Mismatch in Law School

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Abstract
An important criticism of race-based higher education admissions preferences is that they may hurt minority students who attend more selective schools than they would in the absence of such preferences. We use two comparisons to bound so-called “mismatch” effects in law school. We find no evidence of mismatch effects on the graduation or bar passage rates of black students with moderate or strong entering credentials. The data are consistent with negative effects on less-qualified black students who typically attend either 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. When looking at employment outcomes, we find no evidence of negative effects on any black students.

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I. Introduction

Most selective colleges and universities in the United States give admissions preferences to students from certain underrepresented groups. Such students are able to attend schools from which non-preferred applicants with identical credentials are rejected. Critics have argued that the purported beneficiaries of preferences are inadequately prepared for selective, competitive schools, and would do better—learn more, be more likely to graduate, avoid psychological damage, etc.—if they were admitted only to schools more appropriate for their qualifications (Summers, 1970; Sowell, 1978; Thernstrom and Thernstrom, 1997).²

This so-called “mismatch hypothesis” might be characterized as a negative peer effect: A student’s outcomes will decline if the average qualifications of her classmates rise too high above her own (Loury and Garman, 1995; Light and Strayer, 2000). This negative peer effect need not apply to all students. Rather, the mismatch hypothesis concerns the effect of selective schools on the subset of students whose academic qualifications are much weaker than their potential selective-school classmates but who are nevertheless induced to attend these schools by the availability of preferences.

Simple theory counsels against the mismatch hypothesis: Admissions preferences merely broaden preferred students’ choice sets, so ought not to harm them. Evidence from a variety of contexts, however, suggest that students’ matriculation decisions are not always ex-post optimal (see, e.g., Dynarski and Scott-Clayton, 2006; Avery and Kane, 2005; Manski

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² The Supreme Court has held repeatedly that affirmative action’s relevant benefits are through its effects on diversity of the educational environment, suggesting that effects on white students’ outcomes are the most important. See, e.g., Regents of Univ. of Cal. v. Bakke 438 U.S. 265 (1978), and Grutter v. Bollinger, 539 U.S. 306 (2003). Another important effect is on the white students who are displaced from selective schools by less-qualified minority applicants. Neither of these is our focus here. See Arcidiacono (2005) for a more comprehensive analysis and Holzer and Neumark (2000) for a review of the voluminous relevant literatures.
and Wise, 1983; Manski, 1989). The importance and sign of mismatch effects remains an empirical question, with limited available evidence (Holzer and Neumark, 2000).

The key challenge to identifying mismatch effects is the measurement of counterfactual outcomes. Focusing on black students as the primary preferred group, one needs to know what would have happened to black students admitted to highly-selective schools had they not received admissions preferences. Three forms of counterfactual assumptions have been used frequently.

The first and least convincing strategy compares graduation rates and other outcomes of black and white students attending the same college (Thernstrom and Thernstrom, 1997; Herrnstein and Murray, 1994; D’Souza, 1991). As Kane (1998) notes, gaps in outcomes may simply reflect dramatic differences in entering credentials between these groups, who typically were admitted under different rules. In any event, the two groups being compared attend the same school, so differences between them cannot be attributed to the effects of school selectivity.

A second strategy compares black students attending highly selective schools with other black students with the same entering credentials attending less selective schools. Bowen and Bok (1998) find that the former students experience better long-run outcomes, and conclude that concern about mismatch is misplaced. But this “selective-unselective” comparison may yield upward-biased estimates of the true selectivity effect if students admitted to selective schools are better qualified on unobserved dimensions than those denied admission.³

³ Dale and Krueger (2002) compare pairs of students admitted to the same schools but making different matriculation choices. They find small positive effects of selectivity on employment outcomes, but do not focus specifically on students who might be mismatched (because, for example, their credentials are atypical of the schools they attend).
A third strategy attempts to solve this endogeneity problem by leveraging admission preferences themselves. Affirmative action allows black students to attend dramatically more selective schools than whites with similar entering credentials. If access to selective schools hurts student outcomes, preferences should worsen average black students’ outcomes relative to those of otherwise-similar whites. This suggests a comparison of black students’ outcomes with those of white students with the same credentials, irrespective of the schools that each attend. Mismatch effects will produce a negative black-white gap. This strategy (which we refer to as a “black-white comparison”) can sidestep the potential dependence of admissions and matriculation decisions on unobserved variables, so long as these unobservables are balanced between races.

This paper evaluates the mismatch hypothesis as it applies to black law school applicants. A key advantage of the law school context is the availability of a race-blind achievement measure, the bar exam, that is required for the practice of law. While we also examine more traditional outcome measures like graduation and early-career salaries, the bar exam avoids many (though not all) shortcomings of these measures.

Our analysis focuses on reduced-form implementations of the second and third counterfactual strategies described above. While the identifying assumptions for each approach are likely violated, the biases in the two strategies are in opposite directions, and therefore allow us to bracket the true effect. The selective-unselective comparison is most likely biased upward relative to the true selectivity effect, so understates mismatch; conversely, the black-white comparison most likely overstates mismatch.

We situate our analysis in a model with heterogeneous treatment effects of school selectivity. Our two strategies identify distinct parameters. The selective-unselective

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4 For evidence on law school admissions, the focus of this paper, see Sander (2004) and Rothstein and Yoon (2008). For colleges, see Bowen and Bok (1998) and Krueger, Rothstein, and Turner (2006).
comparison amounts to an OLS estimator of the selectivity effect, controlling for entering credentials. If the exclusion restriction is satisfied, it identifies the average effect of selectivity on all students who attend selective schools; that is, the average effect of the treatment on the treated, or ATT (Imbens and Angrist, 1994).

The black-white comparison can be seen as an instrumental variables (IV) estimator in which the student’s race is used as an instrument for the selectivity of the school that she attends. Under appropriate assumptions, this comparison identifies the local average treatment effect (LATE; see Imbens and Angrist, 1994) of selectivity for black students who “comply” with the affirmative action instrument by attending more selective schools than they otherwise would. This is precisely the population of interest for evaluation of the mismatch hypothesis.

OLS estimates offer no indication of negative effects of selective schools on educational or labor market outcomes. Although school selectivity reduces class rank for both black and white students, this mechanical effect does not carry through to later outcomes: Students attending more selective schools are more likely to graduate from law school, are equally likely to pass the bar exam, and earn higher post-graduation salaries.

The black-white comparison yields more mixed results. Black law students are significantly less likely to graduate or pass the bar exam, on average, than white students with the same entering credentials. Those who do graduate, however, fare as well or better in the labor market. Disparities in academic outcomes are concentrated among students with the weakest entering credentials; among students in the upper four quintiles of the distribution – where apparent affirmative action preferences are the strongest – black students’ academic and employment outcomes are as good as or better than those of whites.
We interpret our results as demonstrating that any mismatch effects are restricted to students with entering academic credentials in the bottom quintile of the matriculant pool. Although one cannot dismiss the bottom quintile as unimportant—87% of black law applicants and 76% of black matriculants have credentials in this range—our results counsel a nuanced view of the mismatch hypothesis. Few bottom-quintile students attend the most selective law schools, with or without preferences; to the extent that mismatch exists, it evidently derives from students attending second- or third-tier schools rather than fourth-tier schools and appears only on academic outcomes but disappears when students enter the job market. Moreover, the attribution of black underperformance to mismatch effects is particularly tenuous for poorly-credentialed students, as we cannot rule out the alternative explanation of differential sample selection arising from the role of affirmative action at the extensive margin: Many black students would not be admitted to any law school without preferences. As we discuss below, these students may have lower ex ante chances of success in the legal profession than do the white students with similar entering credentials who are nevertheless admitted.

We are not the first to examine law schools for evidence on mismatch. Sander (2004) argues that affirmative action acts to the detriment of black law students, undermining both their academic experience during law school and their chances of becoming licensed attorneys. His somewhat structural analysis depends critically on a causal parameter of which he does not have a credible estimate, the effect of law school grades on later outcomes. He estimates this effect via OLS, with no strategy for addressing the

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5 27% of white law school applicants with bottom-quintile credentials are admitted to at least one school, while 40% of black applicants in this range are admitted. This understates the degree of differential selection, as black applicants are more likely to have credentials at the lower end of the bottom quintile, where admissions rates are very low, than are whites.

6 Sander’s analysis and conclusions have been the subject of strong criticism (Ayres and Brooks 2005; Chambers et al. 2005; Dauber 2005; Wilkins 2005; Ho 2005), to which Sander (2005a; 2005b) responded.
omitted variables bias that heterogeneity in unobserved ability would produce. This leads Sander to estimate mismatch effects that are twice as large as what would be consistent with our reduced-form estimates, which rely on weaker assumptions.7

The remainder of the paper proceeds as follows. In Part II we discuss the mismatch hypothesis in greater detail. Part III introduces our identification strategies and our empirical approach. We describe in Part IV the Bar Passage Study (BPS) data that we use for our analysis. Part V examines black-white differences in the selectivity of the school attended, and documents large selectivity effects on the degree to which students are mismatched. Part VI presents estimates of the selectivity effect on student outcomes, and Part VII concludes.

II. Heterogeneous Treatment Effects and Mismatch

A simple model for law students’ outcomes is

\[
y_i = \alpha + X_i \beta + s_i \theta_i + b_i \gamma + \epsilon_i,
\]

where \(y_i\) is some outcome measure, \(X_i\) a vector of entering admissions credentials\(^8\), \(s_i\) a measure of school selectivity (with higher values corresponding to more selective schools), \(b_i\) an indicator for whether the student is black, and \(\epsilon_i\) an error term. For the time being, we treat \(s_i\) as a binary measure.

