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 is concentrated among 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." This claim has been variously described as the "mismatch" (Thernstrom and Thernstrom, 1997) or "fit" (Bowen and Bok, 1998) hypothesis.

It has proven difficult to obtain conclusive evidence regarding mismatch effects. Some studies looking at undergraduate education have concluded that the evidence supports mismatch (Loury and Garman, 1993; Light and Strayer, 2000), while others draw the opposite conclusion (Kane, 1998; Bowen and Bok, 1998).[2]

One source of the discrepant results has been disagreement about the parameter of interest. We provide a new framework that nests many of the different parameters that

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[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.

[2] For an excellent survey of the economic literature on affirmative action, including a review of studies of its use in undergraduate admissions, see Holzer and Neumark (2000).

have been studied. We focus on the effect that motivates most policy discussions of mismatch: the causal effect of admissions preferences on the human capital accumulation of the black students who attend selective schools as a result of these preferences.[3] If this effect is negative, minority students would be made better off by the elimination of race-based admissions preferences; if positive, criticisms of affirmative action as harmful to its purported beneficiaries are unsupported.[4]

Next, we develop a simple statistical model that illuminates the different empirical strategies available for identification of the mismatch effect. We emphasize two potentially informative contrasts: between students of the same race and same (observable) admission credentials who attend more- and less-selective schools; and between students of different races but similar credentials, irrespective of school attended.

We argue that each of these contrasts is likely to yield a biased estimate of the mismatch effect but that the sign of the bias will vary. The first contrast can be expected to understate the mismatch effect, while the second will likely overstate it. Thus, under reasonable assumptions the two comparisons will bracket the true mismatch effect.

Our empirical analysis focuses on law students. The legal education setting is attractive for three reasons. First, much of the recent debate over the mismatch hypothesis has focused on law school (see, e.g., Sander, 2004, 2005a,b; Chambers et al, 2005; Ayres and Brooks, 2005; Ho, 2005; Barnes, 2007). As in the earlier studies of

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[3] A related but distinct question is whether affirmative action increases the production of black graduates. This question turns on whether any negative mismatch effects on graduation rates are large enough to outweigh the extensive-margin effect of preferences on the number of black matriculants. We evaluate this question in a companion study (Rothstein and Yoon, 2008).

[4] We set aside the question of why a student would take up an admission offer at a selective school if the selective school treatment effect is negative. As Arcidiacono et al (2009) emphasize, the mismatch hypothesis requires either that students make irrational choices or that they lack important information.

undergraduate admissions, results have varied widely with the empirical strategy used. Second, law schools offer certain analytic advantages: a common curriculum, particularly in the first year; a common achievement outcome, in the form of the bar exam; and relatively homogeneous student career paths after graduation. Third, there is reason to expect that mismatch effects should be particularly large in legal education, where relative performance – in the form of the first year class rank – is an important determinant of later career opportunities and the common pedagogical use of the Socratic method may aggravate the anxieties of students who believe themselves to be unqualified.

Our analysis indicates that the data are more informative than the disparate conclusions literature to date would suggest. Like previous authors, we find no indication of mismatch effects in selective-unselective contrasts. 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. Black underperformance is nearly entirely attributable to poor outcomes among black students whose admission credentials place them in the bottom quintile of the entering law student population. Few such students attend highly selective law schools, even with preferences. Among more qualified students, blacks graduate and pass the bar exam at similar rates to otherwise similar whites. Moreover, analyses of the least qualified law students are importantly affected by the admissions policies of the least selective law schools, to which poorly qualified white applicants have much greater difficulty gaining admission than do similarly qualified blacks. The resulting sample selection prevents strong inferences about mismatch effects on bottom quintile students.

We therefore conclude that the available data provide little evidence regarding mismatch effects on the least qualified students but suggest that mismatch effects are absent or small for students with moderate qualifications. As one might have expected that affirmative action would be most harmful for these students – many of whom are admitted to the most selective law schools due to the availability of preferences – this casts doubt on many of the strong claims made for the mismatch hypothesis.

