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Essay

IS AFFIRMATIVE ACTION RESPONSIBLE FOR THE ACHIEVEMENT GAP BETWEEN BLACK AND WHITE LAW STUDENTS?

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In *Grutter v. Bollinger*, the Supreme Court upheld some affirmative action programs in legal education as constitutional.[1] The wisdom of affirmative action as a policy decision, however, remains highly contested.[2] The challenge is to determine how affirmative action policies affect law schools,

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[1] Grutter v. Bollinger, 539 U.S. 306 (2003).

[2] *See* RACIAL PREFERENCE AND RACIAL JUSTICE: THE NEW AFFIRMATIVE ACTION CONTROVERSY (Russell Nieli ed., 1991) (an anthology of views on affirmative action); *see also* CAROL COHEN & JAMES P. STERBA, AFFIRMATIVE ACTION AND RACIAL PREFERENCE: A DEBATE (2003) (providing two sides of the moral debate over racial preferences).

law students, and the legal profession. Several scholars have provided nuanced descriptions of the effects of affirmative action on students and graduates at specific elite institutions.[3] These studies demonstrate that the affirmative action policies at these schools are generally consistent with the stated policy goals of affirmative action. Namely, these studies find that students value diversity in the classroom, that black and white graduates have equally successful careers after law school, and that, in some situations, black graduates give back more to the community than white graduates do.[4]

Although these studies demonstrate that black law students are highly successful in a variety of dimensions, there are still troubling aspects of black student performance in law school and thereafter. In a recent article in the Stanford Law Review,[5] Professor Richard Sander focused on one such datum: Half of black law students are in the bottom tenth of their class.[6] Using this datum as evidence of a significant problem in black stu-

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[3] *See, e.g.*, Celestial S.D. Cassman & Lisa R. Pruitt, *A Kinder, Gentler Law School? Race, Ethnicity, Gender, and Legal Education at King Hall*, 38 U.C. DAVIS L. REV. 1209 (2005) (reporting the results of a study of law school culture and its impact on students at U.C. Davis Law School); David L. Chambers, Richard O. Lempert & Terry K. Adams, *Michigan's Minority Graduates in Practice: The River Runs Through Law School*, 25 L. & SOC. INQUIRY 395 (2000) (discussing benefits of diversity to University of Michigan Law School graduates); *see also* Timothy T. Clydesdale, *A Forked River Runs Through Law School: Toward Understanding Age, Gender, Race, and Related Gaps in Law School Performance and Bar Passage*, 29 L. & SOC. INQUIRY 711 (2004) (providing a cross-institution comparison); The Educational Diversity Project, http://www.unc.edu/edp (ongoing major survey project focusing on the effects of diversity in legal education) (last visited Apr. 30, 2007).

[4] *See, e.g.*, Chambers et al., *supra* note 3, at 428–30, 456–58 (describing University of Michigan Law School alumni study, which demonstrated that minority law students who graduated in the 1970s performed significantly more pro bono activities than their white counterparts, and were more likely to work for public institutions).

[5] Richard H. Sander, *A Systemic Analysis of Affirmative Action in American Law Schools*, 57 STAN. L. REV. 367 (2004).

[6] *Id.* at 426. Black students account for 7% of the new matriculants at law school each year. *See* American Bar Association, African American J.D. Enrollment, 1971–2005, at 1, http://www.abanet.org/legaled/statistics/charts/stats%20-%2013.pdf (last visited Apr. 30, 2007) (providing data through academic year 2006–2007); American Bar Association, First Year J.D. and Total J.D. Minority Enrollment for 1971–2005, at 1, http://www.abanet.org/legaled/statistics/charts/stats%20-%208.pdf (last visited Apr. 30, 2007) (providing data through academic year 2006–2007, and showing that the percentage of black first year law students ranged from 6.5% to 7.7% from 2001 to 2006). Total minority enrollment has been approximately 20% of the total first year population of students since the mid 1990s. *See* First Year J.D. and Total J.D. Minority Enrollment for 1971–2005, *supra*, at 1. This datum is accurate overall for law students, and for most law schools, with the major exception of historically black institutions. *See, e.g.*, ABA-LSAC, The Official Guide to ABA-Approved Law Schools, 2008 Edition, Left Page 2 Data (Excel Spreadsheets), http://www.abanet.org/legaled/statistics/charts/OG%20Left%20Page%202%202008.xls (last visited June 14, 2007) (demonstrating that most ABA-approved law schools enroll between 10% and 30% minority students, with most of those schools at 20% or lower; the few schools with 50% or more minority students are historically black institutions or institutions that have large Hispanic student populations); ABA-LSAC, THE OFFICIAL GUIDE TO ABA-APPROVED LAW SCHOOLS 56–65 (2002) (listing the percentage of minority students enrolled at each ABA-approved law school in 2000). As others have noted, some group of people must be in the bottom tenth of the class. David B.

dent achievement, Sander sought to answer empirically how affirmative action as currently practiced affects the number of black law graduates who pass the bar—that is, the number of new black lawyers each year. Based upon his analysis, Sander predicts a 7.9% gain in the number of black lawyers absent affirmative action.[7] According to Sander, this counterintuitive result is the product of the "mismatch hypothesis." The mismatch hypothesis posits that, as a result of current affirmative action policy, blacks systematically are admitted to and attend law schools for which they are underqualified. As a result, they disengage from the learning process, and they do not learn as much as they would have if they had the same qualifications as the white students in the class. Sander's analysis does not withstand scrutiny in many ways.[8] As I demonstrate in Part I, his claim that affirmative action results in a loss of 169 black lawyers each year is not supported by the data.

Taken out of the volatile context of affirmative action, the mismatch hypothesis is not an unreasonable theory, despite Sander's flawed analysis. Many people, Sander includes, remember instances when they were outmatched and learned little or nothing in a class.[9] Sander provides an anecdote about his attempt to take college German at Harvard.[10] Lacking a facility with foreign languages, Sander soon found himself adrift, and, as the semester progressed, disengaged from his German class more and more. This anecdote sounds like a typical learning experience of a young student at an elite college: for (perhaps) the first time, the student finds himself outmatched, responds by disengaging from the class, and therefore learns less from the class than he would have from a less ambitious one. Certainly, this is a plausible explanation for Sander's failure to learn German. Returning to the context of race, however, other theories are also plausible. Hostile learning environment, stereotype threat,[11] and race-based barriers

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Wilkins, *A Systematic Response to Systemic Disadvantage: A Response to Sander*, 57 STAN. L. REV. 1915, 1917 (2005). And before blacks were admitted to law school, few worried that the white men at the bottom of the class were "outmatched" by their peers. Still, that half of all black students are in the bottom tenth of the class at least suggests that law schools may not be providing as good an education to their black students, for whatever reason. Even if this is not the case—again, some group of students has to be at the bottom of the class—law schools should think carefully about the stereotypes that this fact perpetuates.

[7] Sander, *supra* note 5, at 473.

[8] *See infra* note 44 for a list of the primary ways in which Sander's analysis is flawed.

[9] As a self-proclaimed "math geek," I hear confessions from many people about prior math classes. The mantra "I'm just not good at math" may be, at least in part, a widely held, culturally accepted disengagement with the process of learning mathematics. *See* Elena Nardi & Susan Steward, *Is Mathematics T.I.R.E.D? A Profile of Quiet Disaffection in the Secondary Mathematics Classroom*, 29 BRIT. EDUC. RES. J. 345, 362 (2003) (noting that "self-images of mathematical ability [were] overwhelmingly negative" and that this contributed to less willingness to engage with mathematics).

[10] Sander, *supra* note 5, at 449–50.

[11] Stereotype threat is a term used to describe the phenomenon of minority groups performing more poorly than expected because of the stereotype that the minority groups will, in fact, perform poorly. *See generally* Claude M. Steele & Joshua Aronson, *Stereotype Threat and the Intellectual Test Perform-*

are among the other logical theories to explain why minority groups do not perform well, even though they may not explain Sander's personal experience. Of course, each of these theories may be at work at the same time—one might be somewhat mismatched, for example, but the problem compounds with discriminatory treatment.

Like Sander, I have a personal anecdote that suggests, at least for me, that these separate theories might coexist. I began college as a physics major. Initially, I did quite well, excelling at introductory and intermediate classes, with my performance based primarily on my mathematics ability. By the time my junior year arrived, however, I hit a wall: I found out that math skills—my real strength—only take one so far in physics; after that point (for me, a potential theory seminar), a real intuition into physics is necessary. I was, to put it bluntly, outmatched by my classmates. But this fact alone did not lead me to disengage from the learning process; at first, I worked quite hard in the class. As the class went on, however, it became clear that my fellow students (primarily male) did not believe that I—or the other women in the class—was capable of doing the work. In my case, they were right. In my female classmates' cases, they were wrong. Both of the other women went on to get PhDs in related fields. Nonetheless, in all three of our cases, our male classmates relied on gender stereotypes to conclude that the women in the class did not belong there.

Thus, there were two reasons why I did not do well in my physics class. First, I was outmatched; my math ability no longer was sufficient to get me through. Second, I was mistreated, and angry about my mistreatment. Looking back, I let that mistreatment affect my performance. I studied less than I should have and could not focus when I needed to because my mistreatment rankled.[12] This experience characterizes what some term "hostile learning environment."[13]

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ance of African Americans, 69 J. PERSONALITY & SOC. PSYCHOL. 797 (1995). As a concrete example, black students taking the SAT hear and internalize the stereotype that blacks do not do well on the SAT. They worry about the prospect, they get angry at the stereotype, and they feel that others expect them to do poorly. All of these factors distract them from the task at hand—taking the SAT—and therefore make optimal performance more difficult. *Id.* at 799. Signals as small as checking a box to indicate the test taker's race have been shown to trigger stereotype threat. Jessi L. Smith, *Understanding the Process of Stereotype Threat: A Review of Mediational Variables and New Performance Goal Directions*, 16 EDUC. PSYCHOL. REV. 177, 181 (2004).

[12] Several readers of drafts of this Essay commented that such treatment would simply make them work harder. I have two responses to this suggestion. First, as educators, we should not condone a discriminatory culture because it "makes students work harder" or somehow instills values of perseverance. Second, as an individual student, I certainly learned from this experience and did not allow such treatment to affect my performance in subsequent classes. The underlying point, however, is that the educational environment should be created in such a way as to facilitate learning for all students, without creating barriers to learning based upon arbitrary factors such as gender or race.

[13] Wills v. Brown University, 184 F.3d 20, 26 (1st Cir. 1999) (defining a hostile environment claim as "harassment severe enough to compromise the victim's employment or educational opportunities").

I take from this story the intuitive sense that the mismatch theory is not unreasonable, but that discriminatory behaviors that affect performance are also viable explanations for the law school achievement gap that Sander highlights.[14] If either of these theories is accurate, legal academia has failed black students. However, the appropriate response to this potential failure differs drastically depending on what one thinks the original problem is.[15]

There are three potential explanations for the gap in achievement between black and white students as measured by law school grades. First, the mismatch theory suggests that blacks are outmatched by their classmates, disengage from the learning process, and receive poor grades as a result. Second, it may be that the law school atmosphere creates substantial barriers to high achievement for black students. Third, black law students may simply be performing to their potential, as measured by their lower incoming credentials. Under this third theory, black law students have not disengaged from the learning process and would not learn more at a less demanding school, nor do they face special barriers to achievement.

If correct, each of these theories would require a different calculus for the appropriate response. First, the mismatch theory implies that law schools are doing a disservice to black students by admitting them when they will likely fail, but would have done better elsewhere. One straightforward policy change on this basis would be to refuse to admit such underqualified students by removing the "boost" that affirmative action provides black applicants in the admissions process. Another policy change would be to recognize that it is not the underqualification, but the disengagement with learning that makes mismatch a bad deal for black students. Schools could therefore implement academic outreach programs to keep the students engaged.[16] Second, the barriers theory suggests that law school culture must change to remove race-based barriers to achievement. By race-based barrier theory, I do not intend to invoke solely a description of intentional discrimination. Instead, I mean to describe the myriad ways that law school atmosphere can be uninviting to black law students, whether intentionally

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[14] I recognize that there are many measures of law school achievement; grades, participation in moot courts, law review, trial teams, and other activities are just a few. The only measured achievement gap upon which I focus is grades; this is the only measured achievement within law school. I focus on grades primarily because of the common perception that grades are the primary determinant of job prospects after law school and because it is a very stark statistic that I believe deserves some attention.

[15] Some critics acknowledge the troubling aspect of the data but suggest that Sander's remedy of dismantling affirmative action is inappropriate. *See* Ian Ayres & Richard Brooks, *Does Affirmative Action Reduce the Number of Black Lawyers?*, 57 STAN. L. REV. 1807, 1808–09 (2005). Indeed, just because the legal academy may be doing a poorer job of educating its black students does not mean that the legal academy should get out of the business of doing so.

[16] Many schools have some program like this, at least informally. For example, the Law School Admission Council (LSAC) sponsors conferences for academic support personnel. *See* LSACNet.org: Events & Dates: Calendar of Events, http://www.lsacnet.org (click on "Event Calendar") (last visited June 14, 2007) (listing events including the LSAC Academic Assistance Training Workshop, held in June 2007).

or not. Finally, if neither mismatch nor the race-based barriers theory has a significant impact on black law student achievement, then schools must determine as a normative matter whether the appropriate response is to change nothing. One could argue that while not directly hurting black law students, affirmative action perpetuates the stereotype that black students are less able than their white classmates.[17] Schools could also provide additional support services for struggling students, or simply acknowledge that while stereotype reinforcement might be a small effect, the pedagogical and societal advantages of diversity are more important.[18]

Sander's article perfunctorily dismisses a race-based barriers theory and focuses instead on the mismatch theory.[19] In addition, Sander focuses on only one possible policy intervention based upon the supposed accuracy of the mismatch theory: He asserts that the proper response to poor law school performance by black students is to curtail affirmative action.[20] Several authors reanalyzed the data Sander relied upon, all coming to the opposite conclusion from Sander;[21] still others argued about matters other than his empirical study.[22] In the two years since publication, Sander and other

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[17] This is a consistent argument against affirmative action. *See* Faye Crosby, Aarti Iyer & Sirinda Sincharoen, *Understanding Affirmative Action*, 57 ANN. REV. PSYCHOL. 585, 593 (2006) (discussing psychological experiments that at best provide limited empirical support for this theory).

