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## Michigan's Minority Graduates in Practice: Answers to Methodological Queries

Richard O. Lempert, David L. Chambers, and Terry K. Adams

Before making a few remarks in response to those who commented on our article (Lempert, Chambers, and Adams 2000), we would like to express our gratitude to the editors of *Law and Social Inquiry* for securing these commentaries and to the people who wrote them. The comments both highlight the potential uses to which our research and similar studies may be put and give us the opportunity to address methodological concerns and questions that other readers of our article may share with those who commented on it.

The responses to our work are of two types. Professors Nelson, Payne, and Sander focus largely on methodological concerns and the limits of what can be learned from our study. Professors Guinier, Russell, and Wilkins draw on our work to pursue their own themes. Except for clearing up some understandable but nonetheless mistaken impressions of Professor Russell, we shall not discuss the Guinier, Russell, and Wilkins essays, for we think they stand on their own as articles and do not call for a response. For those who may similarly seek to draw on our research, however, we wish to emphasize a point that Professor Sander makes and that is at the heart of Professor Wilkins's important contribution. Our study looks at the graduates of only one law school, a school that is among the most selective and prestigious of American law schools. In comparison to the graduates of most law schools, for example, far more Michigan graduates work in large private firms, and far fewer work as solo practitioners or in very small firms. While we are confident in the general validity of our results as they apply to Michigan graduates and believe that our findings are likely to generalize to graduates of most so-called elite American law schools, our findings cannot be safely

generalized to graduates of other law schools. We are not saying that our findings will not generalize beyond the graduates of elite law schools, but we are noting that no strong claim can be made that they will.

Russell understandably thinks that our work was patterned on Bowen and Bok's and was undertaken in response to the lawsuit that has been brought to end affirmative action at the University of Michigan. Neither assumption is true. Our data were in hand before we knew of Bowen and Bok's study and, although we knew that Michigan was among the schools that might be the target for a lawsuit, planning for our study began more than a year before the suit was filed.<sup>1</sup>

Russell, along with Guinier, wonders about the Michigan law faculty's involvement in setting the school's admissions policy. Russell writes, "I cannot tell whether an associate dean wrote the policy and the faculty approved it without discussion, or if the faculty debated and thoughtfully considered the policy" (2000, 511 n.2). In fact, a faculty committee (chaired by Lempert) worked on the school's admissions policy for the better part of a year, and the law faculty approved the policy after considerable discussion. Having decided on its admissions policy, the faculty at Michigan has looked largely to its professional admissions staff, in consultation with a faculty admissions committee, to carry out the faculty's wishes.

We turn now to methodological issues. Professor Sander has a number of concerns that we are glad to have the opportunity to address. First, he asks about the weight given to LSAT/UGPA index scores relative, we assume, to letters of recommendation, impressions in interviews, and other softer measures an application reveals. Nelson and Payne are similarly curious about the details of Michigan's admissions process.

It is difficult to specify the relative weights accorded the different factors that figure in Michigan's admissions decisions. The admissions index is almost dispositive in establishing eligibility for consideration for admission. Minority and white applicants with indexes below a certain level are almost always rejected. At the other extreme, applicants with very high index scores relative to the rest of the applicant pool are almost certain to be admitted. But below the top level, there are many more acceptable applicants than there are spaces in an entering class. While at all levels, higher index scores enhance prospects for admission, many people who are admitted have the same or lower index scores than many applicants who are not admitted. Indeed, as our article notes (2000, 482–83), for most of the years of our study, the Michigan admissions system was designed to focus many admissions decisions on softer data, after so-called hard credentials were

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1. The study's subject is a long-standing interest of the authors. More than a decade ago Chambers and Lempert applied for, but did not receive, an NSF grant to study minority lawyers in practice, and Adams and Chambers, in their work on the Michigan alumni survey, have for years been interested in the careers of minority alumni.

used to admit some people automatically and to establish a pool for the remaining positions. In practice this process meant that applicants with higher admissions indexes would not always fare better than those with lower ones.

Historically, Michigan's admissions policies have paid special attention to a variety of factors. One is in-state residence. Hence, many Michigan residents are admitted to the law school with index scores below those of most admitted and some rejected out-of-state applicants. A second is the potential contribution of an applicant to the diversity of the school's student body, including especially, its ethnic diversity. Hence, members of certain minority groups are frequently admitted with index scores below those of most admitted and many rejected whites applicants. We can get some perspective on how this latter factor has affected admissions decisions by looking at the LSAT scores and UGPAs of minorities and whites in our sample. Over the 27-year period of our study, the median LSAT percentile ranking of our white respondents is 94, and their median UGPA is 3.51. The median LSAT percentile for minority respondents is 66, and their median UGPA is 3.1. The LSAT scores of Michigan's minority students have, however, increased considerably over time and at a faster rate than the scores of whites have increased. So minority students who graduated toward the end of our sample period have LSAT scores and admissions indexes that are, on average, closer to the average scores of white admittees than are the scores of minorities who graduated toward the beginning of the period we study.

