Northwestern University Law Review
Printed in U.S.A.
Vol. 105, No. 2

IS AFFIRMATIVE ACTION RESPONSIBLE FOR THE ACHIEVEMENT GAP BETWEEN BLACK AND WHITE LAW STUDENTS? A CORRECTION, A LESSON, AND AN UPDATE

By Katherine Y. Barnes[*]

INTRODUCTION

In 2007, the *Northwestern University Law Review* published an essay that I wrote entitled *Is Affirmative Action Responsible for the Achievement Gap Between Black and White Law Students?*[1] The essay joined a scholarly debate regarding the potential deleterious effects of affirmative action in the law school admissions process. The debate was rekindled by an empirical study published in the *Stanford Law Review* by Professor Richard Sander in 2004 that suggested that affirmative action policies were counterproductive,[2] followed by a series of replies from other academics and rejoinders from Professor Sander.[3]

---

[*] Professor of Law, James E. Rogers College of Law, University of Arizona; Professor of Economics (courtesy), University of Arizona; Director of the Rogers Program on Law & Society.

[1] Katherine Y. Barnes, *Is Affirmative Action Responsible for the Achievement Gap Between Black and White Law Students?*, 101 NW. U. L. REV. 1759 (2007).

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

[3] Many of the replies to Professor Sander questioned his methods and results. *See, e.g.*, Ian Ayres & Richard Brooks, Response, *Does Affirmative Action Reduce the Number of Black Lawyers?*, 57 STAN. L. REV. 1807 (2005); David L. Chambers et al., *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); Michele Landis Dauber, *The Big Muddy*, 57 STAN. L. REV. 1899 (2005); Daniel E. Ho, Reply, *Affirmative Action's Affirmative Actions: A Reply to Sander*, 114 YALE L.J. 2011 (2005); Daniel E. Ho, Scholarship Comment, *Why Affirmative Action Does Not Cause Black Students to Fail the Bar*, 114 YALE L.J. 1997 (2005) (reviewing Sander, *supra* note 2); Jesse Rothstein & Albert Yoon, Mismatch in Law School (2009), http://gsppi.berkeley.edu/faculty/jrothstein/workingpapers/rothstein_yoon_may2009.pdf. Others questioned the conclusions that should be drawn from his results. *See, e.g.*, David B. Wilkins, *A Systematic Response to Systemic Disadvantage: A Response to Sander*, 57 STAN. L. REV. 1915 (2005). Professor Sander replied to several critics directly. *E.g.*, Richard Sander, Reply, *A Reply to Critics*, 57 STAN. L. REV. 1963 (2005); Richard Sander, Response, *Mismeasuring the Mismatch: A Response to Ho*, 114 YALE L.J. 2005 (2005). There are also several other articles that are not empirical in nature. *See, e.g.*, Kevin R. Johnson & Angela Onwuachi-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, 4 (2005) (arguing that a more appropriate response to the achievement gap between black and white stu-

The purpose of my essay was to provide a framework with which to test different theories regarding the effects of affirmative action. The essence of Professor Sander's claim was that minority students matriculate to law schools that are above their capabilities because of affirmative action. This mismatch, in turn, largely explained the worse outcomes that black students obtained.[4]

Although I recognize that the mismatch hypothesis valuably questions whether students and their institutions maximize the students' success in law school and in law life, I argued that worse outcomes for black students may be the product of other cultural differences across schools.[5] Indeed, the insight behind the mismatch hypothesis is unrelated to race: mismatch relies solely on the interaction of student ability and institutional quality. I framed a broader test of the mismatch hypothesis that separated mismatch in general, which affects all students with low credentials, from these cultural aspects, which affect all black students.

In 2008, Professors Doug Williams and Richard Sander contacted me regarding replication of my results. Unfortunately, I had changed institutions between the time the essay was slated for publication and this contact. Due to my own negligence, although I thought I had transferred all of my files to my computer at my new institution, I had not. Thus, I did not have the original programs that I used to analyze the data. I reconstructed the programs for Professors Williams and Sander and their colleagues Dr. Roger Bolus and Dr. Marc Luppino but was unable to replicate the same results as presented in the original essay. Because my first commitment is to the truth, or as much thereof as the limits of logic, method, data, and human capacities allow, this Revision followed.

Research is a process of formulating and reformulating theories on the basis of new information. Empirical research, in particular, involves the often public debate regarding the appropriate methods, analysis, and conclusions to be drawn from data. By its nature all empirical research is imperfect in some way. Some imperfections are correctible, and although all empirical researchers hope that mistakes in analysis are infrequent, the academic process of replication, further investigation, and debate (like the methods of science more generally) is built to find flaws in current research in order to improve knowledge.

---

dents would be "an open and honest dialogue about the problem of minority underrepresentation in law schools").

