

Do Racial Preferences Affect Minority Learning in Law Schools?

*Doug Williams**

An analysis of the The Bar Passage Study (BPS) reveals that minorities are both less likely to graduate from law school and less likely to pass the bar compared to whites even after adjustments are made for group differences in academic credentials. To account for these adjusted racial gaps in performance, some researchers put forward the “mismatch hypothesis,” which proposes that students learn less when placed in learning environments where their academic skills are much lower than the typical student. This article presents new results from the BPS that account for both measurement-error bias and selection-on-unobservables bias that makes it more difficult to find a mismatch effect if in fact one exists. I find much more evidence for mismatch effects than previous research and report magnitudes from mismatch effects more than sufficient to explain racial gaps in performance.

I. INTRODUCTION

Racial preferences are a pervasive aspect of college and graduate school admissions.¹ Although these preferences aim to create more diverse campus environments and to expand the opportunities available to targeted groups, some social scientists argue that large racial preferences may have harmful, unintended consequences for the intended beneficiaries.² A poverty of good data has hindered efforts to test these arguments, 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

*Frank W. Wilson Professor of Economics, University of the South, Sewanee, TN 37383; email: dwilliam@sewanee.edu.

¹Although the term “racial preferences” is often used synonymously with “affirmative action” (and is sometimes used that way in this article), 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 article.

²Early proponents of the mismatch hypothesis include James Davis (1966), Clyde Summers (1970), and Thomas Sowell (1985).

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

performance but, more importantly, *that these gaps persist even after controlling for credentials*.⁴ In other words, minorities are both less likely to graduate from law school and less likely to pass the bar compared to whites even after adjustments are made for group differences in entering academic credentials.

Advocates of the “mismatch hypothesis” argue that such gaps are both unsurprising and explicable because racial preferences, by creating educational settings where minority students have much lower academic credentials than the majority of their classmates, will cause the students receiving large preferences to learn less and to accumulate less human capital than they would in a classroom setting where they were better “matched.”⁵ As a consequence of mismatching engendered by preferences, 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 article also uses the BPS but builds on 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. Prior research has generally neglected bar passage and has focused instead on how racial preferences ultimately affect the number of minority lawyers.⁶ In particular, this article addresses the effect of preferences on the quality of human capital rather than the effect of preferences on the diversity of the lawyer population. Second, this article avoids unobservables bias issues related to race by restricting the article to within-race analyses. Third, this article 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 biases have made it empirically challenging in previous research to find a mismatch effect if in fact one exists. By presenting consistent measurements of many outcomes across a variety of tests, this article helps integrate and explain the findings of past mismatch scholarship. Finally, the article presents results for a larger minority subgroup that includes blacks, Native Americans, and Hispanics (prior 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 article proceeds with a literature review (Section II) and a more in-depth examination of mismatch theory (Section III). Section IV introduces the BPS in greater detail and summarizes the extent of credential disparities across racial groups, and both the

⁴The use of preferences will create a student population where the mean academic credentials of whites exceed 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.

⁵Sander (2004).

⁶The 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.

unadjusted and adjusted racial gap in performance. Section V 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 article is found in Sections VI and VII where I present new empirical results that account for measurement-error bias and selection-on-unobservables bias. I summarize the findings in Section VIII.

II. 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.⁷ After establishing that bar passage was more sensitive to class rank than law school eliteness, Sander 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. Ayres and Brooks (2005) were among the first to directly test the mismatch hypothesis, and they employed two different approaches, both of which focused on the probability of becoming a lawyer. 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 data set, 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. In direct opposition to the mismatch hypothesis, they found that relative tier has a positive and significant effect on the likelihood of becoming a lawyer for all races.⁸ They also found no significant interaction effects for race and relative tier, with the exception of Hispanics, whose interaction effect is negative (a result consistent with mismatch). Ayres and Brooks did not present results for blacks and whites separately, and they omit the bottom tier (historically black law schools)

⁷Most 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 variable 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 benefit 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 percent more observations than Hispanics, is the largest underrepresented racial group in the BPS. Second, blacks receive the largest preferences so that an empirical test that measures preferences imperfectly is more likely to detect a mismatch effect for blacks than for other minority subgroups who receive smaller preferences. Third, blacks are much more likely to be regarded as a monolithic group than are Asians or Hispanics and, therefore, treated more consistently by law school admissions offices in allocating preferences.

⁸The authors use the *LAWYER COMPLETION* variable described below as their lawyer completion outcome variable.

from their analysis. Presumably, Ayers and Brooks excluded blacks at historically black schools because of a concern 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 the minorities who are “well matched,” the exclusion of these “better-matched” students 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, this test will be biased against the mismatch hypothesis. Finally, Ayres and Brooks do not attempt to reduce the noise in the BPS’s tier variable. Because the tier designation depends on other factors besides the academic credentials of students, there is likely to be significant overlap in school eliteness across tiers.