The causal effect of attending a selective school on student i’s outcome is \(\theta_i\).\(^9\) While it is possible that selective schools have negative effects on all students, a more realistic hypothesis is that selective schools benefit some students but not others. For example, \(\theta_i\)

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7 A companion paper (Rothstein and Yoon, forthcoming) revisits Sander’s claim that affirmative action reduces the net production of black lawyers. Our reduced-form estimates do not support this claim; we estimate that the elimination of preferences would reduce the number of new black lawyers by over half.

8 We loosen the assumption that \(X\) enters (1) linearly below, by including polynomials and interactions in our primary \(X\) variables in our main specifications and by presenting graphical estimates that allow for an arbitrarily nonlinear relationship between \(X\) and \(y\).

9 Equation (1) characterizes only the outcomes of students who actually matriculate at some law school, so the effect of selective schools is relative to attending a less selective school, not to pursuing an alternative career. We discuss this distinction below.
may be positive for highly-qualified students and negative for students who are less well qualified.\textsuperscript{10} With heterogeneous effects of school selectivity, some students may be mismatched while others are not.

The policy-relevant parameter for evaluation of affirmative action is the average selectivity effect among the policy’s beneficiaries, those black students who are induced to attend selective schools by the availability of affirmative action preferences.\textsuperscript{11} This requires additional notation. Let $s_i(0)$ be the school that student $i$ would attend without preferences, and let $s_i(1)$ be the school that she would attend if offered preferences. The effect of preferences on the school that student $i$ would attend is $s_i(1) - s_i(0)$.\textsuperscript{12} Because black students receive preferences while white students do not, the observed selectivity for student $i$ is $s_i = s_i(b_i) = b_i s_i(1) + (1-b_i) s_i(0) = s_i(0) + b_i (s_i(1) - s_i(0))$.

For many students, $s_i(1)$ will be identical to $s_i(0)$. Some students would be admitted to selective schools with or without preferences and would enroll in either case. Following Angrist, Imbens, and Rubin (1996), we refer to these students as \textit{always takers}. Others, \textit{never takers}, would not be admitted even with preferences, or simply prefer to attend unselective

\textsuperscript{10} There are several reasons to think that selectivity effects may be more negative for black students. An underprepared white student may be able to avoid notice, but the visibility of race may make anonymity impossible for black students. Observers with incomplete information about particular students’ ability may form lower estimates for members of groups that benefit from admissions preferences (Murray, 1994; Steele, 1990; Sowell, 2004). The resulting low expectations may be self-fulfilling for black students, as so-called “stereotype threat” has been claimed to worsen black performance in contexts where racial performance differences are salient (Steele and Aronson, 1998). Finally, the Socratic method of instruction, widely used in law schools, may aggravate these effects by focusing classroom attention on black students as representatives of their race (Guinier et al., 1994).

\textsuperscript{11} If students are aware of their $\theta$s, one would expect them to sort on this basis, so that only students with positive $\theta$s accept admissions offers from selective schools. In this case, affirmative action cannot produce mismatch effects. If students observe their own $\theta$s only with error, however, some students with negative $\theta$s might nevertheless attend selective schools. Ideally, one would want to know whether \textit{any} of the students affected by affirmative action preferences have negative $\theta$s. Because an individual student’s outcome is observed only for one value of $s$, this cannot be tested. The mean of $\theta$ among students affected by preferences is a sufficient statistic for evaluation of the \textit{net} effect of these policies.

\textsuperscript{12} We draw here on the framework developed by Imbens and Angrist (1994) and Angrist, Imbens, and Rubin (1996).
schools (perhaps because they have \( \theta_i < 0 \)). For students in each of these groups, \( s_i(1) = s_i(0) = s_i \).

The most obvious source of differences between \( s_i(1) \) and \( s_i(0) \) is the admissions decision itself: Some students are admitted to selective schools only if they receive preferential treatment. If these students accept the admissions offers that they thereby obtain, they have \( s_i(1) - s_i(0) = 1 \). We refer to these students as affirmative action compliers. A final category, defiers (\( s_i(1) = 0 \) and \( s_i(0) = 1 \)) contains students who would attend selective schools without preferences but with preferences would decline admissions offers from these schools. The schools that complier and defier students attend are determined by their access to preferences and, therefore, by their race. White compliers and black defiers attend unselective schools, while white defiers and black compliers attend selective schools.

The mismatch hypothesis states that selectivity has a net negative effect on the black students who are induced to attend selective schools by the availability of preferences. It is therefore a claim about the local average treatment effect (LATE) of selectivity among affirmative action compliers.

III. Identification and Estimation

A. Two Comparisons

Students’ academic credentials are an important confounding factor for any strategy for identifying mismatch effects. We distinguish between two types of credentials: 1) the LSAT score and the law school GPA, which are observable by researchers; and 2) recommendation letters, application essays, extracurricular activities, and other

\[13 \text{ Another category of compliers consists of students who could be admitted to selective schools even without preferences but who attend them only if preferences are used to create a “critical mass” of black students at these schools. Formally, this sort of decision violates Angrist, Imbens, and Rubin’s (1996) “stable unit treatment value assumption.” This means that our estimates cannot be interpreted as indicating the effect that changing a single student’s access to preferences would have on her outcomes. Rather, they indicate the net effect of the preference regime on black students, which in any case is the effect of interest.}\]
characteristics that are observed by the admissions committee but not by the
econometrician. Referring to the former as $X^1$ and the latter as $X^2$, we can rewrite (1) as
\[ y_i = \alpha + X^1_i \beta^1 + s_i \theta_i + b_i \gamma + (X^2_i \beta^2 + \varepsilon_i), \]
where the composite error term includes both unobserved admissions credentials and a
remaining term, $\varepsilon_i$. The latter can be seen as the admissions office’s prediction error, and we
assume that it has mean zero conditional on the information available in admissions, $E[\varepsilon_i | X^1, X^2, b] = 0$.

1. Within-race, between-school comparisons

OLS-style comparisons identify selectivity effects by comparing mean outcomes of
students at selective and unselective schools, conditional on race and on observed
credentials. Using (2), suppressing subscripts, and removing terms that are identical across
the comparison groups, we can write the difference between these two conditional means as
\[ D_s(b, X) = E[y | b, X^1, s = 1] - E[y | b, X^1, s = 0] \]
\[ = E[\theta | b, X^1, s = 1] + E[X^2 \beta^2 | b, X^1, s = 1] - E[X^2 \beta^2 | b, X^1, s = 0]. \]
\[ + E[\varepsilon | b, X^1, s = 1] - E[\varepsilon | b, X^1, s = 0]. \]

The first term here is the average effect of selectivity on the subset of students who
attend selective schools (i.e., the effect of the treatment on the treated, or ATT). The
remaining terms represent potential biases relative to this ATT. The first derives from the
role of unobserved credentials in determining admission to selective schools. Holding $X^1$
and $b$ constant, the probability of being admitted to a selective school is likely increasing in
$X^2 \beta^2$. If so, the partial correlation between $s$ and $X^2 \beta^2$ is positive. The second bias term in
(3) derives from the matriculation decisions of students admitted to selective schools. If
students with high unobserved (to the admissions office) ability are more likely to take up
offers of admission at selective schools, it will be positive. The likely magnitude of these positive biases is relatively small, however. Law school admissions depend heavily on LSAT scores and undergraduate GPAs, both of which are included in $X^1$, and relatively little on other components of the application.\textsuperscript{14} Thus, much of the variation in $s$ given $X^1$ and $b$ likely reflects student preferences and idiosyncratic factors rather than admissions decisions, and there is little reason to think that these preferences are strongly correlated with outcomes (Krueger and Dale, 2002). OLS should provide a reasonably tight upper bound for the true selectivity effect on students attending selective schools.

2. \textit{Between-race comparisons}

Our second comparison is between black and white students with the same credentials, regardless of the school attended. The conditional black-white gap in mean outcomes is

\begin{equation}
D_b(X^1) = E[y | b = 1, X^1] - E[y | b = 0, X^1]
\end{equation}

\begin{align*}
= E[s \theta | b=1, X^1] - E[s \theta | b = 0, X^1] \\
+ \gamma + E[X^2 \beta^2 | b = 1, X^1] - E[X^2 \beta^2 | b = 0, X^1].
\end{align*}

Each of the first two terms in (4) can be written as the product of the mean of $s$ given $b$ and $X$ with the mean treatment effect of selective schools among students attending them, again given $b$ and $X$:

\begin{equation}
E[s \theta | b, X^1] = E[s | b, X^1] * E[\theta | b, X^1, s=1].
\end{equation}

Because $s$ is binary, its conditional mean is just the fraction of the relevant group attending selective schools. As discussed in Section II, different subsets of black and white students will attend selective schools. Among white students, always-takers and defiers have $s=1$.

\textsuperscript{14} A supplemental analysis, available from the authors, demonstrates that LSAT scores and undergraduate GPAs are several times better at predicting admission to law school than are SAT scores and high school GPAs for college admissions, even at the most selective colleges, leaving little role for other factors that may independently predict $y$. 
Let \( p^b(b, X^i) \) be the fraction of students of race \( b \) and qualifications \( X^i \) in group \( g \) (\( g = \text{at}, \text{nt}, \text{c}, \text{d}, \) for always-takers, never-takers, compliers, and defiers, respectively). Then \( E[s \mid b=0, X^i] = p^{\text{at}}(0, X^i) + p^{\text{d}}(0, X^i) \). Similarly, black always-takers and compliers have \( s=1 \): \( E[s \mid b=1, X^i] = p^{\text{at}}(1, X^i) + p^{\text{c}}(1, X^i) \).