We emphasize, however, that all of the available evidence regarding the mismatch hypothesis derives from observational analyses. Our conclusions rest on unverifiable 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. A reasonable conclusion might be that we simply cannot know whether mismatch effects are important. At the least, however, our analysis suggests that recent claims that the data provide strong evidence for mismatch (see, e.g., Sander, 2004) are dramatically overstated.

The paper proceeds as follows: Section II develops a simple model that clarifies how school selectivity might impact human capital accumulation. Section III develops our two strategies for identifying the effects of school selectivity, in the context of a simple data generating process, and discusses the likely biases in each. In Section IV, we describe the Bar Passage Study (BPS) data that we use for our empirical analysis. Section V presents estimates of the role of affirmative action in law school admissions. We present results in Section VI. Section VII concludes.

II. The Effect of School Selectivity

The mismatch hypothesis concerns the effect of affirmative action-based admissions preferences on black students' human capital accumulation. In this section, we develop a very simple model that clarifies the channels by which any such effect might arise.[5]

It is natural to model human capital accumulation in school, y, as depending on two factors: The selectivity of the school that a student attends, s, and the student's performance within that school, p: $y = f(s, p)$. Plausibly, both factors have positive effects: $\partial f/\partial s>0$ and $\partial f/\partial p>0$. However, advocates of the mismatch hypothesis argue that selectivity has negative effects on performance: $p=p(s)$, with $\partial p/\partial s < 0$. p can seen as the outcome of a tournament within the school: A student who attends a more selective school will face stiffer competition in the tournament for grades, so all else equal will suffer in the rankings. This claim is sometimes known as the "frog pond" hypothesis, as the idea is that it is better to be the big frog in a small pond than the smallest frog in a larger pool (Espenshade et al., 2005).

The net effect of s on y is

(1) $$dy/ds = \partial f/\partial s + (\partial f/\partial p)*(\partial p/\partial s).$$

The sign of this effect depends on whether the positive direct effect of selectivity (the first term) is outweighed by a negative effect operating through grades (the second term).

[5] We focus on the partial effect of the selectivity of the school that an individual student attends, holding other students' attendance constant. This rules out general equilibrium effects. These could go either direction. D'Souza (1991) and Steele (1990), among others, argue that the existence of affirmative action promotes the view that black students are unprepared, potentially harming even those black students who would be admitted to selective schools without preferences. But "critical mass" arguments (see, e.g., *Grutter v. Bollinger et al.*, 539 U.S. 306 (2003)) imply that black students are positively affected by the presence of black classmates, implying that preferences should help these highly qualified black students.

A full understanding of the effects of selectivity would require identification of each of the three derivatives on the right hand side of (1).[6] The greatest challenge is in the estimation of $\partial f/\partial p$, the causal effect of in-school performance on later outcomes. Students are heterogeneous in their ability, and both grades and later human capital are increasing in student ability. Unless ability can be perfectly controlled, the estimate of $\partial f/\partial p$ will be upward-biased. Assuming that $\partial p/\partial s < 0$, this will lead to understatement of the net effect of selectivity, $\partial y/\partial s$ – and overstatement of the importance of mismatch effects – even if the other terms in (1) are estimated without bias.

But it is not necessary to distinguish the direct and indirect effects of selectivity; for evaluation of the mismatch hypothesis, it is sufficient to identify dy/ds itself. If this is negative, on average, for the black students who are induced to attend more selective schools by the availability of admissions preferences, then affirmative action is on net harmful to black students.

Even identification of the reduced-form effect of selectivity in observational data is extremely challenging, albeit less so than identifying the causal effect of grades. In the next Section, we develop a simple statistical model that identifies the likely biases in the available comparisons.

