Mismatch and Bar Passage: A School-Specific Analysis

Richard Sander and Robert Steinbuch

I. Introduction

Do the large admissions preferences used by many law schools hurt their intended beneficiaries by undermining student learning and subsequent performance on bar exams? One of us (Sander) posed this question in early 2005 and presented a variety of data and analyses to suggest that preferences had exactly this effect. Sander argued that law professors (like most teachers) tend to gear instruction to the middle range of scholastic ability in a classroom. For a student admitted with a large preference – meaning the student had substantially lower academic credentials than did her "median" classmate – this might make learning harder. And given the intense pace and difficulty of the law school curriculum, especially in the first year, the greater learning challenge could compound itself, producing poor academic performance and, after graduation, greater difficulty passing the bar. Sander showed that the group of law students who received (on average) the largest preferences (Blacks) tended to have very low law school grades – an effect he found was entirely due to preferences – and these students were far more likely than other students to fail the bar after multiple attempts – another "preference" effect rather than a "race" effect. Sander found further that without preferences, Blacks would have grades comparable to whites and the bar-passage gap would substantially narrow. Using simulations, Sander concluded that the number of Black lawyers would probably increase if preferences were eliminated.

This argument, known as the "law school mismatch hypothesis," generated intense interest, not only within the legal academy but among those engaged in the broader national debate on affirmative action. Several prominent critics emerged, though their critiques were not generally published in peer-reviewed journals and have not held up well under closer examination.[1] Several careful peer-reviewed studies have appeared over the past decade, including a com-

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1. Peter Arcidiacono & Michael Lovenheim, *Affirmative Action and the Quality-Fit Trade-off*, 54 J.

prehensive review essay written by two accomplished labor economists, Peter Arcidiacono and Michael Lovenheim, for the March 2016 *Journal of Economic Literature*. After reviewing much of the back and forth, they put their conclusion this way:

> We find the evidence suggesting that shifting African-Americans to less-selective schools would increase bar passage, particularly for first-time bar passage, to be fairly convincing. This is especially the case since the low quality of the data would tend to bias estimates away from finding mismatch. On the other hand, an argument could be made that the data are too noisy and provide sufficiently imprecise information on actual law-school quality that they preclude one from drawing any concrete conclusions regarding mismatch. Regardless, the law-school debate makes clear that this is a question that merits further attention, where more definitive answers could be answered with better data. Our hope is that better datasets will soon become available.[2]

Arcidiacono and Lovenheim singled out the data problem with good reason: all of the major articles on law school mismatch have relied on one data source – the Bar Passage Study (hereinafter "BPS") conducted by the Law School Admission Council ("LSAC") in the 1990s. The BPS is not only a quarter-century old now, but, for reasons we elaborate in Part II, the BPS allows only indirect measurement of how "mismatched" individual students might be.

Excellent data to test mismatch exists, and even better data could be readily developed, but leaders of the legal academy have been remarkably unified in blocking the use or creation of such data. In 2006-07, when the California Bar was favorably considering a collaborative study of mismatch with its unique, large dataset on the background and outcomes of bar-takers, the Society of American Law Teachers and a group of California law school deans intervened to dissuade Bar officials from doing so.[3] The National Committee of Bar Examiners rejected requests to study the mismatch issue or to make its data available for such research. The Law School Admission Council threatened to defund the "After the JD" study if it did not firmly dissociate itself from research on mismatch. And we could offer many other, similar examples.[4]

Econ. Lit. 3, 18-20 (2016), discuss the law school mismatch debate in some detail and point out several problems in the critiques; so does Doug Williams, *Do Racial Preferences Affect Minority Learning in Law Schools?*, 10 J. Empir. Legal Stud. 171 (2013). Richard Sander, *Replication of Mismatch Research: Ayres, Brooks and Ho*, 58 Int'l. Rev. L. & Econ. 75 (2018), shows several key errors in a leading critique of the mismatch effect which, when corrected, alter the authors' results and provide strong support for mismatch.

2. Arcidiacono and Lovenheim, *supra* note 1, at 20.

3. The Society of American Law Teachers' letter and other extensive materials from the battle over California Bar data are posted at *California Bar Lawsuit*, Project Seaphe, http://www.seaphe.org/topic-pages/california-bar-lawsuit.php (last visited March 3, 2023); *see also* Richard H. Sander & Stuart Taylor, Jr., Mismatch: How Affirmative Action Hurts Students It's Intended to Help, and Why Universities Won't Admit It chp. 15 (2012).

4. Sander & Taylor, *supra* note 3, at chp. 5.

Why has the legal education establishment become so hostile to the development or release of data on the determinants of individual success in law school and the bar? This is an interesting and important question, but it is not the primary subject of this article. We report here, instead, on some valuable data that we *have* obtained, which allows us to perform new and (in significant ways) better tests of the mismatch hypothesis, and thus to advance our understanding of the subject. In particular, we present here the first analysis of law students that can estimate "mismatch" levels for individual students, and thus test how students with similar credentials, but varying levels of mismatch, fare when they take bar exams.

Our major findings are these:

1. In the three schools we examine, greater levels of mismatch are strongly associated with weaker first-time performance on state bar exams. Indeed, our analyses suggest that one's relative position in one's law school class (in terms of academic credentials) matters more than the absolute level of one's credentials. To put it differently, the improved measure of mismatch we are able to create with this data suggests that the harmful effect of law-school mismatch upon bar-passage rates is larger than earlier research by Sander (2005) or Williams (2013) documented. This is not altogether surprising, since, as Arcidiacono and Lovenheim pointed out, the weaker measures of mismatch used before would tend to bias mismatch estimates downward.
2. When we control for mismatch effects and LSAT scores, racial deficits in bar-passage rates substantially shrink. When we add measures of undergraduate grades ("UGPA") into the analysis, racial deficits virtually disappear. Our results imply that Black and Hispanic bar performance could improve dramatically if student levels of mismatch were reduced or the learning loss associated with mismatch was otherwise successfully addressed.
3. Our dataset has important limitations. It covers only three schools, and one of them is in a different bar jurisdiction from the other two. For one school, our only "credential" data are LSAT scores. For all three, we have no data on *outgoing* transfer students, which limits our ability to compare outcomes across the entire class of entering students. This is not, of course, a randomized experiment, so student choices may produce student bodies at the various schools that are different in substantive ways we cannot ascertain. These are all reasons to interpret our findings cautiously and to reiterate calls for more and better data. However, by using a variety of techniques, and subjecting our data to a mix of tests, we can evaluate the robustness of our findings – and they hold up well.
4. Using ABA data on the racial makeup of law students and lawyers, we examine aggregate attrition rates for minorities from the legal profession. This analysis shows that *something* very disproportionately causes attrition among Blacks, Hispanics, and American Indians from the ranks

of would-be lawyers. The severity of this attrition calls for immediate investigation and corrective strategies from both the legal academy and the legal profession.

II. The Bar Passage Study Data and Its Limitations in Studying Mismatch

The issue of whether mismatch effects exist, and are large enough to worry about, has been around for a long time. James Davis raised the issue in his classic 1966 paper, "The Campus as a Frog Pond," and it was discussed at some length by Christopher Jencks and David Reisman in their influential 1969 book, *The Academic Revolution*. In 1970, Clyde Summers identified mismatch as a potentially key flaw in law-school affirmative-action plans, which were then just getting established,[5] and Thomas Sowell made similar points in broader critiques of racial preferences.

Most law schools, then and now, based admissions decisions largely on two academic credentials: an applicant's LSAT score and her undergraduate grade point average (UGPA). Other factors are used, of course, but a good deal of research has shown that the vast majority of admissions decisions at a given law school can be predicted from these two factors. Moreover, schools tend to give somewhat more weight to LSAT than UGPA, apparently because the available research shows that LSAT does a better job of predicting law school grades. Some schools combine LSAT and UGPA into a single "index" to compare students, and for ease of discussion we will do the same. If LSAT scores run from 120 to 180 and UPGAs run from 0 to 4.0, then the "academic index" we use below is calculated as (10*(LSAT-120)) + (100*UGPA), which scales student credentials from 0 to 1000.