[18] The briefing in *Grutter v. Bollinger*, 539 U.S. 306 (2003), provides a thorough airing of the advantages of diversity in education. *See, e.g.*, Consolidated Brief of Lt. Gen. Julius W. Becton, Jr. et al., as Amici Curiae in Support of Respondents at 13–18, *Grutter*, 539 U.S. 306 (No. 01-1015), 2003 WL 1787554 (discussing the need for affirmative action to maintain an integrated officers corps in the military); Brief of General Motors Corp. as Amicus Curiae in Support of Respondents at 12, *Grutter*, 539 U.S. 306 (No. 01-1015), 2003 WL 399096 (arguing that diverse educational environments provide cross-cultural skills necessary to compete in the global economy); *see also* Brief of the American Educational Research Ass'n. et al. as Amici Curiae in Support of Respondents, *Grutter*, 539 U.S. 306 (No. 01-1015), 2003 WL 402134 (compiling research on the importance of educational diversity).

[19] *See infra* note 44 and accompanying text for a further discussion of Sander's mistaken conclusion that discrimination cannot be a factor in law student performance.

[20] Sander, *supra* note 5, at 468–78.

[21] *See generally* Ayers & Brooks, *supra* note 15 (analyzing the LSAC data by comparing the performances of black and white law students who have similar credentials and matriculate at the same tier of school); David L. Chambers, Timothy T. Clydesdale, William C. Kidder & Richard O. Lempert, *The Real Impact of Eliminating Affirmative Action in American Law Schools: An Empirical Critique of Richard Sander's Study*, 57 STAN. L. REV. 1855 (2005) (reanalyzing the data, correcting several mistakes that Sander makes in coding the data); Daniel E. Ho, Scholarship Comment, *Why Affirmative Action Does Not Cause Black Student to Fail the Bar*, 114 YALE L.J. 1997 (2005) [hereinafter Ho, *Scholarship Comment*]; Daniel E. Ho, *Affirmative Action's Affirmative Actions: A Reply to Sander*, 114 YALE L.J. 2011 (2005) [hereinafter Ho, *Affirmative Actions*]; Jesse Rothstein & Albert Yoon, *Mismatch in Law School* (Northwestern Law & Econ., Research Paper No. 881110, 2006), *available at* http://www.princeton.edu/~jrothst/rothstein-yoon_2006june15.pdf (using non-parametric reweighting to analyze the data); Daniel E. Ho, Evaluating Affirmative Action in American Law Schools: Does Attending a Better Law School Cause Black Students to Fail the Bar (Mar. 9, 2005) (unpublished manuscript), *available at* http://people.iq.harvard.edu/~dho/research/sander.pdf [hereinafter Ho, *Social Science Critique*] (using propensity score matching to analyze the data).

[22] *See generally* andré douglas pond cummings, *"Open Water": Affirmative Action, Mismatch Theory and Swarming Predators: A Response to Richard Sander*, 44 BRANDEIS L.J. 795 (2006) (argu-

authors have written at least twelve articles, comments, and rebuttals discussing Sander's findings.[23]

In this Essay, I focus on the two theories that seek to explain black students' depressed achievement in law school: the mismatch theory and the race-based barriers theory. Although Sander focuses solely on the achievement gap between black and white law students, and his critics generally follow suit, these two theories have the same implications for other minority students. Thus, I also investigate how affirmative action affects the achievement of other groups of minority students. I take as a starting point Sander's argument and his critics' empirical critiques. All of these prior researchers attempt to answer whether mismatch is a problem for black students in law schools. Unfortunately, their methods of testing the mismatch theory suffer from a misunderstanding of what the mismatch theory is, and therefore confound the effects of mismatch and discrimination in their empirical tests. One goal of this Essay is to clarify what the mismatch theory implies about the relative performance of blacks and whites in law school. In doing so, I hope to clarify the empirical debate between Sander and his critics.[24] For all of his statistical models and 116 pages of discussion, Sander's conclusion is based upon a very straightforward analysis. Similarly, Sander's critics focus on only a few methodological flaws of Sander's

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ing, inter alia, that Sander's article is paternalistic and normatively flawed because it does not take into account many positive aspects of affirmative action); Michele Landis Dauber, *The Big Muddy*, 57 STAN. L. REV. 1899 (2005) (arguing, inter alia, that Sander's assumption, that absent affirmative action black and white law students would perform similarly, is seriously flawed, and that his poor analysis should not have been published because it did not satisfy peer review and replication standards); Kevin R. Johnson & Angela Onwuanchi-Willig, *Cry Me A River: The Limits of "A Systemic Analysis of Affirmative Action in American Law Schools,"* 7 AFR.-AM. L. & POL'Y REP. 1 (2005) (noting that other explanations for the performance gap between black and white law students are hostile learning environment and the extensive community service that black law students perform); Beverly I. Moran, *The Case for Black Inferiority? What Must be True if Professor Sander is Right: A Response to a Systemic Analysis of Affirmative Action in American Law Schools*, 5 CONN. PUB. INT. L.J. 41 (2005) (listing six "truths"—logical leaps in his argument—that Sander makes, but fails to justify); Wilkins, *supra* note 6 (focusing on the value of having black law students graduate from elite institutions). Sander's critics do not always defend affirmative action explicitly, but in evaluating Sander's methodology and results, it is clear that his critics take as a baseline the use of affirmative action. By doing so, Sander's critics require that any deviation from current affirmative action policy be justified by empirical proof. Even the title of the response of Chambers et al., *The Real Impact of Eliminating Affirmative Action,* emphasizes that affirmative action is the baseline from which there might be a policy change. Chambers et al., *supra* note 21.

23 *See* Ayers & Brooks, *supra* note 15; Chambers et al., *supra* note 21; cummings, *supra* note 22; Dauber, *supra* note 22; Johnson & Onwuanchi-Willig, *supra* note 22; Moran, *supra* note 22; Richard H. Sander, *Mismeasuring the Mismatch*, 114 YALE L.J. 2005 (2005) [hereinafter Sander, *Mismeasuring*]; Richard H. Sander, *A Reply to Critics*, 57 STAN. L. REV. 1963 (2005) [hereinafter Sander, *Reply*]; Wilkins, *supra* note 6; Ho, *Scholarship Comment*, *supra* note 21; Ho, *Affirmative Actions*, *supra* note 21; Rothstein & Yoon, *supra* note 21; Ho, *Social Science Critique*, *supra* note 21.

24 To date, I have been unable to find a published article or working paper in an academic venue that defends Sander's work, other than his own. *See* Sander, *Mismeasuring*, *supra* note 23; Sander, *Reply*, *supra* note 23.

analysis, although they do so using very different methods. Thus, an additional goal of this Essay is to simplify the debate.

This Essay proceeds as follows. Part I begins by discussing the mismatch and discrimination theories, and provides a template for testing both theories. To clarify what the implications of the mismatch and discrimination theories are in the context of affirmative action, I contrast my testing design against the designs of other researchers who have also tested mismatch in law schools. These researchers all use tests of the mismatch theory that confound race-based barriers and mismatch theories, making their results suspect. Part I concludes with the results of the tests I propose on three performance measures: graduation status, bar passage, and obtaining a well-paying job.

In Part II, I focus on potential counterfactuals: What would happen if affirmative action did not exist, or if alternative affirmative action policies were in place? I simulate different admission policies, all of which utilize some combination of undergraduate grade point averages (UGPA), Law School Admission Test (LSAT) scores, and race.

In Part III, I discuss the data in depth, and the reasons why all the analyses to date are incomplete and cannot fully answer the questions at hand. I conclude by providing a blueprint of what data would be necessary to estimate the effects of affirmative action on black students' bar passage rates, and a model of law student matriculation that would provide insight into what the appropriate policy responses to the troubling data are. Because affirmative action is a divisive policy with entrenched positions on either side, I advocate making the data available in order to determine appropriate policy and move the debate beyond rhetoric and into the practical results of affirmative action.

I. TESTING THE THEORIES

In order to clarify issues that often can be muddled in the context of affirmative action, I first describe the mismatch theory in more detail. The mismatch theory is that students who are outmatched in class disengage from the learning process and, in turn, do not learn as much as those students with the same credentials[25] who matriculate to schools at which they are not outmatched. Thus, the mismatch theory predicts that when compar-

[25] As I use the phrase, "credentials" or "student credentials" is a broad term encompassing any information known to admissions officers that might impact law school performance. LSAT and UGPA are two objective measures of student credentials, but other variables, ranging from difficulty of undergraduate program to maturity level to family support network, may impact law school performance. Consistent with my broad definition, the phrase "student credentials" encompasses all of these variables. In this discussion, I assume that one can accurately measure student credentials, although the empirical tests I report use only the imperfect measures of LSAT and UGPA. It is, however, theoretically possible to measure student credentials, even when broadly construed to include any attribute that will affect student performance. *See infra* Part III.B.

ing students with the same credentials across schools, those who are outmatched at higher-ranked schools will learn less and therefore perform worse on assessments that measure what they have learned (e.g., graduation or bar-passage rates). The mismatch theory also postulates that, to the extent that other measures of success, such as a well-paying or prestigious job, are related to the amount of information a student learns in law school, outmatched students will also fall behind in these areas. Note, however, that mismatch has nothing to do with race: All students who matriculate to a law school where their classmates outmatch them are vulnerable to disengaging and falling behind. Sander's critics implicitly discuss mismatch as if it is a problem only for black students because of affirmative action,[26] but affirmative action and the mismatch theory are not necessarily intertwined. Any preference based on something other than student credentials can potentially lead to mismatch. Thus, admissions based upon legacy, donor, and residency preferences are all examples of admissions criteria that could generate mismatch. Indeed, mismatch theory may apply to the very bottom group of law students in any class, no matter the details of the admissions policies.

Because mismatch theory is unrelated to race, testing whether mismatch occurs by making cross-race comparisons is inappropriate. Testing mismatch using cross-race comparisons confounds race-based differences in performance with effects due to mismatch, making the test results invalid.[27] Despite this, several researchers have tested mismatch by comparing the performance of white and black students with similar credentials.[28] This

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[26] Ayres & Brooks, *supra* note 15, at 1808–09; Ho, *Scholarship Comment*, *supra* note 21, at 1998, 2000; Rothstein & Yoon, *supra* note 21, at 3–4. Chambers et al. do not directly argue with the mismatch theory per se, but instead focus on the performance of *black* law students if affirmative action were to be eliminated. *See generally* Chambers et al., *supra* note 21. Ayres and Brooks recognize that the credentials of white and black students overlap, but do not explicitly state that this means that white students could be mismatched. Ayres & Brooks, *supra* note 15, at 1811–12, fig.1.

[27] Donald B. Rubin, *Estimating Causal Effects of Treatments in Randomized and Nonrandomized Studies*, 66 J. EDUC. PSYCHOL. 688, 692–93 (1974) (discussing variables that might confound the analysis in two examples, and arguing that causal relationships are difficult to prove absent randomization due to confounding).

[28] Ayres and Brooks, *supra* note 15, compare black and white students within a given tier of law schools. *Id.* at 1813–17. Rothstein and Yoon, *supra* note 21, compare within tier across race. *Id.* at 24–28. In addition, these researchers make other comparisons, some of which are more appropriate. Ho makes several comparisons: he compares those students who matriculate at top tier schools versus those who do not, both for all students and for the subset of only black students. Ho, *Social Science Critique*, *supra* note 21, at 9. In addition, Ho provides some detail on the same comparison for white law students. Ho, *Scholarship Comment*, *supra* note 21, at 2002. Finally, Ho uses propensity scores to create more complex comparison groups. Ho, *Social Science Critique*, *supra* note 21, at 9 & tbl.2, fig.5. Ayres and Brooks, *supra* note 15, also compare black students across tiers, *id.* at 1825, and Rothstein and Yoon, *supra* note 21, compare both within race across tier as well, *id.* at 24–28. Thus, many researchers compare black students who attend different school types, confounding different school cultures with mismatch, and, more troubling, white students with black students at the same school type. This latter comparison is based upon the demonstrably false assumption that white and black students

test is based on the assumption that black law students with the same credentials as white law students will, on average, matriculate at higher ranked schools. This test cannot differentiate between a performance gap due to mismatch and a performance gap due to law-school specific cultural differences that affect black and white students differently. Thus, an accurate test of the mismatch theory must control for student credentials.

After controlling for student credentials, the only variable relevant to determining mismatch is school rank. Thus, a comparison of black and white students with the same credentials within the same type of school tells one nothing about mismatch since both sets of students will be equally mismatched in the classroom. Any systematic difference between black and white students with the same credentials at the same schools must be due to some manifestation of discrimination.[29] After all, the implication is that after controlling for other variables (including student credentials), race is an important predictor of performance. Similarly, an intra-race comparison (comparing black students with the same credentials) across differently ranked schools also demonstrates a misunderstanding of the mismatch effect. At best, an intra-race comparison drops a large portion of the data and consequently loses statistical power to discern differences across school rank.[30] In addition, in making a black-black comparison, one implicitly makes the troubling and demonstrably counterfactual assumption that only the black students have poor enough credentials to be vulnerable to mismatch. Finally, if schools differ in their cultures and the effects thereof on black students, making a black-black comparison will confound the different potential effects of discrimination across schools with any potential mismatch effect. Ayres and Brooks, Ho, and Rothstein and Yoon attempt to test mismatch using black-black comparisons.[31] Tests of mismatch that study white students avoid this problem and appropriately test the mismatch theory, but do not provide as much insight into the effects of affirmative action specifically.[32]

Ideally, any test of the mismatch theory would simply compare the performance of students with the same absolute credentials across schools. If mismatch were present, students with the lowest credentials in a given

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with similar credentials attend different schools. While perhaps true on average, making this comparison confounds mismatch effects with discrimination effects.