III. A Simple Statistical Model

Post-schooling outcomes depend not just on the effects of selectivity, direct and indirect, but also on students' prior characteristics. To fix ideas, we focus on a binary conception of selectivity, and adopt a potential outcomes framework. We assume that if

[6] Loury and Garman (1995) focus on the difference between the selectivity effect in a regression for wages when college grades are and are not controlled. This amounts to an analysis of the various terms of (1). Sander (2004) also uses analyses of the separate derivatives on the right side of (1) to study the mismatch hypothesis.

student i attended an unselective school, her outcome would be y_i^0, but that if she attended a selective school she would have outcome y_i^1. $\theta_i = y_i^1 - y_i^0$ represents the reduced-form effect of selectivity from the model above, dy/ds, on student i.

Let b_i represent the race of student i, and let X_i be a vector of her other characteristics on school entrance (including her admissions test scores and other observed measures of academic preparation). We can write the potential outcome at an unselective school as a linear projection onto these observed variables plus an orthogonal error,[7]

$$(2) \qquad y_i^0 = \alpha + X_i \beta + b_i \gamma + \varepsilon_i.$$

The residual term here reflects both the component of ability that cannot be proxied by the observed variables (which typically can explain only a small share of the variation in academic outcomes) and any random shocks that arise while the student is in school. The observed outcome is then

$$(3) \qquad y_i = y_i^0 + s_i\theta_i = \alpha + s_i\theta_i + X_i \beta + b_i \gamma + \varepsilon_i.$$

Equation (3) makes clear the challenge of identifying even the central tendency of θ_i. The most straightforward and common empirical strategy (see, e.g., Kane, 1998; Bowen and Bok, 1998; Chambers et al., 2005, and Ho, 2005) is to examine the mean difference in y_i between students attending selective schools and observably similar students who attend less selective schools,

$$(4) \qquad \begin{aligned} D_s(X, b) &= E[y_i \mid X, b, s_i = 1] - E[y_i \mid X, b, s_i = 0] \\ &= E[\theta_i \mid X, b, s=1] + (E[\varepsilon \mid b, X, s = 1] - E[\varepsilon \mid b, X, s = 0]). \end{aligned}$$

[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.

The necessary assumption for this comparison to be informative is that the term in parentheses in (4) equals zero. If so, $D_s(X, b)$ identifies the average effect of selectivity on those students who actually attend selective schools (i.e., the effect of the treatment on the treated).

But the exclusion restriction is not very plausible. Selective schools require extended applications and employ large admissions staffs, part of whose job it is to try to tease out information about ε from essays, recommendation letters, and other signals that are not typically observed by the researcher. Thus, we can expect that admission to a selective school will be positively correlated with ε. Matriculation decisions may be positively correlated with ε as well, if students with high unobserved (to the econometrician) ability are more likely to take up offers of admission at selective schools.

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.[8]

Unfortunately, it is extremely difficult to identify a source of exogenous variation in s. No researcher to date has identified a plausible natural experiment in this area.[9] However, it is at least possible to identify comparisons that will be subject to *different* biases than those that plague the "OLS"-style analysis. In particular, some researchers (e.g., Bowen and Bok, 1998) have estimated differences in outcomes between black and

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[8] A similar bias applies to tests based on estimates of $\partial D_s(b, X)/ \partial X$ and $\partial D_s(b, X)/ \partial b$, as in Barnes (2007). There is every reason to expect that the bias term in (4) will vary with both X and b, providing evidence of mismatch even if θ is identically 0. If selectivity depends on $X \psi_X + b \psi_b + u$, for example, with u and ε bivariate normal, the bias term will contain the expression $\lambda(a+c(X \psi_X + b \psi_b)) + \lambda(-a-c(X \psi_X + b \psi_b))$, where a and c are constants and $\lambda()$ is the inverse Mills ratio.