4 The response to this Revision describes the mismatch hypothesis as meaning that students with low credentials will learn less in classrooms aimed at the middle student. *See* E. Douglass Williams et al., *Revisiting Law School Mismatch: A Comment on Barnes (2007, 2011)*, 105 NW. U. L. REV. 813, 813 (2011). This is imprecise: for mismatch to result in worse outcomes, low credential students must learn less than they would have at another school, not less than the middle-range students at their current institution.

5 Barnes, *supra* note 1, at 1770.

I am very grateful to Professor Williams, Professor Sander, Dr. Luppino, and Dr. Bolus for their effort in replicating my results and their diligence in helping to advance our understanding of the empirical validity of the mismatch hypothesis. I mix a sense of embarrassment at the problems with my original analysis with the pleasure of seeing the methods of science and scholarly discourse work the way they should.

Before revisiting the main arguments and results of my 2007 essay, I would like to take a moment to reflect on the process by which law professors publish their research and how others can avoid my error in the future. The preferred method to avoid errors is, of course, not to make them. This, however, is not always possible.

For myself, my practice for submitted papers has changed. The best practice to avoid mistakes in coding is to double code every program,[6] and I now do so. I also keep a pristine copy of the program used for the results in the submitted version of the paper in a separate "read-only" directory, where future changes in the program will not be confused with the analysis from the submitted paper.

Beyond the personal responsibility of researchers, law reviews also have the responsibility to facilitate replication. Journals should have a systematic policy that programs that support results must be submitted with the article,[7] that programs and datasets must be posted on the law journal's website, and that technical appendices must be published on the law journal's website to explain more of the details of the analysis. Because of the large number of law journals and the fact that their editorships change every year, long-term policies are more difficult to implement and retain. Although difficult, law reviews should commit to these best practices when evaluating and publishing empirical research. Indeed, it is optimal to have a more comprehensive clearinghouse, such as SSRN, which would alleviate these structural pressures by providing one well-maintained location to house programs and datasets. Although I believe such policies would help prevent future errors and should therefore be implemented widely, I take full responsibility for my error in not maintaining my program appropriately, as well as any errors in analysis which that program would have illumi-

[6] Optimally, double coding requires two different people independently coding the same general algorithm using two different statistical packages and then making sure that the output matches. This is a time-consuming and expensive safeguard, but it can detect most errors before they are published. This tests whether the logic of the algorithm is implemented correctly and helps find errors in the logic itself.

[7] Transparency is the best way to curtail errors. For example, Professor Sander makes all of his programs and data available on his website, as I also have done for this Revision. *See* KATHIE BARNES DATA SETS, http://www.law.arizona.edu/faculty/barnesDataSets.cfm (last visited June 26, 2011). Although I commend this practice because it facilitates replication, law reviews should not leave the decision whether to make this information available to the individual researcher. Moreover, in the interest of transparency, all modeling decisions should be explicit. Unfortunately, in the context of a law review article written for a nontechnical audience, including every specific decision is unrealistic, but technical appendices, published on the law review's website, can help offset this concern.

nated. Nonetheless, I am pleased that *Northwestern University Law Review* has agreed to make available a copy of all my data and programs on their website to aid future research and policy on this topic.[8]

I. THE ORIGINAL 2007 ESSAY

My 2007 essay engaged in the debate over the effects of affirmative action on law student outcomes in four primary ways. First, the essay clarified the argument over what mismatch is—specifically, that the mismatch theory is unrelated to the race of the students who are at risk of being outmatched by their classmates.[9] Second, it explicitly separated two theories that may explain black law students' poorer law school outcomes and tested these theories separately: the mismatch theory, which suggests that affirmative action programs put black students at higher risk of being academically outmatched, and what I termed the "race-based barriers" theory, which suggests that individual law school culture creates or perpetuates barriers to success that are associated with racial categories.[10] Third, the essay reported the results of alternative policy simulations to determine the effect of affirmative action on the number of new black lawyers each year, the number of black graduates, and the number of black law students who obtain well-paying jobs after graduation; the goal was to test whether different policies produce significantly different numbers, as Professor Sander argued in his 2004 article.[11] Fourth, the essay discussed the significant limitations of the data, including several coding issues and significant selection bias problems, and it provided an experimental design that would alleviate these data problems.[12] In addition, the modeling structure I used to test the two theories and simulate alternative policies allowed for a more flexible relationship between student credentials, specifically Law School Admission Test (LSAT) scores and undergraduate grade point averages (UGPA), and student outcomes after law school.

There are three primary limitations to the data in these studies. As I articulated in the original essay, they are "(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."[13] Incomplete

[8] *See Data Sets for Northwestern University Law Review 105:2*, NW. U. L. REV. (Oct. 1, 2011), http://www.law.northwestern.edu/lawreview/issues/105.2.data.html (calculations by Katherine Barnes); *see also* KATHIE BARNES DATA SETS, *supra* note 7 (providing same data).

[9] Barnes, *supra* note 1, at 1767–68.

