Do Racial Preferences Reduce Minority Learning in Law Schools?

Doug Williams[*]

September, 2011

Please Do Not Quote Without Permission

[*] Frank W. Wilson Professor of Economics, University of the South, Sewanee, TN, 37383;

Email: dwilliam@sewanee.edu

1. Introduction

Racial preferences are a pervasive aspect of college and graduate school admissions.[1] Although the goals of these preferences is to create more diverse campus environments and to expand the opportunities available to targeted groups , some social scientists[2] argue that large racial preferences may have harmful, unintended consequences on the policy's targeted beneficiaries. A lack of data needed to test for these harmful effects has hindered moving the debate beyond armchair theorizing. But the release of **The Bar Passage Study (BPS)**, a large and comprehensive panel data set on law students, held out the promise of studying the effects of racial preferences in the nation's law schools.[3] An analysis of the BPS reveals not only that there are large racial gaps in performance[4] but, more importantly, *that these gaps persist even after controlling for credentials.* In other words, minorities are both less likely to graduate from

[1] Although the term "racial preferences" is often used synonymously with "affirmative action," (and is sometimes used that way in this paper), preferential admissions are only one part of most affirmative action programs, including those in higher education. Affirmative action also embraces such activities as targeted outreach, reexamination of traditional admissions criteria, and preferential financial aid. None of these activities are implicated by the mismatch hypothesis discussed in this paper.

[2] Thomas Sowell (1986) was among the first social critics to articulate the mismatch hypothesis.

[3] Richard Sander (2004) was the first to use this new data set to examine the effects of racial preferences on law school performance.

[4] The use of preferences will create a student population where the mean academic credentials of whites exceeds the mean academic credentials of blacks. It would be surprising if this racial gap in credentials did not by itself lead to some racial gap in performance.

law school and less likely to pass the bar compared to whites even after adjustments are made for group differences in entering academic credentials.

To account for these adjusted racial gaps in performance, some social scientists argue that racial preferences[5], by creating educational settings where minority students have much lower academic credentials than the majority of their classmates, will cause these minorities to learn less and to accumulate less human capital than they would in a classroom setting where they were better "matched". As a consequence of mismatching engendered by preferences, the "mismatch hypothesis" predicts that students who receive preferences are less likely to graduate and less likely to pass the bar than whites *with the same credentials*.

With the exception of Sander (2004), previous research using the BPS has concluded that the balance of evidence is against the mismatch hypothesis. This paper also uses the BPS but builds upon and differs from previous research in three ways. First, it focuses on bar passage as the most direct important measure of learning outcomes found in the BPS, in contrast to previous research which neglected bar passage and chose instead to focus on how preferences ultimately affect the number of minority lawyers.[6] In particular, this paper addresses the effect of preferences on the quality of human capital rather then the effect of preferences on the diversity of the lawyer population. Second, this paper presents new estimates of mismatch effects after accounting both for the measurement error bias due to the absence of specific information about law school selectivity and for selection on unobservables bias. Both of these biases have made it

[5] Sander (2004).

[6] The effect of racial preferences on the number of minority lawyers is of obvious importance, but Sander's observation that preferences, by reducing graduation and bar passage rates, might decrease the number of minority lawyers was at least partially responsible for this focus.

empirically challenging in previous research to find a mismatch effect if in fact one exists. Finally, the paper presents results for a larger minority subgroup that includes blacks, native Americans and hispanics while previous research has focused only on blacks. I find much more evidence for mismatch effects than previous research; indeed, I find that several different approaches to measuring mismatch consistently produce estimates that appear to largely account for minority underperformance on the bar exam.

The paper proceeds with a literature review (section 2) and a more in-depth examination of mismatch theory (section 3). Section 4 introduces the BPS in greater detail and presents summary data of both the unadjusted and adjusted racial gap in performance as well as summary data on the extent of credential disparities by race. Section 5 introduces the primary empirical model used in the analysis along with a discussion of the main methodological issues that surround a test of the mismatch hypothesis using the *Bar Passage Study*. The core of the paper is found in sections 6 and 7 where new empirical results are presented that account for measurement error bias and selection on unobservables bias. The findings are summarized in section 8.

2. Review of the Literature on Law School Mismatch

Making use of the unprecedented amount and quality of micro data made available by the Bar Passage Study, Sander (2004) first stirred interest in the relevance of mismatch to law students and brought widespread attention to an important puzzle: blacks fail the bar and drop out of law school at much higher rates than whites with similar entering credentials. He proposed that much of the racial difference in graduation rates and bar passage rates could be attributed to educational mismatch brought about by affirmative action. He then went on to

establish several important findings that must hold for the mismatch thesis to explain this puzzle: (1) blacks[7] receive significant admission preferences compared to whites; (2) black disparities in entering credentials cannot alone explain racial differences in dropout and bar passage rates; and (3) bar passage and law school completion are much more sensitive to class rank than the

[7] Most of the recent literature on law school mismatch has focused on blacks. Because mismatch is usually proxied, there is good reason to separate racial groups for purposes of testing the mismatch hypothesis. The researcher could pool all underrepresented minorities and include a dummy variables for each racial group to capture race differences in unobservables. However, these race dummies might also pick up the race differences in average group mismatch or 'distance'. If this were the case, the reliability of any estimated coefficient on a variable included to test for mismatch effects would be reduced. But why focus on blacks and not on some other racial group? Part of the reason is the relative importance of various groups in the affirmative action debate. Blacks are the largest group in law school that benefits from affirmative action and, therefore, a natural group to study.

There are also practical reasons to focus on blacks that are guided by the requirements of empirical research. First, the sample of blacks, with 50% more observations than Hispanics, is the largest underrepresented racial group in the BPS. Second, blacks receive the largest preferences so that an empirical test which measures preferences imperfectly is more likely to detect a mismatch effect for blacks than for other minority subgroups who receive smaller preferences. And third, blacks are much more likely to be regarded as a monolithic group than Asians or Hispanics and, therefore, treated more consistently by law school admissions offices with regard to preferences.

eliteness of school attended. This last finding is consistent with the hypothesis that a good match is more important than the eliteness of school attended in determining outcomes. Although Sander's work corroborated each of these three key ingredients and thus provided indirect evidence for the affirmative action/mismatch thesis, he did not advance a direct test of the mismatch hypothesis.

Ayres and Brooks (2005) were among the first to directly test the mismatch hypothesis, and they employed two different approaches. In their first test, Ayres and Brooks assigned each student a relative tier measure, defined to be the difference between the student's actual tier and the median tier attended by white law students with the same entering credentials. In the Bar Passage Study dataset, law schools are classified as belonging to one of six clusters that roughly correspond to six tiers of relative eliteness with six being the most elite. If a minority law student attends tier five and the median white with the same credentials attends a tier three school, then the minority student's relative tier would be two (five minus three). Ayres and Brooks argue that relative tier should proxy the degree of mismatch so that higher relative tier should reduce the likelihood of graduation and passing the bar if the mismatch hypothesis is correct. In opposition to the mismatch hypothesis, they find that relative tier has a positive and significant effect on the likelihood of becoming a lawyer for all races.[8] They also find no significant interaction effects for race and relative tier, with the exception of Hispanics, whose interaction effect is negative. Ayres and Brooks do not present results for blacks and whites separately, and they omit the bottom tier (historically black law schools) from their analysis. Presumably, Ayers and Brooks exclude blacks at historically black schools because of a concern

[8] The authors use the *lawyer* variable described below as their lawyer completion outcome variable.

that these schools (or their attendees) might be somehow different than traditional law schools (or their attendees). But because the historically black schools include a disproportionate number of minorities who are better matched, the exclusion of these better matched blacks substantially reduces the variation in the sample and biases their empirical test against the mismatch hypothesis. In addition, their results are likely plagued (as they point out) by selection on unobservables bias. Students attending higher relative tiers are likely to have higher values of "unobservables", which are indicators of ability hidden to the researcher but revealed to admissions officers through recommendation letters, writing samples and interviews. If this is the case, students attending higher relative tier schools started out more likely to succeed compared to their peers at lower relative tiers with the same observed credentials. Since the empirical model assumes that sorting across tiers is random after controlling for credentials, this test will be biased against the mismatch hypothesis.