Even at the time that Clyde Summers was writing, and probably even more so today, there was a widely recognized hierarchy for law schools within which we commonly speak of several "tiers" of schools, with the higher-tier schools able to attract stronger students than the lower-tier ones. We can certainly observe in admissions data that the academic index scores of students at a highly-ranked school barely overlap with those of a school that is, say, 25 places down in various academic rankings, such as that provided by U.S. News and World Report. Thus, at the University of Virginia, a "Tier One" school, the median academic index for matriculants in 2006-07 was 862 and the 25th percentile was 829, while at William and Mary, a "Tier Two" school also in Virginia, the median academic index was 802 and the 25th percentile was 761. Since law schools routinely use race-based admissions preferences equivalent to over one hundred academic

5. James A. Davis, *The Campus as a Frog Pond: An Application of the Theory of Relative Deprivation to Career Decisions of College Men*, 72 Amer. J.L. Soc. 17 (1966); Christopher Jencks & David Riesman, The Academic Revolution (1968); Clyde W. Summers, *Preferential Admissions: An Unreal Solution to a Real Problem*, 2 U. Tol. L. Rev. 377 (1970).

index points,[6] a student accepting such a preference would (usually unwittingly) enter a school where her index was far below those of nearly all her peers.[7]

It was well recognized by the 1980s that "minority" students – particularly Black students -- had bar passage rates well below those of whites. Concern was great enough to motivate the LSAC to launch, in 1989, the BPS, an unprecedented and still unique effort to study in depth the progress of a national cohort of students through law school and their attempts to pass state bar examinations. The study gained the cooperation of nearly every state bar in the nation along with 155 of the 172 accredited "mainland" law schools that then existed.[8] The BPS tracked some twenty-seven thousand students who began law school in the fall of 1991. Participating students completed a detailed questionnaire soon after they arrived at law school, and a large subsample of students completed three follow-up surveys during law school and after graduation. Participating schools provided data on student grades and graduation outcomes. LSAC gathered data on bar outcomes either from the State Bars themselves or from published lists of bar passers. Along many dimensions, the quality of data obtained in the BPS was exceptionally high.

At the outset, and in approaching the various schools, state supreme courts, and bar associations from whom they sought data, the BPS organizers promised to study a range of possible explanations for low minority bar passage rates. Broadly speaking, there were three distinct theories about what might be happening. One theory was that bar examinations were racially biased – either asking questions in a way that disfavored minority test-takers, or actually scoring exams in a racially discriminatory way. We will refer to this as the "discrimination" hypothesis. A second theory was that minority students did worse because, on

6. Project Seaphe gathered admissions and enrollment data from over forty public law schools across the United States for the entering classes of 2006 and 2007. Some examples from that data illustrate our point: at the University of Virginia in 2006-07, 72% of enrolled Blacks had an academic index below 760, while 97% of enrolled whites had an index above 800. The racial gap in median index was 145 points. At Ohio State in 2006-07, 72% of enrolled Blacks had an index below 680, while 96% of enrolled whites had an index above 700; the racial gap in median index was 128 points. At the University of Houston, 88% of Blacks in the entering classes of 2006-07 had an index below 600; 80% of whites had an index above 616; the racial gap in median index points was 101. Calculations by the author of publicly available law school admissions data from Project Seaphe.

7. Some evidence that Blacks are generally unaware that they are receiving large racial preferences comes from the BPS itself, which asked entering law students many questions about their law school expectations. One question asked students to "rate how you expect to compare with your first-year classmates at the law school you are attending [on] academic ability." About 30% of Blacks thought they were in the "top 10%" of their classmates in academic ability, comparable to the 27% of whites who thought they were in the "top 10%." Authors' calculations from original BPS data, field "esq65b." With the rise of social media and websites such as "Law School Numbers," it is of course possible that students now have a better sense of their comparative credentials.

8. A total of 163 law schools agreed to participate in the BPS, but data from only 155 schools are included in the results, so some law schools presumably ran into logistical difficulties in the course of the study. *See* Linda Wightman, *User's Guide: LSAC National Longitudinal Data File, LSAC* 2, 16 (1999).

average, they entered law school with lower academic credentials. Since LSAT and UGPA were highly correlated with bar outcomes, groups of students with lower credentials would be expected to pass bar exams at lower rates. We will refer to this as the "credential" hypothesis.

We now know that the BPS leaders also considered the mismatch theory. In the confidential letters it sent to state bars in 1989, seeking their cooperation in the project, the BPS described the mismatch hypothesis and explained that the BPS would enable this issue to be studied:

> Do students with comparable credentials when they enter law school perform differently on the bar examination as a consequence of the relative abilities of others in their class at the law school they chose to attend? For example, does a student who chooses to attend a law school where he or she ranked near the bottom of the class perform differently on the bar examination than a student matched on entering credentials who attended a school with a less able entering class?[9]

Former LSAC officials, requesting anonymity, have told us that when some law-school administrators objected to "putting affirmative action on trial," the mismatch hypothesis was quietly dropped. Even though it was one of the putative bases on which state bars, supreme courts, and law schools were persuaded to join the Bar Passage Study, LSAC never undertook to actually examine mismatch. But worse was to come. Many deans and other law professors complained that the BPS data, once assembled, would provide documentation of the degree to which each law school used racial preferences, so that they might be taken to court literally, not just figuratively. The LSAC therefore took steps to cripple the data, even in versions available only to scholars. LSAC removed any means of directly identifying individual schools, and instead "clustered" schools into six tiers. The state where a student took the bar exam was removed too, replaced by a variable indicating one of twelve national "regions" where each student took the bar. And although LSAC did standardize law school grades with each school (which also made schools more anonymous), it did not standardize LSAT scores and UGPAs within schools, so that one could not compare the entering credentials of students against their peers. [10] Thus sanitized, as it were, at its inception, the BPS was unable to provide direct answers to many of the most interesting questions it had been intended to study. This vast effort, with direct costs of over $5 million and many more millions in time and effort by participating students, schools, and bars, was engineered to be a second-rate research tool.[11]

9. Linda Wightman, *LSAC Bar Passage Study: Study Design*, LSAC 9 (March 1991) (noted on page 154 in the materials produced by the State Bar of California on October 5, 2009, in *Sander et al v. State Bar*).

10. Wightman, *supra* note 9, at 15.

11. The informal word is that LSAC took these steps partly to provide further protection for student anonymity and partly because some law schools feared that if the makeup of their enrolled students could be analyzed, the size of the racial preferences used by the schools could be inferred – causing the schools to risk becoming litigation targets.

III. What the BPS Showed on the "Three Questions"

Despite its limitations, the BPS did contain a wealth of information on student experiences, attitudes, academic background, performance, and outcomes. By working around its limitations, one could learn much about the questions that provided the original motivation of the study.

First, the BPS provided reasonably strong evidence that bar exams were not racially discriminatory, in the sense that they did not discernibly penalize (for example) Black students relative to objectively similar white students. In other words, if one predicts bar passage outcomes with the BPS, and controls for the LSAT scores, UGPA, and law school grades of bar-takers, there is no statistically significant difference in the bar passage outcomes of Blacks and whites.[12] This corroborates the research of other scholars, such as Stephen Klein, who have examined the question with more detailed data within a single jurisdiction and found that, with proper controls, "race" does not predict bar passage.[13]

Second, analysis of the BPS data confirms that differences in entering credentials do explain part of the racial gap in bar passage. LSAT scores are strong predictors of bar passage (the correlation of LSAT scores and raw bar scores is well over .5), and UGPA is also a statistically significant, if weaker, predictor of bar scores. Since Black students entering law school at the time of the BPS had LSAT scores that were about one standard deviation lower than non-Hispanic whites, and substantially lower UGPAs as well, then it makes sense that Black bar-passage rates would be lower as well. However – and this is a key point – these lower scores and grades were found by Williams and by Rothstein and Yoon to explain only about half of the bar-passage gap.[14] And neither Williams nor Rothstein and Yoon could easily control, in the same analysis, for individual levels of student mismatch. Since, as we shall see, the "credential gap" and "mismatch" effects are correlated, these studies may have overestimated the pure "credential" effect.