[10] *Id.* at 1763–65. Although I use the term "race-based barriers," I cannot make the claim that these barriers are caused by race; instead, my model measures the association of race, school types, and their interaction with student outcomes.

[11] *Id.* at 1796–1800; *see* Sander, *supra* note 2, at 368.

[12] Barnes, *supra* note 1, at 1801–06.

[13] *Id.* at 1801.

measurement of student credentials is the most troubling because it creates selection bias: students who matriculate to high-ranking schools with low measured credentials likely have unusually high *unmeasured* credentials, and vice versa. Selection bias can influence statistical analyses significantly, making the resulting inferences less certain. In addition, the data only indicate to which of six broad groups of schools a student matriculated; I consolidated these into four school types roughly similar to *U.S. News & World Report* tiers.[14]

The 2007 results generally found relatively strong evidence of the opposite of a mismatch effect (an antimismatch effect), and some evidence of cultural differences across schools that affect minority students differently.[15]

II. REVISED RESULTS

Tables 1A, 2A, and 3A provide the results of the logistic regression models that are relevant to the mismatch test, while Tables 1B, 2B, and 3B provide the results of the models that are relevant to the race-based barriers test.[16] For convenience, the tables also provide the original results from my 2007 essay.

As detailed in the original essay, mismatch predicts a particular pattern of outcomes across school types.[17] Specifically, the probability of a positive outcome—graduating from the law school, for example—should be higher for lower ranked schools. Mismatch might only occur for the students with very low credentials, but there is little theoretical basis to conclude that mismatch would happen in some schools but not others. Because Table 1A reports the difference in probability of graduating from the same law school to which one initially matriculated between the listed school type and mid-range schools, the pattern consistent with mismatch is a monotone, though not necessarily linear, negative progression in graduation rates as school quality increases. In addition, because the results here and in the original essay did not control for selection bias, I remain "cautious about drawing conclusions from the results due to significant data limitations."[18] Thus, one requires clear evidence of mismatch or antimismatch to make tentative conclusions either way. The results here do not meet this standard.

---

[14] The four school types are historically black schools, low-range schools, mid-range schools, and top 30 schools.

[15] *Id.*

[16] All report results are from logistic regression models, which predict the probability of a positive outcome (graduation, bar passage, or obtaining a well-paying job) given a flexible function form for student credentials (allowing up to cubic powers of LSAT, UGPA, and their interactions—nine variables), race (white, black, Asian, other), school type, school type × race interactions (nine variables), and school type × credentials interactions (twenty-seven variables).

[17] Barnes, *supra* note 1, at 1769.

[18] *Id.* at 1807.

Table 1A provides four panels of comparisons, based on different student credentials. In the 2007 essay, I neglected to explicitly define how I determined the specific percentiles of student credentials. To be explicit here, I defined the percentile levels based upon a weighted average of LSAT and UGPA, as Professor Sander did in his original article.[19] None of the panels demonstrate this mismatch pattern. Historically black schools (HBS) have higher graduation rates than mid-range schools for students with very low credentials. At higher credential levels, top 30 schools have higher graduation rates than mid-range schools.[20]

Table 1B reports graduation rates under the race-based barriers portion of the model. The results here have not changed substantially: the only statistically significant result is that, compared to their white peers, Hispanic students at Top 30 schools have a slightly lower graduation rate.

---

[19] *See* Sander, *supra* note 2, at 393 (providing an equation for the academic index, which is a weighted average of LSAT and UGPA scores). Professor Williams and his coauthors state that I used separate values for the fifth percentile of LSAT and fifth percentile of UGPA. Williams et al., *supra* note 4, at 818. This is incorrect. As my original essay stated, for the fifth percentile, I compared against students whose "credentials are in the fifth percentile of the entire data set." Barnes, *supra* note 1, at 1776 n.61. Indeed, this value is far below the fifth percentile of overall student credentials because most students who have the LSAT scores in the fifth percentile also have better UGPAs and vice versa. Indeed, less than three-quarters of one percent of students have credentials at or below both fifth-percentile levels.

[20] Professor Williams and his coauthors report significance levels for tests comparing against top 30 schools, which demonstrate that top 30 schools often have statistically significant difference outcomes from other school types. Williams et al., *supra* note 4, at 827–28 tbl.3. However, this formulation of testing obscures the fact that the remaining school types—HBS, low-range schools, and lid-range schools—generally do not have significantly different outcomes.