We can also decompose the second term in (5) as a weighted average of the mean of \( \theta \) in the relevant groups. Defining \( \theta(b, X^i, g) = E[\theta \mid b, X^i, \text{group} = g] \), we have:

\[
(6) \quad E[s \theta \mid b = 0, X^i] = p^{\text{at}}(0, X^i) \theta(0, X^i, \text{at}) + p^{\text{d}}(0, X^i) \theta(0, X^i, \text{d}) \text{ and } \\
(7) \quad E[s \theta \mid b = 1, X^i] = p^{\text{at}}(1, X^i) \theta(1, X^i, \text{at}) + p^{\text{c}}(1, X^i) \theta(1, X^i, \text{c}).
\]

Returning to (4) and re-arranging terms,

\[
(8) \quad D_b(X^i) = p^{\text{c}}(1, X^i) \theta(1, X^i, \text{c}) - p^{\text{d}}(0, X^i) \theta(0, X^i, \text{d}) + p^{\text{at}}(1, X^i) \theta(1, X^i, \text{at}) - p^{\text{at}}(0, X^i) \theta(0, X^i, \text{at}) \]
\[
+ \gamma + E[X^2 \beta^2 \mid b = 1, X^i] - E[X^2 \beta^2 \mid b = 0, X^i].
\]

The first term is the one of interest. Remaining terms represent sources of potential bias.

We begin by stating five assumptions under which these bias terms disappear, and then consider the implications of likely violations:

(i) \( p^{\text{d}}(0, X^i) = 0 \). There are no white students who attend selective schools but would attend unselective schools if affirmative action preferences were eliminated.\(^{15}\)

(ii) \( p^{\text{at}}(0, X^i) = p^{\text{at}}(1, X^i) \). Equal fractions of white and black students with credentials \( X^i \) fall into the “always taker” category of students who would attend selective schools regardless of the availability of preferences.

(iii) \( \theta(0, X^i, \text{at}) = \theta(1, X^i, \text{at}) \). Local average treatment effects are the same for black and white always-takers.

(iv) \( \gamma = 0 \). There is no direct effect of race on outcomes.

(v) \( E[X^2 \beta^2 \mid b = 1, X^i] = E[X^2 \beta^2 \mid b = 0, X^i] \). There are no mean differences between black and white students in unobserved credentials, conditional on observed credentials.

\(^{15}\) In a more flexible model where \( \beta \) might vary by race, we would also require that \( p^{\text{at}}(1, X^i) = 0 \). This might be violated: Under statistical discrimination, affirmative action policies may reduce the signaling value of a selective school degree for black students who could be admitted even without preferences, leading some such students to choose unselective schools though they would have chosen selective schools in the absence of preferences. Similarly, some white students might attend selective schools only if preferences produce diverse classes at these schools. Both groups of defiers seem likely to be small.
With these, (8) becomes

\[(9) \quad D_b(X^1) = p^s(1, X^1) \ast \theta(1, X^1, c).\]

This is the product of the complier share with the parameter of interest, the LATE of selectivity for black compliers with observed credentials \(X^1\). To recover the LATE, we need only an estimate of the complier share. The black-white difference in the fraction of students attending selective schools is

\[(10) \quad E[s \mid b = 1, X^1] - E[s \mid b = 0, X^1] = p^s(1, X^1) + p^c(1, X^1) - p^s(0, X^1) - p^d(0, X^1).\]

Under assumptions (i) and (ii), this reduces to \(p^c(1, X^1)\). We can thus compute \(\theta(1, X^1, c)\) as:

\[(11) \quad \theta(1, X^1, c) = \frac{D_b(X^1)}{p^c(1, X^1)} = \frac{E[y \mid b = 1, X^1] - E[y \mid b = 0, X^1]}{E[x \mid b = 1, X^1] - E[x \mid b = 0, X^1]}.\]

This is an IV estimator, using race as an instrument for selectivity while controlling for observed credentials. Equations (9) and (10) are, respectively, the reduced-form and first-stage models.

An important advantage of the between-race comparison is that \(D_b(X^1)\) can be computed without reference to the selectivity of the schools that individual students attend, which can be difficult to measure. It can be interpreted as the net effect of affirmative action on black students’ outcomes, combining the effect of preferences on the schools that students attend with the effect of those schools on the affected students’ outcomes.

Equation (8) suggests several biases that will affect the black-white comparison if the strong assumptions enumerated above are violated. Assumptions (i) and (ii) are relatively innocuous. Using just these, we can write

\[(12) \quad D_b(X^1) = p^s(1, X^1) \ast \theta(1, X^1, c) + p^s(0, X^1) \ast \theta(0, X^1, at) - \theta(0, X^1, at) + \gamma + E[X^2 \beta^2 \mid b = 1, X^1] - E[X^2 \beta^2 \mid b = 0, X^1].\]
The bias terms in (12) reflect differences in average treatment effects between black and white always-takers; direct effects of race on outcomes; and differences in unobserved credentials between black and white students. We discuss these in reverse order.

Research on the prediction of college grades (e.g. Rothstein, 2004; Young, 2001) generally indicates that white college students outperform black students with the same observed admissions credentials at the same colleges. Similar patterns have been found in law schools (Wightman, 2000; Wightman and Muller, 1990; Anthony and Liu, 2003; and Powers, 1977). A natural explanation is that black students, on average, have weaker unobserved credentials than do observably similar white students. In other words, $E[X^2 \beta_2 | b = 1, X^1] - E[X^2 \beta_2 | b = 0, X^1] < 0$.

The second bias term is $\gamma$, the direct effect of race on outcomes. Its sign is likely to vary depending on the particular outcome measure used, and we defer discussion of it to Section IV. The final bias term, $p^s(0, X^1)^s(0(1, X^1, at) - \theta(0, X^1, at))$, is probably negative. The logic of the mismatch hypothesis suggests that $\theta_i$ declines with admissions qualifications, both observed and unobserved. If black students indeed have lower $X^2 \beta_2$ than do white students with the same $X^1$, we should expect the black-white difference in $\theta_i$ (conditional on $X^1$) to be negative. It seems plausible that this will hold as well among always-takers.

Taken together, then, we have two negative biases and one that varies depending on the outcome. When $\gamma$ is positive, the sign of the bias is ambiguous. When $\gamma$ can be assumed to be non-positive, however, the black-white comparison and the IV estimator derived from it will yield lower bounds for the effect of admissions preferences on black outcomes and for the selectivity effect, respectively.\textsuperscript{16}

\textsuperscript{16} It seems natural to combine the selective-unselective and black-white comparisons by differencing both across type of school and between race. Rothstein and Yoon (2006) show that this does not identify a parameter of interest. They also show that a quasi-difference-in-differences estimator can be constructed that
B. Implementation

As written, our model (1) assumes that the effect of X^1 on y is linear and is constant across race. If so, the estimation strategies discussed above can be implemented via pooled OLS and IV regressions that control linearly for the X^1 variables, the LSAT score and the undergraduate GPA. Our main specifications loosen this in two ways: We include quadratic terms in these controls (and an interaction between them), and we estimate the selective-unselective comparison separately for each race, in effect fully interacting model (1) with a race indicator. If this flexibility is sufficient to capture the true effects of X^1 on outcomes, our OLS and IV specifications yield estimates of the average of the treatment effects in question (ATT for OLS, LATE for IV) across the X^1 distribution.17

We use two approaches to further relax our assumptions about X^1. First, we present graphical estimates of mean outcomes for each comparison (among students of the same race at more- and less-selective schools, or among black and white students irrespective of school) as semiparametric functions of an index of the LSAT and undergraduate GPA.18 These graphs indicate that conditional means are approximately linear and treatment effects are approximately constant at all but the lowest X^1 values. Accordingly, we also present regression estimates that use only the subsample whose LSAT scores and undergraduate GPAs place them in the upper four quintiles of the entering credentials distribution. These identify the ATT (OLS) and LATE (IV) conditional on having credentials in this range.

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17 The various estimators implicitly weight the X^1 distribution differently. OLS weights it by the distribution of X^1 among students attending selective schools; IV by the distribution among black compliers. See Angrist and Imbens (1995) and Frölich (2007).

18 We estimate these with local linear kernel regressions, using an Epanechnikov kernel and a rule-of-thumb approximation to the optimal bandwidth (Fan and Gijbels, 1996, p. 111). Explorations with alternative bandwidths do not reveal much sensitivity within a reasonable interval. We obtain pointwise confidence intervals for our graphical estimates via a bootstrap.
A final issue concerns the measurement of selectivity. Our data set (described below) has only an imperfect proxy for the selectivity of the school attended. We focus on a binary categorization of schools as “highly selective” and “less selective,” though there is likely important unobserved variation within each category. With some distributional assumptions, we can use latent variable models to recover the average true selectivity given race and observable credentials. Specifically, we suppose that latent selectivity, $s^*$, is normally distributed conditional on race and credentials,

$$s^* = \pi_0 + X^1\pi_1 + b\pi_2 + \mu,$$

where $\mu \sim \text{N}(0, \sigma^2)$, and that our binary measure is an indicator for $s^* > 0$. We can estimate the $\pi$ coefficients with a probit model. Recall that our “first stage” regression amounts to the black-white difference in mean selectivity conditional on $X^1$. This difference is simply $\pi_2$. A 2SLS estimator for the effect of a one-unit increase in latent selectivity can therefore be computed as the ratio of the black coefficient in a model for $y$, controlling for $X^1$, to the corresponding coefficient from (13), $\pi_2$. The units of $s^*$ are arbitrary; we normalize it to have unit standard deviation among white students (unconditional on $X^1$) in our sample.