Ayres and Brook's second test attempts to address this selection bias by exploiting a unique feature of the BPS dataset. Students were asked if the school they were attending was their first choice school and whether they were admitted to their first choice school. These "first choice students", who were admitted to their first choice school but who may be attending their first, second or lower choice school, are arguably more alike with respect to the unobservables than the sample as a whole. Admission to one's first choice school should be a positive signal about the quality of an applicant's unobservables that is independent of what school the applicant actually attends. Moreover, minorities attending their first choice schools do receive larger preferences and have a greater academic distance from the median student at their school compared to minorities attending their second choice or lower school.[9] If minorities attending

[9] See table 5.

their first choice school experience greater distance than if they attended their second or lower choice, then the mismatch hypothesis predicts these minorities will be less likely to pass the bar compared to their first choice counterparts who choose to attend their second or lower choice school. Using logit regressions, Ayres and Brooks find that students attending their first choice school are as likely to graduate and become lawyers compared to otherwise similar students not attending their first choice school. However, they report that the students attending their first choice schools do appear to take longer to become lawyers.[10] An important omission from the Ayres and Brooks paper is an analysis of whether mismatch affected bar exam success for law graduates who actually took the bar. As argued below, bar outcomes conditional on taking the test are the best outcome measures available in the BPS to measure educational achievement.[11]

In an unpublished paper, Yoon and Rothstein (2008a), who again focus on blacks, also provide two direct tests of the mismatch hypothesis -- one that they argue is biased against finding mismatch and the other that they argue is biased toward finding mismatch. Their first test, in the spirit of Ayres and Brooks, considers whether going to a more selective school (controlling for entering credentials) harms blacks. Again, the idea is that blacks attending more selective schools will be more mismatched compared to blacks with similar entering credentials attending non-selective schools. To conduct this test, they regress an outcome measure on a binary indicator of school selectivity (schools in the first two tiers are designated as selective)

[10] In particular, first choice students actually attending their first choice school are less likely to have become a lawyer after 4 years of beginning law school compared to first choice students attending their second or lower choice school.

[11] The three bar outcome measures used in this paper that condition on taking the test are pass bar first time, pass bar ever and adjusted pass bar ever that are discussed below.

and controls for entering credentials. They find no evidence that attending a selective school negatively affects the likelihood of students either graduating from law school or eventually becoming a lawyer. As the authors point out, the "selective test" is biased against the mismatch hypothesis because of the selection bias already discussed. Yoon and Rothstein's second test is less a direct test of the mismatch hypothesis than a confirmation of Sander's initial finding that much of the black-white difference in outcomes is left unexplained after controlling for credentials. They conduct this test by regressing outcomes on entering credentials and a dummy variable for "black". They find large negative and significant effects of "black" on their outcome measures. If one interprets "black" as an indicator for receiving affirmative action, then their test provides evidence for the mismatch hypothesis. But since "black" could be a proxy for other unobservables, a more conservative interpretation of their findings is that about half of the black-white difference in outcomes remains even after controlling for credentials. This finding is consistent with the mismatch hypothesis but also consistent with other hypotheses where black is a proxy for other race-related effects, such as traditional discrimination or stereotype threat. An important critique of the Yoon and Rothstein paper is that the authors, like Ayres and Brooks, omit from their analysis the bar outcome variables for law graduates that actually take the test and that provide the best available measures of educational achievement.[12]

[12] In a companion piece published in the University of Chicago Law Review, Yoon and Rothstein do show results for passing the bar the first time for blacks relative to whites, holding entering credentials constant. While the magnitudes of the black-white differences are large and statistically significant, these results for black-white differences are consistent with alternative hypotheses as well.

While both Ayres & Brooks and Rothstein & Yoon focus only on assessing "mismatch" as an explanation of outcome disparities, Barnes (2007) attempts to compare mismatch against an alternate explanation, which she calls the "race-based barrier" hypothesis. According to this hypothesis, blacks are less likely to graduate and pass the bar because of discrimination faced in law school. Barnes suggests that discrimination might take a variety of forms, such as a "hostile learning environment", "direct discrimination in learning outcomes" and "stereotype threat". To test these two competing theories, Barnes uses logit to regress various outcomes on credentials, race, law school tier, credential/tier interactions and race/tier credentials. Her test of the mismatch hypothesis is whether the credential/tier interactions are jointly statistically significant and whether the predicted outcome of a low credential student is worse at more elite schools. Her test of the race-based barriers hypothesis examines whether the race/tier interactions are jointly statistically significant and whether minorities fare worse at higher tiers. Although Barnes initially concluded that her results supported the race-based barrier hypothesis and weighed against mismatch, she has now acknowledged that her computations in the 2007 paper are incorrect. A replication by this author and three others produces results that are generally consistent with the mismatch hypothesis.[13]

To sum up, each of the papers reviewed here conclude that there is no mismatch effect. One of these (Barnes) makes computational errors and the corrected model provides support for mismatch. The other two papers mostly present results rejecting mismatch. Both the "relative tier" test (Ayres and Brooks) and the "selective" test (Yoon and Rothstein) reject mismatch. The exceptions are the finding (A&B) that first-choice students who attend their first choice school take longer to become lawyers and the finding (Y&R) that blacks are less likely to graduate and

[13] Williams et al, 2011.

pass the bar, holding credentials constant. As discussed below, both the first choice test and the selective test are sensitive to specification and the use of subsamples.

3. Mismatch Theory

The mismatch hypothesis is based on the assumption that classroom instruction is directed to the median student. If this assumption is valid, students too far below the median may struggle to understand class discussions and to keep up with the pace of instruction. Consequently, mismatched students learn less and may even reduce their effort if they become discouraged, leading to even less human capital accumulation. To justify this "median student assumption", this section outlines a theory of the level of classroom instruction. This theory can then be used to predict how the level of classroom instruction will change once racial preferences are introduced.

Embedded within the median student assumption is the notion that classroom instruction is a public input. An instructor can only teach at one level, and each student benefits, more or less, from whatever level of instruction is delivered. Although the level of instruction is multi-dimensional and includes pace as well as level of sophistication, it is useful to assume, for simplicity, that the instruction level can be captured by the single dimensional index L. Suppose that student academic credentials can be captured by the index C, and that for a given value of C, there is a unique level of instruction L that maximizes the value-added by the instructor. It is also reasonable to assume that the optimal level of instruction, L, is increasing in C. That is, more able students require more challenging levels of instruction to maximize the value-added of the instructor. To illustrate, consider hypothetical students 'A' and 'B'. If Student A is in a class where the level of instruction is more difficult than the optimal level associated with her

credentials, she is gaining less value-added than she would if the instructor were teaching at her optimal level. Intuitively, this is true because she will have difficulty keeping up with the pace and sophistication of the instruction. Conversely, if Student B is in a class where the level of instruction is less difficult than the optimal level associated with her credentials, she is also gaining less value-added than she would if the instructor were teaching at her optimal level. Intuitively, this is true because her abilities are not being pushed to capacity. One can define "negative mismatch" as the condition facing Student A; Student B is "positively" mismatched.

