Mismatch and Bar Passage: A School-Specific Analysis

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Are law students admitted with large preferences put at a disadvantage in passing bar exams? The debate on this question – often called the "law school mismatch hypothesis" – has often generated more heat than light, related as it is to broader questions about affirmative action and the use of racial preferences in higher education. Two highly-respected labor economists, from opposite sides of the debate about affirmative action, collaborated on an authoritative study of higher education preferences published in March 2016 by the *Journal of Economic Literature*. Here is their answer to the question we pose:

> 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.[1]

The various institutions of legal education, and their leaders, have not taken up this challenge. Indeed, they have gone out of their way to prevent the release of the sort of data that would resolve the mismatch debate.[2] Nonetheless, we have obtained reliable data on student credentials and in-state bar outcomes for three public law schools with differing levels of school eliteness and thus, differing "median" student credentials. This makes possible the first analysis of bar passage that can estimate "mismatch" levels for individual students, and test how students with similar credentials, but varying levels of mismatch, fare when they take bar exams. Our major findings are these:

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[1] Peter Arcidiacono and Michael Lovenheim, *Affirmative Action and the Quality-Fit Tradeoff*, 54 J. ECON. LIT. 3, 20 (2016).

[2] 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. The MBRE rejected requests to study the mismatch issue or make its data available for such research. The Law School Admissions Council threatened to defund the After the JD study if its members pursued research on mismatch. And so on.

1) Mismatch during law school is a major – probably *the* major – determinant of bar passage among law graduates. Substantial mismatch dramatically lowers one's chances of passing the bar on the first attempt. Indeed, all of our analyses suggest that a deficit in credentials relative to one's peers has a larger effect on bar passage than the absolute level of entering credentials.

2) When we control for mismatch effects and LSAT scores, racial deficits in bar passage rates substantially shrink. When we control for LSAT as well as UGPA, racial deficits appear to disappear altogether. Our results suggest that most of the racial gap in bar passage rates at these schools would disappear if the mismatch effect could be eliminated or successfully addressed

3) Our data has important limitations. it covers only three schools, and one of them in a different bar jurisdiction from the other two. For one school, our only "credential" data is on LSAT scores. For all three, we have no data on *outgoing* transfer students, which limits our ability to compare outcomes for the entire class of entering students. 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. But given the strength and seriousness of what we find, the case for developing better datasets is all the more urgent and compelling. It is of great importance that available data sources for studying mismatch more generally – in particular, the California Bar's admissions dataset – should be made available for objective research.

The Bar Passage Study and Its Limitations

In 1989, the Law School Admissions Council ("LSAC") launched its Bar Passage Study (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 161 of perhaps 180 accredited law schools that then existed. 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.

Sadly, LSAC made a series of decisions that crippled the utility of the BPS. First, LSAC decided to mask the identity of individual law schools by grouping schools into six "clusters." LSAC assigned schools to clusters based on seven factors, including school selectivity, size, cost, faculty/student ratio, percent minority, and numerical student credentials.[3] LSAC also grouped

[3] *User's Guide: LSAC National Longitudinal Data File*, p. 15 (1999).

state bar exams into twelve geographic regions. 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.

The resulting blurriness of the BPS data dramatically reduced its utility, since it made it largely impossible to relate the characteristics of a school, the makeup of its students, or student attitudes to one another or to student outcomes.

Studying Mismatch with the BPS

Most law schools base admissions decisions in large part on the LSAT scores and undergraduate grades ("UGPAs") of applicants. The LSAT runs on a scale from 120 to 180, and most colleges use a 4-point scale of grades, with nearly all students having GPAs of 2.5 or higher. In some of our analyses, we combine these two measures into a single "academic index," scaled from 0 to 1000.[4]

We know from a variety of research on individual law schools that the academic index of students correlates with their law school grades. The correlation is modest – in a -1 to 1 range, it usually falls around 0.4 – partly because of something called the "restriction of range" problem (most students at a particular law school have very similar credentials, which lowers the potential correlation) and partly because many other factors go into law school success. Nonetheless, as a prediction of individual-level human outcomes, a 0.4 correlation translates into a strong and reliable prediction of outcomes for groups.[5]

Similarly, the academic index correlates with performance on the bar exam. It is much easier to study this correlation with data on actual bar *scores*,[6] but one can fairly observe the association even with simple pass-fail data.