IV. Data

Our data come from the Law School Admission Council’s (LSAC) Bar Passage Study (BPS; Wightman 1998, 1999), which attempted to survey all students matriculating at accredited law schools in the fall of 1991. Survey responses were matched with administrative records on admissions qualifications and academic progress, and with information from state bar associations about bar passage outcomes through July 1996. The BPS contains information on over 27,000 students, about 62% of the 1991 cohort.19

19 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 differed 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% are black. The Data Appendix describes our sample and variable definitions in detail. 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\(^1\) variables are the LSAT score and the undergraduate grade point average (UGPA). LSAT scores ranged from 10 to 48 in 1991, with mean 36.8 and standard deviation 5.5 among BPS respondents. The UGPA, computed from student transcripts, ranges from 1.5 to 4.0 on the traditional scale, with an A grade corresponding to a 4.0, a B to a 3.0, etc.\(^{20}\) The BPS contains no information about the college attended, and UGPAs are not adjusted for the difficulty of the undergraduate curriculum.\(^{21}\) For graphical analyses, we form an index of these two variables, using weights of 0.4 and 0.6 on the standardized UGPA and LSAT, respectively,\(^{22}\) then convert this index to a percentile score based on the distribution within our matriculant 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 in the BPS. Three quarters of black matriculants are in the bottom quintile of the distribution.

For confidentiality reasons, the BPS masks the identity of the law schools that students attend, grouping them into six “clusters” of schools that are similar along

\(^{20}\) We drop 28 observations with reported UGPAs outside the (0, 4.0] range.
\(^{21}\) Surprisingly, most law school admissions offices do not seem to take account of the undergraduate institution in their evaluation of undergraduate GPAs (Hagle, undated).
\(^{22}\) These weights are taken from Sander (2004), who describes them as a close approximation to those used in law school admissions. A probit model for attendance at the most selective cluster of law schools yields nearly identical weights, as does a model predicting law school GPAs within clusters.
dimensions like size, cost, selectivity, tuition level, and minority representation. 24% of respondents are in two clusters that contain the most selective schools, one third in the so-called “Elite” cluster, the most selective, and the rest in the “Public Ivy” cluster (Wightman 1993).23 The remaining four clusters overlap substantially in the credentials of their students and have relatively similar admissions rates, so provide little information about school selectivity.24 We treat the Elite and Public Ivy clusters as highly selective (s=1) and the remaining clusters as less selective (s=0). Table 1 indicates very little difference between whites and blacks in the fraction attending schools in either highly selective cluster. Within each race, students at the most selective schools have much better credentials than students at less selective schools.

We examine several categories of outcome measures, each with advantages and disadvantages for our investigation. First, we consider law school grades. We focus on grades during the first year of law school, when curricula are typically standardized and grades issued on strict curves.25 First year grades are important determinants of access to prestigious internships and post-graduation clerkships (which are typically offered in the second year). The first row in the third panel of Table 1 reports the grade point average, which in the BPS is standardized within law schools. The second row converts this to a class rank measure, under the assumption that GPAs are normally distributed within each law school. The average black student is at the 23rd percentile of her class and the average white student at the 54th percentile.

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23 We focus exclusively on the cluster in which the student matriculated. Though a few students transfer subsequently, more than 98% of students in our sample who graduate from any BPS law school do so from one in the same cluster as the one in which they matriculated.

24 These four clusters are distinguished on other dimensions, like public funding and school size, that have little relevance for our analysis. Our results are robust to defining only the “Elite” cluster as highly selective.

25 Analyses of cumulative grades yield similar results.
Our second category of outcomes relate to attainment. We use three measures here: An indicator for graduating from law school and two indicators of bar exam passage. The latter differ only in the treatment of the 7% of graduates who do not attempt the bar exam. As some law school graduates choose not to practice law—so need not take the exam—it is not clear whether a non-taker should be counted as a failure. Our first measure does so, while our second omits graduates who did not attempt the exam. In each case, we count any passage—regardless of the number of attempts—by July 1996 as a positive outcome.

Our final category of outcome measures concern post-graduation labor market experiences. We rely on a follow-up survey given to a subset of BPS respondents in late 1994, four to six months after their scheduled graduation. Few non-graduates responded to this post-law-school survey, so we restrict attention to graduates. We construct three measures: An indicator for full time employment; a measure of the quality of the job; 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. For the job quality and salary measures, we restrict attention to respondents with full-time jobs, although results are not sensitive to this. 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 closely related with students’ capacity to keep up with their classmates. On the other hand, grades are difficult to compare across schools.

26 There is some misclassification in the latter, as 14 states (accounting for about 4.4% of students who pass the bar) do not report failed attempts at the exam. A back-of-the-envelope calculation suggests that about 3% of the graduates that we count as non-takers in fact failed the exam in one of these states.
27 97% of students who pass by July 1996—two years after the scheduled graduation date—do so by July 1995, so the BPS’ truncated record of exam outcomes likely misses only a few passages. Analyses of first-time passage rates yield similar results.
28 Similarly, results for the employment outcome are not sensitive to the coding of those employed part time.
in the best of circumstances; when they are standardized within school as in the BPS, only comparisons relative to other students at the same school are possible. We therefore interpret our class rank analyses as measures of how mismatched students are rather than as measures of the effects of mismatch. The achievement threshold required for graduation may also vary somewhat across schools (and possibly even across race within schools; see Mansfield, 2001), though this is certainly closer to an absolute measure than is class rank.

Employment outcomes are subject to somewhat different biases. Employers of young lawyers may themselves practice affirmative action, resulting in better outcomes for black than for white lawyers at the same level of achievement. This corresponds to a positive $\gamma$ in equation (1), and biases upward the black-white comparison relative to the effect of affirmative action on academic achievement. Of course, this bias may be offset by the biases discussed earlier, which operate in the opposite direction. Moreover, 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 conditional on admissions credentials can be interpreted as a measure of the combined effect of admissions and hiring preferences.

The bar exam outcome differs from the others in important ways. Crucially, bar exams are graded blind, with the test-takers’ identities anonymous, so there should be no direct effects of race or of school quality beyond the effects of each on academic achievement. Nearly all past studies of mismatch (e.g. Kane, 1998) have been forced to

29 The exam’s difficulty varies across states, and graduates need only take the exam in the state where they hope to practice. Our measures can be seen as an indicator for whether the student’s achievement level permits her to pass the exam in any of the states where she would be willing to practice. They would provide a
rely on grades, graduation, and salaries as outcome variables, and have therefore been subject
to the potential biases outlined above. Our examination of bar exams provides a useful
check on results based on these more commonly available measures.

V. Measuring Preferences

Even the least selective law schools have competitive admissions, and only 56% of
the 92,648 applicants from the BPS cohort were admitted to any law school (Barnes and Carr, 1992; see also Wightman, 1997). This contrasts with undergraduate education where,
as Kane (1998) notes, most colleges do not have selective admissions. Figure 2 relates
admissions rates for cells defined by race, LSAT, and UGPA (from Barnes and Carr, 1992)
to the admissions index percentile of typical students in the cells. White students whose
credentials would have placed them in the bottom quarter of the BPS distribution were more
likely to be rejected from all the schools to which they applied than to be accepted anywhere.
Conversely, black admission rates were above 50% everywhere above the fifth percentile,
and were double or more those of similarly-qualified whites through the lower part of the
distribution.

The significant racial gap in the probability that poorly-credentialed applicants will be
admitted to any law school raises the possibility of selection bias in black-white comparisons.
A likely explanation for the gap in admissions is that even the least selective schools apply
lower thresholds for admission to black than to white applicants. If these schools consider
both observed (LSAT and UGPA) and unobserved (e.g., personal statements, letters of
reference, extracurricular and work experience, etc.) credentials, their use of affirmative
action will differentially truncate the distribution of unobserved credentials among black and

biased measure of true achievement if selectivity or race were correlated with students’ geographic preferences. As a check, we have re-run our analyses including controls for the (possibly endogenous) geographic region in which the student took the exam. This has essentially no impact on the results.
white students who make it into our sample of matriculants. \(^{30}\) This will produce negative black-white differences in \(X^2 \beta_2\) conditional on \(X^1\) in the population that attends law school relative to the population of law school applicants, biasing our black-white comparison against black students and leading us to overstate mismatch effects. The bias is largest at the lowest \(X^1\) values, where the gap in the probability of gaining admission is the greatest. Black-white comparisons in this range must be interpreted with caution. Figure 2 indicates that comparisons based on the top four quintiles of the admissions index distribution will be relatively free of bias.

Figure 3 displays the fractions of white and black students in the BPS sample—which includes only matriculants—who attend schools in the two highly selective clusters, as functions of the admissions index percentile. Throughout the index distribution, black students are much more likely to attend schools in these clusters than are white students.