To make a prediction about what level (L) the instructor will choose for the classroom, one must know something about the objectives of the instructor and the distribution of academic credentials of the students in the classroom. A reasonable assumption is that the instructor maximizes the total value-added of her instruction to the classroom. If all of the students in the classroom are homogenous in terms of academic credentials, then the instruction will teach at the level that maximizes the value-added for the homogenous credential. Hence, the optimal instruction for the homogeneous case is equivalent to the case where there is a single student. The tradeoffs involved in choosing the optimal instruction for the case of a heterogeneous classroom can be illustrated by the example of a classroom with two students of different ability. If the instructor seeks to maximize the total value-added for the two students, her strategy will follow one of two paths. One outcome is a separating solution, which is particularly likely if the credentials of the two students are far apart; teaching to the middle may produce very little value-added for either student, so it is optimal for the instructor to focus on only one student. If, in contrast, the two students are not so far apart, the instructor may follow a "pooling" solution. Here, moving the level of instruction away from one student and toward the other involves a

trade-off of value-added gained and value-added lost, and the level of instruction that maximizes total value-added will lie in between the optimal levels for each student.

To see how this model of classroom instruction applies to the law school classroom environment, consider the nature of the law school classroom absent racial preferences. There are roughly two hundred law schools in the United States, and they are unusually hierarchical – even within the elite-conscious norms of American higher education. Students perceive their success as closely linked to the eliteness of the school they attend (Korobkin, 1998), and therefore apply to large numbers of schools and tend to attend the most elite school that will have them. Law schools are also highly conscious of rankings, and have become more so since the advent of the US News rankings a generation ago (Stake, 2006). Law schools have always relied heavily on "hard" credentials like LSAT scores, and this reliance has increased as the rankings explicitly incorporate such credentials as the 25th percentile and 75th percentile of a law school's LSAT distribution. For all of these reasons, students at most law schools fall within a relatively narrow "credentials band". Given a tight, bell-shaped distribution of student credentials, an objective of maximizing total value added will lead to an instruction level aimed toward the center of the distribution, presumably not distant from the median. Intuitively, this 'median' level of instruction maximizes total value-added because it is the level that will be closest to the optimal level for most students. Hence, the mismatch hypothesis posits that instructors will follow a pooling solution in the classroom and that measures of central tendency such as the median are reasonable proxies for this pooling solution.

What happens when racial preferences introduce new students whose credentials are far below the median, creating a bump or spike in the left tail? It would be possible, of course, for the instructor to lower substantially the level of instruction in order to add value to these students

in the left bump of the credential distribution. This would mitigate a negative mismatch effect but would plausibly harm many students in the top half of the distribution. Some instructors may well choose to do this. But given the tight distribution among students who have not received preferences, it is likely that most instructors will continue to teach to the "median" student, meaning that instruction will be geared far more closely to the non-preferenced students than to the small numbers with preferences. Consequently, students far below the median will be mismatched in the sense that they will be at risk of not comprehending the instruction and falling behind.

4. Measuring Mismatch and Its Consequences with the BPS

A direct prediction of the mismatch hypothesis is that students receiving preferences will learn less. To test this prediction, a common measure of acquired knowledge is needed for matched and mismatched students. For most forms of education, such a measure does not exist. For example, there is no universal exam for college graduates that measures what they learned from their undergraduate education. The lawyer licensing system does, however, attempt one such measure: almost all law graduates who wish to obtain a law license must pass a state bar examination. While the bar examination intends to test minimum proficiency only, it is, as a practical matter, a universal test of educational achievement. The bar exam is validated using law school grades, and even the most able law school graduates take the exam very seriously.

While legal education, because of the requirement to pass the bar, is somewhat unique in having an outcome variable for measuring learning outcomes, the data requirements for testing the mismatch hypothesis pose a significant hurdle for successfully implementing an empirical test. At a minimum, data for individuals is needed on bar outcomes, college academic

credentials, LSAT scores and the median LSAT score of the law school attended. In addition, data on family background and college quality would be helpful. The best data set available for studying legal education mismatch is the Bar Passage Study (BPS). The Bar Passage Study (BPS) was commissioned and conducted by the Law School Admission Council (LSAC) in the 1990s to study, among other things, whether bar exams had discriminatory effects upon minority bar-takers. The study tracked some two-thirds of all students who started law school in 1991 through their law school careers and bar exam experiences. All twenty-seven thousand participants completed surveys as they began law school, and several thousand members of a subsample participated in three follow-up surveys. For all participants, LSAC gathered data on undergraduate grades, LSAT scores, and law school performance, and it tracked results for the great majority of participants who took bar exams during the three years after graduation. Although the BPS is a very rich source for studying the process and effects of legal education, it has some serious weaknesses. The most glaring weakness is the absence of specific information about the selectivity of the law school attended as measured by either the LSAT median or the undergraduate GPA median. Instead, only the LSAT median and the undergraduate GPA of the "tier' to which a student's law school belongs is available. Even with its weaknesses, the BPS is by far the best data available for testing the mismatch hypothesis since it combines data on bar outcomes, academic credentials and law school characteristics on a micro level. Ideally, one would like to know the bar score of each taker scaled to be directly comparable across jurisdictions. The BPS did not collect bar scores; rather, it tracked each bar exam taken by participants, recorded whether the participant passed a given exam, and noted the region (not the jurisdiction) in which the exam was taken.[14] Unlike previous analyses that code individuals

[14] By not including the actual score on the bar exam, the variation in performance is reduced,

who do not attempt the bar exam as failing, this paper focuses on bar passage measures that only use observations for individuals who actually take the exam. The advantage of only including people who took the bar in the analyses is a reduction in measurement error of the dependent variable and, consequently, smaller standard errors for the estimated coefficients. The three distinct measures of bar performance used in the paper are: *pass bar first time*, *pass bar ever* and *adjusted pass bar ever*. *Pass bar first time* measures whether an individual passed the bar or not on the first attempt. *Pass bar ever* measures whether an individual ever passed the bar or not in all the attempts tracked by the BPS (up to seven attempts). Unlike *pass bar first time* and *pass bar ever* which are binary variables, a*djusted pass bar ever* incorporates information about the number of attempts required to pass the bar; this variable takes on the value '1/n' if the test taker passed the bar on the n^{th} try and '0' if the test taker never passed. Conceptually, this variable makes more of a distinction between someone who passes on the first rather than the second attempt, than between someone who passes on the fourth rather than the fifth attempt. Each of these bar passage variables has measurement error as measures of educational achievement since different states have different passing thresholds. Since the BPS provides no information on the state where the bar was taken, it is not possible to control for variation in bar difficulty. For purposes of assessing the effects of mismatch on learning, the outcome on the first time bar attempt is a far better measure than eventual bar passage, the measure emphasized by previous research. First, passing the bar requires surpassing a threshold, and repeated test taking allows for a greater role of luck in eventually passing that threshold. More importantly, additional

---

making it more difficult to detect mismatch effects. By not including jurisdiction, it is impossible to standardize bar outcomes across individuals, thereby increasing measurement error and making it more difficult to find mismatch effects.

preparation for repeat exams may span over several months or even years so that repeaters will have a longer (unobservable) legal education than non-repeaters.[15] This shadow education will be customized to a bar failer's abilities, allowing the effects of mismatch to be undone over time.