29 Again, this assumes that student credentials are accurately measured; I discuss the consequences of biased measures of student credentials in Part III.

30 Power is a term of art in statistics that measures the ex ante probability of rejecting the null hypothesis of a test when the null hypothesis is in fact false. The power of a statistical test increases as sample size increases because with more data, one can discern the difference between random variation and a true effect more accurately.

31 *See* Ayres & Brooks, *supra* note 15, at 1828–38; Ho, *Social Science Critique*, *supra* note 21, at 9 & fig.4; Rothstein & Yoon, *supra* note 21, at 24–25.

32 Ho, *Scholarship Comment*, *supra* note 21, at 2002–04 & fig.1, provides some detail on the cross-tier comparison for white law students.

school would perform more poorly than students with the same credentials at lower ranked schools. In statistical terms, this would mean that in a regression, school-credentials interactions would be significant.[33] In other words, holding credentials constant, school type would be a predictor of performance for students whose credentials put them at risk of mismatch. In addition, the mismatch theory posits a specific direction in which the effect must work. Controlling for credentials, the mismatch theory predicts that matriculating to a higher ranked school lowers a student's performance if the student's credentials are sufficiently low to trigger mismatch. The statistical test of significance of the school * credentials interaction only tests whether the interaction terms affect performance or not; it does not test *how* the school-credentials interactions affect performance. Therefore, the test is only one-way: if the test is not significant, it demonstrates that mismatch is not supported by the data; if the test is significant, then one must investigate the direction of change—whether performance suffers from matriculating to a higher-ranked school, as mismatch requires, or whether a different pattern, such as reverse mismatch, emerges.

Ironically, considering all the ink spilled over testing the mismatch theory more appropriately than Sander does, it is only Sander's analysis that tests mismatch without using race. Sander does not, however, test the interaction of credentials and schools. Instead, he tests only whether the two variables separately affect student performance, implicitly assuming that all students who matriculate to a particular school get the same benefit of higher performance, regardless of credentials. For example, any performance benefit of matriculating to a particular school would be the same for students in the bottom tenth of the class and those in the top tenth of the class, as well as for all students in between. This assumption directly contradicts the mismatch theory, which posits that students with lower credentials see a negative impact on their performance by matriculating at a higher ranked law school. While Sander does not test the mismatch theory by using race, he implicitly assumes that only minority students can be mismatched,[34] and thereby also falls prey to the red herring that the mismatch

[33] Statistical models can contain "main" effects and "interaction" effects. "Main" effects estimate the average performance effect for the group. For example, the "main" effect of school type will estimate the average graduation rate for each school type separately, and the credentials "main" effects will estimate the average performance boost a student receives based upon a boost in their credentials. Without interaction terms, however, the model would assume that the effect of credentials upon performance and the effect of school type on performance is simply additive; the two do not interact at all. For example, students in a highly-ranked school would receive the same the same performance boost from being at that school whether their credentials suggested that they were mismatched or not. *See generally* JAMES JACCARD & ROBERT TURRISI, INTERACTION EFFECTS IN MULTIPLE REGRESSION (2d ed. 2003) (providing a description of when interaction effects are required and how to implement them in multiple regression models).

[34] Sander, *supra* note 5, at 452–53. This assumption is factually inaccurate. *See* Ayres & Brooks, *supra* note 15, at 1812 fig.1 (describing the overlap in credentials between black and white law students matriculating at schools in the same tier).

theory somehow relates to race. In addition, Sander's analysis is seriously flawed because he assumes, without adequate empirical support, that discriminatory law school culture does not influence black achievement in law schools.[35]

While the mismatch theory is unrelated to race, the race-based barriers theory is explicitly related to race. The phrase "race-based barriers," as I use it, is a somewhat fluid concept that incorporates many types of behavior. I use this phrase to signal that the specific cultures of individual law schools are quite different and, therefore, treatment that differs across race (intentionally or not) may also be quite different across schools. Race-based barriers include: hostile learning environment,[36] direct discrimination in outcomes,[37] and other issues like stereotype threat.[38] Most of these race-based barriers are school-specific and are based upon the individual law school's culture. However, other barriers, such as stereotype threat, implicate a broader societal stereotype and therefore generally are not school specific.[39]

Because race-based barriers may be general or school-specific, statistical testing of discrimination should allow for both possibilities. Statistical tests can accomplish this by testing race * school interactions for school-specific race-based differences, as well as race main effects (which measure the average performance gaps between racial groups across all schools) for more general race-based differences.[40] Unfortunately, without data on the cultures of specific schools or students' experiences during law school,[41] one cannot differentiate between the different types of barriers listed above. The question one can ask is this: After controlling for possible mismatch

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[35] Sander, *supra* note 5, at 439 tbl.5.6, 444 tbl.6.1 (providing Sander's empirical argument); Ho, *Scholarship Comment*, *supra* note 21, at 2000 (criticizing Sander's empirical analysis because of post-treatment bias).

[36] Title IX provides a cause of action for hostile learning environment. *See, e.g.*, Davis v. Monroe County Bd. of Educ., 74 F.3d 1186 (11th Cir. 1996), *aff'd* 526 U.S. 629 (1999) (finding that Title IX provides a cause of action for hostile learning environment in the context of sex discrimination); Bonnell v. Lorenzo, 241 F.3d 800 (6th Cir. 2001) (recognizing a school's interest in curtailing a professor's speech in order to prevent hostile learning environment, also in the context of sex discrimination).

[37] Direct discrimination in outcomes could include class opportunities, job opportunities, research fellowships, and non-anonymous final grades.

[38] In stereotype threat, an individual student incorporates the stereotype that he or she will perform poorly in a specific situation (for example, a final exam), and because of that stereotype, actually performs worse than should be expected given the knowledge the student has obtained. *See generally* Steele & Aronson, *supra* note 11 (describing the stereotype threat theory).

[39] Because stereotype threat may be counteracted by simple statements suggesting that the test administered is unbiased, in some situations stereotype threat may appear to be a school-specific phenomenon, as some schools counteract the threat while others do not. Smith, *supra* note 11, at 181–82 (noting that the stereotype threat effect was nullified when a black professor introduced a test by stating that the test was "the first step in an attempt to develop a culturally unbiased test").

[40] See *supra* note 33 for further discussion of main and interaction effects.

[41] The Educational Diversity Project, *supra* note 3, hopes to provide just such data on student experiences in law school; analysis of the data is still ongoing.

and student credentials, does race predict performance? Put this way, it is clear that the test cannot determine whether race-based barriers exist; it can only demonstrate whether race is an important predictor of performance. If race is an important predictor of performance, then race-based barriers might be the cause.

An alternative explanation for race as a predictor of performance is that unmeasured student credentials[42] differ across race. If this is the case, these unmeasured credentials will be confounded with race, making the variable of race appear to be an important predictor of performance when, in fact, it is the unmeasured student credentials that are important predictors.[43] Because the data available focus primarily on pre-law school admissions and post-law school outcomes, they provide little information about student experiences during law school. Thus, only a rough statistical test of the race-based barriers theory is possible, even under the assumption that unmeasured student credentials are not strongly correlated with race.

In his analysis of affirmative action and mismatch in law school, Sander attempts to counter the argument that race matters in law school performance. Sander does so in order to argue that mismatch must explain the weaker performance of black students. Sander does not, however, allow for race * school interactions, implicitly assuming that individual school cultures are unimportant. If this assumption is incorrect, the model produces biased and inaccurate results. Various other restrictive modeling assumptions, described in detail elsewhere,[44] make his tests highly suspect. For example, Chambers and his coauthors demonstrate that Sander's conclusion that race is not a factor in student performance misclassifies students who

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[42] Recall that student credentials are attributes that affect student performance about which admissions offices have information. *See supra* note 25. Because applications for admission cannot include every possible attribute that might affect student performance, there will always be unmeasured attributes that affect student performance, that is, unmeasured credentials. Many of these will be "soft" variables: ability to work with others well and to persevere, familial support, familiarity with the law and lawyers, etc.

[43] This does not rule out the possibility that race is an important predictor because of discriminatory barriers as well as unmeasured student credentials.

[44] See Chambers et al., *supra* note 21, for a complete description of the poor modeling choices that Sander makes. In brief, Chambers et al. focus on five criticisms. First, Sander measures eliteness (or "tier") as a linear variable, implicitly assuming that the difference between the top and second tiers is equal to the difference between the second and third tiers. *Id.* at 1872–73. Second, in one of the data sets Sander uses, Sander coded all individuals who declined to provide racial data as white. This is inappropriate, and other methods for dealing with missing values provide substantially different results. *Id.* at 1878–79. Third, Sander uses 2001 data to predict trends for 2004; Chambers et al. argue that 2001 and the adjacent years were anomalous, and note that the pool of potential law students has changed significantly enough in the past three years to call this assumption into question. *Id.* at 1860–61 & tbl.1. Fourth, Sander misuses test statistics, *id.* at 1869–71, and as a result, makes overbroad assertions about his results. Finally, Chambers et al. look beyond Sander's analysis to argue that the pool of law school applicants is not a fixed entity that would remain unchanged absent affirmative action: Black students, when assessing their options, may be less likely to attend law school if it is a less attractive option to them. *Id.* at 1862–68.

decline to provide their racial background as white students. Reanalyzing the data based on a more appropriate classification demonstrates that race is a factor in law students' performance.[45]

Thus, the appropriate statistical test for the mismatch theory is to determine if school * credentials interactions are statistically significant predictors of performance. If these interactions are statistically significant, then school rank will affect similarly credentialed students differently, which is the key observable characteristic of the mismatch theory. Similarly, the correct statistical test for the race-based barriers theory is to determine if race and school * race interactions are statistically significant predictors of performance after controlling for student credentials.

Both the mismatch and racial barriers tests are rendered difficult by two measurement issues.[46] First, because of privacy concerns, the Law School Admission Council data (LSAC data) do not contain a record of which school each student attends. Instead, the participating schools are grouped into six clusters based on criteria such as school size, percentage of minority students, cost, and average student LSAT and UGPA scores. Sander reorders the clusters into a rank based upon the average student LSAT. Labeling the resulting variable "tier," Sander asserts that while not ideal, "tier" is a good proxy for school rank.[47] Sander's critics follow suit,[48] taking issue only with the specific form in which the ranking is used, but not with the ranking itself. The creators of the data, however, specifically warn against using clusters as a rank order of schools.[49] Because of this, I label the clusters "school type" and, where possible, do not rely on any ranking system.

In order to develop a reasonable measure for the rank of school attended, which testing the mismatch theory requires, I am forced to follow earlier researchers by using what I have named "school type" as a proxy for rank. I recognize that the results have limited applicability as a consequence.[50] The school types, based on the six clusters in which LSAC researchers grouped the schools, are best described as Small Top 30 law

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[45] *Id.* at 1878–79.

[46] Luckily, a third important variable, race, is accurately measured in the data and therefore does not present a problem. As Chambers et al. point out, Sander also uses a different set that does have measurement problems with respect to the race variable. *Id.* at 1878–79. As Chambers et al. demonstrate, how one controls for this data problem affects the results of the estimation. *Id.*

[47] *See* Sander, *supra* note 5, at 415 ("[School tiers] correspond roughly to tiers of law school prestige.").

[48] Ayres & Brooks, *supra* note 15, at 1812, 1817; Chambers et al., *supra* note 21, at 1884; Rothstein & Yoon, *supra* note 21, at tbl.1; Ho, *Social Science Critique*, *supra* note 21, at 10 n.6.

[49] LINDA F. WIGHTMAN, USER'S GUIDE: LSAC NATIONAL LONGITUDINAL DATA FILE 15 (1999), *available at* http://bpsdata.lsac.org/ [hereinafter WIGHTMAN, USER'S GUIDE] (file may be downloaded upon submitting personal information) ("There is no explicit or implicit rank ordering associated with the assigned cluster numbers.").

[50] In Part III, *infra*, I detail the additional data necessary to provide more accurate results.

schools, Large Top 30 law schools, Mid-range Public law schools, Mid-range Private law schools, Lower Ranked law schools, and Historically Black law schools.[51] I present them here in the rank order used by Sander and his critics, which is based upon the average LSAT score for each cluster. Given these descriptions, there appears to be significant overlap in the ranks of schools between the top two clusters (Small Top 30 and Large Top 30 law schools) and the mid-range clusters (Mid-range Public and Mid-range Private law schools). This overlap can mask the effects of mismatch because an individual mismatched at a higher ranked school might, in fact, be in a lower cluster. Thus, the overlap means that the data mismeasures rank in many cases. Because such overlap can mask the effects of mismatch, I collapse the school types into four: Top 30 Schools, Mid-range Schools, Low-range Schools, and Historically Black Schools.[52]

Second, the data do not contain information about all possible student credentials. Generating a reasonable measure for student credentials is more complicated than it is for school rank. The two objective variables included in the LSAC data are LSAT score and UGPA. At base, all researchers, myself included, measure student credentials as a function of LSAT and UGPA scores: $Credentials = f(LSAT, UGPA)$.

Flexibility when using LSAT and UGPA to measure credentials is important because it allows LSAT and UGPA to affect performance in nonlinear ways. Sander provides the least flexible definition of credentials: a fixed linear combination ($Credentials = 0.6 \times LSAT + 0.4 \times UGPA$), which he terms an "academic index."[53] This academic index assumes that credentials are linear in form—that the difference between an UGPA of 3.3 and 3.4 is the same as the difference between 3.9 and 4.0. All prior researchers, except Ho, rely on a similar academic index as the measure of student cre-

[51] *See* LINDA F. WIGHTMAN, LSAC NATIONAL LONGITUDINAL BAR PASSAGE STUDY, LSAC RESEARCH REPORT SERIES 8–9 (1998) [hereinafter WIGHTMAN, LSAC STUDY] (describing the school clusters by various demographic characteristics, such as percentage of institutions in the cluster that are public and percentage of minority students in the cluster).