It is therefore unsurprising (and not disputed) that, other things being equal, students with lower academic indices will fail the bar exam more often. For example, in the BPS, law students with an academic index of 800 or more had a first-time bar passage rate of 97%, while

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[4] Our formula for the academic index is ((LSAT-120)*10)+(100*UGPA); LSAT thus contributes 600 points to the scale, compared to 400 points for UGPA. This greater weighting of LSAT is also common in law school admissions offices, presumably because LSAT does a better job in predicting both law-school grades and bar outcomes than does UGPA. UGPA can be made more predictive by taking into account other information, such as the undergraduate school attended, the grading scale used at that school, or courses and majors taken; but we lack any of that additional information here.

[5] See Sander, "A Systemic Analysis of Affirmative Action in American Law Schools," 57 Stanford L. Rev. 367, 418-25 (2004).

[6] Since bar scores are continuous, one can much more powerfully compare them with other continuous measures (e.g., law school grades, LSAT scores) than one can compare a simple pass/fail outcome. Research by Stephen Klein, a longtime consultant to the California Bar, found correlations of .55 to .60 between pre-law credentials like the LSAT and college GPA, and eventual scores on the bar exam.

those with an index of 500 or less passed at a rate of 39%. This relationship – called the "credential effect" – is sometimes confused with mismatch. But the mismatch hypothesis concerns an entirely different point: it posits that a student with a particular index (say 700) will learn more at School Y, where the median index of all students is approximately 700, than at School Z, where the median index of all students is approximately 800. If true, this implies that accepting a large admissions preference will hurt a student's learning in law school.

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,[7] and Thomas Sowell made similar points in broader critiques of racial preferences.

Since a key goal of the BPS was to understand why minority students had lower bar-passage rates than whites, it was odd that when the study was announced, there was no indication that mismatch would be among the hypotheses tested. As we now know, the BPS planners were well aware of the mismatch question, and 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?[8]

Former LSAC officials, requesting anonymity, have told us that when some law-school administrators objected to "putting affirmative action on trial," the mismatch question was quietly dropped. What is clear, in any case, is that even though this question 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 it.

As noted earlier, the elimination of individual law school identities makes it challenging to test the mismatch hypothesis with BPS data. The reason is easy to see. The six "clusters" of law schools in the BPS do correlate with the academic index: the mean index of Cluster 5,

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[7] James A. Davis, The Campus as a Frog Pond: An Application of the Theory of Relative Deprivation to Career Decisions of College Men," 72 American Journal of Sociology 17 (1966); Jencks and Riesman, The Academic Revolution (Doubleday, 1968); Clyde Summers, "Preferential Admissions: An Unreal Solution to a Real Problem," 2 U. Tol. L. Rev. 377 (1970).

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

which included many elite, private schools, is 845; the median index of Cluster 4, which included somewhat less elite (and generally public) schools, is 782, and so on. But within each of these clusters are law schools with very different levels of competitiveness and, thus, academic levels; Cluster 5, for example, appears to have included schools with median indices as low as 775 (USC, perhaps) and as high as 900 (e.g., Yale). A student with an index of 750 would have only a small academic gap with the USC student body, but a large gap with the Yale student body. But since both schools are in the same cluster, we cannot measure the individual level of potential mismatch.

However imperfect, the BPS has been the only game in town – that is, the only public dataset that allows one to compare school outcomes of similar students across different law schools. In a 2004 article on the scope and effects of affirmative action in legal education, one of us (Sander) dealt with the cluster problem by comparing black and white students. Data from the BPS, buttressed by admissions data from half-a-dozen law schools, showed that a large majority of African-American law students received significant admissions preferences and attended law schools with median-student credentials well below theirs. Sander therefore used race as a rough surrogate for whether a student was potentially mismatched. His findings implied that the mismatch effect on bar passage was not only real, but fairly large – equivalent to subtracting about 120 points from a student's academic index.

In the ensuing, often raucous debate, critics of the mismatch hypothesis seized on the fuzziness of the BPS to construct alternative measures of academic gaps, and to argue that these alternative measures failed to show a problem. These critiques, however, have not stood up well under close scrutiny.[9]

By far the most comprehensive examination of the BPS data and the mismatch question – and one of very few to appear in a peer-reviewed, empirical journal, is Doug Williams' 2013 article, "Do Racial Preferences Affect Minority Learning in Law Schools?" Williams carefully examined the work of mismatch critics and devised clean, consistent tests for comparing different approaches to study the mismatch effect with the BPS data. He also devised innovative ways of addressing the weaknesses of the BPS data, such as controlling for the region where study subjects took the bar exam and measuring successful outcomes in many alternate ways. Williams not only found that mismatch effects were significant in nearly all his models; he also concluded that the mismatch effect could fully explain the underperformance of African-Americans on bar exams.[10]

