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MISMATCH IN LAW SCHOOL

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ABSTRACT

An important criticism of race-based higher education admission preferences is that they may hurt minority students who attend more selective schools than they would in the absence of such preferences. We categorize the non-experimental research designs available for the study of so-called "mismatch" effects and evaluate the likely biases in each. We select two comparisons and use them to examine mismatch effects in law school. We find no evidence of mismatch effects on any students' employment outcomes or on the graduation or bar passage rates of black students with moderate or strong entering credentials. What evidence there is for mismatch comes from less-qualified black students who typically attend second- or third-tier schools. Many of these students would not have been admitted to any law school without preferences, however, and the resulting sample selection prevents strong conclusions.

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I. Introduction

Critics have long argued that the use of affirmative action in college and graduate school admissions harms students from underrepresented groups who are the apparent beneficiaries of admission preferences. These critics claim that students who do not qualify for ordinary admission are in fact inadequately prepared, and would do better—learn more and be more likely to graduate—if they were admitted only to schools better matched to their qualifications (Summers, 1970; Thernstrom and Thernstrom, 1997).[1] Sowell (1978, p. 41), for example, writes that when "Ivy League schools and the leading state and private institutions" use affirmative action, "[t]he net result is that thousands of minority students who would normally qualify for good, non-prestigious colleges where they could succeed are instead enrolled at famous institutions where they fail."

Much of the recent debate about what is known as "the mismatch hypothesis" has focused on law schools. Sander (2004, 2005a,b) finds that large mismatch effects result from affirmative action preferences for black applicants, but subsequent authors, using the same data but different estimation strategies, find no evidence of mismatch (see, e.g., Ayres and Brooks, 2005; Ho, 2005; Chambers, et al, 2005; Barnes, 2007).

In this paper, we use simple, reduced-form strategies to re-examine the evidence regarding mismatch in law school. We find that the data are more informative than the literature to date would suggest. What evidence there is for mismatch comes from the least qualified law students. Identification of mismatch effects is particularly difficult for

[1] The Supreme Court has held repeatedly that affirmative action's relevant benefits are through 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 Holzer and Neumark (2000) for a review of relevant literatures.

these students; in any event, they are unlikely to be admitted to the most selective law schools even with preferences. When we focus on students in the top four quintiles of the entering credentials distribution, we find no evidence for mismatch effects. Even methods that we would expect to overstate mismatch indicate that affirmative action benefits rather than harms black students.

We begin by developing a framework for classifying the empirical strategies used in the literature to date. Three contrasts have been used: between students of the same race and same (observable) admission credentials who attend more- and less-selective schools; between black and white students with the same credentials who, because of affirmative action, typically attend different schools; and between students at the same school who differ in their law school grades.

Each of these contrasts is likely to yield a biased estimate of the mismatch effect – the effect of attending a highly selective school rather than one that is less selective – though the form and magnitude of this bias varies. To sign the likely biases, we consider a simple data generating process, incorporating both observed student credentials and other qualifications considered in admissions but unobserved by the econometrician. The first contrast can be expected to understate the mismatch effect, while the second and especially the third will likely overstate it. As Sander (2004) relies heavily on the third contrast (while invoking the second), and his critics unanimously rely on the first, the predictable biases can explain the divergent pattern of results seen in the literature.

We next select the two strategies – "selective-unselective" and "black-white" comparisons – that we expect to be the most informative, and we implement simple, reduced-form versions of each. Our illustrative data generating process indicates that under reasonable assumptions these will bracket the true effects of mismatch on law school graduation and bar exam passage.[2] While the selective-unselective comparison has been widely used, the black-white comparison is new to the law school literature.[3]

Consistent with earlier work, we find that students attending highly selective schools have better academic and labor market outcomes than equally qualified students attending less selective schools, the opposite sign from that predicted by the mismatch hypothesis. By contrast, black students have lower graduation and bar passage rates (although better employment outcomes) than whites with the same admission credentials. While this result appears consistent with the mismatch hypothesis, further investigation suggests a more nuanced view. The black-white gap is driven entirely by students whose admission credentials place them in the bottom quintile of the law student population, few of whom attend highly selective law schools. Among more qualified students, blacks graduate and pass the bar exam at similar rates to otherwise similar whites. Moreover, for the least-qualified law students, black-white comparisons are subject to an important sample selection bias, deriving from the frequency with which poorly qualified applicants are rejected by even the least selective law schools. As a consequence, results based on the bottom quintile cannot support strong inferences about mismatch. We therefore conclude that the available data provide little evidence regarding mismatch effects on the least qualified students and strongly suggest that mismatch effects are absent for students with moderate or better qualifications.

We emphasize, however, that all of the evidence regarding the mismatch hypothesis derives from observational analyses. Our conclusions rest on unverifiable

[2] We also consider employment outcomes, for which our estimates may not bracket the true effect.
[3] Bowen and Bok (1998) present estimates of this form for undergraduate admissions.

assumptions about the signs of the biases in the comparisons we examine. Because all black students in the last several decades have had access to admission preferences, more robust strategies are unavailable. Research and policy must therefore proceed on the basis of the sort of assumptions – strong, but explicit – that we rely on here.

The paper proceeds as follows: Section II develops our typology of strategies for identifying the effects of school selectivity. Section III presents a simple statistical model illustrating the likely biases in the various strategies. In Section IV, we describe the Bar Passage Study (BPS) data that we use for our analysis. Section V presents estimates of the role of affirmative action in law school admissions. We present empirical result from our two comparisons in Section VI. Section VII concludes.

II. A Typology of Identification Strategies for the Selective School Effect

The mismatch hypothesis is a claim about the effect of attending a selective school, relative to one that is less selective. If selective schools have negative effects on students who are admitted only because of the availability of admission preferences, then the elimination of such preferences would raise these students' outcomes.[4]

The strategies that have been used to isolate the effects of selectivity from those of other potentially confounding variables, particularly the academic credentials that determine admission to selective schools, can be classified into three broad categories.

The first is a simple comparison between students attending more- and less-selective schools, controlling for differences in observed credentials. Kane (1998) and

[4] There are also plausible claims (see, e.g., D'Souza, 1991, and Steele, 1990) that the existence of affirmative action harms black students who would be admitted to selective schools even without preferences by promoting the view that black students are unprepared. On the other hand, if black students are positively affected by the presence of black classmates (as "critical mass" arguments would imply), preferences might help these highly qualified black students. We focus on the partial equilibrium effects of selectivity on individual students, holding other students' schools fixed.

Bowen and Bok (1998) use this type of comparison to examine the effects of attending a selective college, while Chambers et al. (2005), Ho (2005), Ayres and Brooks (2005), and Barnes (2007) use versions of the selective-unselective comparison for law students. None of these studies finds large negative effects of school selectivity.