TABLE 1A: LOGISTIC REGRESSION OF GRADUATION RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO MISMATCH TESTING

Variable	*ΔPr(Graduate)*[21]	
	2007 Essay	*Revised Results*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 5th percentile)		
Comparison (Baseline) Probability	83.3%	85.4%
Historically Black Schools	-4.1%	5.9%*
Low-Range Schools	-11.3%	-4.7%
Mid-Range Schools	—	—
Top 30 Schools	5.3%	0.8%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 10th percentile)		
Comparison (Baseline) Probability	87.2%	86.4%
Historically Black Schools	-4.8%	5.4%*
Low-Range Schools	-9.8%	-3.6%
Mid-Range Schools	—	—
Top 30 Schools	4.4%	0.7%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 25th percentile)		
Comparison (Baseline) Probability	89.7%	90.7%
Historically Black Schools	-1.8%	2.1%
Low-Range Schools	-4.9%	0.2%
Mid-Range Schools	—	—
Top 30 Schools	4.6%	2.2%*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 50th percentile)		
Comparison (Baseline) Probability	91.7%	91.6%
Historically Black Schools	-0.1%	1.6%
Low-Range Schools	-0.8%	1.8%
Mid-Range Schools	—	—
Top 30 Schools	4.3%	3.1%***
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$		

[21] 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 1B: LOGISTIC REGRESSION OF GRADUATION RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO RACE-BASED BARRIERS TESTING

	ΔPr(Graduate)[22]	
Variable	*2007 Essay*	*Revised Results*
Student Race (compare to White) (Historically Black Schools)		
Comparison (Baseline) Probability	91.5%	93.3%
Black	-6.7%	-5.9%*
Hispanic	-6.4%	-5.5%
Asian	-3.7%	2.9%
Student Race (compare to White) (Low-Range Schools)		
Comparison (Baseline) Probability	90.9%	93.5%
Black	-1.5%	-0.9%
Hispanic	1.9%	1.5%
Asian	5.1%	3.7%
Student Race (compare to White) (Mid-Range Schools)		
Comparison (Baseline) Probability	91.7%	91.6%
Black	-3.5%	-3.6%
Hispanic	-1.1%	-1.2%
Asian	-1.5%	-1.5%
Student Race (compare to White) (Top 30 Schools)		
Comparison (Baseline) Probability	95.9%	94.8%
Black	0%	0.1%
Hispanic	-2.5%	-3.1%**
Asian	0.6%	0.8%
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$		

22 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).

Tables 2A and 2B report results for the bar passage rate.[23] In Table 2A, testing mismatch, the results are significantly different from the 2007 essay. First, there is no evidence of an antimismatch effect. Second, the magnitudes of all effects are much smaller, particularly for HBS. Going to a Top 30 school with low credentials is a riskier proposition than matriculating to other school types, but there is no evidence that other schools are different from each other. Thus, the revised results do not demonstrate the monotonic pattern across the four school types indicative of the mismatch effect.

Table 2B investigates race-based barriers in bar passage. Here, the results are not substantively different, but the magnitude of the changes is generally smaller. Hispanic and black students are less likely to take and pass a bar exam from HBS; in other schools, no effects are statistically significant.

---

[23] The bar passage results published in this Revision contain a subtle error in the way that bar passage was defined. As Professors Williams, Sander, Luppino, and Bolus point out in their response, the bar passage results drop those individuals who graduated from law school but chose not to take a bar exam; these individuals should have been coded as not passing a bar exam. *See* Williams et al., *supra* note 4, at 817–18. Correcting this coding error, the bar passage results differ slightly, but the conclusions from the results remain the same. The professors alerted me to this error in a draft of their response provided in January 2011, and I responded by providing new results after fixing my coding error. Unfortunately, the *Northwestern University Law Review* editors made the decision not to allow me to report the corrected results in this publication. My understanding is that their decision is based upon the timing of the editing process, which was delayed at many points in the publication process, several of which were my fault, and a prior understanding that the professors could rely on the model and results from my earlier draft in writing their response. Although I believe that their decision to publish incorrect results does not reflect best practices, I have chosen to allow publication of my Revision with incorrect results to engage Professor Williams and his coauthors on the mismatch debate itself. I urge readers to disregard the bar passage results published here and instead rely upon the correct results, published on SSRN, *see* Katherine Y. Barnes, *Is Affirmative Action Responsible for the Achievement Gap between Black and White Law Students?* A Correction, a Lesson, and an Update (Aug. 12, 2011), http://ssrn.com/abstract=1908530, available on my website, *see* KATHIE BARNES DATA SETS, *supra* note 7, and available at the *Northwestern University Law Review*'s website, *see Data Sets for Northwestern University Law Review 105:2*, *supra* note 8. This affects only the bar passage results, which are contained in Tables 2A–B, 5, and 7.