[52] I have also performed the analysis with the original school type structure. The results from Part II are substantially similar; the results from Part I differ in two ways: first, the racial barriers tests demonstrate that each of the six school types have different average school cultures that affect minority students differently; second, the mismatch tests show no discernible pattern (either mismatch or reverse mismatch), likely because of the overlap in school ranks across school types. Tables for this alternative analysis are available from the author upon request.

[53] To put LSAT and UGPA on the same scale, Sander first normalizes both variables, so the full function is:

$$Credentials = 0.6 \times \frac{1000 \times (LSAT - 10)}{38} + 0.4 \times \frac{1000 \times UGPA}{4}$$

Sander, *supra* note 5, at 393. My formula differs from that in Sander's article because Sander uses normalized values for LSAT and UGPA; the formula above does the normalization as well.

dentials.[54] However, they differ in the flexibility in which they allow this one-dimensional index to affect law student performance.

Rothstein and Yoon provide the most flexible form of the academic index by reweighting the data so that average academic index scores are the same across races. Their estimation of the entire distribution of academic index credentials does not depend on any particular functional form for the relationship between student achievement and academic index, and therefore allows for any relationship between academic index and student achievement. It still, however, assumes that the academic index, with its specific linear relationship between LSAT and UGPA, is the best way to measure student credentials. Rothstein and Yoon therefore trade having little flexibility in measuring student credentials for instead having complete flexibility in how the measured student credentials affect student achievement. I make a different trade-off, allowing both parts of the model to have significant, but not complete, flexibility. Thus, I achieve flexibility without assuming that LSAT and UGPA have a specific linear relationship to student credentials. I allow the student credentials measure to differ across different student performance measures, and assume that $f(\bullet)$ is a polynomial expression of order 3. This means that terms up to cubes enter into the function. Specifically[55]:

$$\begin{aligned} Credentials \quad = \quad & \beta_1 + \beta_2 LSAT + \beta_3 UGPA + \beta_4 LSAT \times UGPA + \beta_5 LSAT^2 + \beta_6 UGPA^2 + \\ & \beta_7 LSAT^2 \times UGPA + \beta_8 LSAT \times UGPA^2 + \beta_9 LSAT^3 + \beta_{10} UGPA^3 \end{aligned}$$ [56]

Theoretically, including all polynomial terms (through infinity) and interaction effects, one can estimate any potential function $f(\bullet)$, no matter how strange. Limiting the order of the polynomials to 3 (for a total of 10 variables) provides a reasonably flexible functional form for student credentials as measured by LSAT and UGPA. Allowing a flexible function for the measurement of credentials means that I do not assume the relationship be-

[54] Ayres & Brooks, *supra* note 15, at 1817 (using the same academic index that Sander creates); Chambers et al., *supra* note 21, at 1884 (using the same academic index that Sander creates); Rothstein & Yoon, *supra* note 21, at 3 (using the same academic index that Sander creates); Ho, *Social Science Critique*, *supra* note 21, at 5–6 (matching on LSAT and UGPA separately, or controlling for LSAT and UGPA via propensity scores).

[55] In practice, I do not estimate student credentials separately, but include this polynomial of LSAT and UGPA into each logistic regression of the achievement measure of interest.

[56] The β coefficients are estimated via logistic regression. The function allows significantly more flexibility than a linear or quadratic function. Specifically, it allows for very low credentials—those most at risk of being mismatched—to affect performance differently than all other credentials. This is the purpose of choosing at least a cubic polynomial: I investigated higher-ordered polynomials of degree 4 or 5, but found little additional benefit from these more complicated models. I also investigated how different my results would be if I used the academic index that Sander created; the results are substantially different. Using the less flexible form significantly underestimates the black student graduation rate and bar passage rate, as compared to the more flexible form.

tween student credentials and LSAT or UGPA is linear. As the LSAT is explicitly set on a nonlinear curve and many undergraduate institutions do not grade linearly, this is necessary to model student credentials accurately.

Thus, while somewhat problematic, I have devised the three variables—race, school type, and credentials—that I will use to test the mismatch and race-based barriers theories and how these theories affect various performance measures. I focus on three performance measures: bar passage, graduation, and obtaining a well-paying first job after law school. Although these performance measures are by no means exhaustive, they provide a snapshot of potential performance measures, are readily available in the data, and measure performance on goals that many law students share. In particular, to the extent that the affirmative action debate has focused on the yearly number of new black lawyers under different policies, bar passage is directly relevant. Graduation status, the second performance measure I use, is a goal of all entering law students and provides a measure of the number of black and other minority students who complete legal training each year, whether they become lawyers or not.

Finally, a well-paying first job (which I define as earning $40,000 or more in 1995)[57] is another goal many law students share. I choose this cutoff because I am not trying to measure only the most elite jobs.[58] In law, this would be impossible via salary data alone since many high-prestige jobs pay relatively poorly. Though salary is not an ideal measure, I include it for another reason: to provide some information about how affirmative action policies affect low-probability events. Most law students graduate from law school, and most law graduates who take a bar exam pass it eventually. But not all law students—indeed, only 15.1% of those students who responded to the question on the LSAC survey about jobs and salary—obtain well-paying jobs. Given that most of these well-paying jobs go to students at top institutions, this is an important benefit of matriculating to an elite institution. Unfortunately, this measure of student achievement is limited by a small sample response. Only 11.8% of students in the database completed this question on the LSAC survey. Thus, nonresponse bias is a real concern. The data provide some indication, however, of the overall probability of obtaining a well-paying job. As David Wilkins argues in his critique of Sander's argument, these lower-probability, high-prestige events matter because blacks will not become a larger part of the "elite" without access to such opportunities.[59]

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[57] This is approximately $51,260 in 2005 dollars. *See* Federal Reserve Bank of Minneapolis, Consumer Price Index Calculator, http://minneapolisfed.org/Research/data/us/calc (last visited Apr. 16, 2007).

[58] The cutoff is low enough that it includes some federal clerkships, depending on location. I have also investigated other cutoffs, including $50,000 in 1995 dollars ($64,075 in 2005 dollars). *Id.* The results do not change substantially, although fewer students meet this criterion in general.

[59] *See* Wilkins, *supra* note 6, at 1934–36 (describing the myriad ways that elite institutions train students to become a part of the elite law firm).

Tables 1A and 1B provide the results of a single model of graduation status.[60] First, Table 1A focuses on the mismatch test.[61] In order to make the results more accessible, I report the predicted probability of successful graduation from law school, which provides a sense of the substantive results from the model. In Table 1A, I use the probability of graduation for white students in Mid-range Schools as a baseline. From that baseline, I report the predicted change in the probability of graduation for students with LSAT and UGPA credentials fixed at the fifth, tenth, twenty-fifth, and fiftieth percentiles of the entire matriculant sample. The results show little support for the mismatch theory.[62] For the mismatch theory to be correct, the change in graduation rate should decline as one compares similarly credentialed students at higher ranked schools. Thus, according to the mismatch theory, for students at any given credential level, Historically Black Schools should have the highest graduation rate and should exhibit a large positive change in graduation rate when compared to the baseline graduation rate of white students at Mid-range Schools. Low-range Schools should have a smaller but still positive difference in graduation rates from the baseline. Finally, Top 30 Schools should have a negative difference in graduation rates from the baseline.

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[60] Specifically, the model is a logistic regression of graduation status on student credentials, type of school, race, race * school type interactions, and student credentials * school type interactions.

[61] Tables 1A through 3B all report results from logistic regressions of a performance measure on student credentials, type of school, race, race * school type interactions, and student credentials * school type interactions. The "A" tables report results relevant to the mismatch theory, and the "B" tables report results relevant to the race-based barriers theory. Because of the large number of interaction terms for each table, I report the significance level of a joint test whether or not the interactions are statistically significant. For the "A" tables, this is the mismatch theory test; for the "B" tables, this is the race-based barriers test. In addition, rather than report each parameter for the 27 (school type * credentials) interactions and the 12 (school type * race) interactions, I provide the change in estimated successful performance relative to a "baseline" group of students who are described in each panel. For example, the first three rows of Table 1A report the change in graduation rate for students at Historically Black, Low-range, and Top 30 Schools, respectively, as compared to a baseline of students who matriculated at Mid-range Schools, whose credentials are in the fifth percentile of the entire data set. These "baseline" students have an 83.3% chance of graduating from law school.

[62] Again, because of the serious limitations in the data described above, I do not want to overclaim. The results suggest that the mismatch and discrimination theories are likely in evidence, but they cannot demonstrate conclusively that either theory affects law students' graduation rates.

Table 1A: Logistic Regression of Graduation Rate Allowing for Mismatch and Discrimination—Results Relevant to Mismatch Testing

Variable	*ΔPr(Graduate)*[63]
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 5th percentile, with baseline probability of 83.3%)	
Historically Black Schools	-4.1%
Low-range Schools	-11.3%
Top 30 Schools	5.3%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 10th percentile, with baseline probability of 87.2%)	
Historically Black Schools	-4.8%
Low-range Schools	-9.8%
Top 30 Schools	4.4%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 25th percentile, with baseline probability of 89.7%)	
Historically Black Schools	-1.8%
Low-range Schools	-4.9%
Top 30 Schools	4.6%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 50th percentile, with baseline probability of 91.7%)	
Historically Black Schools	-0.1%
Low-range Schools	-0.8%
Top 30 Schools	4.3%
Overall School Type * Credentials Interactions (27 variables)[64]	p-value = 0.060

None of the comparison groups—with credentials at the fifth, tenth, twenty-fifth, or fiftieth percentiles—demonstrate this pattern. In contrast, for students below the fiftieth percentile, matriculating at a Low-range or Historically Black School *lowers* their chances of graduating from law school. In addition, students who matriculate to Top 30 Schools consistently exhibit a four to five percentage point increase in graduation rate over

[63] This number provides the change in probability between the given characteristic and the control, holding credentials at the specified value and race at its modal value, white.

[64] For these interaction terms, I provide only an overall likelihood ratio test to test the null hypothesis that all interactions are jointly equal to zero.

the baseline. Overall, these differences are marginally statistically significant, with a p-value of 0.060.[65] Top 30 Schools provide a consistent bump of an approximate 4–5% increase in the probability of graduation. Low-range Schools are particularly risky propositions for students with low-percentile credentials. Even after controlling for these credentials separately, students in the fifth and tenth percentiles have 11.3% and 9.8% lower probabilities of graduating than they would if they attended Mid-range Schools. With baseline probabilities of 88.3% and 87.2%, respectively, this is a significant difference. The only comparison between school types that supports the mismatch theory is that white students have a better chance of graduating from a Historically Black School than a Low-range School, particularly for those white students with the lowest credentials. However, this result appears in only one of six comparisons between school types, with only marginal significance of any differences across school types for students with different credentials. This single comparison therefore provides negligible support for the mismatch theory.

Table 1B provides the results for the race-based barriers tests by reporting comparisons between minority students of different races and white students and providing separate estimates at each possible school type. Specifically, I report the correlation between a student's race and likelihood of graduation, and the manner in which that correlation changes depending on the type of school the student attends. Here, I fix student credentials at their median values. The results indicate that race does matter, likely because different types of schools have different law school cultures that affect the learning experience of minority students. In particular, for all school types except Top 30 Schools, black students with the same credentials as white students are less likely to graduate. Compared to similarly credentialed white students, the black students attending Mid-range Schools are 3.5% less likely to graduate.

The results also show that minority students attending Historically Black Schools are less likely to graduate than white students. The graduation rate for black students at these institutions is 6.7 percentage points lower than the graduation rate for white students. This general trend is also present at Mid-range Schools, although the substantive difference is less pronounced. Top 30 Schools generally have a smaller difference between graduation rates for minority and white students. Hispanic students, with a 2.5 percentage point lower probability, provide the only substantive difference in graduation rates as compared to white students in Top 30 Schools. Overall, the test for race-based differences is statistically significant, with a

[65] A p-value measures how likely it is that the observed difference between the model estimates and a "null hypothesis" can be explained by random variation in the data. Traditionally, values less than 0.05 (on a scale of 0 to 1) are considered statistically significant; values between 0.10 and 0.05 are considered marginally significant. For Table 1A, the null hypothesis is that mismatch theory does not affect student graduation rates.

p-value of less than 0.001. In general, the race-based differences are smaller than the differences between school types, suggesting that the school type at which a student matriculates is more important to success than the race of the student.

Table 1B: Logistic Regression of Graduation Rate Allowing for Mismatch and Discrimination—Results Relevant to Discrimination Testing

Variable	*ΔPr(Graduate)*[66]
Student Race (compare to White) (control: Historically Black Schools, with baseline probability of 91.5%)	
Black	-6.7%
Hispanic	-6.4%
Asian	-3.7%
Student Race (compare to White) (control: Low-range Schools, with baseline probability of 90.9%)	
Black	-1.5%
Hispanic	1.9%
Asian	5.1%
Student Race (compare to White) (control: Mid-range Schools, with baseline probability of 91.7%)	
Black	-3.5%
Hispanic	-1.1%
Asian	-1.5%
Student Race (compare to White) (control: Top 30 Schools, with baseline probability of 95.9%)	
Black	0%
Hispanic	-2.5%
Asian	0.6%
Overall School Type * Race Interactions (12 variables)	p-value = 0.001

Moving to the results for bar passage, the overall test of the school type * credentials is statistically significant. Table 2A provides the results for

[66] This number provides the change in probability between the given characteristic and the control, holding all other factors at their median or modal value (LSAT = 37; UGPA = 3.3; race = white).

each pairing, and provides strong evidence that law school is a risky proposition for law students with low-percentile credentials, assuming that those students hope to pass the bar exam. This risky proposition is made much riskier by matriculating at a school outside of the Top 30 grouping: For students with credentials in the fifth and tenth percentiles, matriculation at a Historically Black or Mid-range School lowers the probability of passing the bar by 50.1 and 46.8 percentage points, respectively. These effects are huge. Even comparing Historically Black Schools with the next highest rank, Low-range Schools, the difference in probability of bar passage is 20 percentage points for these students.[67]

Even if these stark numbers are only suggestive of what true estimates using better data would be, the results are disturbing. These results, however, must be tempered with the caveat that I do not control for bar exam difficulty, which varies widely across states. Although it would be ideal to include as a variable the state in which a student takes a bar exam, the data do not exist. To the extent students from different racial groups cluster to take the bar exams for specific states, bar exam difficulty may be confounded with group status. For example, Historically Black Schools are concentrated in the South; their graduates may be more likely to take bar exams in the South as well, in which case bar exam difficulty may be confounded with school type.