As we noted in the introduction, Sander used the BPS to explore both of these issues along with the third hypothesis: whether mismatch contributed to the bar passage gap. Since the BPS made it impossible to measure the degree of credential "mismatch" of individual students, Sander used Black law students as

12. One might suggest that such an analysis is itself biased, because of large racial disparities in LSAT scores. But this argument is fallacious. To the extent LSAT scores show any racial effect upon law school grades, it is that (in some analyses) they slightly *overpredict* nonwhite grades. This would imply a (very slight) bias in the tests *in favor* of nonwhites, not a bias against.

13. Klein established the absence of a "racial" effect in bar outcomes as early as 1979, in a RAND report presented to the National Conference of Bar Examiners, "An Analysis of the Relationships Between Bar Examination Scores and an Applicant's Law School, Admissions Test Scores, Grades, Sex, and Racial/Ethnic Group." *See* http://www.seaphe.org/pdf/past-bar-research/An_Analysis_of_the_Relationships_Between_Bar_Exam_Scores.pdf.

14. For example, Williams, *supra* note 1 at 181, found that "about one-third to one-half of the race gap [in various law school and bar outcomes] cannot be explained by race differences in entering academic credentials."

an imprecise, collective proxy for law school mismatch. The BPS and another contemporaneous study – the National Survey of Law Student Performance – established the following: (1) Black students entered most law schools with far lower credentials than most of their classmates; (2) controlling for credentials, Blacks received roughly the same grades as whites at the same school with similar credentials, but (3) given that a dramatically higher proportion of Blacks than whites received large admissions preferences, their grades at any given school were correspondingly much lower than those of their classmates as a whole;[15] (4) controlling for credentials and law school grades, Blacks passed the bar at the same rate as whites; but (5) controlling for LSAT and UGPA (that is, entering law school credentials) only, Black bar-passage rates were substantially below white rates (because law-school grades better predict bar passage than admissions metrics). These five findings are easily explained by the mismatch hypothesis: students (in general, of whatever race) entering law school with a large preference will tend to earn much lower grades than they would at a school granting them no preference (or a much smaller preference). If the lower grades are so low as to signify much less learning (at least, bar-exam-relevant learning), then bar performance will suffer as a result. Sander's data suggested that "mismatch" could fully explain half of the Black-white bar passage gap; combined with the "credential" effect, the full Black-white gap could be accounted for.[16]

The many critiques of Sander's argument that emerged over the years following publication of Systemic Analysis rarely disputed that Sander's descriptive findings were correct.[17] And no critic attempted to explain how, if those five findings were true, mismatch could *not* occur. Nor did they offer testable alternate theories of the racial bar passage gap. Instead, the critiques usually devised some different test of mismatch and argued that the alternative test's results contradicted the predictions of the mismatch hypothesis. However, these alternative tests were not generally published in peer reviewed journals,[18] in part,

15. About half of Blacks in the BPS had GPAs in the bottom decile of their school cohort.

16. Richard H. Sander, *A Systemic Analysis of Affirmative Action in American Law School*s, 57 Stan. L. Rev. 367 (2004).

17. *See*, for example, Ian Ayres & Richard Brooks, *Does Affirmative Action Reduce the Number of Black Lawyers?*, 57 Stan. L. Rev. 1807 (2005), (replicating the tables in *Systemic Analysis)*. The only descriptive conclusion Ayres and Brooks (or other critics) disagreed with was Sander's claim that Black students perform as well in law school as do white students with similar grades. Ayres and Brooks argued that Black students performed slightly worse. Sander showed that with a better measure of student credentials, Black student performance exactly matched white student performance, but in any case, small differences would not have much effect on the key mismatch argument. Richard H. Sander, *A Reply to Critics*, 57 Stan. L. Rev. 1963, 1967-79 (2005).

18. The one mismatch critique that was published in a peer-reviewed journal (though one in statistics, not social science) was Alice Xiang & Donald B. Rubin, *Assessing the Potential Impact of a Nationwide Class-Based Affirmative Action System*, 30 Stat. Sci. 297 (2015). Xiang and Rubin use BPS data to simulate the effects of using class-based, rather than race-based, preferences, and find that attrition rates for Black students do not decline, which they take as evidence against the mismatch hypothesis. One central problem with their approach is that the BPS data on socioeconomic status was entirely self-reported and poorly specified, so that it produces

we suspect, because they consistently suffered from one of two flaws. One flaw was to treat the law school "clusters" (used by the BPS to group schools) as rigidly hierarchical tiers, which they were not.[19] The other, surprisingly common flaw, was for critics to make outright and serious errors in their analyses, which, when corrected, either rendered their results inconclusive or, in several cases, turned them into strongly supportive findings for the mismatch hypothesis.[20]

In any case, all of these BPS studies, including Sander's, suffered from a substantial disadvantage: none of them measured "mismatch" directly. All these studies relied on contestable inferences and, by using indirect measures, as our introductory quote from Arcidiacono and Lovenheim suggests, tended to bias results towards finding a smaller mismatch effect than actually existed.

IV. Our Data

The innovation in this study is quite simple: we obtained data from three law schools – UCLA, UC Davis, and the University of Arkansas, Little Rock -- on the credentials of each student at the school who sat for the in-state bar exam over multiple years. All told, our data covers nearly four thousand such students. This makes it possible to do something that could not be done with the released BPS database: construct a direct measure of "mismatch" for each individual student, and evaluate whether a student's level of mismatch helps to predict whether she passed or failed the bar on the first attempt. The Addendum at the end of this article explains how we obtained this data and how interested researchers can obtain a copy of the data.

Each of the three schools provided a slightly different set of information, as Table 1 details, and this complicated some aspects of our analysis. But we think we obtained the key fundamentals necessary to estimate individual mismatch levels and evaluate their impact upon bar passage outcomes. First, we have

SES descriptions inconsistent with other, more precise data. Xiang and Rubin also relied heavily on the assumption that the BPS clusters were rigidly hierarchical (*see infra* note 20 and accompanying text). Moreover, like the other BPS-based research described here, including Sander's original work, the authors had no direct measure of student mismatch.

19. For example, a critique by Daniel Ho arranged the six BPS clusters in a hierarchy and showed that Black students in "adjacent" clusters, matched by incoming credentials, had similar bar outcomes. But since "adjacent" clusters in fact included many pairs of law schools that had similar average credentials and used similar preferences, Ho's results were not informative. Richard H. Sander, *Replication of Mismatch Research: Ayres, Brooks and Ho*, 58 Int'l Rev. Law & Econ. 75 (2019).

20. *See, e.g.,* Doug Williams et al., *Revisiting Law School Mismatch: A Comment on Barnes (2007, 2011)*, 105 Nw. L. Rev. 813 (2011). Katherine Barnes developed a model to test the mismatch hypothesis, and her initial research purported to strongly contradict the mismatch hypothesis. But when Williams et al. could not replicate her results, she produced a revised model that, most observers would agree, produced results highly consistent with the predictions of the mismatch hypothesis. Similarly, Ian Ayres and Richard Brooks developed a "second-choice" model which, they claimed, undercut the mismatch hypothesis, but when Sander corrected obvious errors in this model, the results were highly consistent with the mismatch hypothesis. Ayres & Brooks, *supra* note 19; Sander, *supra* note 21.

LSAT scores from all three schools, and both LSAT and UGPA for 80% of the students. Second, we know enough about the universe of fellow students to measure each student's "credential distance" from her classmates. Third, at each of these schools the vast majority of graduates take the in-state bar exam, and we know the pass/fail outcome of those exams. For nearly all students, we know a specific graduation year, so that we can compare students with their actual classmates.[21] Fourth, for the two more elite schools in our sample, we are able to exclude incoming transfer students, who can otherwise confound a mismatch analysis for elite schools.[22]

On two important issues (utilizing undergraduate grades in scaling credentials and measuring mismatch; and limiting the analysis to students taking the same bar), we were able to use subsets of two schools to extend our analyses.

Table 1: Characteristics of the Three Law School Datasets

Data characteristic	UCLA	Davis	UA Little Rock
LSAT scores	Yes	Yes	Yes
UGPA	No	Yes	Yes
Index	No	Calculated	Calculated
Ethnicity	Yes	Yes	Yes
Law school grades	No	Yes	Yes
Incoming Transfers excluded?	Yes	Yes	No
In-state bar results?	Yes	Yes	Yes
Graduating years covered	2000, 2001, 2005	1997-2011	2005-2011
Number of distinguishable cohorts	3	5	1

21. This matters mainly for Davis, because Davis provided data on fifteen classes and admissions became more competitive over time.