TABLE 2A: LOGISTIC REGRESSION OF BAR PASSAGE RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO MISMATCH TESTING

	$\Delta Pr(Pass\ Bar)$[24]	
Variable	*2007 Essay*	*Revised Results*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 5th percentile)		
Comparison (Baseline) Probability	63.0%	73.7%
Historically Black Schools	-50.1%	4.2%
Low-Range Schools	-16.2%	-2.3%
Mid-Range Schools	—	—
Top 30 Schools	1.7%	-9.5%*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 10th percentile)		
Comparison (Baseline) Probability	73.8%	76.1%
Historically Black Schools	-46.8%	3.2%
Low-range Schools	-15.1%	-1.7%
Mid-Range Schools	—	—
Top 30 Schools	1.9%	-7.9%*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 25th percentile)		
Comparison (Baseline) Probability	80.3%	86.0%
Historically Black Schools	-17.9%	-1.7%
Low-Range Schools	-9.1%	-0.4%
Mid-Range Schools	—	—
Top 30 Schools	3.5%	0.5%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 50th percentile)		
Comparison (Baseline) Probability	84.8%	89.1%
Historically Black Schools	-7.6%	-1.4%
Low-Range Schools	-6.6%	-0.9%
Mid-Range Schools	—	—
Top 30 Schools	3.5%	2.5%***
$^{*}\ p < 0.05$; $^{**}\ p < 0.01$; $^{***}\ p < 0.001$		

24 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 2B: LOGISTIC REGRESSION OF BAR PASSAGE RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO RACE-BASED BARRIERS TESTING

	Δ*Pr(Pass Bar)*[25]	
Variable	*2007 Essay*	*Revised Results*
Student Race (compare to White) (Historically Black Schools)		
Comparison (Baseline) Probability	77.1%	87.7%
Black	-11.9%	-8.1%**
Hispanic	-7.1%	-7.0%
Asian	-26.0%	-14.5%
Student Race (compare to White) (Low-Range Schools)		
Comparison (Baseline) Probability	78.1%	88.2%
Black	-7.7%	-4.8%
Hispanic	5.6%	2.7%
Asian	-0.8%	-1.1%
Student Race (compare to White) (Mid-Range Schools)		
Comparison (Baseline) Probability	84.8%	89.1%
Black	-8.6%	-7.5%
Hispanic	-5.8%	-5.2%
Asian	-6.4%	-4.0%
Student Race (compare to White) (Top 30 Schools)		
Comparison (Baseline) Probability	88.3%	91.6%
Black	-0.9%	-1.7%
Hispanic	-1.6%	-3.5%*
Asian	1.0%	-0.3%
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$		

[25] 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). Bar passage is defined in the same way as in Table 2A. *See supra* note 23.

Tables 3A and 3B present the results of the logistic regression model that investigates the rate of reporting a well-paying job, defined as a job that pays more than $50,000 in 1995 dollars.[26] These results were not statistically significant: overall, the school type × credentials interactions were not different from zero.[27] This was also true in the 2007 essay. Thus, the results provide no evidence of a mismatch effect. One should note, however, that these data are particularly problematic because many students chose not to answer this question.

Finally, Table 3B also provides the results of the race-based-barriers theory on obtaining a well-paying job. Only in Top 30 schools were there any statistically significant differences. In those schools, black and Hispanic students were more likely to report having obtained a well-paying job than their white counterparts. But that statistical significance does not take into account the potential for bias in the results due to nonresponse or other reasons why minority students might search for a well-paying job more diligently than white students, such as their higher average debt loads, which make well-paying jobs more of a necessity.

Overall, the mismatch results have changed substantially. The reported results from the 2007 essay demonstrated an antimismatch effect. The corrected results do not. Nor do the results support the mismatch hypothesis.

---

[26] Professor Williams and his coauthors note that the results are based upon the definition of a" well-paying job" as a job that pays more than $50,000 per year rather than the $40,000 per year stated in the 2007 essay. The results are essentially unchanged across the two cutoffs. Results with the $40,000 cutoff are available from the *Northwestern University Law Review*, *see Data Sets for Northwestern University Law Review 105:2*, *supra* note 8, and my website, *see* KATHIE BARNES DATA SETS, *supra* note 7.

[27] One comparison, between black students and white students who have credentials at the fifty-percent level and have matriculated to Top 30 schools, is statistically significant at the five-percent level. The fact that black students at high-ranked schools are more likely to report obtaining a well-paying job does not support the mismatch hypothesis.

TABLE 3A: LOGISTIC REGRESSION OF WELL-PAYING JOB RATE[28] ALLOWING FOR BOTH MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO MISMATCH THEORY

	ΔPr(High Salary)[29]	
Variable	*2007 Essay*	*Revised Results*
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 5th percentile)		
Comparison (Baseline) Probability	6.1%	6.0%
Historically Black Schools	-5.0%	-5.5%
Low-Range Schools	-2.7%	2.0%
Mid-Range Schools	—	—
Top 30 Schools	2.4%	1.1%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 10th percentile)		
Comparison (Baseline) Probability	7.6%	6.7%
Historically Black Schools	-7.1%	-6.1%
Low-Range Schools	-2.3%	2.0%
Mid-Range Schools	—	—
Top 30 Schools	2.1%	0.9%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 25th percentile)		
Comparison (Baseline) Probability	9.6%	11.4%
Historically Black Schools	-9.4%	-8.5%
Low-Range Schools	-0.7%	-4.5%
Mid-Range Schools	—	—
Top 30 Schools	7.1%	1.4%
Mismatch (compare to Mid-Range Schools) (Fixed student credentials at 50th percentile)		
Comparison (Baseline) Probability	14.1%	14.0%
Historically Black Schools	-13.4%	3.3%
Low-Range Schools	-5.0%	-10.1%
Mid-Range Schools	—	—
Top 30 Schools	20.7%	6.4%*
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$		