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[9] See Sander, "Replication of Mismatch Research: Ayres, Brooks, and Ho," 49 *International Review of Law and Economics* (forthcoming, 2019); Williams, Sander, Luppino & Bolus, "Revisitnig Law School Mismatch: A Comment on Barnes," 105 Northwestern L. Rev. 813 (2011). These articles show that leading critics of law school mismatch made outright errors or poor methodological choices that, when corrected, flip their results to either confirm the law school mismatch hypothesis, or to provide no evidence against it.

[10] The wording is important here: since African-Americans in the BPS have lower average credentials than whites, part (half or slightly more than half) of their lower performance on the bar exam might be attributed to the "credential effect." But a substantial bar passage deficit for African-Americans remains after taking this effect into account, and Williams found that this underperformance below that expected by credentials could be fully explained by mismatch. See also *infra*, discussion in Conclusion.

As Williams noted, the strength of his results was particularly striking given the fuzziness of the BPS data; like Arcidiacono and Lovenheim, he noted that "the limitations of the BPS intrinsically tend to bias any test against a finding of mismatch." Williams concluded:

> 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.

In this paper, we use data that avoids at least some of the limitations of the BPS data, thus making possible a more direct assessment of mismatch and its effects on bar passage.

Our Data

The innovation in this study is quite simple: we obtained data from three law schools on the credentials of each student at the school who sat for the in-state bar exam over multiple years. All told, our dataset covers nearly five thousand students, nearly four thousand of whom took in-state bar exams. This makes it possible to do something that could never be done with the BPS: construct a direct measure of "mismatch" for each individual student, and to evaluate whether a student's level of mismatch helps to predict whether they pass or fail the bar. The Addendum at the end of this report explains how we obtained this data from two law schools in California, and one in Arkansas, and how interested researchers can obtain a copy of the data. For purposes of this draft, we identify the law schools as School A, School B, and School C. School A is an elite law school in California, with student credentials in the range found in the law schools at UC Berkeley, UCLA, UC Irvine, and USC. School B is a somewhat less elite, but still highly-ranked law school in California, with student credentials in the range found at UC Davis, UC Hastings, and Loyola. School C is a lower-ranked public law school in Arkansas.

Our dataset is hardly ideal. Each school gave us slightly different types of information, as Table 1 suggests. 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 LSAT scores from all three schools. Second, we know enough about the universe of fellow students to measure each student's "credential distance" from her classmates. Third, in all of these schools the vast majority of graduates take the in-state bar exam, and we know the pass/fail outcome of those exams. Fourth, for the two elite schools in our sample, we are able to exclude incoming transfer students, who can otherwise confound a mismatch analysis.[11]

[11] Many elite schools admit into their second-year classes students whose credentials were not strong enough to win admission as 1Ls, but who 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 School C, we could not identify incoming transfers, but School C 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.

On two important issues (utilizing undergraduate grades in measuring credentials, and limiting the analysis to students taking the same bar), we were able to use two of the three schools in additional analyses.

Table 1
Characteristics of the three law school datasets

Data characteristic	School A	School B	School C
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
All entering students included?	No	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, 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.

Initial Explorations with the Data

Table 2 shows for five of the cohorts at our three schools a simple cross-tabulation of first-time bar passage by LSAT score.[12] The data show some clear regularities that we will examine more robustly below. First, if we examine any of the "passing %" columns from top to bottom, there seems to be a strong credential effect: at each school, bar passage rates rise fairly steadily as LSAT scores go up.[13] Second, if we examine any of the first six rows of data,

[12] Three of School B's cohorts had a median LSAT close to 162, as did one of School A's cohorts; since we would not expect these cohorts to provide any "contrast" with one another, we excluded them from this table. All cohorts are included in the regressions which follow.

[13] It is worth noting that this "credential effect" seems to lose force at the top of the credential distribution. We suspect that if the outcome variable here were bar scores, rather than bar passage, the credential effect would be large throughout the distribution. But it makes sense that once one's credentials are high enough to put one in a

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 School A to B to C; 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.