Ayres and Brooks’s second test attempted to address this selection bias by exploiting a unique feature of the BPS data set. 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. If minorities attending their first-choice school experience greater distance from their median peer than if they attended their second- or lower-choice school, then the mismatch hypothesis predicts that 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 found 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.⁹

Rothstein and Yoon also focus on blacks in an unpublished NBER paper (2008a) and a companion published article (2008b).¹⁰ In their unpublished paper, Rothstein and Yoon provided two direct tests of the mismatch hypothesis—one that they consider biased against finding mismatch and one that they consider biased toward finding

⁹In particular, first-choice students actually attending their first-choice school are less likely to have become a lawyer after four years of beginning law school compared to first-choice students attending their second- or lower-choice school. They also report that first-choice students are less likely to become lawyers on their first bar attempt.

¹⁰A paper that uses a similar framework to Rothstein and Yoon is Camilli et al. (2011). They use a matching framework, comparing students from the top two tiers to those from the bottom four tiers, to determine how going to a more selective school affects outcomes. The results from their matching method closely mirror the results from the regression method used by Rothstein and Yoon, finding weak support for a negative mismatch effect. They also show results for black test-takers for first-time bar passage with magnitudes similar to those that I show in Table 3. Their result is not statistically significant (my result is marginally significant), which could be explained by the much smaller sample they obtain with their matching procedure. They do not offer a correction for the noisiness of the tier variable nor do they offer a solution to the selection-on-unobservables problem.

mismatch.¹¹ 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 nonselective schools. They therefore regress an outcome measure on a binary indicator of school selectivity (schools in the first two tiers are designated as selective) with 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. Moreover, Rothstein and Yoon do not address the “noisy” tier variable issue. Rothstein and Yoon’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 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. In their companion piece published in the *University of Chicago Law Review*, Rothstein and Yoon do show results for passing the bar the first time for black test-takers relative to white test-takers that are consistent with the mismatch hypothesis. Although the magnitudes of the black-white differences they estimate are large and statistically significant, their results for black-white differences are consistent with alternative hypotheses, as discussed above.

Both Ayres and Brooks and Rothstein and 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

¹¹I focus primarily on Rothstein and Yoon’s unpublished article, which presents regression results for the selective-unselective comparisons that this article focuses on. The published article only presents black-white comparisons, which I argue is an unpersuasive test. Below, I do discuss the pertinent, additional results presented in their law review article for the black-white comparisons.

barriers hypothesis examines whether the race/tier interactions are jointly statistically significant and whether minorities fare worse at higher tiers. Barnes did not attempt to address selection-on-unobservables bias. 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 (Barnes 2011). A published replication by this author and three others produces results that are generally consistent with the mismatch hypothesis (Williams et al. 2011).

To sum up, each of the papers reviewed here concludes that the balance of the evidence provides little support for the mismatch effect in the production of lawyers. Yet each of these papers (including Barnes's corrected results) produced some results consistent with mismatch, and many of the models have omissions or incorporate biases that, if corrected, might produce a different empirical picture. Moreover, these papers generally neglected tests specifically aimed at examining whether mismatch reduces learning in law school, but focused instead on the production of lawyers. Let us consider how to address these issues.

III. 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 teach only at one level, and each student benefits according to the level of instruction delivered. Although the level of instruction is multidimensional and includes pace as well as complexity, it is reasonable here to model the instruction level as 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 the students in the classroom are homogenous in terms of academic credentials, then the instructor 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 tradeoff 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.

Now let us consider this model of classroom instruction in the real-world setting of legal education. There are roughly 200 law schools in the United States, and they are unusually hierarchical—even within the elite-conscious norms of U.S. higher education. Students perceive their success as closely linked to the eliteness of the school they attend (Korobkin 1998), and therefore apply to a large number 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 these reasons, in the absence of racial preferences, students at most law schools would 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 nonpreferenced 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.

IV. 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 require almost all law graduates who wish to obtain a law license to pass a state bar examination. While the bar exams are intended to test minimum proficiency only, they are tests of educational achievement taken by the vast majority of law graduates. The bar exam is validated using law school grades, and even the most able law school graduates take the exam very seriously.

Legal education, because of the requirement to pass the bar, is somewhat unique in having an outcome variable for measuring learning outcomes; however, the data requirements for properly testing the mismatch hypothesis pose a significant hurdle for successfully implementing an empirical test. At a minimum, data for individuals are needed on bar outcomes, college academic credentials, LSAT scores, and the median LSAT score of the law school attended. Data on family background and college quality would be helpful as well. The only data that come close to meeting these requirements are, as I have suggested, the Bar Passage Study (BPS). The 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 on 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 27,000 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 for our purposes. 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. Ideally, one would like to know the bar score of each taker scaled in a way 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.¹² Unlike most previous analyses that code individuals who do not attempt the bar exam as failing, this article 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 article 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, ADJUSTED PASS BAR EVER incorporates information about the number of attempts required to pass the bar; this variable takes 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. Because 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 preparation for repeat exams may span over several months or even years, so that repeaters will have a longer (unobservable) legal education than nonrepeaters.¹³ This shadow education will be customized to a bar failer's abilities, allowing the effects of mismatch to be undone over time.