[28] Professor Williams and his coauthors note that, in my 2007 essay, I reported an unweighted population percentage for the percentage of students who obtained a well-paying job. *See* Williams et al., *supra* note 4, at 821 (citing Barnes, *supra* note 1, at 1775). This was an error: using the weighted average is correct in this situation. This does not, however, affect the results of my logistic regression model, for which using unweighted values is the most appropriate method. *See* Charles F. Manski & Daniel McFadden, *Alternative Estimators and Sample Designs for Discrete Choice Analysis*, *in* STRUCTURAL ANALYSIS OF DISCRETE DATA WITH ECONOMETRIC APPLICATIONS 2 (1981).

[29] 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: LOGISTIC REGRESSION OF WELL-PAYING JOB RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—RESULTS RELEVANT TO RACE-BASED BARRIERS TESTING

	$\Delta Pr(High\ Salary)$[30]	
Variable	*2007 Essay*	*Revised Results*
Student Race (compare to White) (Historically Black Schools)		
Comparison (Baseline) Probability	0.7%	17.3%
Black	7.6%	56.4%
Hispanic	0.1%	2.9%
Asian	†	†
Student Race (compare to White) (Low-Range Schools)		
Comparison (Baseline) Probability	9.1%	3.9%
Black	11.3%	5.5%
Hispanic	2.1%	1.0%
Asian	†	†
Student Race (compare to White) (Mid-Range Schools)		
Comparison (Baseline) Probability	14.1%	14.0%
Black	7.9%	8.2%
Hispanic	5.9%	6.0%
Asian	6.1%	6.1%
Student Race (compare to White) (Top 30 Schools)		
Comparison (Baseline) Probability	34.8%	20.4%
Black	29.7%	26.7%***
Hispanic	9.7%	7.6%*
Asian	6.3%	4.7%
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$		
† Insufficient variation in the data to estimate this parameter.		

30 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).

Tables 4, 5, and 6 combine the two possible effects—a mismatch effect with a race-based barriers effect—for black law students in an attempt to provide the best advice for these students: should they follow conventional wisdom and go to the best school to which they are admitted or go to a lower ranked school to avoid being outmatched by their classmates?

The results from the corrected model are straightforward. If one has the option, go to a Top 30 school, particularly for better graduation rates; otherwise, there is not a statistically significant difference in outcomes.

TABLE 4: LOGISTIC REGRESSION OF GRADUATION RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—COMBINATION OF MISMATCH AND RACE-BASED BARRIERS THEORIES FOR BLACK STUDENTS

	Pr(Graduate)[31]	
Variable	*2007 Essay*	*Revised Results*
Fixed student credentials at 5th percentile		
Historically Black Schools	66.3%	84.0%
Low-Range Schools	68.5%	78.6%
Mid-Range Schools	77.0%	79.2%
Top 30 Schools	88.5%	86.6%***
Fixed student credentials at 10th percentile		
Historically Black Schools	70.9%	84.8%
Low-Range Schools	74.4%	80.8%
Mid-Range Schools	82.2%	81.0%
Top 30 Schools	91.6%	87.4%*
Fixed student credentials at 25th percentile		
Historically Black Schools	79.0%	86.7%
Low-Range Schools	82.6%	89.8%
Mid-Range Schools	85.4%	86.8%
Top 30 Schools	94.3%	93.0%**
Fixed student credentials at 50th percentile		
Historically Black Schools	84.9%	87.4%
Low-Range Schools	89.4%	92.6%
Mid-Range Schools	88.1%	88.0%
Top 30 Schools	95.9%	94.9%***
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$		

[31] This number provides the probability of graduation for black students matriculating to given school types holding credentials at the specified value.

TABLE 5: LOGISTIC REGRESSION OF BAR PASSAGE RATE ALLOWING FOR MISMATCH AND RACE-BASED BARRIERS—COMBINATION OF MISMATCH AND RACE-BASED BARRIERS THEORIES FOR BLACK STUDENTS