Table 2A also demonstrates a consistent reverse mismatch trend: the lower ranked the school type, the lower the probability that a student will pass the bar exam. No matter what a student's credentials, students matriculating to Top 30 Schools are most likely to pass the bar, while students at Historically Black Schools are least likely. Thus, the results suggest that students who attend more demanding schools rise to the challenge of competing with their classmates rather than disengaging from the learning process.

[67] Specifically, the difference is 28 percentage points for those in the fifth percentile of student credentials, and 20 percentage points for those in the tenth percentile. In a logistic regression, the change in probabilities is not additive, because of the logistic functional form. Thus, to compare Historically Black Schools to schools other than Mid-range Schools (the baseline), one has to return to the underlying coefficients of the model.

Table 2A: Logistic Regression of Bar Passage Rate Allowing for Mismatch and Discrimination—Results Relevant to Mismatch Testing

Variable	*ΔPr(Pass Bar)*[68]
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 5th percentile, With baseline probability of 63.0%)	
Historically Black Schools	-50.1%
Low-range Schools	-16.2%
Top 30 Schools	1.7%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 10th percentile, With baseline probability of 73.8%)	
Historically Black Schools	-46.8%
Low-range Schools	-15.1%
Top 30 Schools	1.9%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 25th percentile, With baseline probability of 80.3%)	
Historically Black Schools	-17.9%
Low-range Schools	-9.1%
Top 30 Schools	3.5%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 50th percentile, With baseline probability of 84.8%)	
Historically Black Schools	-7.6%
Low-range Schools	-6.6%
Top 30 Schools	3.5%
Overall School Type * Credentials Interactions (27 variables)	p-value = 0.006

Turning to Table 2B and the race-based barriers theory, the statistical test again finds that race and race * school type interactions are significant. As with graduation rates, Historically Black Schools demonstrate the largest difference between white and minority student bar passage rates, with minority students having a bar passage probability of 7.1 to 26 percentage points lower than white students. These drops in bar passage rates are very different across races, making it clear that law school cultures affect different minority groups differently. In contrast, for Mid-range Schools, the

[68] This number provides the change in probability between the given characteristic and the control, holding credentials at the specified value and race at its modal value, white.

drop in bar passage rates for minority students is more consistent across races (from 5.8 to 8.6 percentage points). Once again, minority students attending Top 30 Schools are the least likely of all minority students to experience a racial barrier to bar passage. As with graduation status, the race-based differences are smaller than the school type differences on average.

Table 2B: Logistic Regression of Bar Passage Rate Allowing for Mismatch and Discrimination—Results Relevant to Discrimination Testing

Variable	*ΔPr(Pass Bar)*[69]
Student Race (compare to White) (control: Historically Black Schools, With baseline probability of 77.1%)	
Black	-11.9%
Hispanic	-7.1%
Asian	-26.0%
Student Race (compare to White) (control: Low-range Schools, With baseline probability of 78.1%)	
Black	-7.7%
Hispanic	5.6%
Asian	-0.8%
Student Race (compare to White) (control: Mid-range Schools, With baseline probability of 84.8%)	
Black	-8.6%
Hispanic	-5.8%
Asian	-6.4%
Student Race (compare to White) (control: Top 30 Schools, With baseline probability of 88.3%)	
Black	-0.9%
Hispanic	-1.6%
Asian	1.0%
Overall School Type * Race Interactions (12 variables)	p-value = 0.000

[69] This number provides the change in probability between the given characteristic and the control, holding all other factors at their median or modal value (LSAT = 37; UGPA = 3.3; race = white).

Finally, I test the mismatch and race-based barriers theories with respect to obtaining a well-paying job. Table 3A provides the results for the mismatch test. With a p-value of 0.220, the data provide no support for Sander's mismatch theory, or the more general test that school type * credentials is statistically significant. These results rely on a much smaller sample size, and therefore have a lower ability to discern true effects.

Table 3A: Logistic Regression of Well-Paying Job Rate Allowing for Mismatch and Discrimination—Results Relevant to Mismatch Testing

Variable	*ΔPr(High Salary)*[70]
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 5th percentile, with baseline probability of 6.1%)	
Historically Black Schools	-5.0%
Low-range Schools	-2.7%
Top 30 Schools	2.4%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 10th percentile, with baseline probability of 7.6%)	
Historically Black Schools	-7.1%
Low-range Schools	-2.3%
Top 30 Schools	2.1%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 25th percentile, with baseline probability of 9.6%)	
Historically Black Schools	-9.4%
Low-range Schools	-0.7%
Top 30 Schools	7.1%
Mismatch (compare to Mid-range Schools) (Fixed student credentials at 50th percentile, with baseline probability of 14.1%)	
Historically Black Schools	-13.4%
Low-range Schools	-5.0%
Top 30 Schools	20.7%
Overall School Type * Credentials Interactions (27 variables)	p-value = 0.220

[70] This number provides the change in probability between the given characteristic and the control, holding credentials at the specified value and race at its modal value, white.

Table 3B provides the results for race-based differences in well-paying job rates. Despite the smaller sample size, Table 3B shows that, in general, minority students are more likely to obtain well-paying jobs than white students across all school types.[71] This result provides strong support for Wilkins's argument that affirmative action provides access to elite jobs for minority students, particularly at the most elite schools, where the probability of obtaining a well-paying job is largest by far.[72] This result may also reflect the different average monetary situations of minority and white students. Because minority students have larger student loans on average,[73] they may be more likely to choose a well-paying job than a white student who has the same job opportunities but can afford (and prefers) to take a job that pays less. Controlling for family wealth as a proxy for one's ability to pay for law school would alleviate this potential problem with the data. Unfortunately, the data on family wealth in the LSAC database is spotty at best.

[71] In addition to a small sample size, the data on salaries has a low response rate. In all, approximately 3251 of 27,458 students responded, for a response rate of 11.8%. This means two things: first, there is significantly less power to determine differences (particularly for those groups that are a relatively small percentage of the entire data set), and second, that the results are further suspect because of the possibility that the data are missing non-randomly (that is, that a student's characteristics—like race or school type—would affect their probability of answering the salary question in the first place).

[72] *See* Wilkins, *supra* note 6, at 1936.

[73] GITA Z. WILDER, LAW SCHOOL DEBT AMONG NEW LAWYERS: AN AJD MONOGRAPH 3 (2007), *available at* http://www.nalp.org/assets/645_ajddebtmonograph2007final.pdf (demonstrating that Hispanic students have higher median and mean debt levels, and black law students have higher mean debt levels). Black and Hispanic law students are also significantly more like to graduate with debt than white law students. *Id.* at 9.

Table 3B: Logistic Regression of Well-Paying Job Rate Allowing for Mismatch and Discrimination—Results Relevant to Discrimination Testing

Variable	*ΔPr(High Salary)*[74]
Student Race (compare to White) (control: Historically Black Schools, with baseline probability of 0.7%)	
Black	7.6%
Hispanic	0.1%
Asian	†
Student Race (compare to White) (control: Low-range Schools, with baseline probability of 9.1%)	
Black	11.3%
Hispanic	2.1%
Asian	†
Student Race (compare to White) (control: Mid-range Schools, with baseline probability of 14.1%)	
Black	7.9%
Hispanic	5.9%
Asian	6.1%
Student Race (compare to White) (control: Top 30 Schools, with baseline probability of 34.8%)	
Black	29.7%
Hispanic	9.7%
Asian	6.3%
Overall School Type * Race Interactions (12 variables)	p-value = 0.000

† Insufficient variation in the data to estimate this parameter.

The results I have presented thus far look separately at the mismatch and race-based barriers theories, and seek to disentangle the two effects. Tables 4 and 5, however, reunite the two effects and focus on black students. The tables seek to answer the more direct question: What advice should one give a black undergraduate student in deciding where to go to law school? The typical advice is "go to the best school you get into."

[74] This number provides the change in probability between the given characteristic and the control, holding all other factors at their median or modal value (LSAT = 37; UGPA = 3.3; race = white).

Based on mismatch theory, Sander argues that for black applicants, this is bad advice.

Tables 4 and 5 report the graduation and bar passage rates for black students with different credentials.[75] Each of these tables demonstrates clearly that black students should indeed be encouraged to matriculate to the highest rank school to which they are admitted. For each given set of credentials, black students have higher graduation rates and bar passage rates at higher ranked schools. These effects are consistent across different levels of credentials and for every school type. Table 6 provides the results for well-paying jobs. These results are less stark, as the probability of obtaining a well-paying job does not increase monotonically across school types for all sets of credentials. Still, a poorly credentialed black student's best chance of getting a well-paying job is to go to a Top 30 School, if admitted.

[75] Unlike Tables 1A through 3B, Tables 4 and 5 report the actual graduation or bar passage rate, rather than the change in the rate relative to the baseline.

*Table 4: Logistic Regression of Graduation Rate Allowing for Mismatch and Discrimination—Combination of Mismatch and Discrimination Theories for Black Student*s

Variable	*Pr(Graduate)*[76]
Fixed student credentials at 5th percentile	
Historically Black Schools	66.3%
Low-range Schools	68.5%
Mid-range Schools	77.0%
Top 30 Schools	88.5%
Fixed student credentials at 10th percentile	
Historically Black Schools	70.9%
Low-range Schools	74.4%
Mid-range Schools	82.2%
Top 30 Schools	91.6%
Fixed student credentials at 25th percentile	
Historically Black Schools	79.0%
Low-range Schools	82.6%
Mid-range Schools	85.4%
Top 30 Schools	94.3%
Fixed student credentials at 50th percentile	
Historically Black Schools	84.9%
Low-range Schools	89.4%
Mid-range Schools	88.1%
Top 30 Schools	95.9%

[76] This number provides the absolute probability of graduation for black students, holding credentials at the specified value.

Table 5: Logistic Regression of Bar Passage Rate Allowing for Mismatch and Discrimination—Combination of Mismatch and Discrimination Theories for Black Students

Variable	*Pr(Pass Bar)*[77]
Fixed student credentials at 5th percentile	
Historically Black Schools	7.6%
Low-range Schools	36.9%
Mid-range Schools	49.5%
Top 30 Schools	62.7%
Fixed student credentials at 10th percentile	
Historically Black Schools	17.0%
Low-range Schools	48.6%
Mid-range Schools	61.8%
Top 30 Schools	74.1%
Fixed student credentials at 25th percentile	
Historically Black Schools	48.0%
Low-range Schools	62.2%
Mid-range Schools	70.1%
Top 30 Schools	82.6%
Fixed student credentials at 50th percentile	
Historically Black Schools	65.2%
Low-range Schools	70.3%
Mid-range Schools	76.1%
Top 30 Schools	87.3%

[77] This number provides the absolute probability of passing the bar for black students, holding credentials at the specified value.

Table 6: Logistic Regression of Well-Paying Job Rate Allowing for Mismatch and Discrimination—Combination of Mismatch and Discrimination Theories for Black Students

Variable	*Pr(Well-Paying Job)*[78]
Fixed student credentials at 5th percentile	
Historically Black Schools	12.7%
Low-range Schools	8.1%
Mid-range Schools	10.0%
Top 30 Schools	24.0%
Fixed student credentials at 10th percentile	
Historically Black Schools	6.1%
Low-range Schools	12.5%
Mid-range Schools	12.4%
Top 30 Schools	26.8%
Fixed student credentials at 25th percentile	
Historically Black Schools	3.2%
Low-range Schools	20.0%
Mid-range Schools	15.5%
Top 30 Schools	40.5%
Fixed student credentials at 50th percentile	
Historically Black Schools	8.3%
Low-range Schools	20.3%
Mid-range Schools	22.0%
Top 30 Schools	64.5%

In sum, the three counterfactual models presented in this paper refute Sander's mismatch theory that students are more successful at lower ranked schools. In fact, the three models suggest the opposite: Matriculating to higher ranked schools provides significant benefits. The results regarding race-based barriers are more mixed. In some cases, there seems to be at least the suggestion of some sort of race-based barriers; the results for Historically Black Schools are particularly stark.[79] Mid-range Schools also have consistently lower bar passage and graduation rates for minority stu-

[78] This number provides the absolute probability of obtaining a well-paying job for black students, holding credentials at the specified value.

[79] Historically Black Schools are, as others have recognized, a special case. *See* Sander, *supra* note 5, at 416; Ayres & Brooks, *supra* note 15, at 1825–26. Because of this, it is important not to overemphasize the results of the model with respect to these schools. However, this result may be in part due to the greater diversity in these schools, differentially leading to higher white student performance. This is mere speculation; research into different law school cultures and how they affect student performance is outside the scope of this Essay.

dents. Top 30 Schools have fewer differences, except with respect to well-paying jobs, where they provide a significant boost for black students in particular, but also to a lesser extent for Hispanic and Asian students.