Selective-unselective comparisons identify the effect of attending a selective school only if the type of school attended is random, conditional on the included control variables. This is unlikely if admission to selective schools depends on variables that are not controlled in the statistical analysis. Accordingly, each of the above analyses controls for students' observed admission credentials, particularly scores on standardized entrance tests. But this may be insufficient: Admission decisions reflect other factors – e.g., letters of recommendation, personal statements, unusual academic and non-academic experiences – that are unobserved by the econometrician and therefore cannot be controlled.[5] If these factors are predictive of later outcomes conditional on observed credentials, the selective-unselective comparison will be biased.

A variant of the selective-unselective comparison focuses on the matriculation decisions of students admitted to several schools, using actual admission decisions to control for both observed and unobserved credentials. Dale and Krueger (2002) compare students attending highly selective colleges with others admitted to these schools but enrolled elsewhere. This eliminates endogeneity due to admission decisions, as the treatment and comparison students are, by construction, equally admissible. It identifies the selectivity effect provided that students' post-admission matriculation decisions are uncorrelated with other determinants – e.g., ambition – of future outcomes. Dale and

[5] Ho (2005) matches on a long list of student background characteristics. Even his list cannot include the unmeasured, non-quantitative admissions credentials that are the most likely source of bias.

Krueger find no effect of school selectivity on average, but find that the effect is positive for low-income students.

Ayres and Brooks (2005) and Sander (2005b) attempt to approximate the Dale and Krueger strategy by comparing law students who report attending their first choice schools with those who say that they are attending their second choices because their first choices were too expensive or too far from home. There is no reason to think, however, that the latter group was – or could have been – admitted to the schools attended by the former group. As a consequence, the "second choice" estimates are likely subject to the same biases as are selective-unselective comparisons.

A second strategy for identifying the effect of selectivity relies on comparisons of black and white students with similar observed credentials. This strategy attempts to isolate exogenous variation by leveraging admission preferences for black students who, because of their preferential treatment, have access to more selective schools than do whites with similar entering credentials.[6] Use of this variation to identify selectivity effects requires school selectivity to be the only source of differences between average black and white outcomes, conditional on observed credentials. Any uncontrolled factors leading to differences in outcomes will confound the selectivity effect.

Sander's (2004) original study combines the black-white comparison with both selective-unselective comparisons and contrasts between students with better and worse grades at the same schools. He uses a black-white comparison to estimate a negative effect of selectivity on law school grades. He then includes both selectivity and grades in equations for graduation and bar passage. Both selectivity and grades have positive

[6] We present some evidence on this below. See also Sander (2004) and Rothstein and Yoon (2008) for law school and Bowen and Bok (1998) and Krueger et al. (2006) for undergraduate admissions.

coefficients, the latter much larger than the former. This leads Sander to conclude that, on net, preferences depress black outcomes, as the large negative effect operating through law school grades swamps the positive effect operating through selectivity.

As Ho (2005) emphasizes, Sander's inclusion of law school grades, an intermediate outcome, in his final equation makes his analysis fundamentally different from the selective-unselective comparison previously discussed. Identification of the causal effect of grades requires that the unobserved determinants of law school grades and later outcomes are completely uncorrelated. Unmeasured academic ability is an obvious omitted variable. High ability students most likely earn higher grades and better long-term outcomes than low ability students, even conditional on entering credentials. As a result, Sander's analysis can be expected to substantially overstate the mismatch effect.

While this is an important drawback, Sander's essential logic, that the mismatch hypothesis implies that "blacks have much higher failure rates on the bar than do whites with similar LSAT scores and undergraduate GPAs" (Sander, 2004, p. 373), is sound. This claim can be evaluated via simple, reduced-form black-white comparisons. One important advantage of the reduced-form strategy is that the required assumptions are transparent and much weaker than those needed for Sander's structured approach. Nevertheless, they too may be violated; we discuss the likely consequences of this in the next section. A second advantage is that the black-white comparison does not require a selectivity measure. The law school data contain only a crude proxy for school selectivity, seriously limiting selective-unselective comparisons.

III. A Simple Statistical Model

To fix ideas, we consider a simple model relating law school outcomes to entering credentials, race, and the selectivity of the school attended. Let y_i be the outcome for student i, generated by:

$$y_i = \alpha + X_i \beta_X + Z_i \beta_Z + b_i \gamma + s_i \theta + \varepsilon_i. \quad (1)$$

[7]

Here, X_i and Z_i are the student credentials that are considered by a selective school's admission office. X is also observed by the econometrician, but Z is not. b_i is an indicator variable for being black. β_X, β_Z, and γ are projection coefficients, capturing the predictive power of credentials and race for student outcomes rather than the causal effects. In particular, γ captures both the direct effect of student race on outcomes and any systematic difference in unobserved (by the admission office) credentials between black and white students. s_i is a measure of school selectivity, with higher values corresponding to more selective schools. θ is the causal effect of attending a selective school, relative to a less selective school, on student outcomes.

ε is an error term, encompassing the portion of student ability that is not observed in admissions as well as any post-admissions shocks to student outcomes. By construction, ε is orthogonal to X, Z, and b in the population of law school applicants and is uncorrelated with the admission decisions of selective schools. However, students' decisions to accept or decline admission offers at selective schools may be endogenous. This may produce a correlation between ε and s, conditional on the other variables.

As written, (1) assumes that θ is constant across students. A realistic version of the mismatch hypothesis requires heterogeneity in the effect of selective schools, which

[7] As written, we assume that outcomes are linear in the predictor variables. In our empirical analysis, we use probit models for binary outcomes; y can be seen as the underlying latent variable.

might have positive effects on well-prepared (high X and Z) students but negative effects on underprepared students. We return to the potential heterogeneity of θ below.

A. Selective-unselective comparisons

Using (1), the difference between the mean outcomes of students attending selective and unselective schools conditional on race and observed credentials is:

$$(2) \quad D_s(b, X) \equiv E[y \mid b, X, s = 1] - E[y \mid b, X, s = 0]$$

$$= \theta + E[Z\beta_Z \mid b, X, s = 1] - E[Z\beta_Z \mid b, X, s = 0]$$

$$+ E[\gamma \mid b, X, s = 1] - E[\gamma \mid b, X, s = 0].$$

θ is the selectivity effect. The remaining terms represent potential biases.

The first bias term, $E[Z\beta_Z \mid b, X, s = 1] - E[Z\beta_Z \mid b, X, s = 0]$, derives from the role of unobserved credentials in determining admission to selective schools.[8] Holding X and b constant, the probability of admission to a selective school is increasing in $Z\beta_Z$. This creates a positive partial correlation between s and $Z\beta_Z$, biasing $D_s(b, X)$ upward.

The second bias term, $E[\varepsilon \mid b, X, s = 1] - E[\varepsilon \mid b, X, s = 0]$, derives from the matriculation decisions of students admitted to selective schools. If students with high unobserved (to the admission office) ability are more likely to take up offers of admission at selective schools, this bias is positive as well.