The estimates in Figures 2 and 3 reflect a series of student decisions—about where to apply in each figure, and in Figure 3 about where to enroll—in addition to the direct effects of admissions preferences. Krueger, Rothstein, and Turner (2006) show that black college applicants tend to apply to more selective schools than do whites with the same credentials. If this pattern holds in law school as well, Figures 2 and 3 likely understate and overstate, respectively, the direct effects of preferences on the relevant admissions decisions.

Ignoring this issue, and assuming that black and white students would matriculate at selective schools at the same rates (conditional on observed credentials) if there were no race-based preferences, \(^{31}\) the difference between the black and white series in Figure 3

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\(^{30}\) Specifically, assume that student \(i\) is admitted to some law school only if \(X^1 \delta_1 + X^2 \delta_2 > c(b)\), where \(\delta_1\) and \(\delta_2\) are positive and \(c(b)\) is a race-specific constant with \(c(0) > c(1)\). Then \(E[X^2 \mid X^1, b, \text{admitted to some school}] = E[X^2 \mid X^2 > \delta_2^{-1}(c(b) - X^1 \delta_1)]\). For reasonable joint distributions of \((X^1, X^2, b)\), this is decreasing in \(b\), particularly at \(X^1\) values for which the admissions constraint is most binding.

\(^{31}\) In other words, the distribution of students across the always-taker, never-taker, complier, and defier groups is the same for black and white students conditional on \(X^1\). We also assume that the same subset
measures the complier share, the fraction of black students who attend highly selective schools with preferences but would attend less selective schools if preferences were not available. The left panel of Figure 4 displays this difference, along with a 90% confidence interval. It indicates that black students near the middle of the BPS credentials distribution are around forty percentage points more likely to attend highly selective schools than are whites with the same credentials. The difference is largest for the highest-scoring students and lowest at the bottom of the distribution.

As noted earlier, the highly-selective category likely does a poor job of capturing the variation in the selectivity of schools attended by low-credentials students. We can interpret our binary selectivity measure as an indicator for continuous underlying selectivity, and use the fraction of students that we classify as attending highly selective schools to recover the mean of the underlying distribution. The right panel of Figure 4 shows the estimated black-white difference in latent selectivity. This is computed as $\sigma(\Phi^{-1}(f(1, X^1)) - \Phi^{-1}(f(0, X^1)))$, where $f(b, X^1)$ is a local linear estimate of the fraction of race-b, credentials-X^1 students attending s=1 schools and $\sigma$ is the residual variance of latent selectivity. 32 This is the semiparametric analogue to the $\pi_2$ coefficient in the parametric model (13). The probit transformation makes the complier share much more stable, around 0.75 – 1 through most of the distribution.

Table 2 presents regression estimates of the overall complier share, averaged across the index distribution. The first two columns show the coefficients from linear probability models for attendance at a school in one of the two highly selective clusters. When we do of applicants would ultimately enroll in law schools in the absence of preferences, and that the change in admissions rules would affect only their distribution across types of schools. As noted earlier, this is a strong assumption, particularly with regard to students with low LSATs and UGPAs.

32 $\sigma$ is chosen to ensure that the variance of selectivity equals one in the white sample.
not control for entering credentials (col. 1), we find no black effect on selectivity. When we add quadratic controls for (LSAT, UGPA), in column 2, the black effect turns large and positive, indicating that blacks are nineteen percentage points more likely to attend highly selective schools than whites with similar credentials. This effect is robust to the inclusion of higher-order terms in the observed credentials. Column 3 reports the black coefficient from a probit model, which we have rescaled to make the latent variable have unit variance among whites in the sample. This corresponds to the black-white difference in latent selectivity that is shown in Figure 4B. Black students attend schools that are about three quarters of a standard deviation more selective than do whites with similar entering credentials.

A potential concern with our use of the full sample is that in the absence of preferences, many low-scoring black students would not be admitted to any law school. If these students differ in unobserved ways from those who would be admitted, this can introduce a sample selection bias in our black-white comparisons. As noted earlier, we can largely avoid this bias by focusing on the upper four quintiles of the admissions index distribution. Columns 4-6 repeat the first stage specifications for this subsample. The black effect on binary selectivity is nearly double that seen in the full sample. This is primarily a function of the binary measure’s inability to capture variation in selectivity at the bottom of the distribution; the change in the latent selectivity estimate is much smaller.

It remains to be shown that selectivity causes greater mismatch between students and their peers, holding entering credentials constant. To examine this, we look to students’ first-year class ranks. For any given student, a more competitive environment should lead to a lower rank. Figure 5 presents mean rank in class (computed from first year grades and scaled so that the top rank is 1 and the bottom 0) as a function of race, school type, and the
admissions index. More qualified students have higher ranks than those with lower index scores, and students at less selective schools higher ranks than similarly-qualified students at more selective schools. Our selectivity measure is this validated as a measure of the degree of mismatch. Note, however, that white students achieve higher ranks than blacks, even within school selectivity categories. 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 this comparison is biased against blacks by differences in unobserved ability or by direct race effects.

Table 3 presents regression estimates of selectivity effects on rank. The first two columns show OLS estimates of the effects of attending a school in one of the highly-selective clusters, separately for whites and for blacks. We control for a quadratic in (LSAT, UGPA), as in Table 2, but allow these coefficients to vary by race. Attending selective-cluster schools appears to lower rank by about 0.06 for whites, and by twice that for blacks. Effects on blacks in the top four quintiles, shown in the lower panel, are even larger.

The next three columns present the black-white comparison for class rank. Column 3 is the reduced form: When differences in entering credentials are controlled, black students have ranks about 0.19 lower than whites (or 0.23 in the upper four quintiles). Column 4 presents IV estimates of the effect of moving from the “unselective” to the “selective” category, using race as the instrument. These estimates indicate impossibly large negative selectivity effects, suggesting that a shift to from less- to more-selective schools reduces rank by nearly the entire possible range. The most plausible explanation is that the black-white difference in the selectivity of the school attended is larger than is captured by our binary proxy. Our latent selectivity measure should help to address this. Indeed,

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33 The series are truncated at the 2.5 and 97.5 percentiles of the race-specific distributions.
Column 5 shows that the IV estimate of the effect of a one standard deviation increase in latent selectivity is a more reasonable -0.26 (-0.31 in the upper quintiles).

Regardless of the comparison used, then, selectivity appears to have substantial negative effects on class rank. IV estimates are much larger than OLS, perhaps reflecting an upward bias in OLS deriving from differences in the unobserved abilities of students attending more- and less-selective schools or a downward bias in IV.

VI. Results

Figure 4 and Table 2 indicate large differences in the selectivity of the schools attended by black and white students with similar entering credentials, and each of our comparisons in Table 3 indicates that selectivity leads to greater mismatch. If mismatch lowers outcomes for marginal students, both the selective-unselective and black-white comparisons should show negative effects on “real” outcomes.

We begin with graphical analyses of the bar passage outcome. We use our primary bar passage measure here, coding students who never attempted the exam as failures. Figure 6 shows average bar passage rates by race, selectivity of the school attended, and percentile of the admissions index. Above about the 20th percentile of the distribution, these are quite linear and there are only small differences between groups, in favor of whites relative to blacks and, within race, showing a slight edge for students at selective schools. Below the 20th percentile, however, several of the series curve sharply downward, and fewer than one third of the black students at less selective schools with the lowest entering credentials ultimately pass the bar exam.

Figure 7 shows the graphical version of the selective-unselective comparison. The point estimates are simply the differences between the relevant series in Figure 6. We also show 90% pointwise confidence intervals, obtained by bootstrapping the local linear
regressions used to compute these series. The differences between bar passage rates of students at more and less selective schools are small, and generally positive. The one exception is black students with the lowest entering credentials, among whom the selective-unselective difference is negative, reasonably large, and sometimes significant. This comparison is based on a small sample, however: Only 14% of black students with bottom-quintile credentials attend selective schools.

Figures 6 and 7 support our assumption that bar passage rates can be modeled with a simple polynomial in students’ entering credentials, at least above the very bottom of the credentials distribution, and that the shape of this polynomial does not vary dramatically across race or selectivity categories. Table 4 reports regression versions of the selective-unselective comparison for all of our outcomes, controlling for our usual quadratic in (LSAT, UGPA). For white students (columns 1-2), the selectivity effect is positive and significant on five of our six outcomes, with an insignificant negative effect only for full-time employment. The estimated selectivity effects for black students (columns 3-4) are positive and significant for graduation and salaries; all others are small and insignificantly different from zero. The final columns show p-values for the hypotheses that the white and black effects are equal or are both zero. We marginally reject equality for three outcomes, reflecting larger effects on white bar passage (using our second measure) and job quality and smaller effects on white employment. By contrast, we decisively reject the hypothesis that the effects are both zero for four outcomes and marginally reject for a fifth.

As noted earlier, OLS may yield upward-biased estimates of the selectivity effect. We turn next to our black-white comparison, which is likely to be biased in the opposite direction. We begin with the reduced-form, focusing on black-white differences in
outcomes, and then convert these to IV estimates of the selectivity effect for black compliers.

Figure 8 presents the black-white gap in bar passage rates as a function of students’ entering credentials. At the very bottom of the index distribution, white passage rates exceed those of blacks by about 15 percentage points. This gap shrinks as we move up the distribution, however, and is insignificantly different from zero at all points above the 40th percentile. The gap grows above the 80th percentile, but there are very few black students in this range and the confidence interval is extremely wide.