Mismatch could plausibly affect other outcomes as well, such as successful completion of law school. If mismatched students learn less, they may be unable to meet the threshold of performance required for graduation, or they may decide based on their class performance that they are unlikely to become a lawyer, and thus drop out of law school. In this paper, *graduate* is a binary variable indicating whether each law school matriculant eventually graduated from law school. *Graduate* is an important labor market variable but it is problematic as a measure of learning outcomes, since it is under the control of the institution. It is well known that many elite schools make efforts to have no attrition whatsoever. A student taking advantage of admissions preferences, and thereby attending a more elite school, might increase their probability of graduation even if they learn less.[16] A second type of outcome is whether a given law school

[15] These bar performance variables may also be subject to self-selection bias since some graduates elect not to take the bar. The source of this bias is the likely correlation between the disturbance in the selection equation and the disturbance in the outcome equation. Because taking the exam is costly, those students who anticipate failing the bar may not take it. Students with low credentials who choose to take the bar will have high unobservables. If unobservables in the selection equation are positively correlated with unobservables in the outcome equation, then the selection on unobservables bias in the outcome equation will be even more severe.

[16] Because law schools value both diversity and their overall bar passage rate, lowering the graduation threshold poses a tradeoff for law schools. Relaxing graduation standards will increase retention of minorities but also decrease bar passage rates. The principle of diminishing

matriculant eventually becomes a lawyer. *Lawyer* in this paper is a binary variable that takes the value "1" if an individual passed the bar and the value '0' if an individual failed or did not take the bar. Most previous research has used variations of this *lawyer* variable in their tests of mismatch. Like the outcome *graduate*, *lawyer* is an important outcome measure but an imperfect measure of learning. For example, one difficulty with this measure is that is treats an individual in good academic standing who voluntarily drops out the same as an individual who is forced to drop out; these two individuals have almost certainly achieved different amounts of learning.

The final outcome variable is *smooth passage*, which also includes dropouts. This variable takes a value of "1" if an individual graduates and passes the bar on the first try and a value "0" if an individual drops out or takes more than one attempt to pass the bar. Results on *smooth passage* are reported as a check on two possible sources of bias stemming from the exclusion of dropouts. The first bias would involve a self-selection of individuals out of the sample actually taking the bar. These are individuals who drop out of law school because they receive a signal (e.g. law school grades) indicating they will have difficulty graduating and passing the bar. If, other things equal, more mismatched individuals are more likely to receive this signal and less likely to take the bar, then this sample selection bias will make it harder to

---

marginal utility would suggest that the marginal benefit to a school of a 1% increase in the bar passage rate is higher for a school with low bar passage rates compared to schools with high bar passage rates. It is also reasonable that schools with a small number of minorities have greater incentives to retain those minorities. Therefore, more selective schools – which have both high bar passage rates and a small number of minorities – may have a greater incentive to lower graduation requirements relative to less selective schools.

detect a mismatch effect. A second bias, which would favor an erroneous mismatch finding, involves law schools censoring the sample taking the bar. Non-elite schools tend to be more concerned about overall law school bar pass rates and may have a higher threshold for graduation compared to elite schools. If this is the case, students graduating from elite schools and taking the bar will have inferior unobservables compared to similar looking students graduating from non-elite schools and taking the bar.

Table 1 shows the outcomes for various measures by race and the outcome gaps that mismatch theory seeks to explain . The numbers for whites show actual outcomes while the numbers for the other racial groups is the racial difference compared to whites. The "Minority" column shows the aggregate gap for blacks, native Americans and Hispanics as a group.[17] The table reveals large racial gaps for blacks and native Americans and smaller but still substantial gaps for Hispanics. The graduation rate gap is 11 percentage points for blacks, 9 points for native Americans and 5 points for Hispanics. The first time bar passage gap is 31 percentage points for blacks, 26 points for Native Americans and 17 points for Hispanics. The eventual bar passage gap is 19 percentage points for blacks, 14 points for Native Americans and 9 points for Hispanics. The lawyer completion gap is 26 percentage points for blacks, 22 points for Native Americans and 11 points for Hispanics. Much of these gaps is explained by the lower entering credentials of underrepresented minorities compared to whites due to affirmative action. Table 1

[17] Hispanics are identified as either Mexican, Puerto Rican or Other Latino in the BPS; No results are shown for Asians or 'Other race' because these groups receive are more heterogeneous in terms of entering credentials and receive less preferences compared to other race groups (see table 2).

also shows the racial gap that remains after controlling for credentials.[18] Although it varies by outcome variable, about 1/3 to 1/2 of the race gap cannot be explained by race differences in entering academic credentials. After controlling for credentials, blacks (minorities) are 3% (2%) less likely to graduate compared to whites, 12% (10%) less likely to pass the bar the first time, 8% (5%) less likely to ever pass the bar and 8% (7%) less likely to become a licensed lawyer.

The mismatch hypothesis explains these racial gaps in performance as product of too much "distance" between the academic credentials of minority students and the median student:

$$(1) \quad D_i = C_i - \mu_i$$

where C_i is the academic credential of student i and μ_i is the academic credential of the median student at student i's institution. In practice, only a proxy for D can be constructed from the BPS. As discussed above, a major drawback of the BPS is the lack of information on median academic credentials (i.e. LSAT and College GPA) for the specific law schools attended. Different measures of student academic credentials (C_i) exist in the BPS but there is no summary measure of student quality (μ_i) for the actual school attended. To prevent identification of individual law schools in the BPS, the LSAC assigned each student's law school to one of six

[18] To control for credentials, a probit equation for each outcome variable was estimated for whites, using a a quadratic in LSAT score and undergraduate GPA, mother's education, father's education, family income, disability dummies and an English as a second language dummy. These coefficients for whites were then used to predict the outcomes for each racial group using the actual credentials of the group. The remaining gap shown in parenthesis is the difference between the actual group outcome and the predicted outcome using the 'white' coefficients.

clusters of law schools based on school size, tuition, acceptance rate, faculty/student ratio, percent minority, median LSAT score and median undergraduate GPA. The clusters can be ordered by mean LSAT to create "tiers" that proxy for selectivity. Using the tier credential as a proxy for law school credential, one can construct a measure of a student's "credential distance" from his peers by comparing a student's credentials with the median credentials of all students in the same tier. Although this measure of credentials distance is useful, it is a noisy, imperfect measure. Not only is the tier median academic credential a proxy for the student's law school median, a student's law school may be misclassified once "clusters" are interpreted as "tiers". In other words, tiers almost certainly overlap.

Table 2 provides some summary statistics based on two measures of distance for all of the race classifications in the BPS. The Mean *Index Distance* (D)[19] for a group is the difference between an individual's own academic index and the median academic index of the cluster which their school belongs. The *Mean LSAT Distance* for a group is the difference between an individual's own LSAT score and the median LSAT score of the cluster which their school belongs. The distance distribution is shown in terms of standardized index distance (d), which is the *Index Distance* divided by its cluster standard deviation. Negative values for distance imply negative mismatch while positive values imply positive mismatch. Consistent with not receiving any preferences, whites experience the least distance (D=9) and have the largest proportion

[19] The academic index is a weighted average of the undergraduate GPA and LSAT that is widely used by law schools to predict law school performance. It places a 40% weight on applicant undergraduate GPA and a 60% weight on applicant LSAT score, and it is scaled between 0 and 1000. Other researchers including Sander, Ayres and Brooks and Yoon and Rothstein have used this measure in their analyses.

within one standard deviation from the mean (77.2%). All of the included ethnic groups receive significant preferences as measured by D. Blacks have the greatest negative distance (D=-145) and the smallest proportion within one standard deviation from the mean (28.4%). Because Asian and 'Other'[20] are the least mismatched of the various ethnic groups and much more heterogeneous in terms of entering credentials than the other minority race categories, these groups are excluded from the analysis below.