These numbers say something about what affirmative action has meant at Michigan, but one must be cautious in interpreting them as indicators of the extent of affirmative action. We have data only for students who matriculated —at Michigan. Accepted students who did not choose to matriculate at Michigan are likely to have had higher scores than those who came. Thus, the credentials of all Michigan admittees are likely to be higher than what we report, and if competition among law schools for high-scoring minority admittees is more intense than competition for whites, the gap between the admissions credentials of white and minority admittees is likely to be less than we report. More fundamentally, the gap in index scores does not necessarily indicate the degree of affirmative action unless the index is the sole determinant of admissions decisions, which it was not. If, for example, the index had played no role in admissions and admission decisions had been based entirely on a color-blind evaluation of other admissions credentials, minority and white admittees might still have index score differences of the magnitude we found. This is because those in the pool of whites who apply to Michigan have, on average, higher index scores than those in the pool of minorities who apply. Any system that selects applicants from these

pools by criteria unrelated to index scores will select a group of minority students with scores below those of white applicants.

Sander criticizes us for collapsing black, Latino, and Native Americans into the category *minority* because, he suggests, small reported differences between whites and minorities might conceal larger differences between blacks and whites that have been masked by our lumping blacks with assimilated Latino. Yet even if Sander were right in suggesting that we did not attend to black-Latino differences, it would still be correct to combine these groups if the goal is to evaluate the effects of Michigan's minority admissions program since the program includes the three groups we treat together. Moreover, combining the groups would be unlikely to disguise substantial black-white differences since about two-thirds of our minority respondents are blacks. But, although we don't highlight black-Latino differences, the claim that we don't attend to these differences is mistaken. In the equations reported in tables 31, 33, and 35, which examine income, reported satisfaction, and service, we present two models, one of which disaggregates the minority group and provides the separate information for blacks, Latinos, and Native Americans that Sander seeks. We see from these models that blacks do not differ significantly from whites on income or satisfaction, but they engage in more service. Although our models do not directly contrast blacks with Latinos, it appears from them that the two groups are unlikely to differ significantly on income or satisfaction, although on service blacks tend to do more. Moreover, our study did not ignore the possibility of black-Latino differences. We looked for differences on most of the variables in our tables and seldom found them to be significant. When we found significant differences between the two groups we mentioned them (2000, nn.18, 32, 42, 44).

Sander is also concerned that by presenting most of our tabular data in decade cohorts, we are biasing our study against finding significant minority-white differences. We understand the source of Sander's concern: in presenting our data by decades, we reduce the sizes of the groups we compare. But we don't think this distorts our findings. Many of our most important findings come from the regression models with which we conclude our data analysis. These models use all available cases. As we have seen, they fail to show significant minority-white differences in income or satisfaction, reveal a difference favoring minorities in service, and indicate that our admissions index is not a significant predictor of subsequent income or satisfaction.

With respect to the tables, we presented our data as we did not to obscure significant relationships, but because graduation year was associated, for cohort or maturation reasons, with many of the variables we examined, and the proportion of minority students in each class at Michigan increased substantially over time. Not dividing the data into time periods

would have meant that more recent law school graduates disproportionately dominated the minority sample, while respondents in the white sample, because of the way we stratified, were somewhat skewed toward the earlier years.<sup>2</sup> Thus, we might have reported minority-white differences that were more properly attributed to differences in graduation dates. Presenting our data by decade cohorts is a compromise between (1) considering each respondent's graduation year so as not to be misled by time-associated changes in outcome and sample composition and (2) maintaining sufficient sample sizes in our tables so that our significance tests have reasonable power.<sup>3</sup> The use of decade cohorts rather than full-sample regressions does not necessarily bias our tables against finding significant differences, for the biases attributable to this compromise work both ways.

To shed empirical light on Sander's concern, we turned to the regressions we constructed to look for gender and gender/minority status interaction effects.<sup>4</sup> These full sample regressions include as dependent variables most variables that figure in our tables.<sup>5</sup> We can extract from these equations the significance of minority status after controlling for time since graduation and age entering law school. What we find in reviewing these results is that in most cases if the minority-white difference was significant at the .05 level or beyond in the regression, it was also significant in at least one of the decade cohorts in the associated table. Differences that were not significant in the regressions were usually not significant in any cohort in the associated tables.

Sander's intuition is, however, sometimes correct in that some differences between whites and minorities that were not significant in any cohort in the related table were significant in the full-sample regressions. We found five such instances. These are (1) the value of law school as career training in table 3 (2) social satisfaction with law school in table 3, (3) the proportion of graduates taking judicial clerkships in table 9, (4) the proportion of elite law school graduates among the other lawyers in respondents' firms in table 16, and (5) the proportion of time those in private practice devote to government clients in table 17. The regression results, however, do nothing

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2. The weights we used in constructing most of our tables correct for this.