Table 5 presents regression versions of the reduced-form black-white comparison for each of our outcomes. Black students have significantly worse graduation and bar passage results than similarly-qualified whites, with the bar passage coefficient a large -9%. Since black students attend more selective schools than do whites with the same credentials, these estimates are consistent with negative selectivity effects. By contrast, the black effects on employment outcomes are positive, in two cases large and significant. Column 2 presents estimates from the subsample of students with entering credentials above the 20th percentile of the admissions index distribution, among whom sample selection bias is plausibly less severe. All of the point estimates are notably more positive in this subsample, and there is no outcome for which the estimate is significantly negative.

We have explored several alternative specifications for both the black-white and selective-unselective comparisons. Results are robust to semiparametric controls (implemented via matching techniques) for the LSAT score and undergraduate GPA and to
the inclusion of controls for a variety of other student characteristics, including gender, age, and parental education.\textsuperscript{34}

Table 6 presents IV estimates of the selectivity effect corresponding to the reduced forms in Table 5. We focus on estimates of the effect of latent, continuous selectivity (columns 2 and 4), though for completeness we also present results (columns 1 and 3) that use the binary selectivity variable in the first stage. In the full sample, we find that attending a school that is one standard deviation more selective reduces the average black complier’s graduation and bar passage probabilities by 5 and 12-13 percentage points, respectively. Effects on employment outcomes are uniformly positive and significant. Attending a school that is one standard deviation more selective raises the probability of obtaining a prestigious job (conditional on graduating and being employed) by 33 percentage points and raises mean log salaries by 0.19.

When we restrict attention to the top four quintiles (columns 3 and 4), all of the effects become more positive. (Recall that the class rank effect is as negative in this subsample as in the full sample, indicating that selectivity remains a good proxy for mismatch.) We find no significant effects on graduation and bar passage, despite similar precision to the full-sample estimates. The effects on employment outcomes that we found in the full sample are here substantially larger: A standard deviation increase in selectivity raises full time employment rates by 21 percentage points and among the employed raises the probability of having a prestigious job by 39 percentage points and raises salaries by 22 percent.

\textsuperscript{34} Results are reported in Rothstein and Yoon (2006). Frölich (2007) develops the econometric results for the IV estimator with semiparametric controls.
VII. Evaluating a Simple Structural Model

Our results contrast sharply with those of Sander (2004), who concludes that mismatch effects in law school are enormous. Sander does not provide an explicit discussion of identifying assumptions, nor even calculate the magnitude of the selective school effect that is central to his conclusions. He implicitly argues, however, from a three-equation structural model in which (1) race affects school selectivity via affirmative action preferences; (2) selectivity affects student academic performance, as measured by law school grades or class rank, via mismatch effects; and (3) selectivity and academic performance jointly affect later outcomes like graduation and bar exam passage. In the context of this model, even our IV analysis is a reduced form that combines the direct effect of selectivity on outcomes with an indirect effect operating through grades.

Our analysis echoes Sander’s results for the first two equations. The difference is in the third: Sander’s strategy requires him to identify the causal effect of a student’s law school grades on her later outcomes. He estimates this with OLS regressions with controls for the school attended and the student’s race, sometimes also controlling for entering credentials (as in his Table 6.1, p. 444) and sometimes not (Table 7.1, p. 458). With or without these controls, this strategy is not credible. Unobserved student ability has positive effects on both law school grades and later outcomes, biasing the estimated effect of grades upward. As we have seen, both selective-unselective and black-white comparisons indicate negative effects of selectivity on students’ law school grades (relative to their classmates). Thus, the upward bias in Sander’s estimate of the effect of grades on outcomes leads him to a downward-biased estimate of the net effect of affirmative action and to an overstatement of mismatch.
With only one instrument, we cannot distinguish between direct effects of selectivity and indirect effects operating through grades. Nevertheless, the structural analysis – if correct – has implications for the magnitude of the reduced-form black-white difference in outcomes. In an appendix, we compare our estimates with a simplified version of Sander’s strategy. Using identical samples and specifications, the structured approach implies a black-white gap in graduation rates that is more than twice as large as is evident in the data, most likely reflecting upward bias in the estimated causal effect of law school grades.

VIII. Conclusion

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

There are many analytical and substantive reasons to focus on law schools for the study of mismatch, and the Bar Passage Study data are well suited for non-experimental analyses. This paper has explored two exclusion restrictions, with opposing likely biases, using data on law students’ graduation rates, bar exam passage rates, and early career employment outcomes.

We find little convincing support for claims that mismatch is an important consequence of affirmative action in law school admissions. We reject large negative mismatch effects on graduation and bar passage rates for any but the least qualified law school students.

For students in the bottom quintile of the entering credentials distribution, the data are consistent with sizable mismatch effects on black graduation and bar passage rates. This
quintile cannot be ignored, as it contains the majority of black applicants and black law students. Interpretive caution is required, however. In this range, black applicants are much more likely than white applicants to gain admission to some law school. The gap in admissions rates may generate gaps in unobserved credentials between the black and white students who are admitted. With the available data, it is impossible to distinguish true mismatch effects from sample selection biases.

We 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%. As noted above, this could reflect affirmative action on the part of employers rather than “reverse mismatch” (Alon and Tienda, 2005) effects on academic success. A crucial question – which we cannot answer – is whether firms’ hiring patterns would change if law schools were to eliminate affirmative action. If large, high-salary firms would recruit from less selective schools, if necessary, to obtain black lawyers, the black salary premium might persist. Thus, our analysis does not provide clear support for the claim that affirmative action in law school admissions helps black students after law school, as the benefit may derive from affirmative action in employment. It does demonstrate, however, that the combined effect is positive. There is little compelling evidence for negative effects of admissions preferences, and the evidence points decisively against the mismatch hypothesis for all but the least qualified students.

Appendix: Comparing our analysis with Sander (2004)

This appendix attempts to account for the differing results of our study and that of Sander (2004). As noted in the text, Sander uses a somewhat structural model and finds large mismatch effects. We identified an important source of potential bias in his analysis, deriving from the key role of an endogenous variable, students’ law school grades, in his model. We focus here on Sander’s analysis of mismatch effects on dropout rates, and we
follow Sander’s variable and sample construction here despite minor differences from the
definitions used in the main text (explained in the Data Appendix). We also follow Sander
in treating school cluster as a cardinal variable, ranging from 1 to 6 (with 5 and 6 our “highly
selective” group), and we include only linear controls for undergraduate GPA and LSAT
scores. The basic differences between the approaches come through even in these very
simple specifications.

Appendix Table A demonstrates the differences between the “structured” and
reduced-form approaches. Parts III and V of Sander’s paper demonstrate, respectively, that
black students attend substantially more selective schools than do white students with the
same entering credentials and that blacks obtain lower GPAs in law school. Columns 1 and
2 of Appendix Table A present estimates of specifications for law school tier and first year
GPA that, while somewhat different than those that Sander presents, yield similar estimates.
Holding constant LSAT and GPA, there is nearly a full cluster difference between the
schools that black and white students attend. The between-race difference equals the effect
of more than 9 LSAT points (on a 10-48 scale) or 1.2 undergraduate GPA points. Not
surprisingly, this affects black students’ law school GPAs, particularly when they are
standardized within schools as in the BPS: Black GPAs are 0.7 standard deviations below
those of similarly-credentialed whites, an effect as large as that produced by 18.4 LSAT
points or 2.5 undergraduate GPA points.

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

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

As noted in the text, there is reason to worry that the law school GPA coefficient in
column 3 is biased downward relative to the causal effect of law school grades on dropout
rates. Presumably, students with high unobserved ability both earn higher grades and drop

---

35 The LSAT and GPA weights are quite different from those used to compute the admissions index,
but this derives from the odd specification for the dependent variable. When a more sensible variable—e.g. an
indicator for attending a highly selective school—is used the weights are quite close to those used in the index.
36 Sander reports “standardized coefficients,” where we report un-transformed logistic coefficients.
Standardization aside, the estimates in our Column 3 are identical to those that Sander reports in his Table 5.6.
out at lower rates. If so, our formalization of Sander’s argument will overstate the effect of affirmative action on black students’ dropout rates.

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

We found in the main text that the reduced-form black effect is concentrated in the bottom quintile of the entering credentials distribution, with a very small black-white difference conditional on credentials in the upper four quintiles. Column 5 of Table A presents a final specification that estimates separate black effects in the bottom quintile and top four quintiles. As in our main analysis, the black effect appears entirely in the bottom quintile, and in the top four quintiles blacks are slightly (and insignificantly) less likely to drop out than are whites.

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

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

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

---

37 This is quite similar to the 2.1 percentage point marginal effect from a more flexible model reported in Table 2.
Data Appendix

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

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

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

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

A second limitation to the bar exam measure is that 14 states report successful attempts at the exam but not failures. Some students who are not observed to attempt the exam, then, in fact failed it in one of these states. As these states represent only a small fraction (about 4%—Wightman 1998) of observed passages, we expect that the number of misclassifications is small. Misclassification rates for the outcome of the first attempt are likely higher, so we focus on a measure of whether students ever pass the exam. As noted in the text, we use one measure that counts (observed) non-attempters as failures and another that excludes those non-attempters who graduated law school.38

Admissions Index

As noted in the text, our graphical analyses use a percentile rank formed from a single index of the LSAT score and the undergraduate GPA. After converting these two variables to range from 0 to 1—the GPA theoretically ranges from 0 to 4 and the LSAT ranged from 10 to 48 in 1991—we average these with 0.4 weight on the GPA:39

\[
\text{Index}_i = 400 \times \frac{UGPA_i}{4} + 600 \times \frac{LSAT_i - 10}{48 - 10}.
\]

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

39 21 students have reported undergraduate GPAs of 4.1 and 5 have 4.2s. These may come from schools that give extra points for honors courses. We do not censor the data, so there are 6 observations with index values above 1000. Note also that the LSAT has since converted to a different scoring scale.
These weights are those used by Sander (2004). We have explored two alternative weights. The first uses fitted values from a probit model for attending a school in the highly selective clusters, including a black indicator as an additional control. The second uses fitted values from an OLS regression for the first year GPA, with a black indicator and cluster dummies as additional controls. Each of these correlates above 0.999 with Sander’s index, and results are not sensitive to the use of either.40

Outcome Measures

We describe here the construction of our seven outcome measures.