II. SIMULATING ALTERNATIVE AFFIRMATIVE ACTION POLICIES

In Part I, I propose and develop models to test whether, and to what extent, mismatch and race-based barriers theories accurately describe students' achievement in law school. In this Part, I move beyond current practice to investigate what might happen under alternative policies of affirmative action. Investigating counterfactual scenarios—what could have happened, but did not—is difficult. It requires making several assumptions about how the world would not have changed, as well as how it would have.

Before proceeding to the analysis, it is important to state my assumptions. I assume that the models of law student performance I estimate in Part I still hold. This is equivalent to assuming that any mismatch or race-based barriers problems that are currently at play will not change. For example, I assume that the effect of school cultures on minority students will not change much with the addition—or subtraction—of other minority students. I must also assume that application patterns will not change, e.g., that black students will still apply to law schools at the same rate, although the value of law school as compared to other alternatives may have changed. These assumptions do not perfectly mirror the more complicated real world changes that would occur if the alternative affirmative action policies were implemented, but they provide conservative estimates of the results that would stem from each potential policy. The projected effects on black students will accordingly be underestimated.[80] As a basis for comparison, I also provide an estimate of the effect of removing affirmative action policies based on Sander's grid model,[81] in which the only two determinants of performance are school tier and academic index.[82]

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[80] In their critique of Sander's analysis, Chambers et al. make the point that these assumptions are unrealistic. Chambers et al., *supra* note 21, at 1888. Any bias, however, will tend to underestimate the negative effect on black students of getting rid of affirmative action; it is highly unlikely that more black students will apply and matriculate at law schools absent affirmative action.

[81] Sander does not break out his estimate in the grid model in the text of his article; the model I use is based upon Table 6.2 in his analysis. Sander, *supra* note 5, at 446 tbl.6.2.

[82] I specifically use the term "school tier" here because this is the terminology that Sander uses, *see id.* at 415, and to differentiate the six "school tiers" that Sander uses in his analysis from the four "school types" that I use in my own analysis. See *supra* notes 47–49 and accompanying text for a discussion of the school type versus school tier methodological question. In my reanalysis of Sander's model, I use Sander's assumptions with one caveat: I treat the six school tiers as six ordinal variables, rather than one interval scale variable. This flaw in Sander's original analysis, *see, e.g.*, *id.* at 439 tbl.5.6, is one of the important criticisms of Sander's argument in the critique by Chambers et al., *supra* note 21, at 1872–73.

In his article, Sander argues that affirmative action triggers mismatch, which in turn causes black students to perform poorly. As the graduation and bar passage inquiries in Part I demonstrate, one's level of achievement in and after law school depends in part on the type of school attended. Sander's argument therefore sounds quite logical: Affirmative action changes the type of school black students attend; LSAT scores, UGPA values, and school type predict grades; and grades predict bar passage, so affirmative action, by changing the type of school for black students, affects bar passage.

As a technical matter, this logic does not hold. Regression models, particularly underspecified regression models[83] such as the models that Sander uses, are not causal. In a series of unlinked regression models, if *A* predicts *B*, and *B* predicts *C*, one cannot conclude, as a matter of logic, that *A* must predict *C*. A simple example makes this clear. Because blacks, on average, earn less money than whites earn, race predicts salary (*A* = race predicts *B* = salary). Furthermore, because earning a lower salary is correlated with being female, salary predicts sex (*B* = salary predicts *C* = sex). However, knowing an individual's race (*A*) provides no predictive power about the individual's sex (*C*); the chance is still 50% that the individual is female, no matter what the individual's race is. Put simply, regressions measure correlations, and the word "predict" is synonymous, in the regression world, with "is correlated with." In the context of using observational data, regressions do not prove causation.[84]

Neither Sander nor his critics test directly what would happen absent affirmative action. Relying on Sander's assumptions that admission and matriculation patterns would not change in an alternative world without affirmative action,[85] Ayres and Brooks calculate that changing admissions policies to eliminate affirmative action would result in a loss of 12.7% or 9.4% of new black lawyers each year, depending on the model used.[86] Chambers and his co-authors perform a calculation allowing for more realistic and complex changes in application and admission patterns. This calculation suggests that eliminating affirmative action would result in a loss of 30–40% of new black lawyers each year.[87] Sander estimates that remov-

[83] Underspecified regression models are models that do not control for all possible causes of performance.

[84] *See* JAMES T. MCCLAVE ET AL., STATISTICS 503 (7th ed. 1997).

[85] The primary difference between Sander's analysis and Ayres and Brooks's analysis is that Ayres and Brooks do not assume that blacks will suddenly have the same bar passage rates as white students absent affirmative action. *Compare* Sander, *supra* note 5, at 448 (performing calculations that assume that absent affirmative action, black students will have the same bar passage rates as white students with the same credentials), *with* Ayres & Brooks, *supra* note 15, at 1815 (performing calculations assuming that absent affirmative action, black students will have the same bar passage rate as black students currently enrolled in the median school tier for white students with the same credentials).

[86] Ayres & Brooks, *supra* note 15, at 1814, 1816.

[87] Chambers et al., *supra* note 21, at 1891.

ing affirmative action would *increase* the number of new black lawyers by 7.9%.[88] None of these researchers provide a confidence interval or other measure of the variability of their estimates. Therefore, each fails to provide a test to determine whether the number of new black lawyers would be systematically different with and without affirmative action.[89] This Part does exactly that.

First, however, it is important to describe how Sander arrives at the counterintuitive result that getting rid of affirmative action would lead to an annual increase of 7.9% in new black lawyers, as this will provide a starting point for distinguishing my study from prior ones. Sander relies on a grid model first used by Linda Wightman in a report on the potential effect of eliminating affirmative action.[90] Wightman's grid model simply creates a table of LSAT scores by UGPA values and estimates the probability of acceptance at a law school for students in each cell of the table. To determine what the acceptance probabilities of black applicants would be absent affirmative action, Wightman assumes that the acceptance rate would be the same for blacks as it is for whites in every cell.[91]

Sander makes the same assumption as Wightman, but estimates the bar passage rate rather than the law school acceptance rate.[92] Sander further simplifies the grid model by using his specific "academic index" of credentials, a 60/40 linear combination of LSAT and UGPA, as the single explanatory variable. Instead of a two-way grid, which would allow different interactions between LSAT and UGPA, Sander assumes that bar passage is related only to this specific linear combination of LSAT and UGPA. Because one simply is estimating the average bar passage rate for each cell in the grid, using the grid model to predict bar passage—or student admission to law school—is equivalent to using a logistic regression with dummy variables for each of the cells in the grid.

For student admission, Wightman's grid model makes sense, at least as an initial attempt to estimate the effect of ending affirmative action on the number of new black law students. Admission decisions happen before students reach law school and depend heavily on a particular student's LSAT and UGPA.[93] However, Sander's use of the same grid model to pre-

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88 Sander, *supra* note 5, at 473.

89 In order to test any theory and determine whether the result is due to chance, one must have both an estimated value (a "statistic") that provides an estimate of how the theory would alter current numbers (for example, there would be a 7.9% increase in the number of new black lawyers each year) and a measure of the inherent variability of the statistic.

90 *See* Sander, *supra* note 5, at 471–72 (relying on Linda Wightman, *The Threat to Diversity in Legal Education: An Empirical Analysis of the Consequences of Abandoning Race as a Factor in Law School Admission Decisions*, 72 N.Y.U. L. REV. 1, 9 (1997)).

91 Wightman, *supra* note 90, at 9.

92 Sander, *supra* note 5, at 473.

93 Wightman herself notes the caveats to her model, which assumes that black students will not change application or matriculation behavior. Wightman, *supra* note 90, at 22–23.

dict bar passage rate is inappropriate. Sander's use of Wightman's model implicitly assumes that minority and white students with the same LSAT and UGPA would have the same bar passage rate. Sander's model therefore assumes that black students' performance on bar exams will not be influenced by their specific experiences in law school and will be the same as white students possessing equivalent LSAT and UGPAs. This assumption does not account for the different experiences of black and white law students while in school. Essentially, Sander's use of the grid model assumes that everything about black students' experiences during law school that is different from what white students experience will not affect their chances of passing the bar.

Instead of making the assumption that ending affirmative action will make black students' law school experiences identical to that of white students, I assume that black and white student performance is predicted by the same models I report in Part I. Students' performance will change if school type, a significant predictor of performance, changes because of the interaction between school type and credentials. However, my assumption is that the general prediction mechanism, as estimated by the performance models, does not change. Race-based barriers and mismatch (or reverse mismatch) will therefore continue to predict performance to the same extent as under the current affirmative action regime. In addition, the characteristics of individual students (race, LSAT, and UGPA) do not change; the only change between the different policy simulations is the school type to which individual students matriculate.

A. Simulation Description

Using the same LSAC data I used in Part I, I perform four sets of simulations based upon the performance models I presented in Part I. All of the simulations involve assigning each potential matriculant to a type of school or, in some cases, to no school. The assignment to school type is, essentially, the "treatment"[94] each potential matriculant receives and is based upon the underlying admissions policy in effect. Based on the school type assigned, as well as the individual's race and LSAT and UGPA scores, I estimate each potential matriculant's probability of graduation using the model presented in Tables 1A and 1B. I use this probability to simulate whether the individual will in fact graduate. If so, I estimate both the individual's probability of passing the bar using the bar passage model presented in Tables 2A and 2B, and probability of obtaining a well-paying job

[94] In essence, affirmative action becomes the "treatment" that minority students receive; in the thought experiment that one would optimally perform, one would assign individuals randomly to different school types. This would be akin to a particular medical intervention in a clinical trial. *See* Rubin, *supra* note 27, at 689 (discussing general "treatments" in the context of both randomized experiments and observational studies).

directly after law school, using the model presented in Tables 3A and 3B.[95] Finally, I simulate whether the individual did in fact pass the bar or obtain the well-paying job. Thus, each simulation provides an alternative world of matriculants, graduates, bar passers, and students who obtain well-paying jobs based upon the school type into which the matriculants are placed.

I investigate four different treatments: the current affirmative action policy; a policy of no affirmative action; a less expansive policy of affirmative action that I deem "affirmative action light," which provides about half the boost that current affirmative action plans do; and finally, a more aggressive policy of affirmative action that I deem "affirmative action plus," which provides about double the boost that current affirmative action policies do. I detail each of these treatments below.

1. Affirmative Action.—In order to place each individual into a type of school (treatment group), I use a grid model similar to Wightman's. I create a 5 x 3 grid of LSAT vs. UGPA.[96] For each square in the grid, I compute the probability of matriculating to each school type, based upon the 1991 LSAC data on law school matriculants, the same data used for all of the relevant studies. I do this separately for minority applicants who are underrepresented in law schools (black, Hispanic, and Native American applicants) and applicants who are not underrepresented in law schools (white and Asian applicants). Finally, to simulate the data, I randomly assign each individual to a school type based upon the computed probabilities for that individual's credentials index score and race.

2. No Affirmative Action.—Here, I compute the probability of matriculating to a particular school type in exactly the same manner, except that I make two additional assumptions. First, for each school type, I use for *all* potential matriculants the estimated probability of acceptance for students who do not directly benefit from the admissions boost that affirmative action affords. That is, I assume that absent affirmative action, students from underrepresented racial groups would have the same probability of being accepted, based on their credentials, as students from racial groups that are not underrepresented. Second, I assume, as Sander did, that the bottom 14.1% of potential matriculants from underrepresented groups would not be accepted to any school.[97] I also assume in each of the counterfactual simu-

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95 I do not require a student to pass the bar in order to obtain a well-paying job.

96 The grid is created based upon LSAT and UGPA percentiles. Thus, the cut points for LSAT are the twentieth, fortieth, sixtieth, and eightieth percentiles, and the cut points for UGPA are the thirty-third and sixty-seventh percentiles.

97 Chambers et al. point out that the 14.1% figure, based on 2001 data, is a very low percentage compared with other years. Between 1991 and 2004, the percentage of black applicants who would not be accepted at any law school absent affirmative action ranged from 9.1% to 52.5%. Chambers et al., *supra* note 21, at 1861 tbl.1. In addition, the assumption that the bottom 14.1% of applicants would not be accepted is not completely realistic. Based in part on the randomness of the application process, and in part on unmeasured credentials, some individuals with higher LSAT and UGPA scores will not be

lations that matriculation rates for white and Asian students do not change, even though the total number of students does change slightly in each school type with the change in underrepresented enrollment.

3. Affirmative Action Light.—Here, I compute the probabilities of matriculating to particular school types in the same manner as above, with two changes. First, for each school type, I use the average of the estimated probability for underrepresented groups and the estimated probability for non-underrepresented groups as the probability of acceptance. Thus, I assume that underrepresented applicants get half the boost—in terms of probability of matriculation—that they would have gotten under the current affirmative action regime. Second, as with the no affirmative action policy, I assume that some underrepresented applicants who are admitted under the current system would not be admitted under the new one. I assume that the bottom 7% of the applicants from underrepresented groups would not be accepted at any school.[98]

4. Affirmative Action Plus.—Finally, I simulate an affirmative action policy that goes further than the current policy by using race as a stronger factor in deciding whom to admit. Here, I assume that matriculants from underrepresented groups get twice their current the boost over non-underrepresented students, again based upon probabilities. Thus, if under current affirmative action policies a matriculant from an underrepresented group has a 10% probability of being admitted to a particular school type, and a white matriculant has a 7% probability, under the "affirmative action plus" policy, the matriculant from an underrepresented group has a 13% probability of acceptance (10% + (10-7)%).[99]

In this case, I assume that all applicants currently accepted to law school would still be accepted.[100] I cannot, however, estimate which applicants from underrepresented groups who did not matriculate to any law school would do so under this policy because I do not have data on individuals who did not begin law school. Because I do not have the data to test this policy experiment fully, the results for this policy should be considered a lower bound on the total number of matriculants from underrepre-

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admitted, while some within the bottom 14.1% will be admitted to some law schools. Both of these assumptions, like the other assumptions I make regarding the model, would tend to underestimate the negative effects of getting rid of affirmative action, and therefore provide a conservative estimate of what the change would entail.