Table 2
First-time bar passage rates for graduates attempting the in-state bar exam

LSAT Range	School A		School B		School C	
	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[14]	164		160		152	
Total pass rate	89%		81%		78%	
Cohorts:	2002, 2005		2000-02, 2003-05		2005-2012	

We can directly test for mismatch through a regression analysis. The size of a student's LSAT deficit – that is, the number of LSAT points any individual student is below[15] the school median – is our measure of the student's degree of potential mismatch.[16] In a simple logistic regression model, we can estimate how strongly our independent variables, including LSAT and "mismatch," predict first-time bar passage.

Such a simple model has limitations. For example, the "mismatch" effect may well be 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

range likely to pass the bar, the small percentage of failers would be failing for other reasons, such as a failure to study for the exam, personal stress during the exam, and so on.

[14] For the cohorts included.

[15] 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 would probably 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, there is no basis for exploration, and we exclusively focus on "negative" mismatch.

[16] This is the only measure available for all three schools. Below, we combined LSAT and UGPA in models examining School B and School C.

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.

It is also helpful to include in some models "school fixed effects" – i.e., controls for the school each student attended. This helps address possible bias introduced by some students (i.e., those at School C) taking a less difficult bar exam, or if one school generally does a better job of preparing students for the bar than do others.

Table 3
Logistic regression models of first-time bar passage, 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: Categor. Mismatch w/School Fixed Eff.
African-Am.	.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***
School C				.48**		.51**
School B				.45***		.44***
Constant	5.05***	.000***	.003**	.060	.001***	.016
Observations	3,656	3,656	3,656	3,656	3,597	3,597
Somers' D	.13	.35	.36	.39	.37	.39

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

In Table 3, we present results using a series of variations built around these alternate specifications.[17] In all models, we include Somers' D, a summary measure of the explanatory power of the models. These are logistic regression, meaning that the outcome is binary (pass

[17] The results we present here are logistic regressions, and we report odds-ratio coefficients and significance levels. We have also run all of these models as linear probability models (i.e., OLS regressions), and these results are available from the authors.

or fail the bar). A coefficient of exactly "1" means that the independent variable neither increases or decreases one's odds of passing the bar. A coefficient below 1 means that a unit increase in the variable hurts one's odds of passing; a coefficient above 1 means that a unit increase in the variable helps one's odds of passing.

The first two models help us "calibrate" our results by showing patterns that are already well known. Model 1 shows that African-Americans and Hispanics substantially less likely to pass (and more likely to fail) the bar than the omitted groups (mainly Anglos and Asian-Americans).[18] Model 2 shows that when we control for LSAT score, the racial gap is narrowed but still fairly large and statistically significant.[19]

The results from Models 3 through 6 are striking. They consistently support two findings: first, when we add a measure of "mismatch" to the model, race effects become smaller. And second, the mismatch effect itself is consistently large and statistically significant. Indeed, "mismatch," however formulated, appears to be more powerful in predicting bar outcomes than LSAT itself, although that is hard to conclude from these data alone.

Models 3 through 6 test the robustness of the "mismatch" finding by formulating the model in alternative ways. In Model 3, we simply measure the LSAT gap (when negative)[20] between each student's LSAT score and the median LSAT of his school. The coefficient in this model is above 1.0 because having a less negative mismatch value improves one's odds of passing the bar.

In Model 4 we add school "fixed effects." As noted earlier, this is a way of controlling for factors that might create the illusion of mismatch, but actually reflect factors specific to schools in the analysis. The hazard of introducing school fixed effects is that the mismatch variable itself is derived from each person's "school" and "LSAT"; thus putting both school and LSAT into the analysis might create redundancy in the model and obscure the effect of mismatch. This concern is not borne out by Model 4, however. The mismatch coefficient goes up and the LSAT coefficient goes down, apparently because students at School A do better on the bar than do students at B and C (for reasons not captured by the model), and when this effect is controlled, the explanatory power of mismatch increases. Model 4 does not provide a basis (i.e., a high coefficient for School C) to suppose the inclusion of an Arkansas school is distorting the results.

In Model 5, we treat mismatch as a categorical variable rather than a continuous one. Each of the seven levels of mismatch corresponds to a 2-point section of the deficit: Mismatch Level 1 is 0 to 2 points below the median; Mismatch Level 2 is 2 to 4 points, and so on up to

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

[19] For those new to logistic analysis, it may be helpful to point out that although the LSAT effect in Model 2 is closer to 1 than the race effects, it is a much stronger effect. The "race" coefficients measure the "total" race effect, but the reported LSAT coefficient is only the effect of a one-unit increase in LSAT score.