22. Many elite schools admit into their second-year classes students whose credentials were not strong enough to win admission as 1Ls, but who then went to less elite schools and compiled stellar GPAs. Since these students have attended two law schools with very different levels of eliteness, we cannot validly measure their level of mismatch. For UA Little Rock, we could not identify incoming transfers, but UA Little Rock had very few such transfers, and there is no reason to think those transfers that did exist came from generally more elite or less elite law schools.

All entering students included?	No; only eventual in-state bar-takers	Yes	Yes
Observations	752	3,290	899
Observations of in-state bar-takers	752	2,333	723

A final, crucial strength of these data for purposes of studying mismatch is that there is substantial variation in the median credentials of students across the nine available cohorts,[23] but the individual credentials of students overlap substantially (*see, e.g.*, Table 2). We can thus estimate how students at one of the schools might have performed had they attended one of the other schools, and vice versa.

V. Initial Explorations with the Data

Table 2 shows, for six of the cohorts at our three schools, a simple cross-tabulation of first-time bar passage by LSAT score.[24] The data show some clear regularities that we will examine more robustly below. First, if we examine the schools one at a time, we can see a strong relationship between LSAT scores and the probability of first-time bar passage. This is consistent with the "credential" effect we have discussed; usually higher LSAT corresponds to a higher group rate of first-time bar passage. Second, if we examine any of the first six rows of data, there is something that looks very much like a "mismatch" effect – that is, in the lower LSAT ranges, pass rates go up as one moves from UCLA to Davis to UA Little Rock; but at the higher LSAT ranges, this effect disappears. Intuitively, it looks as though students have lower passing rates when their LSAT scores are significantly below those of their classmates.

23. In the UCLA data, we could distinguish individual classes (three total), and at Davis we could distinguish five three-year cohorts. Since the median credentials of classes and cohorts varied somewhat within both schools, we always measured "mismatch" by the most precise cohort we could use. Thus, we used nine cohorts in total.

24. Three of Davis's cohorts had a median LSAT close to 162, as did one of UCLA's cohorts; since we would not expect these cohorts to provide any "contrast" with one another, we excluded them from this table. All nine cohorts, as described in Table 1, are included in the regressions that follow.

Table 2: First-Time Bar Passage Rates for Graduates Attempting the In-State Bar Exam

LSAT Range	UCLA		Davis		UA Little Rock	
	Attempts	Passing %	Attempts	Passing %	Attempts	Passing %
143 or lower	n/a		1	0%	24	37%
144-46	n/a		8	25%	51	51%
147-49	n/a		16	44%	120	75%
150-52	9	22%	37	51%	149	79%
153-55	18	39%	76	71%	165	79%
156-58	27	67%	179	79%	99	86%
159-161	60	88%	305	85%	68	87%
162-64	193	92%	175	86%	29	97%
165-67	198	98%	80	95%	15	94%
168 or higher	126	97%	45	84%	4	100%
Median LSAT[25]	164		160		152	
Total pass rate	89%		81%		78%	
Cohorts:	2001, 2002, 2005		1997-99, 2000-02		2005-2011	

Thus, for example, consider students at the three schools who had an LSAT score between 150 and 152. At UCLA, such students entered law school with scores twelve to fourteen points below the class average, a very large gap. And they passed the bar exam at a very low rate of 22% (two out of nine). At Davis, students with LSAT scores between 150 and 152 entered law school with scores about eight to ten points lower than their median classmate: still significantly mismatched, though less extremely so than at UCLA. These students collectively had a first-time bar passage rate of 51%. At UA Little Rock, students with LSAT scores between 150 and 152 are basically at the school's median. They are not mismatched at all. And their bar passage rate is 79% – that is, about the same as the school-wide first-time rate.

Table 2 repays careful consideration. All by itself, it makes a very powerful case that the mismatch effect not only exists, but is quite large. How else can we explain the enormous differences in first-time bar passage that occur across the "150-52", "153-55", and "156-58" rows? They cannot be readily explained by other "unobserved credentials" of these students, because on any given

25. For the cohorts included.

unobserved measure (UGPA, for example), the students at UCLA are stronger than those of Davis, who are in turn stronger than those of UA Little Rock.[26] This can be verified through many alternative data sources, and it also makes complete sense, since students at a very selective school who are weak on one measure likely secured their admissions spot because they are stronger on other relevant measures.

It is sometimes argued that students at a lower-tier law school will do better on bar exams not because they are generally learning more, but because such schools "teach" to the bar exam, sacrificing other important curricular matters. Alternatively, it is sometimes suggested that students at higher-ranked schools hurt their bar performance through overconfidence. Yet in all the literature on mismatch and bar passage, no one has found that school eliteness, *per se*, harms bar performance. On the contrary, in the BPS, both overall and when we control for LSAT and UGPA, students in **more** elite school tiers have **higher** bar passage rates. It is only when mismatch comes into play that the more elite students do worse, and then, it appears, they may do *much* worse. And, as we shall see below, students at our most elite school (controlling for their credentials and for mismatch) perform best on the bar. This effect might be because the BPS and our data sources don't adequately control for other qualities that students at elite law schools have which enhance their bar performance (*e.g.*, better writing ability). But there is no evidence that, aside from the mismatch effect, lower-tier law schools give their students some secret edge on the bar.

Yet another response to Table 2 might be that UA Little Rock is doing a particularly good job of helping its weaker (i.e., lower LSAT) students to do well on the bar, perhaps through some form of academic support. But this does not bear scrutiny at either a descriptive or analytic level. During the period covered by this data, it was UCLA, not UA Little Rock, which had an acclaimed academic support program.[27] And UA Little Rock has the same drop-off in student performance as UCLA, when we examine students with LSAT scores ten points below the school median (the "153-55" range at UCLA, and the "below 143" range at UA Little Rock). Those students have a 39% pass rate at UCLA, and a 37% pass rate at UA Little Rock. This accords with the basic idea behind the mismatch hypothesis: at all schools, students entering with larger and larger credential deficits tend to learn less and less, producing sharp drop-offs in bar passage rates for students who are far below the median credentials of their classmates *at any school*.

VI. Regression Analyses

Regression analysis provides a more rigorous test of mismatch. In a regression, we can control for (*i.e.*, hold constant) other factors that vary across students

26. We illustrate this *infra*, Table 5 and accompanying text.

27. A detailed description and analysis of the effects of UCLA's academic support program can be found in Kris S. Knaplund & Richard H. Sander, *The Art and Science of Academic Support*, 45 J. Legal Educ. 157 (1995).

and schools, to better isolate what is driven by "mismatch" *per se*. For example, the racial composition of students with LSATs of 150 to 155 varies considerably across our three schools. If it were the case that most of these students at UCLA were Black, while most of these students at Davis were white, and if race had some powerful independent effect on bar passage, then "controlling" for race in a regression would separate out the "race" effect from the "mismatch" effect.

A particularly important control for our regression is LSAT itself. As we have explained, we (and pretty much all the scholars in the field) recognize that credentials (especially the LSAT) are correlated with bar performance,[28] and the research suggests that the "credential effect" (driven particularly by the LSAT) may explain as much as half of the Black-white bar passage gap.

But note that we use LSAT scores **both** to measure student credentials and to measure a student's degree of mismatch. That means that the "credential" variable and the "mismatch" variable will be correlated – perhaps highly correlated. How do we separate out these two effects from one another?

In this first set of regressions, we measure "mismatch" as the LSAT "deficit" between a student's LSAT and the median LSAT of her classmates. As Table 1 showed, we have data on a total of nine student cohorts (three at UCLA, five at Davis, and one at UA Little Rock). If a student's LSAT is 152, and the median LSAT of her cohort is 160, then the student has a mismatch deficit of -8 LSAT points. If a student's LSAT is 164, and the median LSAT of her cohort is 160, then the student is not mismatched, and has a mismatch value of zero.[29] Our "mismatch" measure is thus distinct from the "credential" measure in two ways: it varies across all nine of our cohorts, depending on the median LSAT of each cohort, and it is "zero" for more than half of the students (those at or above each cohort median).