Thus, we expect that the total bias in $D_s(b, X)$ is positive. Analyses that exploit cross-sectional variation in selectivity without isolating an exogenous component are likely to overstate the selectivity effect. A similar bias applies to tests based on $\partial D_s(b,$

[8] This term could also reflect matriculation decisions, if correlated with Z conditional on X.

X)/ ∂X), as in Barnes (2007). There is every reason to expect that the bias terms in (2) will vary with X, providing evidence of mismatch even if θ is identically 0.[9]

Dale and Krueger's (2002) comparison of students attending selective schools with students who declined admission to those schools (discussed above) plausibly eliminates the first bias term in (2). The second bias term may remain, as Dale and Krueger note, if matriculation decisions are correlated with unobserved ability.

Unfortunately, in the available data it is impossible to know whether students at less selective schools who do not attend their first choice schools for reasons of cost or distance – the comparison group for selective school students in the second-choice analyses of Ayres and Brooks (2005) and Sander (2005b) – were in fact admitted to selective schools. If they were not, the first bias term in (2) persists in the second-choice comparison. There is also reason to expect the second bias term in (2) to be important in this comparison. Students in the second-choice sample are systematically poorer than those in the first-choice sample and have less-educated parents—both factors that may be correlated with ε.

B. Between-race comparisons

Black students, by virtue of their access to affirmative action preferences, are admitted to more selective schools than are white students with otherwise identical admission credentials. If school selectivity is harmful, this should be apparent in the reduced-form black-white gap in outcomes conditional on observed credentials:

$$(3) \qquad D_b(X) \equiv E[y \mid b = 1, X] - E[y \mid b = 0, X]$$
$$= \theta\,(E[s \mid b=1, X] - E[s \mid b = 0, X])$$

[9] If X and Z are bivariate normal, for example the first bias term will contain the expression $\lambda(a+cX) + \lambda(-a-cX)$, where a and c are constants and $\lambda()$ is the inverse Mills ratio. This varies with X.

$$+ E[Z\beta_Z \mid b = 1, X] - E[Z\beta_Z \mid b = 0, X] + \gamma.$$

The first term in (3) is the selectivity effect of interest, θ, multiplied by the difference in average selectivity between black and white students conditional on X. The availability of admission preferences for black students ensures that this is positive, and indeed we demonstrate below that it is substantial. Ignoring the remaining terms for the moment, evidence that $D_b(X) < 0$ can therefore be taken as support for the mismatch hypothesis.

Equation (3) shows two biases that might confound the test. These biases are different than those in the selective-unselective comparison and likely work in the opposite direction. The first, $E[Z\beta_Z \mid b = 1, X] - E[Z\beta_Z \mid b = 0, X]$, reflects differences in unobserved (to the econometrician) admission qualifications between black and white students. This is almost certainly negative—average LSAT scores are lower among black students than among white students with the same college GPAs (and vice versa), and it stands to reason that average *unobserved* admission credentials are lower among blacks than whites conditional on observables.

The final term in (3), γ, is the predictive effect of student race within schools, combining the causal effect of race and any difference in unobserved (by the admission office) preparedness between black and white students. The latter is likely negative, by the previous argument. The sign of the causal effect probably depends on the outcome measure used.[10] When y is an employment outcome, the use of affirmative action in hiring plausibly produces a positive γ. When y is an academic outcome, however, explicit and implicit discrimination seem likely to have negative effects (Dauber, 2005).

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[10] To be clear, we do not attempt to estimate the causal effect of race. Because race is not subject to manipulation, its causal effect cannot be identified (and is arguably not even well defined; see Holland, 1986). This does not prevent the black-white comparison from being *informative* about the causal effect of interest, θ, though subject to other potentially confounding influences.

Combining the two confounding factors, the net bias in $D_b(X)$ is likely negative for academic outcomes. This conclusion is supported by research on the prediction of college grades (e.g. Rothstein, 2004; Young, 2001), which generally indicates that white college students outperform black students with the same observed admission credentials at the same colleges. Similar pattern have been found in law schools (Wightman, 2000; Wightman and Muller, 1990; Anthony and Liu, 2003; and Powers, 1977). The net bias in analyses of employment outcomes is more difficult to sign, and could plausibly be either positive or negative.

An additional bias in the black-white comparison is not captured by (3). Even the least selective law schools reject many applicants and many poorly qualified would-be law students are not admitted anywhere. This is much more common for whites than for similarly-credentialed blacks. Thus, even if Z is balanced across races conditional on X among applicants, the admission process truncates the Z distribution, more so for whites than for blacks.[11] Black-white comparisons based only on matriculants will find larger gaps – and therefore more apparent evidence for mismatch – than would be observed if outcomes were measured for the full population of applicants. This bias will be concentrated at X values where non-admission probabilities are relatively high.

C. Heterogeneous effects of school selectivity

In equation (1), the effect of attending a selective school is constant. This permits only a simplistic version of the mismatch hypothesis, $\theta < 0$. A more realistic assumption is that the selectivity effect is heterogeneous. The mismatch hypothesis is then the claim

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[11] Specifically, assume that a student is admitted to some law school only if $X_i \delta_X + Z_i \delta_Z > c(b_i)$, where $\delta_X, \delta_Z > 0$ and $c(b_i)$ is a race-specific constant with $c(0) > c(1)$. Then $E[Z \mid X, b, \text{admitted to some school}] = E[Z \mid Z > \delta_Z^{-1}(c(b) - X\delta_X)]$. If $corr(X, Z) > 0$, this is decreasing in b, particularly at X values for which the admission constraint is most binding.

that θ_i is negative, on average, for students admitted to selective schools only via affirmative action preferences.

This does not fundamentally alter the analysis. With heterogeneous treatment effects, θ is replaced in (2) with the mean of θ_i among students attending selective schools. The logic of mismatch implies that this should be larger than the average effect in the population of affirmative action beneficiaries, producing yet another upward bias on selective-unselective comparisons. Equation (3) is more complex. In an earlier version of this paper (Rothstein and Yoon, 2007), we showed that (3) can be seen as the reduced form for an instrumental estimator in which b_i is used as an instrument for s_i. This implies that the relevant mean in (3) is the local average of θ_i among black students attending selective schools who would have attended unselective schools had they been admitted as white students. These students are precisely the population of interest for the mismatch hypothesis; if it holds, the first term of (3) should be negative.

IV. Data

The data set used for all studies to date examining mismatch in law school is the Law School Admission Council's (LSAC) Bar Passage Study (BPS; Wightman 1998, 1999), a census of students matriculating at accredited law schools in fall 1991. The BPS contains information on over 27,000 students, about 62 percent of the 1991 cohort.[12] Variables include LSAT scores, college GPAs, and measures of law school performance and bar exam outcomes. A subsample was chosen to receive a follow-up survey about employment outcomes four to six months after graduation.

[12] Most non-response was individual: 163 of 172 accredited law schools participated in the study. Entering questionnaire response rates for blacks and whites were 59% and 62%, respectively. We have found no indication that non-response differs systematically by entering credentials.