Variable	*Pr(Pass Bar)*[32] *2007 Essay*	*Revised Results*
Fixed student credentials at 5th percentile		
Historically Black Schools	7.6%	66.0%
Low-Range Schools	36.9%	62.8%
Mid-Range Schools	49.5%	60.4%
Top 30 Schools	62.7%	59.6%
Fixed student credentials at 10th percentile		
Historically Black Schools	17.0%	67.8%
Low-Range Schools	48.6%	66.2%
Mid-Range Schools	61.8%	63.3%
Top 30 Schools	74.1%	63.7%
Fixed student credentials at 25th percentile		
Historically Black Schools	48.0%	74.7%
Low-Range Schools	62.2%	80.0%
Mid-Range Schools	70.1%	76.9%
Top 30 Schools	82.6%	84.0%*
Fixed student credentials at 50th percentile		
Historically Black Schools	65.2%	79.6%
Low-Range Schools	70.3%	83.4%
Mid-Range Schools	76.1%	81.6%
Top 30 Schools	87.3%	89.9%***
$^{*}p < 0.05$; $^{**}p < 0.01$; $^{***}p < 0.001$		

[32] This number provides the probability of bar passage for black students matriculating to given school types holding credentials at the specified value.

TABLE 6: LOGISTIC REGRESSION OF WELL-PAYING JOB RATE ALLOWING FOR BOTH MISMATCH AND RACE-BASED BARRIERS—COMBINATION OF MISMATCH AND RACE-BASED BARRIERS THEORIES FOR BLACK STUDENTS

Variable	*Pr(Well-Paying Job)*[33] *2007 Essay*	*Revised Results*
Fixed student credentials at 5th percentile		
Historically Black Schools	12.7%	6.2%
Low-Range Schools	8.1%	18.3%
Mid-Range Schools	10.0%	10.1%
Top 30 Schools	24.0%	21.0%*
Fixed student credentials at 10th percentile		
Historically Black Schools	6.1%	7.4%
Low-Range Schools	12.5%	19.5%
Mid-Range Schools	12.4%	11.1%
Top 30 Schools	26.8%	22.2%**
Fixed student credentials at 25th percentile		
Historically Black Schools	3.2%	28.9%
Low-Range Schools	20.0%	16.0%
Mid-Range Schools	15.5%	18.5%
Top 30 Schools	40.5%	33.9%*
Fixed student credentials at 50th percentile		
Historically Black Schools	8.3%	73.7%
Low-Range Schools	20.3%	9.4%
Mid-Range Schools	22.0%	22.2%
Top 30 Schools	64.5%	47.2%***
$^{*}\ p < 0.05$; $^{**}\ p < 0.01$; $^{***}\ p < 0.001$		

33 This number provides the probability of obtaining a well-paying job for black students matriculating to given school types holding credentials at the specified value.

The results of the simulations of alternative affirmative action policies depend on the specific model results from above. I present the revised results below. The primary question Tables 7, 8, and 9 attempt to answer is a counterfactual: what would happen under different affirmative action policies, assuming that nothing else pertinent changes, such as the culture of an institution or its applicant pool? Table 7 provides the results of four different affirmative action policies on the number of bar passers and finds that the different policies would have no statistically significant difference on the number of new black lawyers each year.[34] In their response, Professor Williams and his coauthors state that my model suggests that ending affirmative action would increase the bar passage rate for black law students by 27% because of the smaller pool of potential black bar takers absent affirmative action. This 27% statistic is not relevant to mismatch. Moreover, this statistic obscures the fact that these black law students are a significantly different population than the full sample of black law students because the students with the worst credentials were dropped. The fact that these students do significantly better is not surprising. Nor, as Professor Williams and his coauthors also suggest, does affirmative action necessarily increase the *failure* rate because choosing not to take a bar exam is not a *failure*. Law school is a risky proposition for students at the very low end of credentials; this is not an issue of which school these students choose, as the mismatch theory predicts, but rather whether they should take the gamble and go to law school.

Table 8 provides the results for the expected number of minority graduates. Again, there is no statistically significant difference across different affirmative action policies. Finally, Table 9 suggests that there may be one detriment for black law students under a "no affirmative action" policy: fewer well-paying jobs. Given the low response rate for this question, however, this result is suggestive only.

---

[34] The four policies are status quo affirmative action; no affirmative action, in which minority applicants are admitted to institutions based on the probability that a white student with the same credentials would be admitted, assuming that the bottom 14% of minority students would not matriculate to a law school; affirmative action "light," which provides only half the boost in admission rates that minorities currently receive and assumes that 7% of minority students who would otherwise have matriculated to a law school would be denied admission; and affirmative action "plus," which provides twice the boost that minority applicants received. Professor Williams and his coauthors point out in their response that I assume that 14% of underrepresented minority students, rather than 14% of black students, would not matriculate to law schools absent affirmative action. The results remain essentially the same using only black students. Results are available from the *Northwestern University Law Review*, *see Data Sets for Northwestern University Law Review 105:2*, *supra* note 8, and from my website, *see* KATHIE BARNES DATA SETS, *supra* note 7.