To facilitate comparisons across the literature, I include two outcomes that have been studied by earlier mismatch scholars. In this article, 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 law schools make efforts to have no attrition whatsoever. A student taking advantage of admissions preferences, and thereby attending a more elite school, might

¹²By not including the actual score on the bar exam, the variation in performance is reduced, 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.

¹³These 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. 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.

increase his or her probability of graduation even if the student learns less.¹⁴ I also measure whether a given law school matriculant eventually becomes a lawyer. *LAWYER COMPLETION* 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 COMPLETION* variable in its tests of mismatch. Like the outcome *GRADUATE*, *LAWYER COMPLETION* is an important outcome measure but an imperfect measure of learning. For example, one difficulty with this measure is that it 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.

My sixth and 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 of 0 if an individual drops out or makes 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, all else 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 detect a mismatch effect. A second bias, which would favor an erroneous mismatch finding, involves law schools censoring the sample taking the bar. Nonelite 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 true, students graduating from elite schools and taking the bar will have inferior unobservables compared to what appears to be similar students graduating from nonelite schools and taking the bar, thus biasing upward our measurement of mismatch. Hence, *SMOOTH PASSAGE* is included as a robust check for outcome variables that only include test-takers. *SMOOTH PASSAGE* is also an intuitively appealing measure of mismatch because, from the standpoint of matriculating law students, graduating and passing the bar on the first attempt conforms to the typically expected notion of a "successful" academic outcome. Any other outcome reasonably implies that something went wrong.

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 number for the other racial groups is the racial difference compared to whites. The "All Minority" column shows the aggregate gap for blacks, Native Americans, and Hispanics as

¹⁴Because 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 marginal utility would suggest that the marginal benefit to a school of a 1 percent 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.

Table 1: Racial Differences for Outcome Measures

	<i>White</i>	<i>Black</i>	<i>Native American</i>	<i>Hispanic</i>	<i>All Minority</i>
Graduate (%)	92%	-11%	-9%	-5%	-9%
<i>Unexplained gap</i>	0%	-3%	-4%	0%	-2%
Pass bar first time (%)	92%	-31%	-26%	-17%	-25%
<i>Unexplained gap</i>	0%	-12%	-13%	-12%	-10%
Pass bar ever (%)	97%	-19%	-14%	-9%	-15%
<i>Unexplained gap</i>	0%	-8%	-7%	-3%	-5%
Lawyer completion (%)	82%	-26%	-22%	-11%	-20%
<i>Unexplained gap</i>	0%	-8%	-11%	-2%	-7%
Smooth passage (%)	78%	-34%	-29%	-18%	-27%
<i>Unexplained gap</i>	0%	-11%	-15%	-7%	-9%
No. of observations	22,608	1,874	144	1,294	3,312

NOTE: The numbers for whites are actual rates of success. The top number for each outcome variable for nonwhites is the gap compared to whites unadjusted for differences in credentials. The bottom number is the racial gap after adjusting for differences in academic credentials.

SOURCE: The Bar Passage Study.

a group.¹⁵ 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 also shows the racial gap that remains after controlling for credentials.¹⁶ Although it varies by outcome variable, about one-third to one-half of the race gap cannot be explained by race differences in entering academic credentials. After controlling for credentials, blacks (minorities) are 3 percent (2 percent) less likely to graduate compared to whites, 12 percent (10 percent) less likely to pass the bar the first time, 8 percent (5 percent) less likely to ever pass the bar, and 8 percent (7 percent) less likely to become a licensed lawyer.

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

¹⁵Hispanics 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 are more heterogeneous in terms of entering credentials and receive less preferences compared to other race groups (see Table 2).

¹⁶To control for credentials, a probit equation for each outcome variable was estimated for whites, using 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 parentheses, is the difference between the actual group outcome and the predicted outcome using the "white" coefficients.

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

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 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 or her peers by comparing a student's credentials with the median credentials of all students in the same tier. For example, INDEX DISTANCE, which is discussed below, is the difference between an individual's academic index and the median academic index for the tier to which the individual's school belongs. 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 the race classifications in the BPS. The mean INDEX DISTANCE for a group is the mean difference between an individual's own academic index in that group and the median academic index of the cluster to which that individual's school

Table 2: Academic Credentials Distance by Race

	<i>Mean Index Distance</i>	<i>Mean LSAT Distance</i>	<i>Distribution of Individual Index Distance (%)</i>				
			$d > 2$	$2 < d < 1$	$1 < d < -1$	$-1 < d < -2$	$D < -2$
White	9	0.32	1.2	13	77.2	7.4	1.2
Black	-145	-7.36	0.1	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	0.2	2.2	48.7	35.6	13.4
Puerto Rican	-114	-6.00	0	0.6	41.5	34.2	23.8
Other Latino	-69	-3.78	0.2	4.1	57.4	24.7	13.7
Asian	-35	-2.12	0.2	6.6	67.8	19.7	5.8
Other	-32	-1.94	0.3	8.5	66.1	17.8	7.4
Minority subgroup	-119	-6.06	0.1	2.1	38.5	32.2	27.1
Total	-9	-0.58	1	11.4	72	11	4.6

NOTE: The first two columns show the average amount of mismatch by race for the LSAT and the academic index, where mismatch is measured as the difference between an individual student's credential and the median credential at the law school tier where the student attends. The remaining columns show the distribution of mismatch by race where "d" is standard deviations from the tier median.