Summary statistics are reported in the first two columns of Table 1. We focus on the 24,049 black and white students with valid data on entering credentials, of whom 7.6 percent are black. We present means by race in Columns 3 and 4, and by race and selectivity (as defined below) in Columns 5 – 8.

Our two X variables are the LSAT score and the undergraduate grade point average (UGPA). LSAT scores range from 10 to 48, with mean 36.8 and standard deviation 5.5. The UGPA, computed from student transcripts, ranges from 1.5 to 4.0, with an A grade corresponding to a 4.0, a B to a 3.0, etc. For graphical analyses, we form an index, using weights of 0.4 and 0.6 on the standardized UGPA and LSAT, respectively,[13] then convert this index to a percentile score based on the distribution within our sample. The black-white gaps in LSAT scores and UGPAs in our sample are -1.59 and -0.96 standard deviations, respectively, while the gap in index percentiles is -40 (corresponding to a gap of -1.69 standard deviations in the index itself). Figure 1 displays the cumulative distribution of percentile scores among black and white students.

For confidentiality reasons, the BPS groups law schools into six "clusters" based on size, cost, selectivity, tuition level, and minority representation. We focus on a dichotomous categorization, treating the "Elite" and "Public Ivy" (Wightman 1993) clusters as highly selective (s=1) and the remaining clusters – which overlap substantially in the credentials of their students and have relatively similar admission rates, so provide little information about school selectivity – as less selective (s=0).[14] 24% of BPS students attend highly selective schools. Within each race, students at the most selective

[13] These weights are taken from Sander (2004). They are nearly identical to the weights that best predict attendance at a highly selective school, as defined below.

[14] We have also conducted our selective-unselective comparisons across all six clusters, with similar results to those presented below.

schools have much better credentials than students at less selective schools, but the between-race difference in the probability of attending a highly selective school is small.

We consider several categories of outcomes. First, we examine performance during the first year of law school, when curricula are typically standardized and grades are issued on strict curves. First year grades are important determinants of access to prestigious internships and post-graduation clerkships. The BPS grades measure is standardized within law schools. We view it as a purely relative measure, and convert it to class rank (ranging from 0 at the bottom to 1 at the top) under the assumption that GPAs are normally distributed within each school. The average black student is at the 23rd percentile of his or her class and the average white student is at the 54th percentile.

Our second group of student outcome measures has to do with law school graduation and bar exam success. We form a simple indicator for graduation; the BPS permits us to track ultimate graduation even for the few students who transfer schools. Bar passage is somewhat more complex, as some graduates – those who do not plan to practice law – never sit for the exam. We focus on a measure that excludes non-takers.[15]

Our final category of outcome measures concerns post-law school labor market experiences. Few non-graduates responded to the BPS follow-up survey, so we restrict our attention to graduates. We construct three measures: an indicator for full time employment; an indicator for job quality; and the log annual salary. Our job quality measure is based on a subjective classification of jobs into prestigious – clerkships, professorships, large law firms, etc. – and non-prestigious groups. For the job quality and

[15] We count students who did not graduate from law school as failures. A back-of-the-envelope calculation suggests that about 3% of the graduates that we exclude as non-takers in fact failed the exam in one of the 14 states that do not report failed attempts. We discuss below several alternative measures that vary in their treatment of non-takers. Our results are consistent across measures.

salary measures, we restrict attention to respondents with full-time jobs. The sample size for the employment analyses is 3,144, of whom two-thirds had full-time jobs.

Each of the outcome measures has advantages and disadvantages for our purposes. Academic performance within school is most directly tied to mismatch, as a student who struggles to keep up with his or her classmates will earn poorer grades. On the other hand, class rank may reflect mechanical effects of selectivity: the same absolute performance will produce a lower rank at a more selective school simply because the competition is stiffer. We therefore interpret our class rank analyses as primarily measuring the degree to which students are mismatched relative to their classmates, rather than the effects of mismatch. Graduation is a much more absolute measure, though the threshold may vary somewhat across schools.

Bar exams use blind graded and are administered by state bar associations, so in principle there should be no effects of race or of school quality other than those operating through student achievement. However, students choose where to take the exam and the state-specific component of the exam varies in difficulty. The BPS does not report the state where the student took the exam.[16] We expect that selective school students are more likely to take the exam in states with reputations for more difficult exams (e.g. California and New York), which also tend to have larger, more prestigious legal labor markets. If so, selective-unselective comparisons will overstate mismatch effects on bar passage. It is difficult to sign the effect of endogenous state selection on the black-white comparison, although we expect that any such effect is small.

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[16] The BPS does report the *region* in which the exam was taken, though this is a poor proxy for difficulty. Our results are not sensitive to controlling for this.

The most important drawback to our employment outcomes is that they may not be race-blind measures of academic success if employers prefer black job applicants or applicants from elite schools. These will bias both of our comparisons against the mismatch hypothesis.[17] Thus, while we expect the black-white comparison to overstate mismatch effects on graduation and bar passage, this expectation may not hold for employment outcomes.

V. Measuring Preferences & Mismatch

Figure 3 displays the fractions of white and black students in the BPS sample who attend schools in the two highly selective clusters, as functions of the admission index percentile. These are computed from locally linear regressions; dashed lines show pointwise 90% confidence intervals. Throughout the index distribution, black students are much more likely to attend highly selective schools than white students.[18]

The first two columns of Table 2 show probit models for attendance at a highly selective school. The main table shows coefficients; the bottom row shows the implied effect of being black on the probability of attending a highly-selective school, averaged over the covariate variable distribution for black students in the sample. Column 1 includes quadratic controls for LSAT scores and UGPAs, as well as a linear interaction. The black coefficient is large and positive, indicating that blacks are, on average, 16

[17] It is not clear that preferences in employment should be discounted entirely. If law firms are competitive profit-maximizers, a black salary premium would indicate that black lawyers have higher marginal revenue products. A school that hopes to maximize its graduates' productivity should then cater to firm preferences by itself practicing affirmative action. The black-white gap in employment outcomes can be interpreted as a measure of the combined effect of admissions and hiring preferences.

[18] The curves in Figure 3 are flattened by the heterogeneity of selectivity within our "highly selective" category. Sander (2004, Figure 2.8) shows that the probability of admission to the University of Michigan Law School is nearly a step function in the admissions index, with approximately the same leftward shift in the curve for blacks that is seen in Figure 3. This suggests that the lowest-credentialed black and white students in the highly selective BPS clusters probably attend the least selective schools in these clusters.

percentage points more likely to attend highly selective schools than whites with similar credentials. This effect is robust to the inclusion of controls for 15 variables measured at law school entrance, including work experience and several family background measures (Column 2) and for higher-order terms in the observed credentials (not shown).