- 1st year class rank. As noted in the text, the law school GPA variables in the BPS are standardized (to mean zero and standard deviation 1) within each law school. We convert the first year GPA measure to a class rank, assuming that GPAs are normally distributed within each school. Thus, rank = Φ(standardized GPA); it ranges from 0 to 1.

- Graduation status. This is set to 1 for students who graduated law school and to 0 for students who stopped out of law school or who had not graduated by the close of the study. It is set to missing for students who withdrew from the study, either because they transferred to a non-participating school or because they were no longer comfortable participating.

- Bar passage. This is set to 1 for students who were observed to pass the exam and to 0 for all others, regardless of graduation or study participation status. (Note that bar exam outcomes were collected for students who withdrew from the study, though about half are not observed to take the exam.)

- Bar passage if attempt. This is identical to the previous variable, except that students with graduation status equal to 1 who were never observed to attempt the exam are excluded.

- Full time employment. This and the remaining outcomes are available only for students sampled for the follow-up survey (see below).

- “Good” job. This is set to 1 for graduates whose “current work setting” is a judicial clerkship (14.0%), academic (0.8%), a prosecutor’s office (4.4%), a public defender’s office (2.3%), or a large private law firm (20.0%). It is set to zero if the current work setting is a medium (10.6%) or small (19.1%) firm, a solo practice (3.1%), a legislative office (8.3%), a public interest group (2.5%), a business or financial institution (7.7%), “other” law-related work (3.8%), or other non-law related work (2.8%). This classification is of course arbitrary; we have explored other classifications with similar results. Blacks are notably overrepresented (controlling for their admissions indices) in academia, prosecutors’ offices, large private firms, legislative offices, government agencies, and public interest groups; they are substantially underrepresented in mid-size and small firms and solo practices.

- Ln(salary). Salaries are reported in 8 bins. We assign each observation to the middle of the bin (using $10,000 as the middle of the “<20,000” bin and $90,000 as the

40 Note that the models in columns 1 and 2 of Appendix Table 1 assign somewhat greater weight to the UGPA than does our index. The odd specifications in these models—chosen to conform to those used by Sander (2004)—account for this. In any case, indices generated using these models are also highly (>0.99) correlated with our index.
middle of the “>$80,000” bin), then compute the log. Individuals who are not employed full time are excluded.

**Follow up survey**

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

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

**References**


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

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

Figure 2.
Fraction of applicants admitted to at least one school, by race and index percentile (normed to BPS distribution)
Figure 3.
Fraction of blacks and whites at highly selective law schools, by index percentile

Note: Fractions are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index.

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

Note: Fractions are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing 500 bootstrap samples from the underlying data and re-estimating the differences in these samples. In the right panel, the CI is censored from above at +1.5. Series are shown only for points between the 0.25 percentile of the black admissions index distribution and the 99.75 percentile of the white distribution.
Figure 5.
First year class rank by race, law school selectivity, and admissions index

Note: Ranks are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for points between the 2.5 and 97.5 percentiles of the race-specific admissions index distributions.

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

Note: Rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for points between the 2.5 and 97.5 percentiles of the race-specific admissions index distributions.
Figure 7.
Selective-unselective difference in bar passage, by race and index percentile

Note: Passage rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for points between the 2.5 and 97.5 percentiles of the race-specific distributions. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing bootstrap samples from the underlying data and re-estimating the differences in these samples. Estimates and CIs are censored at +/- 0.15.

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

Note: Passage rates are smoothed using a local linear regression smoother (with an Epanechnikov kernel and optimal "rule of thumb" bandwidths) applied to the underlying admissions index. Estimates are shown only for points between the 0.5 percentile of the white distribution and the 99.5 percentile of the black distribution. 90% pointwise confidence intervals are indicated by shaded areas, and computed by drawing bootstrap samples from the underlying data and re-estimating the differences in these samples. CIs are censored at -0.2.
Table 1. Summary statistics

<table>
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<tr>
<th></th>
<th>Full sample</th>
<th>By race</th>
<th>By race and selectivity</th>
<th>Blacks</th>
<th>Whites</th>
<th>Blacks</th>
<th>Whites</th>
<th>Sel. Unsel. Blacks</th>
<th>Sel. Unsel. Whites</th>
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<td>S.D. (2)</td>
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<td>Unsel. (6)</td>
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<td>Unsel. (8)</td>
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<td>100%</td>
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<td>Female</td>
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Admissions credentials

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<td>UGPA</td>
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<td>824</td>
<td>740</td>
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Law school type

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<td>Selective (top 2 clusters)</td>
<td>24%</td>
<td>0.429</td>
<td>23%</td>
<td>24%</td>
<td>100%</td>
<td>0%</td>
<td>100%</td>
<td>0%</td>
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<tr>
<td>Elite (top cluster)</td>
<td>8%</td>
<td>0.271</td>
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<td>8%</td>
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<td>33%</td>
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Outcomes

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<td>0.98</td>
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<td>0.15</td>
<td>-1.15</td>
<td>-0.97</td>
<td>0.17</td>
<td>0.14</td>
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<td>1st year class rank (est.)</td>
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<td>0.29</td>
<td>0.23</td>
<td>0.54</td>
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<td>0.24</td>
<td>0.55</td>
<td>0.54</td>
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<tr>
<td>Graduated from law school?</td>
<td>91%</td>
<td>29%</td>
<td>81%</td>
<td>92%</td>
<td>90%</td>
<td>78%</td>
<td>95%</td>
<td>91%</td>
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<tr>
<td>Ever pass bar exam?</td>
<td>81%</td>
<td>39%</td>
<td>57%</td>
<td>83%</td>
<td>69%</td>
<td>53%</td>
<td>86%</td>
<td>82%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ever pass bar (if attempted)?</td>
<td>86%</td>
<td>35%</td>
<td>61%</td>
<td>88%</td>
<td>75%</td>
<td>57%</td>
<td>93%</td>
<td>87%</td>
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<tr>
<td>Empl. full time (if grad.)</td>
<td>66%</td>
<td>47%</td>
<td>63%</td>
<td>67%</td>
<td>74%</td>
<td>60%</td>
<td>70%</td>
<td>65%</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&quot;Good&quot; job (if employed)</td>
<td>40%</td>
<td>49%</td>
<td>44%</td>
<td>40%</td>
<td>53%</td>
<td>40%</td>
<td>58%</td>
<td>34%</td>
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<td>Salary (if FT; $1,000s)</td>
<td>$39.8</td>
<td>$18.8</td>
<td>$38.0</td>
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<td>Log salary (if FT)</td>
<td>10.51</td>
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<td>10.35</td>
<td>10.72</td>
<td>10.44</td>
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Table 2. Black-White differences in selectivity

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<th>Top 4 quintiles</th>
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<td></td>
<td>Linear probability</td>
<td>Probit</td>
<td>Linear probability</td>
<td>Probit</td>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Black</td>
<td>-0.016</td>
<td><strong>0.191</strong></td>
<td>0.732</td>
<td>0.232</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.012)</td>
<td>(0.038)</td>
<td>(0.022)</td>
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<td>LSAT</td>
<td><strong>-0.092</strong></td>
<td><strong>-0.149</strong></td>
<td>-0.257</td>
<td>-0.554</td>
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<td></td>
<td>(0.005)</td>
<td>(0.018)</td>
<td>(0.014)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>(LSAT/100)^2</td>
<td><strong>8.378</strong></td>
<td><strong>13.671</strong></td>
<td><strong>21.459</strong></td>
<td><strong>44.863</strong></td>
</tr>
<tr>
<td></td>
<td>(0.638)</td>
<td>(2.225)</td>
<td>(1.420)</td>
<td>(4.541)</td>
</tr>
<tr>
<td>UGPA</td>
<td><strong>-0.481</strong></td>
<td>-0.330</td>
<td><strong>-1.403</strong></td>
<td><strong>-2.690</strong></td>
</tr>
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<td></td>
<td>(0.076)</td>
<td>(0.275)</td>
<td>(0.139)</td>
<td>(0.455)</td>
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<tr>
<td>(UGPA/10)^2</td>
<td>-0.124</td>
<td><strong>-9.918</strong></td>
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<tr>
<td></td>
<td>(1.189)</td>
<td>(4.220)</td>
<td>(1.606)</td>
<td>(5.302)</td>
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<td>LSAT * UGPA</td>
<td><strong>0.018</strong></td>
<td><strong>0.041</strong></td>
<td>0.037</td>
<td><strong>0.088</strong></td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.004)</td>
<td>(0.002)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Constant</td>
<td><strong>0.244</strong></td>
<td><strong>1.850</strong></td>
<td>0.117</td>
<td><strong>0.271</strong></td>
</tr>
<tr>
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<td>(0.003)</td>
<td>(0.159)</td>
<td>(0.585)</td>
<td>(0.003)</td>
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<td>24,049</td>
<td>24,049</td>
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<tr>
<td>R2</td>
<td>0.00</td>
<td>0.17</td>
<td>0.01</td>
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Notes: Dependent variable is an indicator for attending a school in the "elite" and "public ivy" clusters. Probit model coefficients are scaled so that the latent variable has standard deviation one among white students in the sample.
Table 3. Selectivity effects on first year class rank, OLS and IV