SOURCE: The Bar Passage Study.

belongs.¹⁷ The mean LSAT DISTANCE for a group is the mean difference between an individual's own LSAT score in that group and the median LSAT score of the cluster to which that individual's school belongs. The distance distribution is shown for standardized index distance (d), which is the individual INDEX DISTANCE divided by the standard deviation of individual INDEX DISTANCE for the individual's cluster. Negative values for distance imply negative mismatch; positive values imply positive mismatch. Consistent with not receiving any preferences, whites experience the least distance (mean INDEX DISTANCE = 9) and have the largest proportion within one standard deviation from the mean (77.2 percent). All the included ethnic groups receive significant preferences as measured by mean INDEX DISTANCE. Blacks have the greatest negative distance (mean INDEX DISTANCE = -145) and the smallest proportion within one standard deviation from the mean (28.4 percent). Because Asian and "other" 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.¹⁸

V. 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:

$$Y_i = \beta_0 + \beta_1[C_i - \mu_i] + \beta_2C_i + \beta_3\mu_i + u_i, \quad (2)$$

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 $D_i = C_i - \mu_i \geq 0$, the distance variable drops out and Equation (2) becomes:

$$Y_i = \beta_0 + \beta_2C_i + \beta_3\mu_i + u_i. \quad (3)$$

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-

¹⁷The 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 percent weight on applicant undergraduate GPA and a 60 percent weight on applicant LSAT score, and it is scaled between 0 and 1,000. Other researchers, including Sander, Ayres and Brooks, and Rothstein and Yoon, have used this measure in their analyses.

¹⁸The "other" category was created by the BPS surveyors to include nonwhite persons not included in one of the specified racial categories.

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 underestimate on average the true distance for those students with high negative values of D and will overestimate 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 (i.e., D is a linear combination of C and μ for each observation). One strategy that avoids perfect multicollinearity is to set $D = 0$ for “matched” students and estimate Equation (4):

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

In practice, however, this equation exhibits high multicollinearity so that the estimates are unreliable.¹⁹

To avoid the problem of multicollinearity, this article 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 VI and VII:

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

Equation (5) is most appropriate as an estimation equation for samples where few students are positively matched. In practice, this means that this equation is appropriate for blacks (who are mostly mismatched) but not for whites (who are mostly matched or positively 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. Rothstein and Yoon 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

¹⁹See Williams (2009).

time make tier an even noisier proxy for true law school selectivity. The model defining selectivity as a binary variable (suppressing the intercept) is:

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

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 nonselective 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 VI, I estimate two models for both blacks only and minorities only. I first estimate Equation (6) using the selectivity variable defined by Rothstein and Yoon, 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 VII, I estimate an instrumental variables (IV) model to correct for selection-on-unobservables bias. The BPS enables the researcher to identify whether a student was admitted to his or her first-choice school and, if so, whether these “first-choice students” are attending their first-choice school or not. As discussed above, Ayres and Brooks (2005) first suggested using the “first-choice students” as an identification strategy. They acknowledged that Dale and Krueger’s identification strategy of matching students with similar application and admission decisions would be ideal but the lack of data in the BPS on where students applied makes it impossible to implement the Dale and Krueger strategy. Although Ayres and Brooks do not estimate an IV model, they are essentially using the second- or lower-choice indicator variable as an instrument.²⁰ 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 for *selective* in Equation (6) that should eliminate unobservables bias. In other words, when the pool considered consists only of students admitted to their first-choice school (regardless of whether they attend), students within a particular racial group should possess similar academic credentials (whether or not observed), and it can be assumed that second-choice schools are less selective. For example, students A and B are both minority students admitted to Cornell. Student A attends Cornell. Student B attends Washington University, her second choice, because she is offered a full scholarship. I am able to identify students who chose not to attend their first-choice school because of cost considerations and geographical constraints and only include these students in the empirical analysis below where I estimate IV equations.²¹ If choices based on cost considerations and geographical constraints are

²⁰The second-choice indicator takes a value 1 if the second- or lower-choice school is attended and a value 0 if the first-choice school is attended.