[9] The closest candidate is Dale and Krueger (2002), who compare students attending selective schools with others who were admitted to those schools but chose not to attend. This comparison eliminates concerns due to the role of unobserved variables in admissions.

white students with similar observed credentials, averaged across more- and less-selective schools. This kind of comparison leverages admissions preferences for black students who, because of their preferential treatment, have access to more selective schools than do whites with similar entering credentials.[10]

We begin by assuming that θ_i is constant across individuals and that $\gamma = 0$ – that race is not predictive of y_i^0 among students with the same X. Then the black-white gap in outcomes conditional on X is

$$(5) \qquad D_b(X) = E[y_i \mid X, b = 1] - E[y_i \mid X, b = 0]$$
$$= \theta^*(E[s_i \mid X, b = 1] - E[s_i \mid X, b = 0]) + b\ \beta_b.$$

This equals the product of the selectivity effect, θ, with the difference in the mean selectivity of schools attended by black and white students with the same observed credentials. θ itself can be identified as the ratio of $D_b(X)$ to this selectivity gap, $(E[s_i \mid X, b = 1] - E[s_i \mid X, b = 0])$. This amounts to a Two Stage Least Squares (2SLS) estimator, using b as an instrument for the endogenous s.

Now we relax the assumption of constant θ_i. Following Imbens and Angrist (1994), we treat s_i itself as a function of b_i. Let $s_i(1)$ be the selectivity of the school that the student i would attend if she were granted the preferences given to black applicants and $s_i(0)$ the selectivity that she would obtain if not granted preferences. It seems reasonable to assume that $s_i(1) - s_i(0) \geq 0$: There is no student who would attend a more selective school if not granted preferences than if preferences were available. If so, then the 2SLS estimator identifies the local average of θ_i among the black students who attend

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

selective schools only due to the availability of preferences (i.e., $s_i(1) = 1$ but $s_i(0) = 0$). These "compliers" are exactly the population of interest for analysis of the mismatch hypothesis, which states that the mean of θ_i in this subpopulation is negative.

The exclusion restriction in the black-white comparison (5) is that $\gamma = 0$. This is as implausible as that in the selective-unselective comparison (4). Recall that γ is the difference in mean outcomes between black and white students with the same observed credentials X if both attend unselective schools. A common result in studies of the prediction of college grades is that black students underperform white students with the same admissions credentials at the same colleges (Rothstein 2004, Young 2001). Similar patterns have been found in law schools (Wightman 2000; Wightman and Muller, 1990; Anthony and Liu, 2003; Powers, 1977). These results strongly suggest that $\gamma < 0$.[11] If so, the black-white comparison will understate the mean of θ_i in the population of compliers, leading to overstatement of any mismatch effect.

In practice, there is an additional potential bias in the black-white comparison that is not captured by our notation. If even the s=0 schools are somewhat selective, so that many students are unable to enroll anywhere, an estimate of $D_b(X)$ from the sample of matriculants may diverge from its expectation in the population of applicants. Specifically, suppose that the s=0 schools' admission decisions depend on dimensions of student ability that are unobserved to the econometrician as well as on the observed credentials, and that these schools give preferences to black students. This will induce a negative correlation between ε and b in the sample of matriculants even if they are

[11] Discrimination in favor of blacks would induce a positive β_b. This is plausible when y is an employment outcome, as employers likely use affirmative action in hiring. When y is an academic outcome, however, explicit and implicit discrimination seem likely to have negative effects, over and above any black-white difference in unobserved preparedness.

uncorrelated in the population of applicants.[12] 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 white non-admission probabilities are relatively high. In our analysis below, we present some estimates that exclude observations with low X in an effort to minimize this bias.

Despite the potential biases, the black-white comparison has an important advantage over selective-unselective comparisons. School selectivity is not a clearly-defined construct, so researchers are forced to rely on imperfect proxies like average SAT scores or the admission rate among all applicants. The resulting mismeasurement likely attenuates estimates of $D_s(X, b)$. By contrast, $D_b(X)$ can be computed without any measure of selectivity at all (although the denominator of the 2SLS estimator might be attenuated by misclassification in s, leading to overstatement of the mismatch effect).