Why do we measure mismatch relative to the median credentials of the student's classmates, rather than relative to the top students? Because under the theory we sketched in our opening paragraph, mismatch arises from the tendency of teachers to aim instruction at the "middle" of their classes. Most teachers instinctively will slow down if most students are not following them, and will move on when most students have caught on. In our experience, students are

28. Stephen Klein finds a correlation of over .9 between a school's mean LSAT score and its bar exam pass rate, though the individual correlation is only moderate, as our models suggest. Stephen P. Klein, *Law School Admissions, LSATs, and the Bar*, ACAD. QUESTIONS 33, 36-37 (Winter 2001–02); *see also* Robert Steinbuch & Kim Love, *Color-Blind-Spot: The Intersection of Freedom of Information Law and Affirmative Action in Law School Admissions*, 20 TEX. REV. L. & POL. 181 (2016).

29. Note that we do not posit any mismatch effect for students with credentials far *above* those of their median classmate. Neither Sander nor Williams posited one, either. It seems plausible to us that such students could also be hurt by mismatch, because a student with a large positive mismatch may not be as academically challenged or motivated as a similar student attending a school where median credentials matched her own. One could explore such an effect with data on actual bar scores, but with only bar passage data (as opposed to bar scores that could let one observe the margin by which a student passed a bar exam), there is not enough variation for a meaningful test. At most law schools, nearly everyone in the top half of the class (measured by GPA) passes the bar.

more reluctant to ask questions if they feel that most of the class is ready to move on. The median students, we suggest, are in an optimal learning environment, and students well below the median will have the most difficulty keeping up.[30]

To make it easier to compare results across our regressions, we standardized LSAT scores to a 0-to-100 scale (a "120" becomes a zero, and a "180" becomes 100), and used those standardized scores to measure mismatch.

Many readers are probably only modestly familiar, and a little uncomfortable, with reading and interpreting regression results. And logistic regressions – the type we use in this article – take some getting used to. We accordingly will err on the side of discussing our results in some detail and explaining the intuitions behind our conclusions.

Since the outcome we are predicting – whether a student passes a bar exam – is an "on or off" outcome ("pass" or "fail") and not a "continuous" outcome with many possible values (like the raw score on a bar exam), we use logistic regressions in our analysis. Each of our models is an equation predicting which students will pass or fail. The "Somers' D" reported at the bottom of each model is an estimate of the explanatory power of the model; roughly speaking, it is describing how much of the guesswork in predicting a typical student's bar outcome is eliminated by observing the variables included in that model. For most of the models in this paper, the Somers' D is between .3 and .4. These are, in our view, a good indication that our models are powerful predictors of bar passage – which is what we would expect, given the marked patterns observable in Table 2. But readers should keep in mind that beneath the orderliness of the aggregate patterns, there is a good deal of variation and unpredictability in individual results. LSAT and objective "mismatch" variables do not dictate individual destinies; they merely show very predictable patterns over large groups of individuals.

For each independent variable in each regression, we report a coefficient. In a logistic regression, each coefficient represents how changes in the independent variable affect the "odds" of the dependent variable (bar passage) being positive (passing) or negative (failing). A coefficient of "1" means that, in the given equation, a one-unit change in the independent variable has a neutral effect – it makes a positive outcome neither more nor less likely. A coefficient below "1" implies a negative effect, and a coefficient greater than "1" implies a

30. This idea is developed in Frederick L. Smyth & John J. McArdle, *Ethnic and Gender Differences in Science Graduation at Selective Colleges with Implications for Admission Policy and College Choice*, 45 Rsch. in Higher Educ. 353 (2004). Their key variable measures student credential distance from the median student. *See also* Esther Duflo et al., *Peer Effects, Teacher Incentives, and the Impact of Tracking: Evidence from a Randomized Evaluation in Kenya*, 101 Amer. Econ. Rev. 1739 (2011), which examined the effect of different assumptions and incentives for where teachers aim their instruction, and found strong experimental evidence for mismatch whether teachers aimed at the middle of the class or the top. Note that if distance from the "median" student leads to mismatch, then students attending schools where their credentials are much higher than the median may be adversely affected as well. If we had bar scores, we could test this idea, but we only have bar passage rates, and bar passage rates are so high for students with high LSAT scores that we could not, even in principle, capture "high-end" mismatch effects.

positive effect. For each coefficient, we also report whether the reported effect is statistically significant – in other words, whether it is quite unlikely that the effect is simply a consequence of random variation or "noise" in the model.

With this background, let us turn to Table 3, which reports a series of models for all three schools, using LSAT scores in the manner we have described to measure both student credentials and mismatch.

Table 3: Logistic Regression Models of First-Time Bar Passage, 9 Cohorts at 3 Law Schools

Independent variables	Model 1: Race alone	Model 2: Add LSAT	Model 3: Add Mismatch Deficit	Model 4: Add School Fixed Effects	Model 5: Categorical Mismatch	Model 6: Categorical Mismatch With School Fixed Effects
Black	.28***	.56**	.71*	.71*	.71*	.72
Hispanic	.41***	.53***	.76*	.79	.77*	.81
LSAT		1.10***	1.05***	1.03*	1.05***	1.04*
Mismatch Deficit			1.13***	1.16***		
Mm Lev 1					.94	.90
Mm Lev 2					.78	.74
Mm Lev 3					.55***	.51***
Mm Lev 4					.36***	.30***
Mm Lev 5					.42***	.37***
Mm Lev 6					.29***	.22***
Mm Lev 7					.15***	.11***
UA Little Rock				.48**		.51**
Davis				.45***		.44***
Constant	5.05***	.000***	.003**	.060	.002***	.013
Observations	3,656	3,656	3,656	3,656	3,656	3,656597
Somers' D	.13	.35	.36	.39	.37	.39

Significance levels are: *p<.1; **p<.05; ***p<.005 (two-sided)

The first two models help us "calibrate" our results by showing patterns that are already well known. Model 1 shows that Blacks and Hispanics were substantially less likely to pass (and more likely to fail) the bar than the omitted groups (mainly non-Hispanic whites and Asian-Americans).[31] In other words, if all we know about a student in these cohorts is her race, then Blacks and Hispanics are much less likely to pass the bar than other students. The low Somers' D for Model 1 is telling us that this model is explaining very little of the vast variation of bar results. That's what we expect, since of course bar success varies greatly within every racial group.

Model 2 adds the "credential" variable, measured by student LSAT scores. In this model, LSAT has a logistic coefficient of 1.10 – a fairly large value, since this means that each **one-unit** increase in the LSAT (on our normalized 100-point scale) increases the odds of passing by a factor of 1.1. The effect is also highly statistically significant. Adding a credential to the model makes the "Black" and "Hispanic" coefficients go up (*i.e.*, rise a little closer to "1"), which means the direct effect of race goes down when we control for LSAT. This is what we expect, because as we noted earlier, a good deal of the racial deficit in bar passage is accounted for by the lower average credentials of Black and Hispanic students.

In Model 3 we add our measure of mismatch. Recall that mismatch as we have defined it can only take on negative values. Here, it is measured as the distance between a student's LSAT score and her school median; if she has an LSAT of 154 and the school median is 162, her mismatch level is "-8." The mismatch effect (we hypothesize) should diminish as the degree of mismatch approaches zero (i.e., as it rises toward zero). In other words, the mismatch hypothesis predicts that the coefficient of mismatch will be greater than "1" in a logistic regression.

The results are striking. The coefficient on mismatch, "1.13," is highly statistically significant, and suggests that a one-point drop in mismatch (e.g., attending a school with a median LSAT one point closer to the student's) is associated with improving a student's odds of passing the bar by a factor of 1.13. Moreover, when we control for mismatch, the direct effect of race shrinks – the coefficients on "Black" and "Hispanic" get closer to zero and become less statistically significant. [32]

Note that in Model 3, the coefficient on mismatch is substantially larger than the coefficient on LSAT itself. It is tempting to infer that the mismatch effect "dominates" the credential effect – in other words, that mismatch is much more important than the direct credential effect of LSAT – but that would be overhasty

31. Across the three schools, somewhat more than a third of Blacks and Hispanics fail their first attempt on the in-state bar exam, compared to a sixth for all others.