²¹These are students who were admitted to their first-choice school but reported they did not attend either because “it was too expensive given the financial aid made available to me” or because “it was too distant from my family or personal responsibilities.”

uncorrelated with unobservable ability, then the second- or lower-choice indicator will be a valid instrument. One concern raised by Rothstein and Yoon (2008b) is that students who attend their second-choice school because of cost considerations may come from families with less income and less parental inputs. If this were the case, then it would be *harder* to find mismatch using the second- or lower-choice indicator variable as an instrument. Even so, I estimate IV regressions that include family income as a control.

VI. OLS ESTIMATES FOR THE SELECTIVE MODEL

This section presents results for the “selective” model first suggested by Rothstein and Yoon. To facilitate a direct comparison with Rothstein and Yoon’s OLS estimates, I also employ OLS.²² But the estimates presented in this section differ from their estimates in two important ways. First, I include measures on bar passage outcomes for law school graduates who actually took the bar exam, and second (in Table 4), I add new estimates for a definition of selectivity that reduces measurement error bias.

Let us first consider Table 3, which follows the Rothstein and Yoon (2008a) approach of comparing the top two law school tiers with the bottom four. The results in Column (2) of Table 3 for GRADUATE and LAWYER COMPLETION correspond to the Rothstein and Yoon estimates (2008a). Column (2) also includes new estimates for the bar passage outcomes for test-takers. 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 percent, are substantial. In contrast to the Rothstein and Yoon results that use graduation and lawyer completion as outcome variables, all 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 percent 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 apply. 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, as already discussed, by a lower threshold of performance required for graduation by more elite law schools. Though small in magnitude, it is curious that the estimated coefficients for SELECTIVE in the white bar passage

²²As Joshua Angrist and Jorn-Steffen Pischke (2008) have argued, the choice of OLS (also known as the linear probability model or LPM) versus logit for binary response models is largely a matter of the modeler’s preference if marginal effects rather than structural parameters are of primary interest. Both are approximations of the true nonlinear function: OLS is a linear approximation, while logit is usually an arbitrarily chosen nonlinear approximation. I show the OLS estimates to allow direct comparison with Rothstein and Yoon (2008a), who estimate their selectivity model with OLS, and to allow direct comparison with the two-stage least square results in Tables 6 and 7. It would be concerning if the OLS and logit results varied greatly. That is not the case here. The estimated marginal effects for the logit model version of Tables 3 and 4 (not shown here but available from the author) are nearly identical to the OLS estimates both in terms of magnitudes and statistical significance.

Table 3: OLS Estimates of the Effect of Selective (Top Two Tiers) Versus Nonselective (Bottom Four Tiers)

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Race Included</i>	<i>Whites</i>	<i>Blacks</i>	<i>Blacks</i>	<i>Minorities</i>	<i>Minorities</i>	<i>Minorities</i>
Graduate	0.027*** (0.004)	0.049** (0.020)	0.046** (0.021)	0.013 (0.015)	0.013 (0.015)	0.014 (0.015)
<i>N</i>	21,629	1,809	1,751	3,214	3,121	3,121
Lawyer completion	0.011* (0.006)	-0.007 (0.028)	-0.008 (0.028)	-0.011 (0.019)	-0.007 (0.02)	-0.007 (0.02)
Smooth passage	-0.0004 (0.007)	-0.023 (0.029)	-0.025 (0.029)	-0.013 (0.02)	-0.012 (0.02)	-0.013 (0.021)
<i>N</i>	21,765	1,836	1,776	3,257	3,161	3,161
Pass_bar_ever	-0.007** (0.003)	-0.042 (0.026)	-0.039 (0.027)	-0.023 (0.017)	-0.018 (0.017)	-0.018 (0.017)
Adj. pass_bar_ever	-0.013*** (0.004)	-0.05* (0.026)	-0.049* (0.027)	-0.025 (0.017)	-0.022 (0.018)	-0.023 (0.018)
Pass_bar_first_time	-0.019*** (0.005)	-0.057* (0.031)	-0.06* (0.032)	-0.025 (0.021)	-0.025 (0.021)	-0.027 (0.021)
<i>N</i>	18,625	1,345	1,303	2,486	2,418	2,418
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	Yes	No	Yes	No	Yes	Yes
Race dummies	No	No	No	No	No	Yes

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Robust standard errors in parentheses. The table reports results for regression models in which the dependent variables are indicated by the rows and the column headings show the groups of subjects to which the regression models are limited. The cell entries are the coefficients and standard errors for the selective variable in each regression model. 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; observations with missing data for these covariates are dropped in regressions (3), (5) and (6), which explains the smaller *N*.

SOURCE: The Bar Passage Study.

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 is 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 the signs are negative and statistically significant for both blacks and the minority subgroup. The magnitude of the coefficients is substantial and is 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.²³ On average, such a movement would eliminate the

²³These numbers were calculated by regressing credentials and SELECTIVE on distance.