5. Testing the Mismatch Hypothesis

The mismatch hypothesis is concerned with students for whom $D < 0$ in equation (1), and the hypothesis predicts that as D becomes more negative, education achievement is lowered. Using the definition of mismatch in equation (1), various tests of the mismatch hypothesis can be motivated by this mismatch measure using the model represented by equations (2) and (3) below. For the negatively mismatched student i such that

$D_i = C_i - \mu_i < 0$, the outcome Y_i can be expressed as:

(2) $Y_i = \beta_0 + \beta_1 [C_i - \mu_i] + \beta_2 C_i + \beta_3 \mu_i + u_i$

where C_i is the student's entering academic credential, μ_i is the median academic credential at the law school attended by individual i and $C_i - \mu_i = D_i$ is academic distance. The mismatch hypothesis is that $\beta_1 > 0$, and it reasonable to hypothesize that β_2 and β_3 are both positive. For a matched student i such that $D_i = C_i - \mu_i > 0$, the distance variable drops out and equation (2) becomes:

[20] The "Other" category was created by the BPS surveyors to include non-white persons not included in one of the specified racial categories.

(3) $Y_i = \beta_0 + \beta_2 C_i + \beta_3 \mu_i + u_i$

Two important challenges to overcome in estimating equation (2) that are generally encountered in using the BPS to test the mismatch hypothesis are selection on unobservables bias and measurement error bias. The selection on unobservables bias results from a regime where affirmative action recipients attend the most selective school to which they are admitted and law schools make admission decisions based on information (essays, interviews, quality of college education) unobservable to the researcher. Students with high unobservables will be selected disproportionately into "reach" schools so that the observable distance variable will under-estimate on average the true distance for those students with high negative values of D and will over-estimate on average the distance for those students with low negative values of D. Consequently, the estimated coefficient for D will be biased toward zero, and this selection on unobservables bias will not only reduce the magnitude of any estimated mismatch effect but may also completely obscure it even when it exists. In addition, measurement error bias, due to the use of tier median selectivity as proxy for a student's law school selectivity, will further bias the coefficients on distance toward zero. Together, selection on unobservables and measurement error bias will make it more difficult to discern a mismatch effect if it exists.

An additional challenge that is specific to estimating equation (2) is multicollinearity. In fact, equation (2) cannot be estimated directly for a sample that satisfies $D_i < 0$ because of perfect multicollinearity. One strategy that avoids perfect multi-collinearity is to estimate equation (4):

(4) $Y_i = \beta_0 + \beta_1 I(D<0) * [C_i - \mu_i] + \beta_2 C_i + \beta_3 \mu_i + u_i$

In practice, however, this equation exhibits high multicollinearity so that the estimates are unreliable.[21]

To avoid the problem of multicollinearity, this paper follows the same identification strategy as previous researchers: terms in equation (2) are combined, resulting in equation (5), which motivates the empirical analyses in sections 6 and 7:

(5) $Y_i = \beta_0 + (\beta_3 - \beta_1) \mu_i + (\beta_2 + \beta_1) C_i + u_i$ for student i such that $D < 0$.

Equation (5) is most appropriate as an estimation equation for samples where all students are mismatched but to varying degrees. In practice, this means that this equation is appropriate for blacks (who are mostly mismatched) but not for whites (who are mostly matched). But even in the case of blacks, equation (5) cannot identify the mismatch effect but only the net effect of the mismatch effect (β_1) and the selectivity effect (β_3).

Although equation (5) avoids multicollinearity, its estimation is still hampered by selection on unobservables bias and measurement error bias. Yoon and Rothstein partially address this measurement error by using a binary variable for selectivity. They designate schools as either selective (top two tiers) or nonselective (bottom four tiers). By combining the bottom four tiers into the nonselective category, they mitigate the problem of overlapping tiers (i.e. schools of identical selectivity appearing in different tiers) but at the same time make tier an even noisier

[21] See Williams, April 2009.

proxy for true law school selectivity. The model defining selectivity as a binary variable (suppressing the intercept) is:

(6) $Y_i = \theta S_i + \beta C_i + u_i$

where S_i is an indicator variable for whether the individual attended a selective school. If students at selective institutions are on average more mismatched relative to students at non-selective institutions and the mismatch disadvantage of attending a more selective school outweighs the selectivity advantage, OLS estimates will yield a negative coefficient for θ. In section 6, I estimate two models. I first estimate equation (6) using the selectivity variable defined by Yoon and Rothstein, and then I estimate a model that defines selective as the top two tiers and nonselective as the bottom two tiers. The omission of the two middle tiers from the selectivity variable makes the categories of selective/nonselective much more homogeneous (and therefore less noisy), makes it even less likely that tiers 'overlap' and, consequently, reduces measurement error bias.

In Section 7, I estimate an IV model to correct for selection on unobservables bias. The BPS enables the researcher to identify whether a student was admitted to their first choice school and, if so whether these "first choice students" are attending their first choice school or not. If the decision of whether or not to attend one's first choice school is uncorrelated with unobservable ability but correlated with selectivity, then the second or lower choice indicator can be used as an instrument[22] for *selective* in equation (6) that should eliminate unobservables bias.

[22] The second choice indicator take a value 1 if the second or lower choice school is attended and a value 0 if the first choice school is attended.

The justification of the IV model can be formalized using equations (2) and (3) from above and adding equation (7) which implicitly assumes that students prefer more selective schools and that attending one's second or third choice school reduces the selectivity of the law school attended:

(7) $\mu_i = C_i + \gamma\, b_i + \delta\, L_i + v_i$

where b_i is in indicator variable for being black and L_i is an indicator for going to one's second or lower choice school. Equation (7) asserts that a student i's predicted law school selectivity is equal to his own credential plus any affirmative action treatment ($\gamma > 0$) plus the effect of voluntarily attending one's second or third choice school ($\delta < 0$) plus a disturbance. The model can be closed by assuming that students choose between their first choice and second choice schools based on the maximization of the present value (PV) of lifetime income, which will depend on school quality (μ_j) and schooling costs (a_j) for school j, where schooling costs can be thought of broadly here to include not only tuition but hedonic costs like distance from home and family:

(8) $L_i = 1$ if $PV(\mu_2, a_2) > PV(\mu_1, a_1)$

0 otherwise.

Because the indicator variable *L* is correlated with *Selective* (equation 7) but uncorrelated with unobservable ability (ability does not appear in equation 8), it should be a valid instrument for *selective* in equation (6).

6. OLS Estimates for the Selective Model

This section presents results for the "selective" model first suggested by Yoon and Rothstein but the estimates presented differ from their estimates in two important ways. First, the focus here, unlike the Yoon-Rothstein paper, is on bar passage outcomes for law school graduates that actually took the bar exam, and second, new estimates are presented for an definition of selectivity that differs from Yoon and Rothstein's definition and that should reduce measurement error bias.

