	0.046	0.045
	(0.042)	(0.041)	(0.046)	(0.047)
p, equal effects	0.51	0.81	0.44	0.52
p, both zero	0.00	0.04	0.20	0.06
Black-White comparison				
	-0.018	-0.016	-0.034	-0.042
	(0.019)	(0.020)	(0.023)	(0.022)

Table 8. Local average treatment effects of school selectivity

	Class rank	Law school graduation	Bar passage		FT emp.	Good job	Ln(salary)
			Any	If attempted			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Binary selectivity							
Full Sample							
Selective vs. unselective	**-0.522**	0.124	0.135	0.116	0.164	0.186	0.649
	(0.076)	(0.075)	(0.117)	(0.111)	(0.167)	(0.232)	(0.442)
Black vs. White	**-1.062**	**-0.219**	**-0.633**	**-0.651**	0.110	**0.945**	0.360
	(0.079)	(0.076)	(0.100)	(0.102)	(0.221)	(0.282)	(0.314)
Top four quintiles							
Selective vs. unselective	**-0.669**	0.072	0.163	0.085	0.199	0.220	0.226
	(0.091)	(0.084)	(0.138)	(0.128)	(0.193)	(0.266)	(0.319)
Black vs. White	**-0.681**	0.013	-0.053	-0.095	**0.395**	**0.831**	**0.438**
	(0.049)	(0.040)	(0.057)	(0.054)	(0.095)	(0.136)	(0.127)
Latent, continuous selectivity							
Full Sample							
Black vs. White	**-0.282**	**-0.058**	**-0.168**	**-0.173**	0.029	**0.251**	0.096
	(0.027)	(0.021)	(0.028)	(0.029)	(0.059)	(0.077)	(0.084)
Top four quintiles							
Black vs. White	**-0.241**	0.005	-0.019	-0.034	**0.140**	**0.294**	**0.155**
	(0.017)	(0.014)	(0.020)	(0.019)	(0.034)	(0.048)	(0.046)

Notes: "Binary selectivity" estimates report effect of a switch from an unselective school to a highly selective school, where the latter is defined as the two highly selective clusters. "Latent, continuous selectivity" estimates report effect of a one standard deviation increase in selectivity. Under the assumptions stated in the text, all estimates are local average treatment effects for affirmative action compliers.

Appendix Table A. Comparison with Sander's results for droput

	Tier	First year law school GPA (standardized)	Drop out (logit)		
			Sander (Table 5.6)		
	(1)	(2)	(3)	(4)	(5)
LSAT	0.099	0.038		-0.073	-0.070
	(0.001)	(0.001)		(0.004)	(0.006)
Undergraduate GPA	0.724	0.280		-0.006	0.010
	(0.016)	(0.015)		(0.056)	(0.060)
First year law school GPA (standardized)			-1.395		
			(0.037)		
Tier			-0.343		
			(0.028)		
Black	0.871	-0.706	-0.155	0.223	
	(0.027)	(0.026)	(0.091)	(0.081)	
Bottom quintile					0.027
					(0.078)
Black*bottom quintile					0.281
					(0.090)
Black*top 4 quintiles					-0.032
					(0.181)
Implied AA effect			0.686		

Notes: All models include controls for family income, part-time status, gender, and three racial/ethnic categories (Asian, Other, and Hispanic) whose coefficients are not shown here. Columns 1 and 2 report OLS coefficents; columns 3-5 report logit coefficients.

Appendix Table B. Comparison with Sander Results: Tabular analysis of non-completion

	Non-completion rates				Fraction of BPS respondents in range		
	Whites	Blacks	Black-White	p-value	All	Whites	Blacks
Index range	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Less than 400	0.0%	46.8%	46.8%	0.20	0.3%	0.01%	3%
400 - 460	22.2%	33.1%	10.9%	0.21	0.8%	0.2%	8%
460 - 520	19.7%	25.6%	5.9%	0.16	2%	0.7%	18%
520 - 580	16.4%	21.1%	4.7%	0.08	4%	2%	23%
580 - 640	12.1%	15.4%	3.3%	0.09	9%	7%	20%
640 - 700	9.6%	10.7%	1.1%	0.55	17%	16%	15%
700 +	7.1%	7.5%	0.5%	0.78	67%	74%	13%