98 This represents about half of the 14.1% of underrepresented students that Sander assumes would not matriculate at any law school absent affirmative action. Sander, *supra* note 5, at 473 tbl.8.2.

99 In addition, I must re-normalize the probabilities of matriculation to make sure that the new probabilities add to 100% (not more). To do so, I assume that students prefer to go to higher ranked schools, using school type as the ranking system.

100 I assume no change in the non-underrepresented group of applicants, which means that more students would matriculate to higher ranked schools overall than under current policy. This difference, however, is small, as the number of underrepresented matriculants is small overall.

sented groups that succeed in the measured way, based solely on the incremental probability that current matriculants from underrepresented groups will succeed at an institution in a different school type cluster. Thus, the only "boost" that applicants from underrepresented groups receive in this simulation is that matriculants, on average, attend a school type with higher average LSAT scores. The simulation therefore isolates the effect of students from underrepresented groups matriculating to a school type more highly ranked from the effect of more applicants from underrepresented groups matriculating to law school in total. For two outcomes that I investigate, graduation and bar passage, this data limitation is likely binding. Some, perhaps many, of the current non-matriculants who would attend law school under this policy regime would graduate from law school and would pass the bar. For the third outcome, whether a graduate obtains a well-paying job after law school, the data limitation is less important. The vast majority of individuals who will not gain admission under current affirmative action policy, but would gain admission to law school because of the additional boost of "affirmative action plus," would matriculate to lower ranked schools and have significantly fewer opportunities for well-paying jobs.

B. Simulation Results

The results of these simulations are provided in Tables 7, 8, and 9. Table 7 reports the results of the bar passage portion of the simulations. The average number of bar passers is given in each cell, with bootstrapped standard errors in parentheses.[101] The first result to note is that the two more intensive affirmative action policies produce *more* black lawyers than a policy of no affirmative action.[102] Overall, there would be a drop in the number of new black lawyers from the 845 lawyers produced by the current system to 732 if affirmative action were no longer an element of law schools' admission decisions. Given a standard error of 22 individuals for this difference of 113 new black lawyers, this result is statistically significant at the 0.01 level.[103] This result, based on a more complete model, directly contradicts

[101] The bootstrap is an extremely useful statistical tool that allows one to calculate standard errors (as well as other quantities of interest) non-parametrically, that is, without making restrictive parametric assumptions, such as the assumption that the data follow a normal distribution. It is particularly useful in complicated models where small deviations from initial parametric assumptions can become magnified into large errors in measurement. See BRADLEY EFRON & ROBERT J. TIBSHIRANI, AN INTRODUCTION TO THE BOOTSTRAP (1994) for details about the bootstrap procedure for determining standard errors.

[102] Note that the small drop of sixteen lawyers from affirmative action to affirmative action plus (about a 2% decrease) is not statistically significant; there is no evidence that this drop is anything but random variation.

[103] The calculations of the p-values and standard errors of the difference also account for the fact that the two numbers are correlated because the two simulations use the same bootstrap sample and the same underlying models of student achievement. The standard error of the difference between any two values in the table is provided by the following formula:

Sander: Ending affirmative action would reduce the number of new black lawyers each year by 13.4% ± 5.2%.[104]

Table 7: Bar Passage Simulation for Three Different Models

	Sander's Model[105]	*Simulation Model*			
	Black Bar Passers	White Bar Passers	Black Bar Passers	Hispanic Bar Passers	Asian Bar Passers
Affirmative Action	1147 (22)	17,039 (105)	845 (28)	774 (29)	585 (204)
No Affirmative Action	1252*** (32)	16,995 (107)	732*** (32)	777 (29)	583 (204)
Affirmative Action Light	—	17,019 (104)	773*** (28)	773 (29)	583 (206)
Affirmative Action Plus	—	17,041 (104)	829 (29)	762 (31)	584 (206)

Another interesting result is that boosting the school type at which successful black applicants matriculate (using the affirmative action plus simulation) does not have a significant effect on the number of new black lawyers. Like the results of complete elimination of affirmative action policies, these results are different from the current affirmative action policy with statistical significance at the 0.01 level. Thus, the results demonstrate an increasing number of new black lawyers as the boost from affirmative action increases, until the limitations in the data—no new matriculants—make it difficult to determine if there is a further increase. Nonetheless, the results still clearly show no decrease, refuting Sander's hypothesis. In con-

$$s.e.(X-Y)=\sqrt{\sigma_x^2-2\rho_{xy}\sigma_x\sigma_y+\sigma_y^2}$$

where σ_x is the standard deviation of X, σ_y is the standard deviation of Y, and ρ_{xy} is the correlation between X and Y. *See* DENNIS WACKERLY ET AL., MATHEMATICAL STATISTICS WITH APPLICATIONS 228 thm.5.12 (5th ed. 1996).

104 This range represents a 95% confidence interval for the percentage loss of new black lawyers absent affirmative action. This means that if one were to rerun the entire experiment with new data one hundred times, on average ninety-five of those times the true value of the percentage decline in new black lawyers would be within the confidence interval estimated. *See* MCCLAVE ET AL., *supra* note 84, at 255.

105 The absolute number of lawyers in Sander's model is larger because my model predicts bar passage for a subset of the LSAC data—respondents whose race is known—while Sander's model predicts bar passage for the larger group of all respondents. Because neither model predicts the total number of lawyers outside the LSAC sample, what is most relevant is the percentage change in numbers, rather than the absolute numbers.

trast to the results for black students, there is not much difference for Hispanic students, likely because of the boost in bar passage rates that Low-range Schools provide to these students (see Table 2B). Asian students are also unaffected, but this may be due to the imprecision of the results; there simply are not enough Asian students to discern much difference.[106] Also, in my affirmative action simulation, Asian students matriculate to the same schools with or without affirmative action because they do not currently get an affirmative action boost, so it comes as no surprise that there is little difference between the current regime and the alternative ones I have investigated.[107]

This basic pattern of results is replicated in the simulations of graduation and well-paying job status. Tables 8 and 9 provide the details. With current affirmative action policy, 1492 black students, or 81.9% of black matriculants, graduate from law school within five years of entering.[108] Without affirmative action, the number of graduates is drastically reduced to 1152. However, because the number of matriculants decreases significantly, the percentage who graduate goes up to 84.1%. Overall, the loss of 337 black graduates is statistically significant at the 0.01 level, and represents a 22.6% drop in the yearly number of new black graduates, with a 95% confidence interval of 22.6% ± 2.4%. Again, the results for affirmative action light lie somewhere in between these two policies. Under affirmative action light, the annual number of black graduates is significantly lower than under the current affirmative action policy (243 fewer black students, or a 16.3% drop), and significantly better than under no affirmative action policy (an 8.1% ± 1.8% increase). Affirmative action plus is not significantly different from the current affirmative action policy in terms of the number of new black law graduates, which suggests that admitting the same black matriculants to school types of higher rank does not significantly affect the number of black law school graduates. In addition, the number of Hispanic graduates drops absent affirmative action. Although this is a relatively small absolute drop of 50 students, or 4.5%, it is statistically signifi-

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106 In 1991, Asian law students represented 4.6% of the LSAC random sample of entering law students, and approximately 2.9% of the general population. *See* WIGHTMAN, USER'S GUIDE, *supra* note 49, at 6 tbl.2 (listing the percentage of Asian law students who were sent a survey); 1990 U.S. Census Data, Table P006, *available at* http://factfinder.census.gov/servlet/DTTable?_bm=y&-geo_id=01000US&-ds_name=DEC_1990_STF1_&-_lang=en&-_caller=geoselect&-state=dt&-format=&-mt_name=DEC_1990_STF1_P006 (listing 7,273,662 Asian Americans and 248,709,873 total Americans). Thus, Asian law students were not particularly underrepresented in the data, but were not a large group of the students surveyed by LSAC.

107 In order to compare my results with those of Sander more directly, I simulate Sander's grid model as well. Due to the random variation in the simulation, I estimate a slightly larger increase in the number of new black lawyers absent affirmative action—9.1%, specifically. By simulating the model several times, I am also able to estimate a 95% confidence interval for this number: 9.1% ± 3.8%. *See supra* Table 7.

108 LSAC, BAR PASSAGE STUDY DATASET, http://bpsdata.lsac.org/ (last visited June 14, 2007) [hereinafter LSAC DATA].

cant at the 0.01 level, with a 95% confidence interval of 4.5% ± 1.6%. Similarly, affirmative action light lowers the percentage of Hispanic graduates by 3.2% ± 1.1%.

Table 8: Simulation of Number of Graduates for Three Different Models

	Simulation Model			
	White Graduates	Black Graduates	Hispanic Graduates	Asian Graduates
Affirmative Action	20,577 (105)	1492 (28)	1099 (29)	756 (206)
No Affirmative Action	20,481 (107)	1155*** (32)	1049*** (29)	752 (204)
Affirmative Action Light	20,524 (104)	1249*** (28))	1064*** (29	752 (206)
Affirmative Action Plus	20,578 (104)	1493 (29)	1100 (31)	755 (206)

Table 9 demonstrates that a similar pattern holds for the number of black graduates who obtain well-paying jobs. Moving to no affirmative action would drop the number of black graduates who obtain well-paying jobs from 297 to 228—a decline of 23%. This decrease is statistically significant at the 0.01 level, with a 95% confidence interval of 23.0% ± 19.6%.[109] Once again, affirmative action plus does not have a statistically significant effect. Although affirmative action plus provides an increase of almost 10% in the number of black graduates obtaining well-paying jobs, the standard errors are large enough that this increase is not statistically significant.

[109] Because of the low response rate on the salary question in the data, the error in the estimates, and consequently the confidence interval, is quite large. It is still clear, however, that the model predicts a statistically significant drop in the number of black law graduates who obtain well-paying jobs, which is also substantively important.

Table 9: Simulations of Graduates with Well-Paying Jobs for Three Different Models

	Simulation Model			
	White Job Takers	Black Job Takers	Hispanic Job Takers	Asian Job Takers
Affirmative Action	4422 (239)	297 (96)	157 (82)	51 (8)
No Affirmative Action	4403 (238)	228** (86)	171 (71)	50 (6)
Affirmative Action Light	4413 (240)	253** (91)	158 (81)	51 (7)
Affirmative Action Plus	4421 (239)	330 (31)	209 (30)	51 (7)

How can one reconcile these results with the very real data that suggest blacks' law school grades are significantly worse than those of whites? My results focus on outcomes—graduation, bar passage, and first jobs—rather than law school grades. Although black law students receive lower grades on average,[110] it appears that this single marker of achievement is inadequate to comprehensively describe black law student performance. At the very least, my results give some reason to doubt Sander's regression that purports to demonstrate that law school grades are the primary determinant of bar passage. Ho suggests that Sander's regression suffers from post-treatment bias,[111] and my analysis supports that suggestion. At least with respect to the three performance measures I investigate, my results demonstrate that the properly modeled data do not support the inference that affirmative action triggers mismatch for black law students. Moreover, the results demonstrate a real cost to eliminating affirmative action. Ending affirmative action would lead to 13.4% fewer new black lawyers, 22.6% fewer new black law graduates, and 23% fewer black law graduates with well-paying jobs. The results presented here are not definitive because they suffer from the same data limitations as the studies of Sander and his critics, but they provide strong evidence that affirmative action has significant benefits and that the evidence of negative consequences Sander provides is highly suspect.

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110 *See* LSAC DATA, *supra* note 108 (showing that the first-year law school GPA for black law students is, on average, one standard deviation lower than the first-year law school GPA for white students).

111 Ho, *Scholarship Comment*, *supra* note 21, at 2000–02. Post-treatment bias is the bias that results from including in a regression variables that occur after the treatment began (post-treatment variables) and therefore may have been affected by the treatment. In Sander's case, the main culprit is law school grades, which are determined *after* the treatment of matriculating at a specific school type.

III. TOWARD AN ADEQUATE MODEL

In Parts I and II, I argue that a model to test the mismatch and race-based barriers theories must include a flexible function of student credentials, as measured by LSAT and UGPA, as well as variables that account for the different cultures of schools and the different student experiences for students of particular racial groups. Sander's model fails to account for even these basic predictors of student performance. But all of the analyses of the LSAC data must contend with the significant limitations inherent in the data. After six sets of researchers have attempted to answer the same question using the same limited data (though very different methods) without obtaining definitive answers, it is time to gather more data. To this end, I describe the LSAC data limitations in more detail below. I also offer research design ideas to obtain more useful data, or to find ways to control for the inherent limitations of this type of observational data. I focus on three primary limitations of the LSAC data: (1) no knowledge of the specific school each student attended; (2) incomplete measurement of student credentials by relying solely on LSAT and UGPA scores; and, to a lesser extent, (3) bar passage results that are not state-specific.

A. School Attended

To protect the identity of individual students and individual schools, the LSAC data do not identify the school to which each student matriculates. Instead, the data identify which of the six different clusters of schools to which the student matriculated. The clusters vary in size from fifty schools to fourteen schools.[112]

Sander and his critics, myself included, use these clusters as a rough measure of school rank. There are two primary problems with using the LSAC clusters as a proxy for rank. First, these school types are not based solely upon rank of school. The clusters are created based upon size of school, cost, selectivity, faculty/student ratio, minority enrollment, median student credentials (LSAT and UGPA), and other variables.[113] Second, even if the school types were based upon school rank rather than other factors, providing information about broad groups of schools grouped into differently sized clusters masks important details about the rank of school that one attends. Essentially, this process assumes that all schools within a cluster are exactly the same.

Why is school identity important in the first place? There are two reasons why researchers need to know the specific school in order to test the

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[112] The clusters were created based upon a statistical procedure called cluster analysis, which groups items based on their similar characteristics. The researcher determines which characteristics are pertinent to the grouping. See WIGHTMAN, LSAC STUDY, *supra* note 51, at 8–9 & n.20, for a more complete description of the clusters.