IV. The Law School Application

Much recent discussion of the mismatch hypothesis has focused on law schools. Unfortunately, perhaps because of the diversity in empirical specifications, the literature has not shed a great deal of light. Sander (2004) attempts to estimate the three derivatives on the right side of (1), and concludes that mismatch effects are extremely important in law school admissions, dramatically depressing black students' chances of graduating from law school and passing the bar exam. As we have argued, estimation of the "structural" equation (1) is not a promising strategy for identifying the mismatch

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[12] Specifically, assume that a student is admitted to some law school only if $X_i \delta_X + \varepsilon_i \delta_\varepsilon + b_i \delta_b > c$, where $\delta_X, \delta_\varepsilon, \delta_b > 0$. Then $E[\varepsilon \mid X, b, \text{admitted to some school}] = E[\varepsilon \mid \varepsilon > \delta_\varepsilon^{-1}(c - b\,\delta_b - X\,\delta_X)]$ is decreasing in b, particularly at X values for which the admission constraint is most binding.

effect. Although Sander appears to endorse the black-white contrast outlined above – writing that mismatch hypothesis implies that "blacks have much higher failure rates on the bar than do whites with similar LSAT [Law School Admission Test] scores and undergraduate GPAs" (Sander, 2004, p. 373) – he does not emphasize this strategy.

A variety of critics have reached different results from Sander. All rely on variants of the selective-unselective comparison (4), and none find evidence of strong negative selectivity effects (Chambers et al., 2005; Ho, 2005; Ayres and Brooks, 2005). Their findings are perhaps unsurprising, given the clear bias against mismatch in their approach. It is therefore instructive to examine other strategies, such as the black-white contrast developed above, that are subject to different biases.

The data set used for all studies to date of the mismatch hypothesis in legal education 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.[13] 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.

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.

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[13] 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.

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,[14] 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).[15] 24 percent of BPS students attend highly selective schools. Within each race, students at the most selective 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

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[14] 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.

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

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.[16] 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 counts non-graduates as failures but excludes other non-takers.[17]

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 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.

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[16] The BPS permits us to track ultimate graduation even for the few students who transfer schools.

[17] 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 have also explored specifications that include graduates who did not take the exam as failures; these yield very similar results.

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 student faces stiffer competition. 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 more absolute measure, though the threshold may vary somewhat across schools.

Bar exams use blind grading 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 in which state 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.[18] 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.

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

[18] 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.

mismatch hypothesis.[19] 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 2 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.[20]

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 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

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[19] 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.

[20] The curves in Figure 2 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 2. 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.

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 3 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 (see Figure 1).

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.[21] As discussed earlier, this will bias black-white comparisons against black students, 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

[21] 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 3 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.

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; positive, large, and insignificant for employment; and negative – trivially so – only for bar passage. 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 bar passage deficit for blacks, on average. 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.

In Section III, we pointed out that the black-white contrast can be seen as the reduced form of an instrumental variables (IV) analysis of the effect of attending a selective school, and that under the identifying assumption that race has no direct effect on outcomes this IV analysis identifies the local average treatment effect (LATE) for black students who attend selective schools as the result of preferences. Table 6 presents IV estimates, both for the full sample and for the top four quintiles.[22] Results mirror those seen in Table 5: In the full sample, the implied LATE of school selectivity is negative and large for graduation and bar passage but positive for employment outcomes. In the top four quintiles, the employment effects are similar, but the graduation and bar passage point estimates shrink dramatically and become statistically insignificant. Although confidence intervals are too wide to rule out moderate-sized effects, there is no indication in the point estimates – which, recall, are biased downward by likely violations of the exclusion restriction – of important mismatch.

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 have 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

[22] In Table 6, we use a linear probability model for the binary outcomes. This does not have a dramatic effect on the results: Point estimates are close to the ratios of the marginal effects of race on outcomes from Table 5 to the marginal effects of race on selectivity from Table 2. We have also extended the IV analysis to allow for a continuous distribution of selectivity, with unobserved variation in selectivity within both the s=0 and s=1 groups. Results are described in Rothstein and Yoon (2007).

excluding students with high-prestige but low-salary jobs (e.g. clerkships) from our analyses of salaries.[23] 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.