Table 4: OLS Estimates of the Effect of Selective (Top Two Tiers) Versus Nonselective (Bottom Two Tiers)

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Race Included</i>	<i>Whites</i>	<i>Blacks</i>	<i>Blacks</i>	<i>Minorities</i>	<i>Minorities</i>	<i>Minorities</i>
Graduate	0.018** (0.009)	0.046 (0.032)	0.034 (0.032)	-0.0002 (0.025)	-0.005 (0.026)	-0.005 (0.026)
<i>N</i>	7,540	840	813	1,462	1,417	1,417
Lawyer completion	0.037*** (0.014)	-0.061 (0.04)	-0.071* (0.041)	-0.079** (0.032)	-0.079** (0.032)	-0.08** (0.033)
Smooth passage	0.047*** (0.015)	-0.098** (0.04)	-0.12*** (0.041)	-0.108*** (0.032)	-0.115*** (0.033)	-0.116*** (0.033)
<i>N</i>	7,586	851	823	1,483	1,436	1,436
Pass_bar_ever	-0.008 (0.009)	-0.088** (0.039)	-0.089** (0.04)	-0.064** (0.031)	-0.06** (0.031)	-0.063** (0.031)
Adj. pass_bar_ever	-0.0003 (0.01)	-0.113*** (0.038)	-0.121*** (0.039)	-0.087*** (0.031)	-0.089*** (0.031)	-0.092*** (0.031)
Pass_bar_first_time	0.007 (0.012)	-0.139*** (0.043)	-0.152*** (0.044)	-0.11*** (0.035)	-0.116*** (0.036)	-0.119*** (0.036)
<i>N</i>	6,517	643	621	1,161	1,124	1,124
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	Yes	No	Yes	No	Yes	Yes
Race dummies	No	No	No	No	No	Yes

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Robust standard errors in parentheses. The table reports results for regression models in which the dependent variables are indicated by the rows and the column headings show the groups of subjects to which the regression models are limited. The cell entries are the coefficients and standard errors for the selective variable in each regression model. 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; observations with missing data for these covariates are dropped in regressions (3), (5), and (6), which explains the smaller *N*.

SOURCE: The Bar Passage Study.

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 percent, eliminating the 12 percent unexplained gap for blacks and the 10 percent 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 percent, eliminating the 8 percent unexplained gap for blacks and the 5 percent unexplained gap for the minority subgroup. In these 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 increase in magnitude (compared to Table 3) and are statistically significant for blacks and minorities.²⁴ And again, the magnitudes of the SMOOTH PASSAGE coefficients (10 to 12

²⁴To assure that these results are not driven by a lack of common support, I also estimated a matching model using the matching estimator suggested by Abadie et al. (2001). The results from the matching model are consistent with the regression results.

percent) are large enough to eliminate the unexplained gaps (9 to 11 percent) in Table 1. Overall, the "selective" test yields both coefficient "signs" and magnitudes that strongly support the mismatch hypothesis.

VII. 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 CHOICE (0 = attending second- or lower-choice school and 1 = attending first-choice school) indicates the matriculation decision of first-choice students and is used as an instrument for selectivity in the selective model.

First-choice students in the BLS are separated into two groups: ATTENDEES1 are first-choice students actually attending their first choice, and ATTENDEES23 are first-choice students who attend their second-choice or lower-choice school and who state they are not attending their first-choice school because it was too expensive given the financial aid available or because it was too distant from family and/or personal responsibilities. An ideal natural experiment for the "selective" test would sort students into selective and nonselective schools by an exogenous mechanism unrelated to preferences or choices, but as long as the choice of whether to attend one's first choice does not depend on unobservables that affect outcomes, the sorting of students into selective and nonselective schools by these choices will be random with respect to the unobservables. Limiting the analysis to students who were admitted to their first choice has the additional likely virtue of creating a better match between the treatment group (students who attend their first choice) and the control group (students who attend their second or lower choice). Since both the controls and treated students with the same observable credentials were accepted into their first choice, they are likely to be more similar with respect to unobservable credentials than randomly selected individuals with identical observable credentials. As a rough check on the matching virtue of the first-choice analysis, Table 5 compares the observable credentials and the outcomes of ATTENDEES1 with those of ATTENDEES23. Academically, ATTENDEES1 are remarkably similar to ATTENDEES23. For blacks, mean LSAT scores and the mean academic index are statistically indistinguishable between ATTENDEES1 and ATTENDEES23, while the differences in mean GPAs for blacks are statistically significant. For the minority subgroup, the mean undergraduate GPA and the academic index are statistically indistinguishable between ATTENDEES1 and ATTENDEES23, while the difference in the mean LSAT for the two groups is statistically significant but small.²⁵ ATTENDEES1 are, as expected, more likely to attend elite schools (schools in the highest tier of the six tiers) and selective schools

²⁵The magnitudes of the statistically significant differences in mean credentials for the ATTENDEES1 and ATTENDEES23 are small: one-sixth of a standard deviation difference for the mean GPA for blacks and one-eighth of a standard deviation difference for the mean LSAT for minorities.