32. In all of our models using mismatch, the effects measured by the regression are, of course, influenced by each individual student in the dataset. Some students with below-median credentials will, we hypothesize, thrive on the challenge of being surrounded by higher-credentialed students, and will perform very well. Others will be overwhelmed and perform terribly. The mismatch variable will not predict bar passage perfectly precisely because individual outcomes will be varied and "noisy."

for, as we have explained, the two variables are substantially correlated in this data, and the LSAT is being measured on a broader scale, with a broader range of possible values, than is mismatch. A conservative interpretation is that both the mismatch and credential effects are significant and important.

In Model 4, we add "school fixed effects," which means that we control for which of the three schools a student attended. Each school provides a unique learning environment, and in addition UA Little Rock students are, of course, taking a different bar exam than UCLA and Davis students. By adding a control for each school, we can see what effect these school-wide effects have on bar passage, and on the other variables.

The coefficients for Davis and UA Little Rock measure how those schools influence bar passage relative to the "omitted" school, UCLA. Both coefficients are well below "1" (.45 and .48, respectively) and highly statistically significant. This means that, controlling for LSAT, mismatch, and race, graduates of UCLA have significantly higher pass rates than Davis and UA Little Rock. This is important, because it helps address a couple of sources of skepticism in interpreting Table 2. Recall that in that simple cross-tabulation, students with relatively low LSAT scores did much, much better on the bar at UA Little Rock compared to Davis, and Davis compared to UCLA. One explanation we discussed was that lower-ranked schools might devote more time and effort to "teaching" the bar exam. Another possible (partial) explanation is that the Arkansas bar exam is somewhat easier than California's. But the school coefficients in Table 3 provide no support for these explanations. With the admittedly limited controls in our model, UCLA students outperform otherwise comparable students at the other schools. That implies that mismatch, not differences in the schools, is really driving the poor performance of low-LSAT students at UCLA. And indeed, the coefficient on "mismatch" is even higher and more statistically significant in Model 4 than in Model 3, and the coefficient on LSAT is lower and less statistically significant. In Model 4, "mismatch" apparently does dominate "absolute credentials" in accounting for bar passage.

Model 5 moves our exploration in another direction. What if the "mismatch" effect is non-linear? Suppose, for example, that a 6-point LSAT deficit pushes one's probability of passing the bar down by 10 percentage points, but a 12-point LSAT deficit pushes it down not twice as much (a linear effect) but three times as much (that is, 30 percentage points). One way to avoid the assumption of linearity is to use a categorical variable for mismatch – *i.e.*, to break the size of the LSAT deficit into a series of small categories and treat each of those as an independent variable. In other words, we separately test the effect of having an LSAT just slightly (*e.g.*, 1 or 2 points) below the median, of having an LSAT a little further (*e.g.*, 3 or 4 points) from the median, and so on. In Model 5, we break "mismatched" students into a total of seven categories, with "MM1" comprising students only slightly below the median, and "MM7" comprising those students furthest from the median.

The results from Model 5 are interesting, but not a dramatic change from Model 3. The seven levels of increasing mismatch are associated with steadily lower chances of passing the bar, dropping quite dramatically at MM6 and MM7. All levels of mismatch above "MM3" are highly statistically significant. Note that this doesn't mean each coefficient is significantly different from its adjacent neighbors, but that it is significantly different from students who are at or above the school's median LSAT (and who therefore have a "0" value for mismatch). The bump up in odds from "MM3" to "MM4" is probably random noise rather than a real difference, though we can't be sure.

Model 6 adds school fixed effects back into the regression, and doing so has similar effects (relative to Model 5) that adding them into Model 4 had relative to Model 3. The "Davis" and "UA Little Rock" effects are substantial, significant, and below "1," meaning that other things being equal, UCLA graduates have higher success on the bar. As in Model 4, adding school controls makes the mismatch coefficients consistently lower in Model 6 – *i.e.*, the mismatch effect is more severe – while slightly weakening the LSAT (credential) effect.

Notably, in Model 6, neither of the "race" variables are statistically significant. As our models have gradually become complex (moving from Models 1 to Model 6), the independent effect of "race" has become steadily less important. This is exactly what we expect, consistent with our earlier work showing that "race" *per se* is not an important part of the explanation of weak minority performance on bar exams. Race becomes important only because schools focus so heavily on race in awarding preferences; when we can effectively measure and control for the individual level of preference, the race effect largely or entirely disappears[33] -- though the real demonstration of this will come in the next section, when we control for UGPA as well as LSAT.

Despite our inclusion of school fixed-effects in Models 4 and 6, one may still wonder how the inclusion of results from two different bar jurisdictions affects our results. In the appendix (Table A), we replicate the analyses in Table 3, but include only the two California schools (UCLA and Davis). In doing so, we lose many observations and also lose much of the variability in mismatch across schools that is obviously important to our analysis. Nonetheless, the results in Appendix Table A parallel in all important respects the results in Table 3. In other words, the results we have described thus far are robust to a smaller, within-state analysis.

VII. Improving the Credential Measure

A significant limitation of the analyses in Table 3 is the reliance on LSAT as our sole measure of credentials and mismatch. We were thus limited because UCLA's data does not include information on the undergraduate GPA of students. However, we do have UGPA for Davis and UA Little Rock. In this section, we will use that data to examine how it affects our results, and then discuss conceptually why the results turn out the way they do.

33. We varied our analysis in other ways as well, such as modeling LSAT as a polynomial, modeling mismatch as a polynomial, and introducing race-mismatch interaction effects. These variations did not produce insights or results that depart in interesting ways from those shown in Table 3.

As we noted in Part II, a common way to combine information on LSAT and UGPA is by creating an "academic index" that weighs the two credentials in a way that roughly maximizes their joint ability to predict performance in law school. We used the data from Davis and UA Little Rock to create such an index, and we scaled it from 0 to 100 so that it would be comparable to the scale used for our LSAT-only measures in Table 3. We also recalculated each student's mismatch level, examining each of our six remaining cohorts separately and determining whether, and how far, each student's index put her below the median index of her cohort.

In Table 4, we revisit two of the models from Table 3, and examine what happens when we use "Index" (the combination of LSAT and UGPA) in place of LSAT alone. Model 7, below, is identical to Model 3, but includes only the students from Davis and UA Little Rock. The coefficients change somewhat, but the general pattern is the same: both absolute credentials (as measured by LSAT) and mismatch are statistically significant, though mismatch seems to play a larger role than the index. Both the Black and Hispanic coefficients are well below "1," though only the Black coefficient is statistically significant.

Table 4: Revisiting Logistic Regression Models 3 and 6, from Table 3 Including Only Davis and UA Little Rock and Using, Alternately, LSAT and Index as Academic Measures

Independent variables	Model 7: Using LSAT	Model 8: Using index	Model 9: Using LSAT, categorical mismatch, school Fixed Effects	Model 10: Using index, categorical mismatch, school Fixed Effects
Black	.64**	1.11	.63**	1.02
Hispanic	.77	.96	.79	.93
LSAT/Index	1.03**	1.02**	1.04*	1.05**
Mismatch Deficit	1.15***	1.22***		
Mm Lev 1			.90	.85
Mm Lev 2			.77**	.54***
Mm Lev 3			.51**	.41***
Mm Lev 4			.33***	.38***
Mm Lev 5			.39**	.20***
Mm Lev 6			.24***	.29***
Mm Lev 7			.14***	.10***
Davis			.87	.73

Constant	.06	.14	.008	.002
Observations	3,005	3,005	3,005	3.005
Somers' D	.36	.39	.38	.39

Significance levels are: *p<.1; **p<.05; ***p<.005 (two-sided)

Note: Forty-four students did not have UGPA data; we omitted these from all four models.

Model 8 is identical to Model 7, and uses the same universe of students, except that Model 8 replaces LSAT with the "Index," which weighs the LSAT and UGPA together. This combination does a slightly better job of predicting individual bar outcomes, as the higher Somers' D indicates. Two other differences between Model 7 and Model 8 are larger, and more important. First, the mismatch coefficient goes up and increases in statistical significance, while the LSAT coefficient becomes smaller.[34] This provides the strongest evidence we have seen thus far that student's relative-credential position in their class is even more important than the level of absolute credentials in determining bar passage. Second, the race effect essentially disappears; as we noted earlier, this is what we expect to happen once we are more fully measuring individual variations in academic preparation, by including both LSAT and UGPA. Model 8 (and 10) provide strong evidence that, in these three schools, race itself does not influence bar passage.