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

[23] We also 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.

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 demonstrate 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 indicates that the combined effect is positive.

Our analysis yields murkier results about possible mismatch effects on the graduation and bar passage outcomes of students with bottom-quintile credentials. 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 that the observed black-white gap simply reflects sample selection bias. Many bottom-quintile applicants of both races are unable to gain admission to *any* law school. As a consequence of the least selective schools' use of affirmative action, however, this outcome is much more likely for white than for black applicants. If 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. Without an exogenous source of variation in sample selection, there is no convincing strategy for avoiding this bias in analyses of bottom quintile students.

How predictive would the admission variables have to be of later outcomes in order to account for the observed data without mismatch effects? 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 even if the true selectivity effect is zero.[24] 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 and other sources of bias 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, the majority of whom become practicing lawyers. 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.

[24] 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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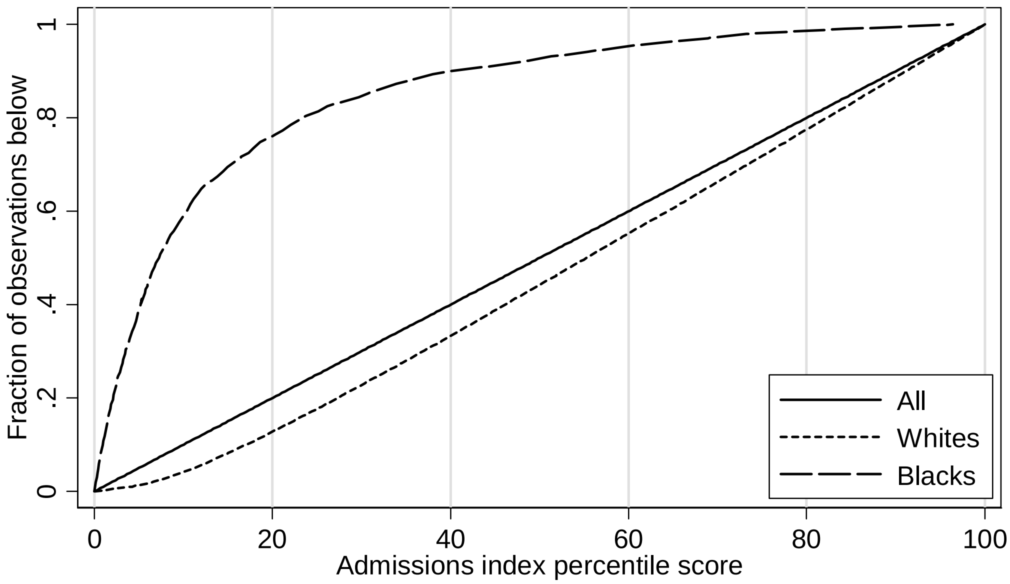
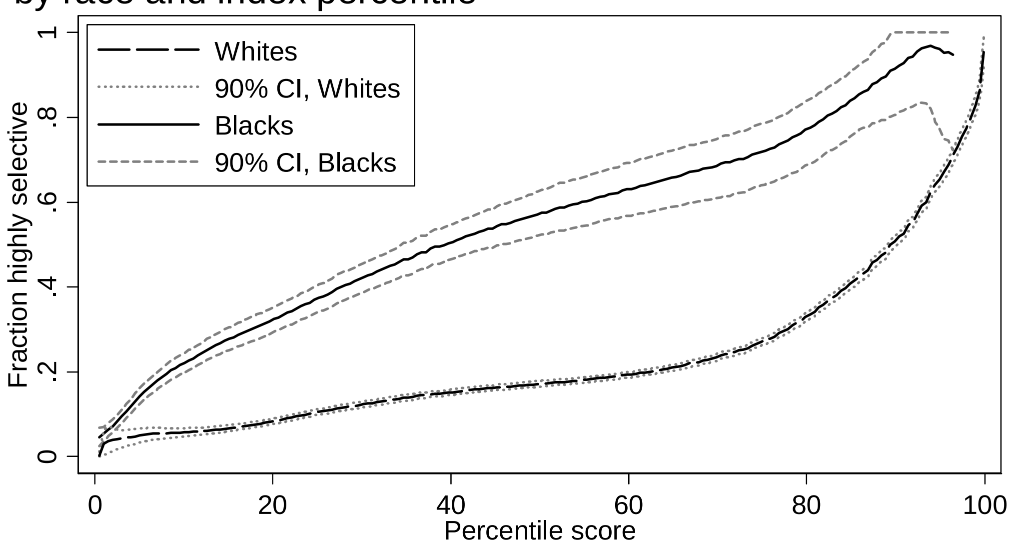
Figure 1. Distribution of admission index percentile scores for black and white matriculants