Table 5: Characteristics and Outcomes for First-Choice Students

	<i>All Blacks</i>	<i>Black Attendees1</i>	<i>Black Attendees23</i>	<i>All Minority</i>	<i>Minority Attendees1</i>	<i>Minority Attendees23</i>
<i>Characteristics</i>						
Undergraduate GPA	2.86	2.97	3.04**	2.95	3.06	3.09
School tier (1 = lowest; 6 = highest)	3.41	3.75	3.49**	3.59	4.00	3.59***
Average LSAT score (range: 10–48)	28.7	30.6	30.3	30.5	32.3	31.5**
Academic index (range: 0–1,000)	569	608	609	605	644	634
Percent at selective schools (two highest tiers)	23	32	23**	27	39	24***
Percent at elite schools (highest tier)	8	13	9	10	16	10**
Student loan debt (1 = lowest; 5 = highest)	3.4	3.3	3.2	3.17	3.13	3.16
<i>Outcomes</i>						
Average law school GPA (standardized)	–1.02	–1.02	–0.76***	–0.84	–0.88	–0.63***
Percent graduating	81	83	88	83	87	87
Percent ever passing the bar who attempted	78	83	86	82	86	88
Percent passing the bar on first attempt	61	66	80***	67	70	78**
Lawyer completion	57	62	67	62	68	70
Smooth passage	45	50	62***	51	56	62*
<i>N</i>	1,874	512	177	3,282	920	209

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. A t test is used to test for a difference in means between Attendees1 on the one hand and Attendees2 and Attendees23 on the other.

SOURCE: The Bar Passage Study.

(schools in the highest two tiers) compared to ATTENDEES23. The selective school difference is statistically significant at a 5 percent 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 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 statistically significant and smooth passage is marginally significant.

The results in Table 5 are supportive of a mismatch effect and can be formalized with an IV model where CHOICE is used as an instrument for SELECTIVE. Although CHOICE is not exogenous, it does satisfy the requirements of an instrumental variable as long as the exogenous factors that determine the value of CHOICE (i.e., choices based on cost considerations and geographical attachments) are uncorrelated with the disturbance in the outcome Equation (6). Tables 6 and 7 provide two-stage least squares (2SLS) results for blacks and minorities using CHOICE as an instrument for selectivity. In Table 6, SELECTIVE indicates top two tiers or bottom four tiers; in Table 7, SELECTIVE indicates top two tiers or bottom two tiers. The results in Table 6 correct for selection-on-unobservables bias while the results in Table 7 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 6, CHOICE is a weak instrument ($F < 10$) for the regressions for blacks and a strong instrument for minorities. All the signs are consistent with the mismatch hypothesis. Moreover, the estimates for the PASS BAR FIRST TIME coefficients are statistically significant for both blacks and minorities. ADJUSTED PASS BAR EVER is statistically significant in three of the four regressions. SMOOTH PASSAGE, which is included as a robust check for selection bias, is statistically significant for both blacks and minorities. In Table 7, which omits the two middle tiers in defining SELECTIVE to minimize measurement error from overlapping tiers, the results are stronger. CHOICE 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 the results for minorities. For the minority regressions in Columns (5) and (6), most of the coefficients are negative and statistically significant. The exceptions are the GRADUATE regression (negative but insignificant in both columns), the LAWYER COMPLETION regression (negative but insignificant in both columns), and the PASS BAR EVER regression (negative and statistically significant in Column (5) but marginally significant in Column (6)). 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 7, the results suggest that eliminating two-thirds of the actual mean academic index distance would increase first-time bar passage by an amount seven times larger than

Table 6: IV (2SLS) Estimates of the Effect of Selective (Top Two vs. Bottom Four Tiers) on Outcomes (IV = Choice)

	<i>Blacks</i>			<i>Minorities</i>		
	<i>OLS</i>	<i>IV</i>	<i>IV</i>	<i>OLS</i>	<i>IV</i>	<i>IV</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Graduate	0.043 (0.03)	-0.433 (0.32) [9.13]	-0.356 (0.291) [9.15]	0.024 (0.021)	-0.049 (0.183) [22.07]	-0.084 (0.202) [16.56]
<i>N</i>	678	678	663	1,190	1,190	1,166
Lawyer completion	-0.017 (0.04)	-0.375 (0.442) [8.08]	-0.348 (0.409) [8.29]	-0.014 (0.30)	-0.151 (0.259) [20.46]	-0.213 (0.284) [16.16]
Smooth passage	-0.069* (0.043)	-0.127** (0.607) [8.08]	-1.24** (0.564) [8.29]	-0.029 (0.032)	-0.579** (0.292) [20.46]	-0.746** (0.339) [16.16]
<i>N</i>	687	687	672	1,206	1,206	1,181
Pass_bar_ever	-0.067* (0.036)	-0.223 (0.308) [8.79]	-0.285 (0.306) [8.17]	-0.032 (0.026)	-0.219 (0.178) [23.03]	-0.285 (0.208) [16.39]
Adj. pass_bar_ever	-0.095*** (0.36)	-0.742* (0.384) [8.79]	-0.855** (0.394) [8.17]	-0.045* (0.025)	-0.468** (0.203) [23.03]	-0.634** (0.251) [16.39]
Pass_bar_first_time	-0.123*** (0.043)	-1.22** (0.521) [8.79]	-1.35** (0.534) [8.17]	-0.05 (0.032)	-0.665*** (0.255) [23.03]	-0.874*** (0.317) [16.39]
<i>N</i>	527	527	515	963	963	943
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	No	No	Yes	No	No	Yes