There are no completely unselective law schools, and only 56 percent of the 92,648 applicants from the BPS cohort were admitted to any law school (Barnes and Carr, 1992; see also Wightman, 1997). The remaining 44 percent are absent from our data. Figure 2 relates the probability of being admitted to at least one school to the admission index percentile, using data on applicants and admissions classified by race, LSAT, and UGPA cells (from Barnes and Carr, 1992). White students whose credentials would have placed them in the bottom quarter of the matriculant distribution were more likely than not to be rejected from all the schools where they applied. Conversely, black admission rates were above 50 percent in every cell above the fifth percentile and were at least double those of similarly-qualified whites through the lower part of the distribution. Partly as a result of this gap in admission rates, blacks are dramatically overrepresented in the left tail of the index distribution of law school matriculants, and about three quarters of black students in the BPS sample are in the bottom quintile.

A likely explanation for the gap in admission rates is that even the least selective schools apply lower thresholds for admission to black than to white applicants.[19] As discussed earlier, this will bias black-white comparisons against black students,

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[19] This contrasts with undergraduate education where, as Kane (1998) notes, only the most selective colleges appear to practice affirmative action. Note that the outcome depicted in Figure 2 is endogenous to application decisions – for example, a student who applies only to Yale Law School might be admitted nowhere even if she would have been admitted to a less selective school. This probably accounts for the non-trivial rates at which even highly qualified students are admitted nowhere.

particularly at low index percentiles where the gap in admission probabilities is the greatest. When we restrict our sample to students in the top four quintiles of the admission index distribution, in Columns 3-4 of Table 2, the black-white difference in the probability of attending a highly selective school nearly doubles.

Figure 4 presents mean first year class ranks as functions of race, school type, and entering credentials. More qualified students have higher ranks than those with lower index scores, and students at less selective schools have higher ranks than similarly-qualified, same-race students at more selective schools. Controlling for selectivity, white students achieve much higher ranks than blacks. As the black-white comparison relies on the assumption that blacks and whites would achieve similar outcomes if they attended the same schools, this result supports our contention that differences in unobserved ability or direct race effects bias the black-white comparison in favor of mismatch.

Table 3 presents regression estimates for class rank. The first two columns show selective-unselective comparisons separately for whites and blacks, again controlling for quadratics in (LSAT, UGPA). Specifications that control for the full set of covariates from Table 2, Column 2, are similar. Attending a highly selective school lowers rank by about 0.06 for whites and by twice that for blacks. Effects on blacks in the top four quintiles, shown in Column 4, are even larger.

Columns 5 and 6 present the black-white comparison for the full sample and for students in the top four quintiles. Because black students attend more selective schools, with stronger students, than do white students with the same entering credentials, we expect negative black coefficients. Indeed, black students have ranks about 0.19 lower than similarly-qualified whites. This gap grows to -0.23 in the upper four quintiles.

VI. Results

Table 2 indicates large differences in the selectivity of the schools attended by black and white students with similar entering credentials. This difference in selectivity occurs across the total sample of students, but is largest in the subsample excluding the bottom quintile of the credentials distribution. Table 3 indicates that each of our comparisons exploits substantial differences in the degree to which students are mismatched during law school, as measured by rank in class.

If mismatch lowers post-law school outcomes for marginal students, both the selective-unselective and black-white comparisons should show negative effects on these outcomes. Figure 5 repeats the estimates from Figure 4, this time for bar exam passage rates. While Figure 4 indicated large selective-unselective and black-white gaps in class rank, no selective-unselective gap is apparent in Figure 5, and the black-white gap is relatively small and concentrated at the lowest percentile scores.

Table 4 reports selective-unselective comparisons for each of our outcomes, with controls for a quadratic in (LSAT, UGPA). For binary outcomes, we show both probit coefficients and marginal effects averaged over the treated sample (in square brackets).

Consistent with Figure 4, the estimates offer no indication of mismatch effects. For white students (Columns 1-2), the selectivity effect is positive and significant on four of our five outcomes, with an insignificant negative effect for full-time employment. The estimated effects for black students (Columns 3-4) are positive and significant for graduation and salaries; all others are statistically insignificant (though the employment point estimate is positive and large). Columns 5 and 6 report p-values for tests of the hypotheses that the white and black effects are equal or are both zero. We (marginally) reject equality in only one case, with a large positive effect on bar passage for whites and a negligible effect for blacks. In contrast, we reject zero effects in four of five cases.

Table 5 presents our black-white comparison. Considering first the full sample, in Columns 1-2, we find that black students have significantly lower graduation and bar passage rates than similarly-qualified whites. Point estimates indicate nearly a ten percentage point average effect on bar passage. Since black students attend more selective schools than do whites with the same credentials, these estimates are consistent with negative selectivity effects. By contrast, the black effects on employment outcomes are positive and in two cases are large and significant.

As discussed earlier, comparisons based on students with very poor credentials are subject to sample selection bias deriving from the comparatively high rates at which white applicants with these credentials are denied admission to any law school. This bias is likely less severe in estimates based on the top four quintiles of the entering credentials distribution, where large majorities of both white and black applicants are admitted to at least one school. (Recall from Tables 2 and 3 that affirmative action preferences are just as strong and black students are just as likely to be mismatched relative to their classmates in this subsample.) Columns 3-4 of Table 5 show black-white comparisons for the subsample of top-quintile students. All of the point estimates are notably more positive than in Column 1. The only negative coefficient is small and statistically insignificant, indicating only a 2.8 percentage point shortfall in black bar passage rates relative to similarly-qualified whites.

We have explored several alternative specifications for both the selective-unselective and black-white comparisons. Our results are robust to semiparametric controls (implemented via matching techniques) for the LSAT score and undergraduate GPA, and to the inclusion of controls for the student characteristics used in Column 2 of Table 2. We also varied the definitions of our dependent variables. For example, we tried coding students who did not attempt the bar exam as failures or successes, rather than excluding them as in our main sample; counting part-time workers as employed; and excluding students with high-prestige but low-salary jobs (e.g. clerkships) from our analyses of salaries.[20] In each case, results were qualitatively unchanged.

VII. Conclusion

The most convincing test of the mismatch hypothesis would require random assignment of students to more- and less-selective schools. Neither this sort of experiment nor a convincing natural experiment is available. Accordingly, research and policymaking must proceed from non-experimental analyses that are identified only via assumptions about counterfactual outcomes.

The Bar Passage Study data are well suited for non-experimental analyses. By focusing on two simple reduced-form comparisons, we have shown that the data speak clearly about the mismatch hypothesis as it applies to students with credentials in the top four quintiles of law school matriculants. Neither selective-unselective nor black-white comparisons offer any evidence for mismatch effects on these students. As the most selective schools admit almost exclusively from this subpopulation, we conclude that the use of affirmative action at these schools does not generate meaningful mismatch effects.

[20] We explored models treating clerkships as an endogenous source of sample selection, using variables measuring students' preferences across job types as stated during the first year of law school as determinants of selection not directly affecting salaries. This had no effect on the results. In another specification, we modeled taking the bar exam as endogenous. We were unable to estimate the selection coefficient in our model for bar passage with any precision, largely because we lack plausible instruments for selection on this margin.