Models 9 and 10 replace the "continuous" measure of mismatch with a series of categorical measures, just as Models 5 and 6 did in Table 3. Both models include "fixed effects" for the schools; Model 9 measures credentials just with LSAT and Model 10 instead uses the superior credentials measure of "index." Model 10, which has the most refined measure of mismatch of all our models, also shows the most powerful mismatch effects.

Let us consider in more detail why "race" becomes insignificant in Models 8 and 10, and why "index" is a better control than "LSAT." First, consider the disappearance of the "race" effect. As we noted in Part II, a number of earlier studies, including "Systemic Analysis," have found that Blacks and Hispanics do as well as whites when one controls for incoming credentials and, crucially, *law school grades*. When one controls *only* for incoming credentials, Blacks and Hispanics perform worse. Why this difference exists is one of the central paradoxes "Systemic Analysis" sought to explain. Sander argued that large preferences led to mismatch, causing the recipients of preferences to underperform in law school, which meant that they received much lower grades than they would have at a school where they were well-matched. That was why, he argued, the racial difference in bar outcomes disappeared only once one controlled for grades, *i.e.*, because the mismatch effect from preferences caused students to do poorly in law school beyond what their incoming metrics would otherwise predict. Once that poor performance was captured in their law-school grades, the expected

34. The z-score on LSAT falls from 2.64 in Model 7 to 2.19 in Model 8, while the z-score on the mismatch variable rises from 6.88 to 8.81.

bar performance manifested, demonstrating that race is not a factor. But with the individual-level data in this paper, we do not need law school grades – we can directly measure and control for mismatch. Models 8 and 10 thus provide strong evidence in support of a fundamental contention of "Systemic Analysis."

Second, consider why mismatch effects go up when we control for both LSAT and UGPA, instead of just LSAT alone. As we have noted earlier in the paper, the presence of important "unobserved" credentials tends to bias any analysis of outcomes towards an underestimate of mismatch. Now we can offer a concrete demonstration of why this is the case.

We know that Davis is more selective than UA Little Rock, because Davis students have, on average, much higher credentials than UA Little Rock students. The average student at Davis (in Table 2) has a 160 LSAT, compared to an average of 152 at UA Little Rock.[35] This means that when a student with an LSAT of, say, 156 is admitted to Davis, there is a good chance that the student has an unusually high UGPA that made her attractive to the admissions committee despite her low LSAT. Conversely, when a student with an LSAT of 156 is admitted to *and enrolls* in UA Little Rock, there is a good chance that she has a below-average UGPA; otherwise she probably would have been admitted to a more elite school than UA Little Rock and chosen to attend there.[36]

Table 5 demonstrates empirically what we expected conceptually. At any given level of LSAT, the students at Davis have higher average college grades than the students at UA Little Rock. In fact, their average college grades are *much* higher. This, of course, makes the poor bar performance of these Davis students all the more striking, and the implicit mismatch effect more serious.

Table 5: Average UGPA by LSAT Score, Davis and UA Little Rock

For students with LSAT scores of…	…Average UGPAs (with # of students in parentheses) were:	
	at Davis	at UA Little Rock
146	3.69 (6)	3.29 (33)
150	3.50 (25)	3.26 (71)
154	3.54 (93)	3.21 (65)
158	3.61 (150)	3.23 (32)

To get an intuitive sense of what this implies about mismatch, examine again Table 2, with its comparisons of student bar-passage rates at particular LSAT levels. At Davis, there are 25 students in the top three rows (LSATs from 143 to 149), with an average first-time bar passage rate of 36%. The students at

35. Note that our data comes from the late 1990s and 2000s; law school admissions have since become more competitive and average student credentials have gone up at many schools.

36. Indeed, our observation is that at many schools, admissions officers have become more deliberate in pursuing these sort of "split credential" students.

UA Little Rock with these LSATs have a first-time bar passage rate of 67% – a 31-point gap. If we group students by index rather than LSAT, we obtain 26 Davis students in the lowest ranges (with index scores from 542 to 636).[37] Their first-time bar passage rate is only 26%, while that of the UA Little Rock students in the same index range is 66% – a 40-point gap. In other words, the mismatch effect is heightened as we measure credentials more accurately, and this is what our regressions show.

VIII. The Scale of the Problem

Colleagues have sometimes told us that while they are prepared to accept that the "mismatch effect" is real, they are not sure it is a problem that needs to be addressed. The bar exam, they argue, is an artificial barrier, so failing the bar has nothing to do with one's future success as an attorney. Even if large preferences cause more students to fail the bar, those students will simply take it again and probably eventually pass. Once they become attorneys, the elite school credential they earned because of the admissions preference will be far more important to their long-term career than a temporary difficulty passing the bar.

This is a seriously misguided response. To the extent the "mismatch effect" actually occurs, it directly reduces learning in law schools. This translates not only into failure on the bar for many; it also means much lower grades for the vast majority of students receiving large preferences. Students of any race attending law school without a preference will earn, on average, grades that place them in the middle of their class. Students receiving large preferences overwhelmingly end up with grades that put them in the bottom fifth of their class. Legal employers, from appellate judges to law firms, care a lot about grades, and for a reason – doing well in law school contributes to better understanding of the law and better performance as a lawyer. And law professors would be hard pressed to accept that our significant grading efforts are overwhelmingly illusory. The best evidence we have of this is that law school grades are highly predictive of which law firm associates will ultimately become partners.[38] Law firms do not consider grades in making partnership decisions, so the very strong association between grades and eventual promotion is hard to explain if law school grades are not themselves related to better performance on the job. It follows, then, that students who graduate with very low grades are permanently handicapped in most legal careers they might pursue.

37. In other words, the 542-636 range captures the lowest-ranking 25 Davis students in the cohorts analyzed in Table 2. We end up with 26, rather than 25, Davis students because two students are tied with indices of 636.

38. Richard Sander & Jane Bambauer, *The Secret of My Success: How Status, Eliteness, and School Performance Shape Legal Careers*, 9 J. Empirical Legal Stud. 893, 911 (2012). Table 9 uses data from the University of Michigan's longitudinal career surveys to show the large and close relationship between higher grades in law school and the odds of becoming a partner at the large firm one joins as an associate. The article provides a variety of other data and analyses on the relationship between law school grades and career outcomes.

Another way to grasp the large-scale harm of mismatch is to measure the aggregate loss of Blacks and (to a lesser degree) Hispanics from the ranks of lawyers. Data from the 1997 Bar Passage Study implied that the higher rates at which minorities failed to graduate, or failed to pass the bar, seriously hurt their odds of becoming lawyers. At the conclusion of the BPS, 57.8% of Blacks in the study had become lawyers, compared to 83.2% of whites.[39] The ratio of these two numbers is .694, implying that Blacks entering law school in the early 1990s were about 70% as likely as whites to become lawyers.

If this attrition is real, and if it has continued at roughly these levels since the 1990s, then its effects should show up in the overall demography of the legal profession. And it does. The ABA's most recent demographic analysis of lawyers, published in 2020, finds that only 5% of lawyers in the United States are Black.[40] Crucially, it also finds that these numbers have been essentially unchanged over the past decade. In contrast, Blacks have consistently made up about 8% of first-year law students for decades. A simple calculation suggests that over the past decade, Blacks starting law school are only about 60-65% as likely as entering whites to become and remain attorneys.

The ABA has also begun, in recent years, to report national data on bar passage rates by race. For 2021, the reported first-time bar passage rate for Blacks was 61% -- the same rate reported by the BPS in the 1990s. Fewer than half of those who fail will pass on a subsequent attempt. When we combine these high failure rates with the disproportionately high attrition rate of Blacks from law schools (most of it due to low grades), we again see evidence pointing to a loss of a third or more of entering Black law students.