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Robust standard errors in parentheses. The table reports results for regression models in which the dependent variables are indicated by the rows and the column headings show the groups of subjects to which the regression models are limited. The cell entries are the coefficients and standard errors for the selective variable in each regression model. The F statistic for the lower-choice variable in the first-stage regression is bracketed. Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. 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; observations with missing data for these covariates are dropped in regressions (3) and (6), which explains the smaller N .

SOURCE: The Bar Passage Study.

the unexplained gap (68 percent compared to 10 percent), would increase ever passing the bar by an amount nine times larger than the unexplained gap (44 percent compared to 5 percent), and would increase lawyer completion rates by an amount six times larger than the unexplained gap (41 percent compared to 7 percent).²⁶ Although the large magnitudes could reflect model misspecification, they could also simply reflect the small

²⁶Minorities attending the top two tiers have an academic index score 93 points below the median law student in their tier on average, while minorities in the bottom two tiers have an academic index score that is 32 points below the median law student in their tier on average.

Table 7: IV (2SLS) Estimates of the Effect of Selective (Top Two vs. Bottom Two Tiers) on Outcomes (IV = Choice)

	<i>Blacks</i>			<i>Minorities</i>		
	<i>OLS</i>	<i>IV</i>	<i>IV</i>	<i>OLS</i>	<i>IV</i>	<i>IV</i>
	(1)	(2)	(3)	(4)	(5)	(6)
Graduate	0.068 (0.049)	-0.507* (0.297)	-0.325 (0.289)	0.019 (0.037)	-0.089 (0.186)	-0.053 (0.186)
<i>N</i>	335	335	328	589	589	578
Lawyer completion	-0.063 (0.059)	-0.633 (0.453)	-0.45 (0.434)	-0.094** (0.048)	-0.29 (0.269)	-0.296 (0.267)
Smooth passage	-0.17*** (0.059)	-1.14** (0.547)	-1.22** (0.553)	-0.178*** (0.049)	-0.59** (0.294)	-0.695** (0.296)
<i>N</i>	341	341	334	599	599	587
Pass_bar_ever	-0.142*** (0.052)	-0.373 (0.260)	-0.561* (0.31)	-0.107** (0.043)	-0.277* (0.158)	-0.392** (0.181)
Adj. pass_bar_ever	-0.214*** (0.05)	-0.622** (0.289)	-0.975*** (0.374)	-0.168*** (0.043)	-0.441** (0.174)	-0.624*** (0.206)
Pass_bar_first_time	-0.279*** (0.059)	-0.882** (0.358)	-1.34*** (0.468)	-0.218*** (0.052)	-0.579*** (0.214)	-0.803*** (0.253)
<i>N</i>	269	269	263	492	492	482
Credentials	Yes	Yes	Yes	Yes	Yes	Yes
Other covariates	No	No	Yes	No	No	Yes

NOTE: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Robust standard errors in parentheses. The table reports results for regression models in which the dependent variables are indicated by the rows and the column headings show the groups of subjects to which the regression models are limited. The cell entries are the coefficients and standard errors for the selective variable in each regression model. The F statistic for the lower-choice variable in the first-stage regression is bracketed. Credential controls are specified as a quadratic in LSAT score and undergraduate GPA. 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; observations with missing data for these covariates are dropped in regressions (3) and (6).

SOURCE: The Bar Passage Study.

sample size. Moreover, more plausible magnitudes much closer to the unexplained gaps can be found within the 95 percent confidence level for all the estimates. Although these IV results should be interpreted carefully because of the magnitudes, they are consistent with mismatch effects.²⁷

²⁷Recall that I control for family income to control for selection on unobservables. Another concern might be that some students go to their second choice rather than their first choice because they receive a better financial package that might include merit aid and that these offers of merit aid from less selective schools create a selection-on-unobservables bias. The logic of this argument would seem to imply that students at more highly ranked schools (first-choice schools) have worse unobservables than students at lesser ranked schools (second-choice schools). This

VIII. CONCLUSION

All the previous papers that have used the BPS and 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 article 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 is needed to fully understand the magnitudes of mismatch effects in law school. Conducting this research will require better data that contain 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.

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seems implausible. However, if this were the case, including an indicator variable for merit aid should make the coefficients in Tables 6 and 7 less negative. In fact, results (not shown here) that include an indicator variable for merit aid show that all the coefficients become more negative, albeit slightly nosier.

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