We similarly find no evidence of mismatch effects on employment outcomes in any portion of the distribution. Black students are much more likely to obtain good jobs than are similarly-qualified white students, with a salary premium around 10-15 percent. This finding might reflect affirmative action on the part of employers. A crucial question is whether firms' hiring patterns would change if law schools eliminated affirmative action. If, in its absence, high-salary firms would recruit from less selective schools to obtain black lawyers, the observed black salary premium might persist. Thus, our analysis does not definitively indicate that affirmative action in law school admission *helps* black students after law school, as the benefit may derive from affirmative action in employment. It only demonstrates that the combined effect is positive.

Our analysis of graduation and bar passage outcomes of students with bottom-quintile credentials yields murkier results. In this subpopulation, which contains the majority of black law students, the black-white comparison is consistent with the presence of mismatch effects deriving from the use of affirmative action by mid-ranked schools to admit students who would otherwise attend the least selective schools. But we cannot rule out an alternative explanation—namely, that the observed black-white gap simply reflects sample selection bias. Many bottom-quintile applicants are unable to gain admission to *any* law school. As a consequence of the least selective schools' use of affirmative action, this outcome is much more likely for white than for black applicants. If the unobserved qualifications (e.g., personal statement, references, employment history) influencing admission decisions are predictive of later outcomes, the resulting sample selection could well produce the observed black-white gaps.

How predictive would the admission variables have to be of later outcomes in order to account for the observed data without mismatch? We estimate that a correlation of 0.25 between the unobserved determinants of admission and graduation would fully explain the black-white gap observed among bottom-quintile students without recourse to mismatch-based explanations.[21] A correlation of this magnitude can by no means be rejected out of hand. Thus, without direct evidence about the selection into law school, the data do not permit strong conclusions about the existence of mismatch effects on the least qualified students' graduation and bar passage rates.

Even granting this limitation, however, it is possible to comment on magnitudes. In a companion paper (Rothstein and Yoon, 2008; see also Ayres and Brooks, 2005), we show that even if the entire black-white gap were attributed to mismatch – that is, even if sample selection were ignored – the implied effects of mismatch on black students' graduation and bar passage probabilities would be dwarfed by the positive effects of preferences on the number of black students admitted to law school. As a consequence, the only result consistent with the data is that the net effect of affirmative action is to dramatically increase the number of black lawyers.

[21] The calculation resembles that used by Altonji, Elder, and Taber (2005). We simulated data with bivariate normal errors in equations for the latent determinants of selection and graduation, assuming no black-white gap in graduation propensity in the population, then imposed the selection rule. With $\rho = 0.25$, the simulated sample selection bias equaled the observed black-white gap in graduation rates.

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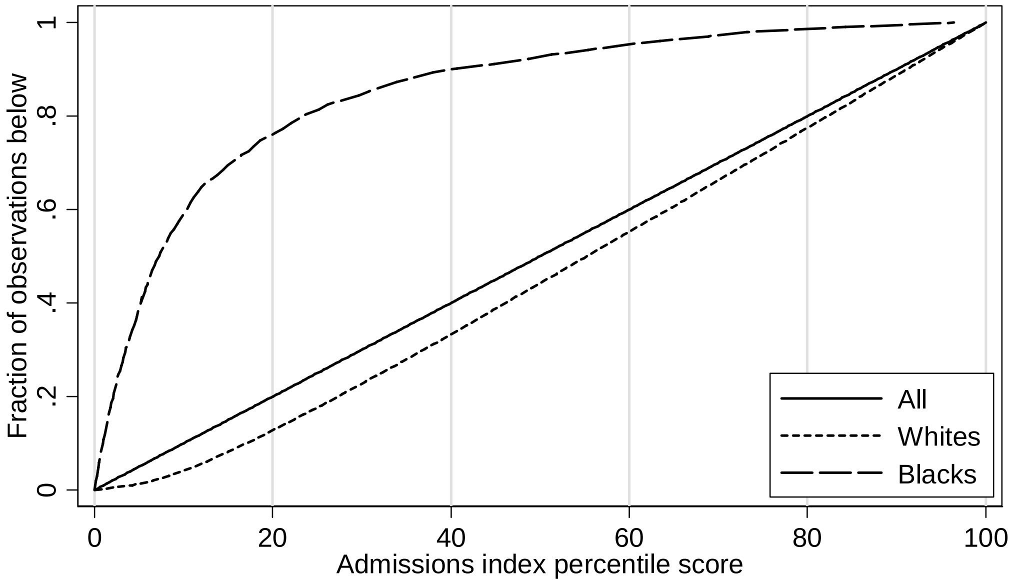
Figure 1. Distribution of admission index percentile scores for black and white matriculants



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

Figure 2. Fraction of applicants admitted to at least one school, by race and index percentile