TABLE 7: BAR PASSAGE SIMULATION FOR THREE DIFFERENT MODELS

Type of Admissions Policy	*White Bar Passers* (s.e.)[35]	*Black Bar Passers* (s.e.)	*Hispanic Bar Passers* (s.e.)	*Asian Bar Passers* (s.e.)
Affirmative Action (Status Quo)	19,762 (73)	1141 (33)	972 (31)	906 (29)
No Affirmative Action (No boost for minority applicants)	19,764 (73)	1134 (33)	986 (30)	905 (30)
Affirmative Action Light (Half the boost for minority applicants)	19,762 (74)	1135 (33)	979 (31)	906 (29)
Affirmative Action Plus (Twice the boost for minority applicants)	19,761 (74)	1152 (33)	967 (30)	906 (29)
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$				

35 "s.e." stands for "standard error," which is a measure of how much variability there is in the given statistic across repetitions of the same simulation.

TABLE 8: SIMULATION OF NUMBER OF GRADUATES FOR THREE DIFFERENT MODELS

Type of Admissions Policy	*White Graduates* (s.e.)	*Black Graduates* (s.e.)	*Hispanic Graduates* (s.e.)	*Asian Graduates* (s.e.)
Affirmative Action (Status Quo)	20,428 (71)	1481 (37)	1112 (32)	988 (30)
No Affirmative Action (No boost for minority applicants)	20,430 (71)	1459 (37)	1118 (32)	988 (31)
Affirmative Action Light (Half the boost for minority applicants)	20,429 (71)	1466 (37)	1115 (32)	988 (30)
Affirmative Action Plus (Twice the boost for minority applicants)	20,428 (71)	1499 (38)	1110 (32)	988 (30)
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$				

TABLE 9: SIMULATIONS OF GRADUATES WITH WELL-PAYING JOBS FOR THREE DIFFERENT MODELS

Type of Admissions Policy	*White Job Takers* (s.e.)	*Black Job Takers* (s.e.)	*Hispanic Job Takers* (s.e.)	*Asian Job Takers* (s.e.)
Affirmative Action (Status Quo)	4616 (61)	310 (18)	236 (16)	252 (16)
No Affirmative Action (No boost for minority applicants)	4612 (63)	265* (16)	220 (15)	252 (16)
Affirmative Action Light (Half the boost for minority applicants)	4616 (63)	284 (16)	227 (16)	252 (16)
Affirmative Action Plus (Twice the boost for minority applicants)	4615 (62)	340 (19)	248 (15)	252 (16)
* $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$				

CONCLUSION

The revised results present a different picture of student outcomes. The data do not support either the antimismatch effect or the mismatch hypothesis: mismatched students do not explain the racial gap in student outcomes. The weakest students do not have systematically different outcomes at HBS, low-range schools, or mid-range schools. Black students have lower bar passage rates at HBS schools than at other institutions. Thus, the results suggest that there remain other factors, which I term race-based barriers, that adversely affect minority law student performance.

Professors Williams, Sander, Luppino, and Bolus write that my conclusions are "exactly opposite" to the conclusions in my 2007 essay, suggesting that my revised results support mismatch.[36] This is incorrect. Their first argument is that ending affirmative action would increase the percentage of black law students who pass the bar by 27%.[37] This is irrelevant to mismatch. Their second argument is that I have miscoded bar passage in this Revision.[38] I fixed this coding but was not permitted to publish it here.

[36] Williams et al., *supra* note 4, at 822.
[37] *Id.* at 814.
[38] *Id.* at 817–18.

Nonetheless, the recoding did not change the conclusions.[39] Their next point is that I "subtly altered" the model by changing the definitions of the percentiles of credentials.[40] The specifications here make sense, are explicitly described, and are not altered from the original model. Finally, the professors note that I used a $50,000 rather than a $40,000 cutoff for high-paying jobs.[41] The results are not sensitive to this change. In summary, the results do not support the mismatch hypothesis.

When I originally began this project six years ago, one goal I had was to incorporate the large amount of uncertainty in the data regarding the results of Professor Sander's 2004 article.[42] This was the impetus for the simulations in Part III of the original essay[43] and the discussion of possible experimental data in Part IV.[44] However, the 2007 results I found were strikingly different from Professor Sander's, and the focus of the essay shifted. Although the revised results demonstrate a more nuanced picture of student outcomes, the underlying data remain uncertain.

Sometimes to answer key empirical questions one must first obtain better data. I advocated for this in my original 2007 essay, and I do so again now. Although the data provide tentative conclusions about whether mismatch or a race-based barriers theory might explain the difference in some outcomes between black and white students, the data, simply put, are not up to the task of definitively determining the cause of outcome differences.

---

[39] *See supra* note 7. I again urge readers to rely upon the corrected results provided at SSRN, Barnes, *supra* note 23, and at my website, KATHIE BARNES DATA SETS, *supra* note 7.

[40] Williams et al., *supra* note 4, at 818.

[41] *Id.* at 821.

[42] Sander, supra note 2.

[43] Barnes, *supra* note 1, at 1801–06.

[44] *Id.* at 1806–08.