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

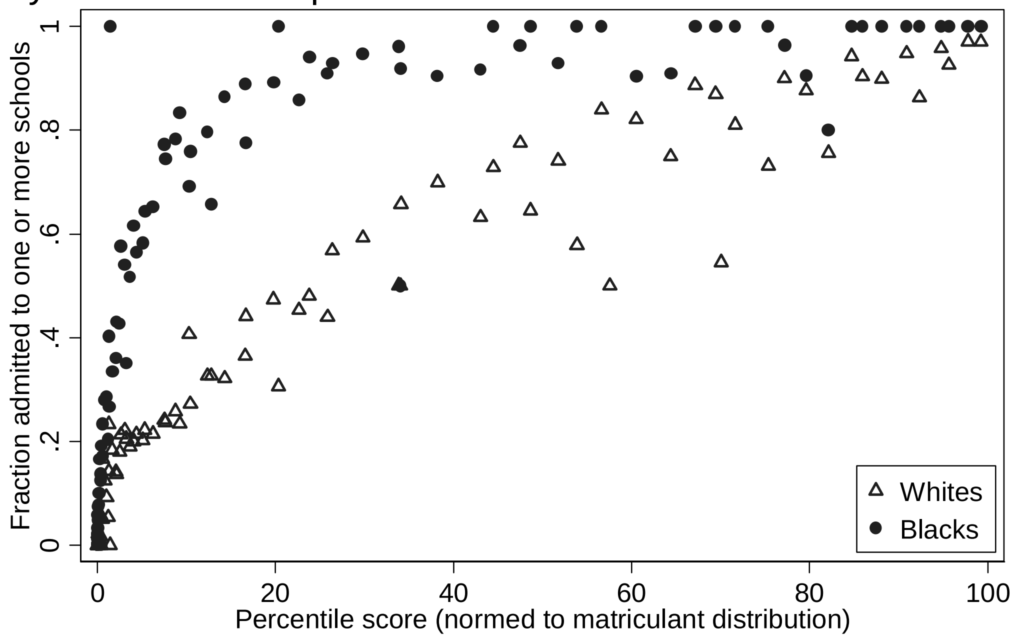
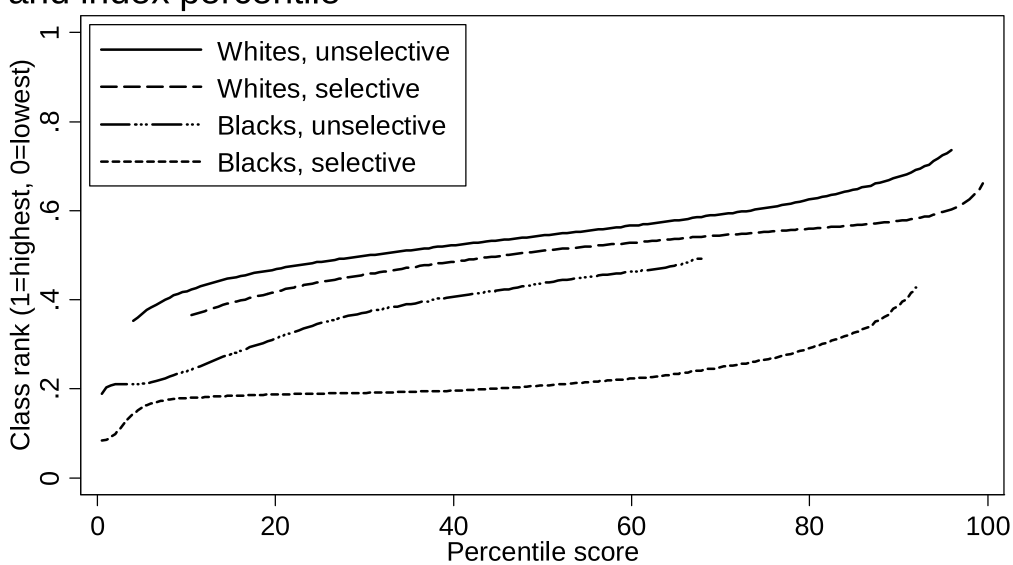
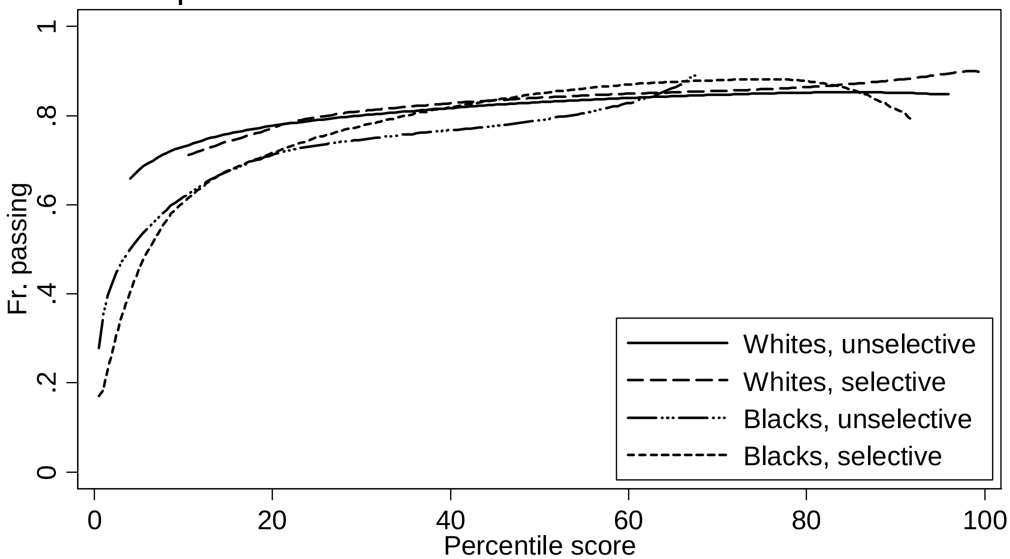


Figure 4. First year class rank by race, law school selectivity, and index percentile



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)
$(\text{LSAT}/100)^2$	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)
$(\text{UGPA}/10)^2$	-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 employed	**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.

Table 6. Selectivity effects implied by black-white comparisons

	Full sample	Top four quintiles
	(1)	(2)
Law school graduation	**-0.171**	0.017
	(0.046)	(0.037)
Bar passage (if attempted)	**-0.487**	-0.078
	(0.062)	(0.044)
Employment		
Has a full-time job	0.258	**0.410**
	(0.197)	(0.199)
"Good" job, if employed	**0.872**	**0.736**
	(0.230)	(0.217)
Ln(salary), if FT employed	**0.411**	**0.402**
	(0.188)	(0.184)

Notes: All specifications are specified as linear models (rather than probit), estimated by two stage least squares. Reported coefficients are on a selectivity indicator, instrumented by student race. Control variables are 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.