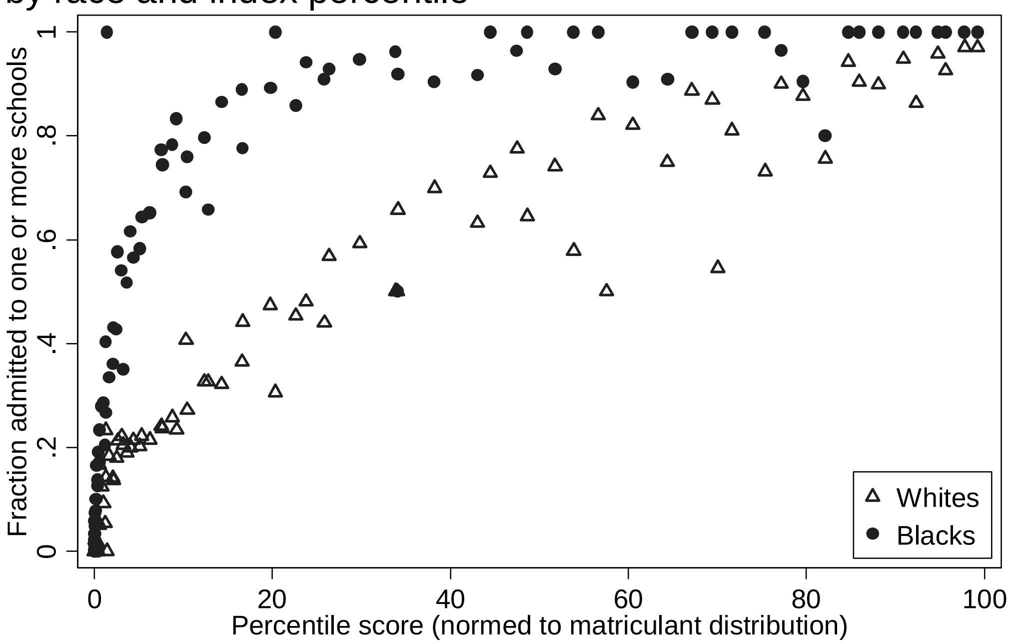
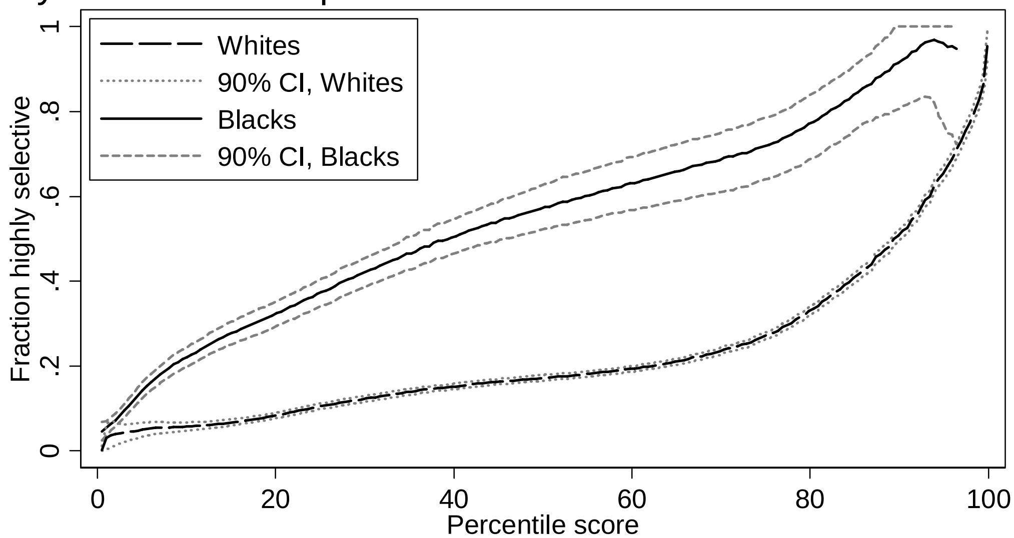
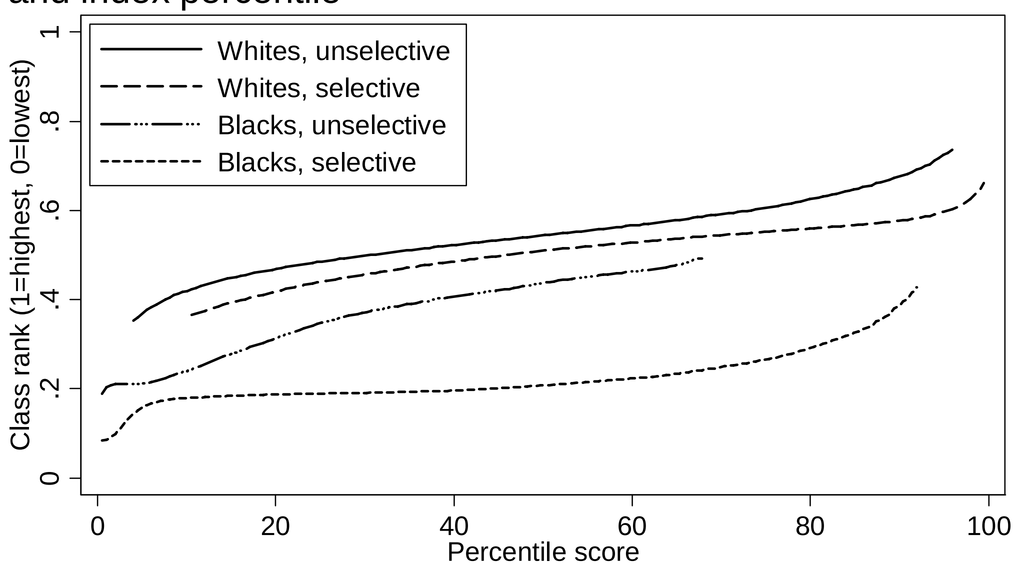


Figure 3. Fraction attending highly selective law schools, by race and index percentile



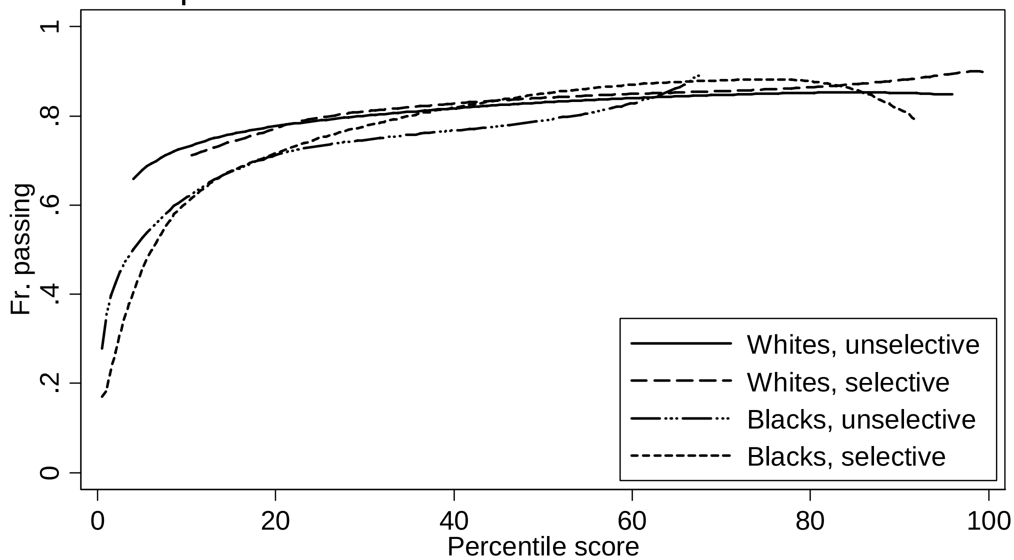
Note: Fractions are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admission index. 90 percent pointwise confidence intervals are computed by bootstrap with 500 replications. Estimates and CIs are censored at 0 and 1, and the series are reported only for the range spanned by each race subsample.

Figure 4. First year class rank by race, law school selectivity, and index percentile



Note: Ranks are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admission index. Estimates are shown only for points between the 1st and 99th percentiles of each subpopulation admission index distribution.

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



Note: Rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admission index. Estimates are shown only for points between the 1st and 99th percentiles of each subpopulation admission index distribution.