[113] *See* WIGHTMAN, USER'S GUIDE, *supra* note 49, at 15.

mismatch theory and the other race-based barriers theories discussed in Part I. First, in order to determine whether mismatch is triggered, one needs to know the specific rank of the school each matriculant attends. To lump thirty schools together in one cluster forces the researcher to assume that students with the same credentials will perform similarly when attending the first or thirtieth ranked schools, but perform better when attending the thirty-first ranked school than either the first or thirtieth ranked schools.[114] This assumption makes little sense and would be eliminated with the addition of school identity to the data. Second, school identity is necessary to test race-based barriers theories. Individual schools have very different cultures, and therefore students' experiences will differ substantially across these schools.[115]

Luckily, this data problem is easy to solve: Future LSAC data sets should simply provide information on each individual school. This can be done while protecting confidentiality by limiting access to the data to researchers. Access may even be restricted to particular computers or particular personnel, who would then run whatever model estimation researchers might propose and provide only the results of the estimations to the researchers. Many public agencies do this regularly.[116] Thus, the first data problem, while a serious impediment to accurate modeling, is reasonably easy to fix.

B. Student Credentials

Controlling for student credentials is critically important to an accurate test of the mismatch hypothesis. The mismatch hypothesis posits that students with the same credentials who matriculate at different schools will perform differently based upon the school attended. In other words, given the same credentials, students at higher ranked schools will do worse. Without an accurate measure of student credentials, the researcher cannot identify students who should perform similarly, based on credentials, and therefore cannot determine whether any difference in performance is based

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[114] Both the Mid-range Public and Mid-range Private school clusters contain 50 schools.

[115] Recall that I cannot test which race-based barriers are present in a given school, but would be able to test the cumulative effect of race-based barriers at each school with information about specific school rank.

[116] For example, the Census Bureau allows researchers to access detailed data at a few census facilities across the country. Researchers must officially be sworn in as census employees to use the data. U.S. DEPARTMENT OF COMMERCE, RESEARCH OPPORTUNITIES AT THE CENSUS BUREAU, DIR/01-RFP, at 4 (July 2001), *available at* http://www.census.gov/prod/2001pubs/dir-01rfp.pdf. Care must still be taken, however, to protect confidentiality in the final product. In one extreme example, the United States Department of Justice issued a report with over 450 tables of information about less than 1000 observations. This level of detail allowed an entrepreneurial researcher to reverse-engineer the tables in order to reconstitute the private data. *See* David J. Algranati, Exploring Racial and Geographical Effects in the Decision to Seek the Federal Death Penalty, 1995–2000 (Dec. 2002) (unpublished doctoral thesis, H. John Heinz III Sch. of Pub. Policy & Mgmt, Carnegie Mellon University).

on mismatch or mismeasurement of credentials. Credentials missing from the LSAC data, but critical to accurate measurement, include difficulty of undergraduate experience, recommendation letters, writing ability, and other degrees or experiences. In addition, when testing for race-based barriers, unobserved credentials will be confounded with race to the extent that credential mismeasurement is different across races. Finally, in determining which effects are most important (mismatch or race-based barriers), one needs to use accurate estimates of all variables in order to get a full picture of which has a stronger impact on student performance.

This second data problem is the most difficult to fix. The LSAC data contain only three pieces of information regarding student credentials: LSAT, UGPA, and the school type attended. LSAT and UGPA are direct measures of student credentials. The school type attended is an indirect measure because one can infer some general information about student credentials from the fact that the student was admitted to the particular type of school.[117] For example, one can infer that a student at an elite institution has stronger credentials (beyond simply LSAT and UGPA) than a student at a lower ranked school. Essentially, information about school admissions provides a window into the broader range of criteria that law school admissions officers use to evaluate student credentials. Because the type of school attended provides only a glimpse out of a clouded corner of the window, it is not very helpful. Indeed, I have focused solely on LSAT and UGPA scores in the analyses above because they are the only straightforward measures of credentials in the LSAC data.

This description highlights the first of two separate errors in using LSAT and UGPA as the sole measures of student credentials. In determining whether to admit students, schools use credentials that are unobserved to the researcher because they are unavailable in the data. Failing to account for these credentials is problematic, because this information is obviously pertinent to admissions decisions. Because students are not randomly assigned to law schools, the selection criteria schools use to admit students biases the results. This bias arises from the unsurprising fact that when comparing two students with the same observed credentials—one who matriculates to University of Michigan, and one who matriculates to Michigan State University—the student who attends University of Michigan likely has better "soft credentials," such as strong recommendation letters and success at a difficult undergraduate institution.

The second error associated with relying upon LSAT and UGPA as the only measures of student credentials is that there are credentials that are not only unobserved in the LSAC data, but also unobserved to law schools. These are factors that individual students use in making their choices to ap-

[117] One can also infer something about a student's range of choices for law school based upon which law school the student attended, but this is a weaker inference, as there are many reasons unrelated to credentials why a student might choose to attend a given school.

ply to specific schools and, finally, to attend one of the schools to which they were admitted. To the extent that students make these choices nonrandomly (and the choices relate in some way to their likely performance in law school), these student choices will bias the results. Thus, the two problems with using only the observed credentials in the data are the nonrandom decisions schools make in deciding whom to admit, and the nonrandom decisions students make in deciding where to apply and, once admitted, which school to attend.

Unfortunately, controlling for this matching problem poses a serious challenge. Traditionally, the way to deal with bias was to include more variables in the regressions in order to incorporate every possible student credential. Essentially, the idea was to measure everything possible. Even if researchers *could* determine all the relevant variables and measure them, determining which variables are most important and how they are important is extremely difficult. Newer methods like propensity scores and instrumental variables can provide greater traction to the problem, but they are still not a solution.[118] With any research design, one must always make the tradeoff between more assumptions (and more sophisticated methods) and better data. Sometimes the best solution, and perhaps the only solution, is to obtain better data.

Indeed, using only observational data may simply be inadequate to the task. As the data are currently presented, all information on each student is based upon the school attended. UGPA, for example, is normalized by school attended, so that one cannot easily compare UGPAs across students. This limits the ability to make cross-school comparisons unless one obtains a credentials measure that crosses schools and is therefore evenly applied across schools. One option may be to ask admissions officers to rank students. The advantage of this is that at some schools, the officers may already rank applicants, and that such a ranking provides a very rough measure of the strength of an applicant's credentials.[119] This option also solves the first of the two credential measurement problems: We obtain an unbiased measure of what the law schools observe each applicant's credentials to be. It does not, however, solve the problem of cross-school comparison. Because this ranking is specific to each school, it is difficult to extrapolate to other schools and compare students across schools—a requirement to test the mismatch theory. One might not worry about this too much if all of the schools provided a consistent ranking (a five means a five at Harvard, Howard, and Hamline Universities), although this consistency

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[118] Ho uses propensity scores in his analysis of the data. Ho, *Social Science Critique*, *supra* note 21, at 8–9. To my knowledge, no one has used instrumental variables. Both are statistical techniques that attempt to allow causal inferences from observational data. *See* Rubin, *supra* note 27, at 700 (discussing matching); WILLIAM H. GREENE, ECONOMETRIC ANALYSIS 397–98 (5th ed. 2002) (discussing instrumental variables).

[119] This ranking may be as simple as three groups: no, maybe, and yes.

is difficult to achieve. But this data still does not take into account the students' choices, and is therefore still subject to bias.

A better option would be to measure student credentials across several schools, chosen at random. The easiest way to do this would be to have the LSAC, which administers the LSAT and provides common electronic applications to law schools, submit each student's application to randomly chosen schools for which their objective credentials (LSAT and UGPA) suggest that they might be admitted. Thus, information on each student would include a series of admittance decisions from *randomly* chosen schools in the objective credentials range of the applicant, which would create an accurate estimate of the student's credentials.[120] Statistical methods can translate these decisions to admit or deny into an accurate credentials scale, based upon a student's entire applicant file.[121] As an alternative to this large project, it may be that one could again use the current LSAC data (rather than starting from the beginning again), and submit these students' applications to other schools. This, of course, would involve serious logistical challenges, likely including further informed consent notification, but may be more cost-effective than starting a new study from scratch.[122] Whether or not the current data can be salvaged or a new study begun, creating an accurate measure of student credentials would be a large step forward in the ability of the data to answer important questions, such as whether affirmative action triggers mismatch or whether race-based barriers affect student performance.

C. State-Specific Bar Passage Results

One smaller issue also impacts the usefulness of the data. In order to estimate bar passage rates accurately, the data need to include the state in which the bar was taken[123] because bar passage rates vary widely between the states. Ideally, one would also have quality-adjusted measures of how difficult each state bar is. This data would allow the researcher to control for bar exam difficulty when estimating a model of bar passage rates. Without this, measured controls that are correlated with difficult state bar

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[120] Details would have to be worked out, but two main things are clear: students would not actually be admitted to the law schools, and the significant expense of looking through many more files would need to be compensated in some way.

[121] *See generally* Andrew D. Martin & Kevin M. Quinn, *Dynamic Ideal Point Estimation via Markov Chain Monte Carlo for the U.S. Supreme Court, 1953–1999*, 10 POL. ANALYSIS 134 (2002) (providing a statistical method to rank individuals based on yes or no answers to a series of related questions).

[122] This would also require admissions committees to compare 1991 applicants to current applicants. One would have to assume that the relative ranking of students would remain reasonably constant across this time period.

[123] Like the school identification information, the specific state or states in which each student passed the bar was in the original, non-public data set. Again, if this data is available in any form, it reduces the need to perform an entirely new study.

exams will have biased estimates. Although it is impossible to tell with current data, it is certainly plausible that the results I present above exhibit this bias. Thus, determining the particular state bar each student attempted is important to determining the effects of mismatch and race-based barriers accurately.

While researchers may point to other problems with the LSAC data, these three problems are the primary stumbling blocks to accurate assessments of both the mismatch and race-based barriers theories of student performance.

D. Individual Law School Culture

While not specifically a problem with the LSAC data, the results presented in Part I suggest that law school culture significantly affects minority student achievement. This is troubling, and suggests that the legal academy should prioritize further investigation to determine what specifically about law school culture has negative (or positive) consequences. The challenge is to determine what in law school culture helps students, particularly minority students, thrive. The first step in this determination is to map law school culture: How do students, faculty, and staff describe the environment, and how do they experience law school? The LSAC study did not focus on this question, and therefore has very little data on law school atmosphere itself. Several other researchers have investigated law school atmosphere for specific schools. One ongoing project, the Educational Diversity Project,[124] is surveying a large number of individuals at different law schools to investigate how diversity in law schools affects students. This is a very important step in determining what individuals subjectively believe affects performance, but it would also be advantageous to take the descriptions of law school atmosphere and correlate them with student performance. If one controlled for student credentials at the same time, a researcher could determine what about law school culture helps minority students succeed or, perhaps more importantly, fail. Although looking at both student credentials and law school culture would greatly expand the scope of the project, it would provide significant benefits by helping to determine why minority students', and particularly black students', achievements in law school lag behind their white classmates' achievements.

IV. CONCLUSION

This Essay attempts to clear away the confusion surrounding the mismatch theory and its relationship to affirmative action policies. Despite much confusion to the contrary, I clarify that race is unrelated to the mismatch theory, and describe appropriate tests of the mismatch theory that flow from that argument. Unlike prior researchers, I differentiate between

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[124] The Educational Diversity Project, *supra* note 3.

tests of mismatch, which rely solely on school rank and student credentials, and tests that determine whether race-based barriers affect students' performance, which depend on individual schools and the race of the students. Using the LSAC data, I test whether the data provide support for either the mismatch or race-based barriers theory. Although I am cautious about drawing conclusions from the results due to significant data limitations, the results suggest that mismatch does not occur. Instead, the data suggest that reverse mismatch—lower credentialed students learn more when challenged by classmates who outmatch them—may be occurring. Race-based barriers, however, remain a problem in some school types, particularly at Historically Black Schools and Mid-range Schools, although race-related unmeasured student credentials may also be the cause of this finding. This suggests that the next step is further research to determine what barriers or unmeasured credentials impact performance.

Beyond describing how the current affirmative action policy and discriminatory treatment may affect student performance, I also investigate the counterfactual worlds of alternative affirmative action policies. I find that ending affirmative action would result in a decrease of 22.6% in the number of new black law graduates, a decrease of 13.4% in the number of new black lawyers, and a decrease of 23% in the number of black law graduates who obtain well-paying jobs. These drops are all statistically significant. In addition, ending affirmative action would result in a loss of 4.5% of new Hispanic graduates. Halving the boost that affirmative action provides underrepresented groups also would create a significant drop in the numbers of new black graduates, lawyers, and graduates who obtain well-paying jobs, although the drops are not as large. Providing an extra boost to black applicants in terms of the school type to which they matriculate would not change the number of new black law graduates and lawyers. The results isolate the effect of a boost in school rank, and, as such, they are an additional test suggesting that the mismatch theory does not explain the achievement gap between white and black law students.

Because the data are inadequate to test definitively whether mismatch or race-based barriers occur, I suggest what data would be necessary to test the mismatch and various race-based barriers theories accurately. At base, my argument is that the observational data are incomplete, and some form of experimental or quasi-experimental data must be incorporated into the study design to control for the selection bias inherent in the matching process that students and law schools go through each year.

Although I acknowledge that this is a large project that will require significant resources, these questions are of the utmost importance to the legal academy. I began this Essay by discussing a troubling statistic regarding black law students' performance: Half of all black law students have grades in the bottom tenth of their class. Unlike Sander's analysis, my analysis suggests that this difference is not attributable to mismatch. Instead, some form of latent race-based discrimination may be at play. The research design I provide above is primarily targeted at determining whether mismatch occurs, but further research must also contend with the specific ways in which discrimination in law schools affects student performance. As a member of a legal institution, I believe it is imperative for the legal academy to confront discrimination as a potential explanation for black students' poor grades, and to work to change the underlying conditions that contribute to these problems.