[20] See note 14, supra.

Mismatch Level 7, which is 12 to 14 points below the median. The coefficients in this model again show the large mismatch effect that we observed in Model 3. In every case, the coefficient measures odds relative to the students with no LSAT deficit. We would expect "Mm Lev 1" and "Mm Lev 2" not to be statistically significant, because the size of those students' LSAT deficit is small; we expect the effect to become steadily larger and more statistically significant as the extent of mismatch increases. That is indeed what we observe. Moreover, although the coefficients bounce around a little, the effect is roughly linear[21] -- more mismatch produces a steadily decreasing probability of passing the bar. The sample size is not large enough to tell whether the smaller coefficient of Mm Lev 4 represents a real non-linearity, or (as seems likely) just random variation from Mm Levels 3 and 5.

In Model 6, we add school fixed effects to the categorical regression. As in Model 4, adding these school controls produces stronger and clearer mismatch effects.

We varied our analysis in other ways as well, such as modelling LSAT as a polynomial, modelling 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.

Despite our inclusion of school fixed-effects in Models 4 and 6, one may still wonder whether the inclusion of results from two different bar jurisdictions renders the analysis suspect. We can test for this by doing the same analyses described in Table 3 for only the California law schools (Schools A and B). These results are reported in the Appendix. The significance levels on mismatch are somewhat lower when we drop School C, presumably because this leaves us with a smaller sample and one with less mismatch variation. But the general import of these results is unchanged; mismatch retains strong explanatory power.

What is the effect of relying on LSAT as the sole measure of academic potential and mismatch in our model? A common criticism of law schools is that they rely too heavily on LSAT. While some of the criticism is overblown – LSAT scores do predict a lot[22] – we think more information is indeed better, both in law school admissions and in modelling mismatch. As we have argued elsewhere and will demonstrate below, not including other measures of academic potential tend to bias our analysis *against* a finding of mismatch.

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[21] Note that although we can tell that the coefficients of Mm Lev 4 and Mm Lev 5 are both statistically distinguishable from zero, the standard errors are too large (because of the small sample size) to tell whether the apparent non-linearity here is real or (more likely) a result of random variation. We also tested a model with a "squared" mismatch term, but this term was not statistically significant, as one would expect from the fairly linear pattern of mismatch in Models 4 and 5.

[22] See note 6, supra. Klein found that LSAT was not only highly correlated with bar exam scores, but that it predicted scores on the performance exam – a written test focused on one's ability to develop a case strategy from primary source materials – pretty much as well as scores on the multiple-choice "multistate" exam. Higher LSAT scores are also a strong predictor of higher earnings, even after controlling for the eliteness of one's school. Sander and Bambayer, "The Secret of My Success: How Status, Eliteness, and School Performance Shape Legal Careers, 9 J. of Empirical Studies 893 (2012).

We can illustrate the biasing effect of unobserved credentials by using data on undergraduate grades ("UGPA"), which we have for Schools B and C. Since School B is more selective than School C, its students will generally have higher credential; other things being equal, a student at School B with a lower-than-average LSAT is likely to have a higher-than-average UGPA. Thus, when we control for LSAT, students at School B will tend to have higher UGPAs than their School C counterparts, and thus should, in the absence of mismatch effects, tend to perform better on bar exams.

Table 4 demonstrates empirically what we expected conceptually. Because this same type of "selection effect" should show up in any measure of incoming credentials we might use (assuming those credentials are used in the admissions process), all of these effects are likely to bias our analysis toward finding a smaller mismatch effect than actually exists.

Table 4
Average UGPA by LSAT score, Schools B and C

For students with LSAT scores of…	…Average UGPAs are given (with number of students) at…	
	School B	School C
146	3.66 (5)	3.24 (41)
150	3.47 (19)	3.25 (77)
154	3.54 (72)	3.19 (70)
158	3.56 (104)	3.22 (33)

Table 5 shows side-by-side analyses utilizing only LSAT (as in Table 3) and analyses in which done with both LSAT and UGPA are combined in an academic index. The table shows Models 3 and 6 from Table 3, first with LSAT as the measure of credentials and mismatch, and then with the LSAT-UGPA index as the measure of credentials and mismatch. We scaled the academic index so that it had the same mean and standard deviation as the LSAT scores of students at Schools B and C, so the LSAT and Index coefficients in these models can be meaningfully compared.