Table 1. Summary statistics

	Full sample		By race		By race and selectivity			
					Blacks		Whites	
	Mean	S.D.	Blacks	Whites	Sel.	Unsel.	Sel.	Unsel.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
N	24,049		1,836	22,213	419	1,417	5,417	16,796
Black	7.6%	0.266	100%	0%	100%	100%	0%	0%
Female	43.7%	0.496	59.5%	42.4%	60.1%	59.3%	43.2%	42.2%
Admissions credentials								
LSAT	36.8	5.5	28.8	37.5	32.7	27.6	40.5	36.5
UGPA	3.24	0.42	2.87	3.27	3.04	2.82	3.43	3.21
Admissions index	747	105	583	761	662	559	824	740
Admissions index %ile	51.6	28.4	14.7	54.6	29.3	10.4	72.2	48.9
Law school type								
Selective (top 2 clusters)	24%	0.429	23%	24%	100%	0%	100%	0%
Elite (top cluster)	8%	0.271	8%	8%	35%	0%	33%	0%
Outcomes								
1st year LGPA	0.06	0.98	-1.01	0.15	-1.15	-0.97	0.17	0.14
1st year class rank (est.)	0.52	0.29	0.23	0.54	0.19	0.24	0.55	0.54
Graduated from law school?	91%	29%	81%	92%	90%	78%	95%	91%
Ever pass bar exam?	81%	39%	57%	83%	69%	53%	86%	82%
Ever pass bar (if attempted)?	86%	35%	61%	88%	75%	57%	93%	87%
Empl. full time (if grad.)	66%	47%	63%	67%	74%	60%	70%	65%
"Good" job (if employed)	40%	49%	44%	40%	53%	40%	58%	34%
Salary (if FT; $1,000s)	$39.8	$18.8	$38.0	$40.0	$47.3	$34.5	$49.1	$36.8
Log salary (if FT)	10.51	0.47	10.44	10.51	10.66	10.35	10.72	10.44

Table 2. Black-white differences in selectivity

	Full sample		Top 4 quintiles	
	(1)	(2)	(3)	(4)
Black	0.858	0.888	1.050	1.064
	(0.045)	(0.046)	(0.063)	(0.064)
LSAT	-0.175	-0.160	-0.631	-0.608
	(0.021)	(0.021)	(0.051)	(0.051)
$(LSAT/100)^2$	16.014	14.466	51.051	48.378
	(2.607)	(2.635)	(5.167)	(5.207)
UGPA	-0.387	-0.469	-3.061	-3.204
	(0.322)	(0.325)	(0.517)	(0.520)
$(UGPA/10)^2$	-11.618	-8.719	-3.082	0.926
	(4.943)	(4.989)	(6.034)	(6.081)
LSAT * UGPA	0.048	0.046	0.100	0.099
	(0.005)	(0.005)	(0.008)	(0.008)
Additional controls	n	y	n	y
Average effect of "black" on probability	0.162	0.164	0.350	0.348

Notes: The dependent variable is an indicator for attending a school in the "elite" and "public ivy" clusters. N=24,049 in full sample (Cols. 1-2), 19,806 in subsample (Cols. 3-4). The table reports probit coefficients and standard errors. The final row shows the increment in the probability of attending a highly selective school associated with being black, averaged over the black students in the sample. Additional controls in Columns 2 and 4 are gender; age (in months) at law school entry and its square; mother's and father's education (plus indicators for missing values); and indicators for disability/handicap, for speaking English as a second language, for taking more than one year off after college, for working full-time for 2 or more years, for legal work experience, for working for pay as an undergraduate, for a father with a white-collar occupation, and for a mother employed outside the home.

Table 3. Selective-unselective and black-white comparisons for first year class rank

	Selective-unselective comparison				Black-white comparison	
	Full sample		Top 4 quintiles		Full sample	Top 4 quintiles
	Whites	Blacks	Whites	Blacks		
	(1)	(2)	(3)	(4)	(5)	(6)
Selective	-0.060	-0.116	-0.060	-0.211		
	(0.005)	(0.015)	(0.005)	(0.026)		
Black					-0.189	-0.226
					(0.008)	(0.013)
LSAT	0.033	-0.015	0.023	0.005	0.019	0.039
	(0.005)	(0.009)	(0.010)	(0.089)	(0.004)	(0.009)
$(LSAT/100)^2$	-1.435	2.963	0.429	-3.628	-0.207	-1.006
	(0.600)	(1.198)	(0.997)	(8.310)	(0.458)	(0.979)
UGPA	-0.193	-0.318	-0.170	-1.273	-0.244	-0.115
	(0.064)	(0.161)	(0.098)	(0.794)	(0.055)	(0.096)
$(UGPA/10)^2$	6.269	4.940	6.835	15.533	6.379	6.888
	(0.916)	(2.568)	(1.124)	(7.410)	(0.854)	(1.106)
LSAT * UGPA	-0.003	0.003	-0.004	0.012	-0.002	-0.006
	(0.001)	(0.002)	(0.001)	(0.012)	(0.001)	(0.001)
N	20,485	1,698	17,854	412	22,183	18,266

Note: Table reports coefficients from OLS regressions. Standard errors are in parentheses.

Table 4. Selective-unselective comparisons for post-law school outcomes

	Whites		Blacks		p values for hypothesis tests	
		N		**N**	**Both equal**	**Both zero**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Law school graduation	**0.235**	22,081	**0.233**	1,809	0.987	0.000
	(0.035)		(0.102)			
	[0.028]		[0.045]			
Bar passage (if attempted)	**0.155**	20,862	-0.002	1,705	0.099	0.000
	(0.032)		(0.089)			
	[0.024]		[-0.000]			
Employment						
Has a full-time job	-0.111	2,306	0.154	838	0.261	0.172
	(0.071)		(0.121)			
	[-0.035]		[0.050]			
"Good" job, if FT employe	**0.278**	1,532	0.050	537	0.409	0.001
	(0.085)		(0.140)			
	[0.103]		[0.018]			
Ln(salary), if FT employed	**0.153**	1,501	**0.227**	528	0.442	0.000
	(0.030)		(0.053)			

Notes: Reported coefficients are for a selective school indicator in OLS (ln(salary)) and probit (other outcomes) specifications. Each specification controls for a quadratic in (LSAT, UGPA). Analyses of employment outcomes use sampling weights. Standard errors are in parentheses. Bold coefficients are significant at the 5% level. Marginal effects, in square brackets, are the change in probability due to attending a selective school, averaged over all selective=1 observations. Tests of equal/zero coefficients are computed from pooled, fully interacted specifications.

Table 5. Black-white comparisons

	Full sample		Top four quintiles	
		N		**N**
	(1)	**(2)**	**(3)**	**(4)**
Law school graduation	**-0.144**	23,890	0.031	19,699
	(0.046)		(0.089)	
	[-0.036]		[0.005]	
Bar passage (if attempted)	**-0.287**	22,567	-0.122	18,615
	(0.042)		(0.076)	
	[-0.095]		[-0.028]	
Employment				
Has a full-time job	0.130	3,144	**0.408**	2,294
	(0.105)		(0.189)	
	[0.049]		[0.143]	
"Good" job, if employed	**0.576**	2,069	**0.759**	1,555
	(0.129)		(0.202)	
	[0.201]		[0.287]	
Ln(salary), if FT employed	**0.100**	2,029	**0.157**	1,525
	(0.045)		(0.071)	

Notes: Reported coefficients are for the black indicator in probit & OLS specifications. All specifications include controls for a quadratic in (LSAT, UGPA). Analyses of employment outcomes use sampling weights. Standard errors are in parentheses. Bold coefficients are significant at the 5% level. Marginal effects, in square brackets, are the change in probability from black=0 to black=1, averaged over all black=1 observations.