The results in column (2) of table 3 for *graduate* and *lawyer* correspond to the Yoon and Rothstein estimates. Column (2) also includes new estimates for the bar passage outcomes. The coefficients for *adjusted pass bar ever* and *pass bar first time* are marginally significant for blacks, and the magnitudes, suggesting that going to a selective school reduces the likelihood of passing the bar on the first try by 5 to 6%, are substantial. In contrast to the Yoon and Rothstein results that use graduation and lawyer completion as outcome variables, all of the coefficients for the bar passage regressions for both blacks and the minority subgroup that actually took the bar are negative as predicted by the mismatch hypothesis. The estimates for the *graduate* regressions indicate that blacks attending a selective law school have a 5% higher graduation probability, while the minority subgroup experiences no boost in graduation probability from attending a selective school. Although this positive coefficient for the black *graduate* equation runs counter to the mismatch hypothesis, two caveats need to be mentioned. First, this estimate, if it is valid, may not carry over to affirmative action beneficiaries who attend nonselective schools. Second, this positive graduation effect could be explained by the potential endogeneity bias discussed earlier.[23] Moreover, as previously mentioned, because graduation is at the control

[23] See footnote 16.

of the institution, there's reason to doubt it as a consistent measure of learning across law schools. Though small in magnitude, it is curious that the estimated coefficients for *selective* in the white bar passage regressions are negative and significant as well, since some whites (e.g. legacies and older students) receive preferences. The *Smooth Passage* variable, which is included as a robust check on sample selection bias from excluding dropouts, is always negative for blacks and minorities but never achieves statistical significance.

Table 4 shows the results for the modified selectivity variable where selective as defined as the top two tiers and nonselective is defined as the bottom two tiers. As previously discussed, this omission of the middle two tiers reduces measurement error bias by creating relatively homogeneous categories of selective/nonselective and eliminating overlap between the selective/nonselective categories. For the bar passage variables, all of the signs are negative and statistically significant for both blacks and the minority subgroup. The magnitude of the coefficients is substantial and is and capable of filling the unexplained gaps in table 1. Controlling for credentials, the reduction in distance as measured by the academic distance in moving from the top two tiers to the bottom two tiers is 216 points for blacks and 204 points for the minority subgroup.[24] On average, such movement would eliminate the actual mean 145 point distance for blacks and the mean 118 point distance for the minority subgroup (see table 2). Correspondingly, the results in table 4 show that moving to a less selective school would increase first time bar passage rates by 11 to 15%, eliminating the 12% unexplained gap for blacks and the 10% gap for the minority subgroup. Likewise, table 4 shows that moving to a less selective school would increase eventual bar passage rates by 6 to 9%, eliminating the 8% unexplained gap for blacks and the 5% unexplained gap for the minority subgroup. In these

[24] These numbers were calculated by regressing credentials and *selective* on distance.

regressions, the effect of attending a selective school on graduation becomes insignificant and the magnitudes of these coefficients are very small. The coefficients on *Smooth Passage*, which recall is included as a check on bias from excluding dropouts, increase in magnitude and are statistically significant for blacks and minorities.[25] Moreover, the magnitudes of the *Smooth Passage* coefficients (10 to 12%) are large enough to eliminate the unexplained gaps (9 to 11%) in table 1. Overall, the "selective" test yields both coefficient "signs" and magnitudes that support the mismatch hypothesis.

7. IV Estimates for the Selective Model

This section addresses selection on unobservables bias by following the suggestion of Ayres and Brooks to incorporate information on "first choice students" into the analysis. The "first-choice" students are the subsample of students who both applied to at least two schools and were accepted to their first choice school. These "first-choice students" can thus choose whether to matriculate at their first-choice school or at a second- or lower-choice school. In this section, the variable *L* that indicates whether or not a first-choice student attended her first-choice school is used as an instrument for selectivity in the selective model.

First-choice students are separated into two groups: *Attendees1* are students actually attending their first choice school and *Attendees23* are students who attend their second choice or lower school. An ideal natural experiment for the "selective" test would sort students into selective and non-selective schools by an exogenous mechanism unrelated to preferences or choices. But since it is not obvious that the choice of whether or not to attend one's first-choice

[25] To assure that these results are not driven by a lack of common support, I also estimated a matching model. The results from the matching model are consistent with the regression results.

school is correlated with unobserved ability, the sorting of students into selective and non-selective schools by these choices form the basis of a quasi-natural experiment. As a rough check on the validity of the first-choice analysis, table 5 compares the credentials and the outcomes of *Attendees1* with those of *Attendees23*. Academically, *Attendees1* are remarkably similar to *Attendees23*. For blacks, mean undergraduate GPA, mean LSAT scores and the mean Academic Index are statistically indistinguishable between *Attendees1* and *Attendees23*. For the minority subgroup, the mean undergraduate GPA is statistically indistinguishable between *Attendees1* and *Attendees23*, while *Attendees23* do have a statistically significant higher mean LSAT score and Academic Index. Still, the magnitudes of the statistically significant differences in mean credentials for the minority subgroup are very small: 1/33 of a standard deviation difference for the Academic Index and 1/13 of a standard deviation difference for the mean LSAT. *Attendees1* are, as expected, more likely to attend elite and selective schools compared to *Attendees23*. The selective school difference is statistically significant at a 5% level for both blacks and minorities, and the elite school difference is statistically significant for the minority subgroup.

Given that the first-choice students have similar credentials, regardless of where they attend, and that first-choice students who choose their first-choice school attend higher tier schools on average, then *Attendees1* will be more mismatched compared to *Attendees23*. Consequently, the mismatch hypothesis predicts that *Attendees1* will have worse outcomes compared to *Attendees23*. The sign of the differences for all outcomes in table 5 supports these predictions generally but the differences are not always statistically significant. For blacks, the differences for passing the bar the first time and for smooth passage are statistically significant,

while for minorities, passing the bar on the first attempt is marginally significant and smooth passage is statistically significant.

The results in table 5 are supportive of a mismatch effect and can be formalized with an IV model where *Attendees23* is used as an instrument for *selective*. Although *Attendees23* is not exogeneous, it does seem to satisfy the requirements of an instrumental variable. It is correlated with selectivity and not intuitively correlated with the disturbance term in the outcome equation (6). Tables 6A and 6B provide results for blacks and minorities using *Attendees23* as an instrument for selectivity. In table 6A, *selective* indicates top two tiers or bottom four tiers. While in table 6B, *selective* indicates top two tiers or bottom two tiers. The results in table 6A correct for selection on unobservables bias while the results in table 6B correct for both selection on unobservables bias *and* measurement error bias. In both tables, the instrumental variable estimates for blacks can be found in columns (2) and (3), and the instrumental variable estimates for the minority subgroup can be found in columns (5) and (6). The F-statistic for the instrument is shown in brackets beneath the standard errors in parentheses. In table 6A, *Attendees23* is a weak instrument ($F<10$) for the regressions for blacks and a strong instrument for minorities. All of the signs are consistent with the mismatch hypothesis. Moreover, the estimates for the *pass bar first time*, *adjusted pass bar ever* and *smooth passage* coefficients are statistically significant for both blacks and minorities. In table 6B, which omits the two middle tiers in defining selective to minimize measurement error from overlapping tiers, the results are stronger. *Attendees23* nearly reaches the status of a strong instrument for the bar passage variables in column (2) regressions for blacks and is a strong instrument for all of the results for minorities. Both negative and significant coefficients are present for all of the outcome variables for minorities except for the *graduate* regressions. The magnitudes of the coefficients are large – in

fact much larger than what is required to explain racial differences. Focusing on the results for the minority subgroup in table 6B, the results suggest that eliminating the actual mean distance would increase first time bar passage by an amount 7 times larger than the unexplained gap (68% compared to 10%), would increase ever passing the bar by an amount 9 times larger than the unexplained gap (44% compared to 5%) and would increase lawyer completion rates by an amount 6 times larger than the unexplained gap (41% compared to 7%). Although the large magnitudes could reflect model misspecification, they could also simply reflect the small sample size. Moreover, more plausible magnitudes much closer to the unexplained gaps can be found within the 95% confidence level for all of the estimates. Although these IV results should be interpreted carefully because of the magnitudes, they are consistent with mismatch effects.