We reiterate that "mismatch" is not about race, but about large admissions preferences that create big credential gaps between the students who receive preferences and their classmates. The racial effects we have been discussing occur because current admissions practices afford preferences more often to racial minorities.[41] But there are many Black and Hispanic students who attend law schools without a preference, and there are some whites and Asians who receive large preferences. If we had data that tracked long-term student outcomes and related this to initial admissions preferences, we would have a much better idea of the magnitude of attrition that preferences produce, and whether there is, for example, some modest level of preferences that does not have harmful effects.[42]

39. Data compiled by the authors from the BPS original data; both this analysis and the original data are available upon request.

40. Am. Bar Ass'n, *ABA Profile of the Legal Profession: 2020*, at 33 (2020).

41. *See*, for example, *supra* note 4.

42. We are aware of one critique of this paper that, while unpublished as of this writing, is available on SSRN. Sherod Thaxton, *When Old Habits Die Hard: A Comment on Sander and Steinbuch's "Mismatch and Bar Passage,"* (Sep. 20, 2022), https://papers.ssrn.com/sol3/papers.cfm?abstract_id=4058858. Thaxton seems to argue that the effects we observe are driven by race–i.e., racial discrimination. However, Thaxton's critique contains so many errors, and so little substantive argument, that we chose not to discuss his critiques here. We instead refer readers to our detailed response, Richard Sander, *A Note on the Thaxton*, https://ssrn.com/abstract=4607503.

Nonetheless, to us, the single most disturbing manifestation of mismatch is its effect upon the legal education and attrition of Blacks aspiring to the legal profession. Two generations of aggressive affirmative action have not succeeded in creating anything close to a proportionate representation of Blacks in the bar. Something terribly wrong is happening between law school entry and entry into the legal profession. It is plausible, indeed likely, that tens of thousands of students who take on enormous financial burdens to attend law school are never getting a chance for a good legal job. In our view, mismatch is the most plausible explanation of a substantial portion of the gap. Were the cause anything other than mismatch, would there not be outrage at the culprit and mobilized demands for investigation and action?

IX. Conclusion

Prior studies of law school mismatch and its hypothesized effect on bar-exam outcomes have uniformly relied upon a single database, the Bar Passage Study. A series of peer-reviewed articles using this data has concluded that mismatch exists and substantially contributes to the racial gap in bar passage; the early critiques have either been disarmed or found to confirm mismatch effects, when errors in analysis are corrected. The comprehensive review of the literature published in the *Journal of Economic Literature* concluded that the case for law school mismatch was "fairly compelling." But both proponents of mismatch, and many critics, agree that the BPS is a clunky dataset for analyzing the mismatch question, and that data allowing one to directly measure mismatch at the individual level would solidify the case.

This study provides that alternative analysis. It, too, is imperfect, since the dataset covers only three law schools, and one of schools is in a different jurisdiction from the other two. But these data at least allow us to estimate each student's credential distance from the middle of her class, and thus allows us to directly estimate mismatch effects and compare them with the absolute effects of credentials, the effect of race, and variations in school effectiveness in preparing students for the bar.

In each of our many alternative model specifications, the mismatch effect is a strong predictor of bar outcomes, and the measured size of the effect matches or exceeds the magnitudes suggested by "Systemic Analysis" and most earlier studies using the BPS. In our models with the best controls, such as when we use both LSAT and undergraduate grades to measure credentials, the mismatch effects are strongest and the direct effects of race upon bar outcomes completely disappear. Moreover, each of the significant variations in results across our models behave the way the "mismatch hypothesis" implies they should.

The mismatch effect in our models accounts for most of the large disparities in bar passage across racial lines. If our findings can be generalized, they largely account for the very serious disparity between the racial makeup of first-year law students and the practicing bar. No other careful, data-driven research has come close to explaining these problems, and they are far too serious to ignore. What then, should be done? There are many possible steps: creating good datasets that link law-student data to data on bar scores; studying possible ways to reduce mismatch through academic support; perhaps even developing controlled experiments to assess how preferences affect long-term outcomes. The most important initial step to take is for the legal academy to simply acknowledge a serious problem that must be addressed, and undertake inclusive, honest, and data-driven conversations.

Appendix Table A

Logistic Regression Models of First-Time Bar Passage, California Law Schools Only

Independent Variables	Model 1: Race Alone	Model 2: Add LSAT	Model 3: Add Continuous Mismatch	Model 4: Continuous Mismatch w/School Fixed Effects	Model 5: Categorical Mismatch	Model 6: Categorical Mismatch w/School Fixed Effects
Black	.25***	.52**	.58**	.60**	.58**	.59**
Hispanic	.37***	.67**	.74*	.76*	.75*	.77
LSAT		1.14***	1.06**	1.01	1.07**	1.01
Mismatch Deficit			1.11***	1.19***		
Mm Lev 1					1.03	.82
Mm Lev 2					.80	.58**
Mm Lev 3					.62*	.39***
Mm Lev 4					.42**	.22***
Mm Lev 5					.58	.30***
Mm Lev 6					.35**	.14***
Mm Lev 7					.18***	.07***
Davis				.43***		.41***
Constant	5.46***	.000***	.000**	2.05	.000**	1.81***
Observations	2,938	2,938	2,938	2,938	2,938	2,938
Somers' D	.13	.35	.36	.33	.37	.33

Significance levels are: *p<.1; **p<.05; ***p<.005 (two-sided)

Addendum: Data Sources and Access

This addendum explains how we obtained the data used in this study, and how any reader can obtain a copy of the data and programs we used to create our tables.

The University of Arkansas, Little Rock ("UA Little Rock") commissioned a consulting firm to conduct a "Bar Passage Correlation Study," which it completed in 2012. Upon the report's release, one of us (Steinbuch) asked the school for a copy of the data, and the school provided a set of pdf forms containing six variables on 899 students: ethnicity, sex, LSAT, UGPA, law school GPA, and first-time bar passage result for students taking the Arkansas bar. Of these 899 students, 723 took the Arkansas bar, and once we tabulated the pdf data on these students, we could reproduce all the results in the consultant's study. The data also matched up well with other sources of information, such as the school's "509" disclosures and results released by the Arkansas bar. We thus have high confidence in the accuracy of this data.[43]

Our data on the University of California at Davis ("Davis") was produced by that law school in August 2014, in response to a public records request one of us (Sander) filed in 2011. For each student admitted from 1994 through 2008, the school disclosed the following variables: three-year admissions cohort;[44] ethnicity; LSAT, UGPA, Davis's academic index, whether the student graduated; whether the student took the California bar and, if so, the pass/fail outcome. Since the fifteen years of data was grouped into three-year cohorts, we have five cohorts of data from the school; as noted in the text, LSAT and/or index deficits were calculated for each cohort. The final two cohorts of Davis data have a good deal of missing data on bar outcomes, but excluding or including these two cohorts did not meaningfully affect our results. By agreement, Davis excluded all incoming transfer students from the disclosed data.

UCLA provided one of us (Sander) with six Excel spreadsheets tabulating bar results for each year over a six-year period (2000 through 2005). The sheets contained information on all students taking the California bar, including ethnicity, LSAT score, and bar outcome. Some years included additional variables, including law school GPA, UGPA, and program affiliations within the law school. For three years (2000, 2001, and 2005), the data identified which students had transferred to UCLA after the first year. As explained in the text, we used only those three years for which we could exclude incoming transfers.

43. Steinbuch had obtained a second dataset from UA Little Rock through a public records request and litigation, but this disclosure did not match up well with independent sources, and we have thus not used any of that data in this article. Steinbuch and Kim Love have written about his efforts to obtain data and bring greater transparency to his law school in *Color-Blind-Spot: The Intersection of Freedom of Information and Affirmative Action in Law School Admissions*, 20 Tex. Rev. L. & Pol. 181 (2015). Since then, Steinbuch made an additional FOIA request, which was satisfied without incident. We expect to use this data in future research.

44. Davis's stated position, at the time they transferred data to us, was that Black students would be grouped in six- or nine-year cohorts, but in the actual data release it was easy to identify the exact cohort of all students.

Scholars interested in reproducing or replicating our results, or exploring the data for other purposes, should contact Sander. The requestor will be asked to sign an agreement to not attempt to reidentify students in the data, and will then receive the data (in either Excel or Stata format), a codebook, and the Stata code we used in our analyses.