Table 5
Logistic Regression Models 3 and 4, from Table 3
Including Only Schools B and C 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 FE	Model 10: Using index, categorical mismatch, school FE
African-Am.	.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***
School B				.73
Constant	.06	.14	.008	.002
Observations	3,005	3,005	3,005	3.005
Somers' D	.36	.37	.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.

Table 5 reveals two interesting things. First, as expected, the mismatch effects are more pronounced when we incorporate UGPA. Second, the race effects appear to completely disappear: for both African-Americans and Hispanics, the coefficients are very close to one and not even weakly statistically significant. Indeed, the comparatively small "credential effect" observed in these models suggests that, if we could eliminate the mismatch effect, black performance on the bar exam might rise to the same level as whites. This finding is illustrated in Table 6, which compares bar passage rates of blacks and non-blacks at Schools B and C when we simply categorize students by their level of mismatch on the academic index. Visual inspection suggests that, when we control for mismatch levels, blacks and non-blacks have quite comparable bar outcomes. The difference in aggregated outcomes, we suggest, stems from the difference in exposure to mismatch: more than four-fifths of the African-American students have an index deficit of -3 or greater, but this is true for only about a fifth of the non-blacks.

Table 6
Index deficit and Bar Passage rates at Schools B and C

Index deficit	Non-black bar takers		African-American bar-takers only	
	Number	% Passing	Number	% Passing
Below -15	7	0%	5	0%
-12 to -15	25	32%	18	28%
-9 to -12	51	39%	20	60%
-6 to -9	156	56%	30	63%
-3 to -6	355	71%	23	52%
0.1 to -3	796	81%	11	73%
No deficit	1495	88%	13	85%
Total	2885	81%	120	56%

Conclusion

In his "Systemic Analysis" paper, laying out the basic case for the existence of law school mismatch effects, Sander estimated from the BPS database that roughly half of the black-white gap in bar passage could be accounted for by mismatch. Using the same data but different techniques, Williams arrived at a roughly similar estimate. Sander's recent reanalysis of Ayres and Brooks critique of mismatch, also using the BPS, finds that when errors in the Ayres-Brooks analysis are corrected, the "second-choice" model they develop suggests quite large mismatch effects for African-Americans, consistent with explaining significantly more than half of the black-white gap in bar passage.[23]

Our results here represent the first analysis of law school mismatch that uses non-BPS data and corrects for the most obvious limitation of the BPS – the absence of a direct measure of mismatch. When we use such a measure, it is not only obvious that mismatch exists; the size of the effect appears to be even larger than Sander and Williams predicted. Indeed, our analysis suggests the mismatch effect may be larger and stronger than the direct credential effect. This raises the possibility that most, and perhaps nearly all, of the racial gaps in bar passage may be accounted for by mismatch.

A few caveats are in order. The database only comprises bar results from three law schools over relatively short periods of time. It does not include data on students taking bar exams out-of-state, and the data on School A, in particular, is quite limited. Our data is also not complete enough to enable us to test "graduation and bar passage" as a combined outcome; we are only examining how law school graduates perform on in-state exams.

[23] See Sander, "Replication of Mismatch Research," *supra* note 9.

But this analysis, particularly in conjunction with other recent work, shows how gravely academic institutions and state bars err in ignoring the mismatch issue. Nearly everyone in authority in legal academic claims that the racial gap in bar passage is a serious problem and that greater diversity in the legal profession is a fundamental goal. If these are more than just words, it is inexcusable not to move forthwith to make the best possible data available for the study of mismatch.

Appendix

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 Contin. Mismatch	Model 4: Contin. Mismatch w/ School Fixed Effects	Model 5: Categorical Mismach	Model 6: Categor. Mismatch w/School Fixed Eff.
African-Am.	.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***
School B				.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.

School C 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.[24]

Our data on School B 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;[25] ethnicity; LSAT, UGPA, School B'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 School B 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, School B excluded all incoming transfer students from the disclosed data.

School C 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 School C after the first year. As explained in the text, we used only those three years for which we could exclude incoming transfers. School C also made an extensive disclosure of data in 2015, in response to a public records request; this data includes

[24] Steinbuch has obtained additional data from School C through public records requests, but these other data disclosures have not matched 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).

[25] School B's stated position, at the time they transferred data to us, was that African-American 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.

non-graduates and graduates who did not take a bar exam, or who took a non-California bar exam. However, for data-masking purposes, the university grouped students in this larger dataset into cohorts of varying lengths and sometimes stretching over many years, making the data unsuitable for defining the relative credentials of students within a fixed cohort.

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.