8. Conclusion

All of the previous papers that have conducted formal tests of the mismatch hypotheses have found little support for mismatch effects in the BPS. An important reason for these conclusions has been insufficient focus on bar passage measures that only include actual test takers. Another reason for these conclusions is the presence of measurement error and selection on unobservables bias that makes it difficult to find a mismatch effect if in fact it exists. This paper demonstrates that a focus on bar passage measures that only include test-takers and corrections for measurement error bias and selection on unobservables bias yields evidence for mismatch effects in legal education. This is true even though the data limitations of the BPS intrinsically tend to bias any test against a finding of mismatch. Moreover, the magnitudes of the coefficients are more than sufficient to account for the underperformance of blacks and other minority groups in law school.

Further research needs to be conducted to fully understand the magnitudes of mismatch effects in law school. Conducting this research will require better data that contains specific information about the quality of law school attended, actual bar scores and information on which state bar examination was taken. Data from large states such as Texas, Florida or California on actual bar scores would be ideal to assure uniformity of grading on bar exams and to provide sufficient variation in law school selectivity.

References

Abadie, Alberto, Drukker, David, Herr, Jane and Imbens, Guido (2001), "Implementing Matching Estimators for Average Treatment Effects in Stata", The Stata Journal, 1:1-18.

Ayres, Ian and Richard Brooks (2005). "Does Affirmative Action Reduce the Number of Black Lawyers?" Stanford Law Review 57(6), May: 1807-1854.

Barnes, Katherine Y. (2007). "Is Affirmative Action Responsible for the Achievement Gap Between Black and White Students?" Northwestern University Law Review 101(4), Fall: 1759-1808.

Ho, Daniel (2005). "Why Affirmative Action Does Not Cause Black Students To Fail the Bar" Yale Law Journal 114: 1997-1004.

Imbens, Guido and Wooldridge, Jeffrey (2007). "Weak Instruments and Many Instruments", NBER Lecture Notes, Cambridge, MA, Summer.

Korobkin, Russell (1998), "In Praise of Law School Rankings: Solutions to Coordination and Collective Action Problems", Texas Law Review 77: 403, 409-10.

Norton, Lynne L., Suto, Debrah A. and Reese, Lynda M. (2006). "An Analysis of Differential Prediction for Racial/Ethnic Subgroups Based on 2002-2004 Entering Law School Classes," LSAC Technical Report 06-01.

Rothstein, Jesse and Albert Yoon (2008a) "Mismatch in Law School". NBER Working Paper Series, Cambridge, MA, August.

Rothstein, Jesse and Albert Yoon (2008b), "Affirmative Action in Law School Admissions: What Do Racial Preferences Do?" University of Chicago Law Review 75(2), Spring: 649-714.

Sander, Richard H. (2004). "A Systemic Analysis of Affirmative Action in American Law Schools," Stanford Law Review 57(2), November: 367-483.

Slake, Jeffrey R. (2006), "The Interplay Between Law School Rankings, Reputations, and Resource Allocation: Ways Rankings Mislead", Indiana Law Journal 81:229.

Sowell, Thomas (1986). "The Plight of Black College Students", *in* Education: Assumptions Versus History, 130-31 (1986).

Williams, Doug (2009), "Does Affirmative Action Create Educational Mismatches in Law Schools?" (Apr. 13, 2009) (unpublished paper)

Williams, Doug et al. (2011), "Revisiting Law School Mismatch: A Comment on Barnes (2011, 2007)," Northwestern University Law Review (forthcoming).

Table 1

Racial Differences for Outcome Measures						
	White		**Black**	**Native American**	**Hispanic**	**All Minority**
Graduate (%)	92%		-11%	-9%	-5%	-9%
Unexplained Gap	0%		-3%	-4%	0%	-2%
Pass Bar First Time (%)	92%		-31%	-26%	-17%	-25%
Unexplained Gap	0%		-12%	-13%	-12%	-10%
Pass Bar Ever (%)	97%		-19%	-14%	-9%	-15%
Unexplained Gap	0%		-8%	-7%	-3%	-5%
Lawyer Completion (%)	82%		-26%	-22%	-11%	-20%
Unexplained Gap	0%		-8%	-11%	-2%	-7%
Smooth Passage (%)	78%		-34%	-29%	-18%	-27%
Unexplained Gap	0%		-11%	-15%	-7%	-9%
No. of Observations	22608		1874	144	1294	3312

Table 2

Academic Credentials Distance by Race							
			Distribution of Distance (%)				
	Mean Index Distance	**Mean LSAT Distance**	**d >2**	**2 < d <1**	**1 < d <-1**	**-1 < d < -2**	**d<-2**
White	9	0.32	1.2	13	77.2	7.4	1.2
Black	-145	-7.36	.1	1.5	28.4	33.9	36.2
Native American	-104	-4.66	0	2.9	48.6	27.9	20.7
Mexican	-89	-4.48	.2	2.2	48.7	35.6	13.4
Puerto Rican	-114	-6.00	0	.6	41.5	34.2	23.8
Other Latino	-69	-3.78	.2	4.1	57.4	24.7	13.7
Asian	-35	-2.12	.2	6.6	67.8	19.7	5.8
Other	-32	-1.94	.3	8.5	66.1	17.8	7.4
Minority Subgroup	-119	-6.06	.1	2.1	38.5	32.2	27.1
Total	-9	-.58	1	11.4	72	11	4.6

Table 3

Estimates of the Effect of Selective (Top Two Tiers) Versus Nonselective (Bottom Four Tiers)						
	(1)	(2)	(3)	(4)	(5)	(6)
Race Included	Whites	Blacks	Blacks	Minorities	Minorities	Minorities
Graduate	.027**	0.049**	.046**	.013	.013	.014
	(.004)	(.020)	(.021)	(.015)	(.015)	(.015)
N	21629	1809	1751	3214	3121	3121
Lawyer Completion	.011*	-.007	-.008	-.011	-.007	-.007
	(.006)	(.028)	(.028)	(.019)	(.02)	(.02)
Smooth Passage	-.0004	-.023	-.025	-.013	-.012	-.013
	(.007)	(.029)	(.029)	(.02)	(.02)	(.021)
N	21765	1836	1776	3257	3161	3161
Pass_Bar_Ever	-.007**	-0.042	-.039	-0.023	-.018	-.018
	(.003)	(.026)	(.027)	(.017)	(.017)	(.017)
Adj. Pass_Bar_Ever	-.013***	-0.05*	-.049*	-0.025	-.022	-.023
	(.004)	(.026)	(.027)	(.017)	(.018)	(.018)
Pass_Bar_First_Time	-.019***	-0.057*	-.06*	-.025	-.025	-.027
	(.005)	(.031)	(.032)	(.021)	(.021)	(.021)
N	18625	1345	1303	2486	2418	2418
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other Covariates	Yes	No	Yes	No	Yes	Yes
Race Dummies	No	No	No	No	No	Yes

*** denotes statistical significance at a 1% level (two-tail test); ** denotes statistical significance at a 5% level (two-tail test); * denotes statistical significance at a 10% level (two-tail test). Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. Other covariates include gender, income, mother's education, father's education, dummy variables for disabilities and an ESL dummy variable.