<table>
<thead>
<tr>
<th></th>
<th>Selective-unselective comparison</th>
<th>Black-white comparison</th>
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<tr>
<td></td>
<td>Binary selectivity</td>
<td>Reduced form</td>
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<tr>
<td></td>
<td>Whites</td>
<td>Blacks</td>
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<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Full sample</td>
<td></td>
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<tr>
<td>Selective</td>
<td>-0.060</td>
<td>-0.116</td>
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<tr>
<td></td>
<td>(0.005)</td>
<td>(0.015)</td>
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<td></td>
<td>(0.008)</td>
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<td>Top 4 quintiles</td>
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<td>(0.026)</td>
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<td>17,853</td>
<td>412</td>
</tr>
</tbody>
</table>

Notes: All specifications control for quadratic terms in the LSAT score and undergraduate GPA and for a linear interaction between the two. See text for discussion of latent selectivity calculations. Standard errors in column 5 are computed using the bootstrap, with 200 replications.
Table 4. Selectivity effects on outcomes: Selective-unselective comparison (OLS)

|                                      | Whites N |          |          | Blacks N |  |          |          |          |
|--------------------------------------|----------|----------|----------|----------|  |----------|----------|----------|
|                                      | (1)      | (2)      | (3)      | (4)      |  | (5)      | (6)      |
| Law school graduation                | 0.029    | 22,081   | 0.049    | 1,809    |  | 0.64     | 0.00     |
|                                      | (0.005)  |          | (0.024)  |          |  |          |          |
| Bar passage                          |          |          |          |          |  |          |          |
| Any                                  | 0.014    | 22,213   | -0.007   | 1,836    |  | 0.20     | 0.11     |
|                                      | (0.006)  |          | (0.029)  |          |  |          |          |
| If attempted                         | 0.025    | 20,862   | -0.002   | 1,705    |  | 0.03     | 0.00     |
|                                      | (0.006)  |          | (0.029)  |          |  |          |          |
| Employment                           |          |          |          |          |  |          |          |
| Has a full-time job                  | -0.040   | 2,306    | 0.054    | 838      |  | 0.02     | 0.05     |
|                                      | (0.025)  |          | (0.043)  |          |  |          |          |
| "Good" job, if FT employed           | 0.105    | 1,532    | 0.021    | 537      |  | 0.06     | 0.01     |
|                                      | (0.031)  |          | (0.052)  |          |  |          |          |
| Ln(salary), if FT employed           | 0.153    | 1,501    | 0.227    | 528      |  | 0.52     | 0.00     |
|                                      | (0.030)  |          | (0.053)  |          |  |          |          |

Notes: Coefficients on a selective school indicator are reported. All specifications include controls for a quadratic in (LSAT, UGPA). P values in columns 5-6 derive from tests conducted on a pooled, fully-interacted model.
<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>Top four quintiles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Law school graduation</td>
<td>-0.033</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.013)</td>
</tr>
<tr>
<td>Bar passage</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Any</td>
<td>-0.089</td>
<td>-0.016</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>If attempted</td>
<td>-0.093</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>Employment</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Has a full-time job</td>
<td>0.051</td>
<td><strong>0.140</strong></td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.064)</td>
</tr>
<tr>
<td>&quot;Good&quot; job, if FT employed</td>
<td><strong>0.208</strong></td>
<td><strong>0.287</strong></td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.075)</td>
</tr>
<tr>
<td>Ln(salary), if FT employed</td>
<td><strong>0.100</strong></td>
<td><strong>0.157</strong></td>
</tr>
<tr>
<td></td>
<td>(0.045)</td>
<td>(0.071)</td>
</tr>
</tbody>
</table>

*Notes*: Coefficients on a black indicator are reported. All specifications include controls for a quadratic in (LSAT, UGPA). All models estimated by OLS (i.e. linear probability for binary outcomes)
Table 6. IV estimates of local average treatment effects of school selectivity

<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>Top four quintiles</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Binary</td>
<td>Latent</td>
</tr>
<tr>
<td>Law school graduation</td>
<td>-0.171</td>
<td>-0.045</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Bar passage</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Any</td>
<td>-0.468</td>
<td>-0.122</td>
</tr>
<tr>
<td></td>
<td>(0.066)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>If attempted</td>
<td>-0.487</td>
<td>-0.127</td>
</tr>
<tr>
<td></td>
<td>(0.062)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Employment</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Has a full-time job</td>
<td>0.469</td>
<td>0.158</td>
</tr>
<tr>
<td></td>
<td>(0.110)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>&quot;Good&quot; job, if FT employed</td>
<td>0.815</td>
<td>0.326</td>
</tr>
<tr>
<td></td>
<td>(0.124)</td>
<td>(0.043)</td>
</tr>
<tr>
<td>Ln(salary), if FT employed</td>
<td>0.460</td>
<td>0.186</td>
</tr>
<tr>
<td></td>
<td>(0.103)</td>
<td>(0.041)</td>
</tr>
</tbody>
</table>

Notes: Instrument in each model is student race. All include controls for a quadratic in (LSAT, UGPA). "Binary selectivity" estimates report effect of a switch from an unselective school to a highly selective school, where the latter is defined as the two highly selective clusters. "Latent selectivity" estimates report effect of a one standard deviation increase in selectivity. These are computed as the ratio of the reduced form model to the first stage, where the latter is computed as described in the text. Standard errors for these estimates are computed by bootstrap, with 200 replications. Under the assumptions stated in the text, all estimates are local average treatment effects for affirmative action compliers.
### Appendix Table A. Comparison with Sander's results for dropout

<table>
<thead>
<tr>
<th>Tier</th>
<th>First year law school GPA (standardized)</th>
<th>Drop out (logit)</th>
<th>Sander (Table 5.6)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>LSAT</td>
<td>0.099</td>
<td>0.038</td>
<td>-0.073</td>
</tr>
<tr>
<td></td>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>Undergraduate GPA</td>
<td>0.724</td>
<td>0.280</td>
<td>-0.006</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.015)</td>
<td>(0.056)</td>
</tr>
<tr>
<td>First year law school GPA (standardized)</td>
<td></td>
<td></td>
<td>-1.395</td>
</tr>
<tr>
<td>Tier</td>
<td></td>
<td></td>
<td>(0.037)</td>
</tr>
<tr>
<td>Black</td>
<td>0.871</td>
<td>-0.706</td>
<td>-0.155</td>
</tr>
<tr>
<td></td>
<td>(0.027)</td>
<td>(0.026)</td>
<td>(0.091)</td>
</tr>
<tr>
<td>Bottom quintile</td>
<td></td>
<td></td>
<td>0.027</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.078)</td>
</tr>
<tr>
<td>Black*bottom quintile</td>
<td>0.281</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.090)</td>
</tr>
<tr>
<td>Black*top 4 quintiles</td>
<td></td>
<td></td>
<td>-0.032</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.181)</td>
</tr>
<tr>
<td>Implied affirmative action effect (marginal effect of Black = 1, at avg. black student's Xs)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Total</td>
<td></td>
<td></td>
<td>0.059</td>
</tr>
<tr>
<td>Bottom quintile</td>
<td></td>
<td></td>
<td>0.041</td>
</tr>
<tr>
<td>Top 4 quintiles</td>
<td></td>
<td></td>
<td>-0.002</td>
</tr>
</tbody>
</table>

Notes: All models include controls for family income, part-time status, gender, and three racial/ethnic categories (Asian, Other, and Hispanic) whose coefficients are not shown here. Columns 1 and 2 report OLS coefficients; columns 3-5 report logit coefficients. "Implied affirmative action effect" in columns 4-5 is the marginal effect of being black, evaluated at the average black students' characteristics. In column 5, this average is computed separately for the bottom and the top four quintiles. In column 3, the implied affirmative action effect is the marginal effect of law school GPA times the black coefficient in column 2 plus the marginal effect of LSAT times the black coefficient in column 1.
### Appendix Table B. Comparison with Sander Results: Tabular analysis of non-completion

<table>
<thead>
<tr>
<th>Index range</th>
<th>Non-completion rates</th>
<th>Fraction of BPS respondents in range</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Whites (1)</td>
<td>Blacks (2)</td>
</tr>
<tr>
<td>Less than 400</td>
<td>0.0%</td>
<td>46.8%</td>
</tr>
<tr>
<td>400 - 460</td>
<td>22.2%</td>
<td>33.1%</td>
</tr>
<tr>
<td>460 - 520</td>
<td>19.7%</td>
<td>25.6%</td>
</tr>
<tr>
<td>520 - 580</td>
<td>16.4%</td>
<td>21.1%</td>
</tr>
<tr>
<td>580 - 640</td>
<td>12.1%</td>
<td>15.4%</td>
</tr>
<tr>
<td>640 - 700</td>
<td>9.6%</td>
<td>10.7%</td>
</tr>
<tr>
<td>700 +</td>
<td>7.1%</td>
<td>7.5%</td>
</tr>
</tbody>
</table>