Table 4

Estimates of the Effect of Selective (Top Two Tiers) Versus Nonselective (Bottom Two Tiers)						
	(1)	(2)	(4)	(5)	(7)	(8)
Race Included	Whites	Blacks	Blacks	Minorities	Minorities	Minorities
Graduate	.018**	0.046	.034	-.0002	-.005	-.005
	(.009)	(.032)	(.032)	(.025)	(.026)	(.026)
N	7540	840	813	1462	1417	1417
Lawyer Completion	.037***	-.061	-.071*	-.079**	-.079**	-.08***
	(.014)	(.04)	(.041)	(.032)	(.032)	(.033)
Smooth Passage	.047***	-.098**	-.12***	-.108***	-.115***	-.116***
	(.015)	(.04)	(.041)	(.032)	(.033)	(.033)
N	7586	851	823	1483	1436	1436
Pass_Bar_Ever	-.008	-0.088**	-.089**	-0.064**	-.06**	-.063**
	(.009)	(.039)	(.04)	(.031)	(.031)	(.031)
Adj. Pass_Bar_Ever	-.0003	-0.113***	-.121***	-0.087***	-.089***	-.092***
	(.01)	(.038)	(.039)	(.031)	(.031)	(.031)
Pass_Bar_First_Time	.007	-0.139***	-.152***	-.11***	-.116***	-.119***
	(.012)	(.043)	(.044)	(.035)	(.036)	(.036)
N	6517	643	621	1161	1124	1124
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other Covariates	Yes	No	Yes	No	Yes	Yes
Race Dummies	No	No	No	No	No	Yes

*** denotes statistical significance at a 1% level (two-tail test); ** denotes statistical significance at a 5% level (two-tail test); * denotes statistical significance at a 10% level (two-tail test). Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. Other covariates include gender, income, mother's education, father's education, dummy variables for disabilities and an ESL dummy variable.

Table 5

Characteristics and Outcomes for First Choice Students						
	All Blacks	Black Attendees1	Black Attendees23	All Minority	Minority Attendees1	Minority Attendees23
Characteristics:						
Undergraduate GPA	2.86	2.97	3.03	2.95	3.07	3.08
School Tier (1=lowest; 6=highest)	3.41	3.76	3.43**	3.59	4.02	3.50***
Average LSAT Score	28.7	30.6	30.3	30.5	32.6	31.3***
Academic Index	569	611	608	605	649	631**
Percent at Selective Schools	23	33	23**	27	39	23***
Percent at Elite Schools	8	13	10	10	16	8***
Student Loan Debt (1=lowest; 5=highest)	3.4	3.3	3.3	3.17	3.1	3.3
Outcomes:						
Average Law School GPA (Standardized)	-1.02	-1.01	-.74***	-.84	-.87	-.62***
Percent Graduating	81	83	88	83	87	88
Percent Ever Passing the Bar Who Attempted	78	83	85	82	86	88
Percent Passing the Bar on First Attempt	61	66	79***	67	71	78*
Lawyer Completion	57	63	68	62	69	71
Smooth Passage	45	51	63***	51	57	72**
N	1874	472	178	1064	858	275

*** denotes statistical significance at a 5% level (two-tail test); ** denotes statistical significance at a 5% level (two-tail test); * denotes statistical significance at a 10% level (two-tail test).

Table 6A

IV Estimates of the Effect of Selective (Top Two v. Bottom Four Tiers) on Outcomes (IV = Attendees23)						
	Blacks			**Minorities**		
	OLS	**IV**	**IV**	**OLS**	**IV**	**IV**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Graduate	0.036	-.414	-.413	0.022	-.119	-.205
	(.03)	(.32)	(.31)	(.021)	(.182)	(.201)
		[8.83]	[8.35]		[21.76]	[16.39]
N	638	638	623	1115	1115	1091
Lawyer Completion	0.0001	-.399	-.376	.003	-.266	-.323
	(.042)	(.45)	(.421)	(.30)	(.259)	(.288)
		[7.84]	[7.73]		[20.98]	[16.07]
Smooth Passage	-.068	-1.28**	-1.31**	-.02	-.62**	-.777**
	(.043)	(.682)	(.599)	(.033)	(.295)	(.344)
		[7.84]	[7.73]		[20.98]	[16.07]
N	647	647	632	3173	1130	1106
Pass_Bar_Ever	-0.045	-.219	-.229	-0.016	-.239	-.258
	(.037)	(.33)	(.329)	(.025)	(.187)	(.220)
		[7.71]	[6.88]		[21.09]	[13.94]
Adj. Pass_Bar_Ever	-0.085**	-.748*	-.851**	-0.035	-.456**	-.579**
	(.38)	(.41)*	(.428)	(.026)	(.212)	(.255)
		[7.71]	[6.88]		[21.98]	[14.95]
Pass_Bar_First_Time	-0.123***	-1.26**	-1.42**	-0.045	-.639**	-.804**
	(.045)	(.56)	(.599)	(.033)	(.265)	(.322)
		[7.71]	[6.88]		[21.09]	[14.95]
N	500	500	488	909	909	889
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes

*** denotes statistical significance at a 1% level (two-tail test); ** denotes statistical significance at a 5% level (two-tail test); * denotes statistical significance at a 10% level (two-tail test). Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. Other covariates include gender, race, income, mother's education, father's education, dummy variables for disabilities and an ESL dummy variable.

Table 6B

IV Estimates of the Effect of Selective (Top Two v. Bottom Two Tiers) on Outcomes (IV = Attendees23)						
	Blacks			**Minorities**		
	OLS	**IV**	**IV**	**OLS**	**IV**	**IV**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Graduate	0.051	-.414	-.219	0.014	-.086	-.021
	(.051)	(.275)	(.258)	(.038)	(.172)	(.165)
		[9.832]	[8.44]		[22.08]	[21.41]
N	318	318	311	552	552	541
Lawyer Completion	-.055	-.627	-.435	-0.089*	-.49**	-.412*
	(.063)	(.42)	(.377)	(.05)	(.245)	(.236)
		[8.21]	[7.94]		[20.62]	[20.49]
Smooth Passage	-.17	-.937**	-.907**	-.169***	-.61**	-.614**
	(.063)	(.473)	(.437)	(.053)	(.272)	(.264)
		[8.21]	[7.94]		[20.62]	[20.49]
N	323	323	316	560	560	549
Pass_Bar_Ever	-0.117**	-.411	-.584*	-0.093**	-.388**	-.443**
	(.055)	(.271)	(.354)	(.045)	(.157)	(.178)
		[9.94]	[8.43]		[23.33]	[20.37]
Adj. Pass_Bar_Ever	-0.196***	-.575**	-.868**	-0.153***	-.471***	-.577***
	(.054)	(.294)	(.428)	(.0461)	(.173)	(.199)
		[9.94]	[8.43]		[23.33]	[20.37]
Pass_Bar_First_Time	-0.269***	-.781**	-1.15***	-0.20***	-.545**	-.683***
	(.065)	(.359)	(.436)	(.056)	(.218)	(.242)
		[9.94]	[8.43]		[23.33]	[20.37]
N	256	256	250	463	463	453
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other Covariates	No	No	Yes	No	No	Yes

*** denotes statistical significance at a 1% level (two-tail test); ** denotes statistical significance at a 5% level (two-tail test); * denotes statistical significance at a 10% level (two-tail test). Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. Other covariates include gender, race, income, mother's education, father's education, dummy variables for disabilities and an ESL dummy variable.