3. Given our sample size, taking account of each respondent's graduation year would have precluded presenting our data in table form.

4. See the model specified in the text at footnote 6 of our article.

5. The gender-effect models we constructed did not include regressions with income or the satisfaction or service indexes as dependent variables, since the gender variables had been included in the regressions we ran for presentation in tables 31–36. In dealing with our current concern, we did not examine the regressions for the variables that figure in many of the tables that employed chi square tests of significance, but almost all of these report finding at least some significant relationships, so reporting the data by decades and running chi square tests separately for each decade did not disguise significant associations. Also, we did not construct regressions for the dependent variables in some of the subtables we present, such as the tables that look at the dimensions of satisfaction separately for respondents in private practice.

to change our interpretation of the data. We can be somewhat more confident that Michigan's minority graduates value law school as career training more than its white graduates, and that if in private practice they spend a bit more time than its white graduates working for government clients. But these trends seemed likely from eyeballing the tables. We noted the difference between whites and minorities in their social satisfaction with law school, citing the regression analysis, and we also noted that this is not a main effect of minority status, but rather appears due to the low social satisfaction of minority women. Finally, the significance of the difference in clerkship attainment is no surprise because table 9 revealed marginally significant differences ( $.05 < p < .10$ ) in clerkship attainment in the 1970 and 1980 cohorts. The fact that the significance level increases when the full sample is analyzed does not affect the finding that in the 1990s there was no significant difference in the tendencies of whites and minorities to take judicial clerkships. By revealing the lack of a significant difference in the 1990s, along with the marginally significant differences in earlier decades, the table creates the more accurate impression, although it is also right to say that over the period of our study minorities were significantly less likely than white graduates to take judicial clerkships.

These five instances are fewer than the dozen in which Sander thought our decision to present data by cohorts obscured significant differences. Moreover, we did not find a significant difference between minorities and whites in overall satisfaction with law school despite Sander's claim that the table shows a full-sample difference significant at the .02 level. Indeed, we are somewhat puzzled that Sander used table 3 for his example because we report in our article that a full-sample regression revealed no significant white-minority difference in overall satisfaction and that further analysis suggested that the difference in the table is attributable to the fact that minorities tended to receive lower grades than whites. Controlling for grades, minorities appear more satisfied than whites with their law school experience.

Using decade cohorts to present our data, rather than drawing on full-sample regressions, did not create a bias against finding statistically significant differences between whites and minorities. Counterbalancing the five instances where our regressions showed significant differences not present in any decade cohort are six instances where our tables show differences between whites and minorities significant in at least one decade cohort that were insignificant in the associated full-sample regression. These are (1) satisfaction with law school as an intellectual experience in table 3, (2) the value of being called on in table 4, (3) first-job firm size in table 11, (4) attainment of supervisory or managing attorney status in table 13, (5) attainment of partnership status in table 15, and (6) satisfaction with co-worker relations in table 22. These results do not necessarily mean that the

statistically significant differences shown in the tables are misleading, for there may have been a statistically significant difference between whites and minorities in one decade but not in all decades taken together.<sup>6</sup>

Sander is also concerned about the fact that the minorities we surveyed responded at a lower rate than whites, a point he emphasizes by making the 10 percentage point difference in the proportion of whites and minority alumni responding appear to be twice that by treating the difference as a percentage of the white response rate. Sander, however, correctly realizes that the issue is not one of response-rate differences, but is rather whether these differences introduced serious biases into our analysis. Here we think Nelson and Payne have it right when they say that we “effectively answered these concerns.” Sander apparently disagrees, and to justify his concern he points out that the nonresponse rate among the 174 minority graduates we knew on the basis of pre-survey data to be working in firms of 50 or more lawyers was 41%, only 2% higher than the nonresponse rate for whites. What Sander does not consider is that this effort to identify Michigan’s minority alumni working in firms of 50 or more almost certainly missed some alumni. Thus, the minority response rate he constructs is likely to be higher, and may be quite a bit higher, than the actual response rate for minority alumni working in medium-sized and large law firms.

Nelson and Payne echo some of Sander’s concerns, and to the extent they do, we have already dealt with them. They would additionally like to know the simple bivariate relationship between minority status and earnings, for they see this as an important social fact in its own right. We disagree. Because time since graduation figures so importantly in earnings (for quite understandable reasons) and because a higher proportion of our minority sample than of our white sample consists of recent graduates, a significant association between minority status and earnings might tell us nothing more than that the minority graduates in our sample have, on average, not been in the workforce as long as our white graduates. Nevertheless, because we believe in empirical answers to empirical questions, we regressed log of income first on time since graduation and minority status and then on

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6. There are several reasons why a relationship might be significant in one or more decade cohorts but not in a full-sample regression with time since graduation controlled. First, the relationship may have been strong only in a particular decade, and adding cases from other decades might dilute the strength of the association. Second, the relationship might not have been significant, controlling for time since graduation, even within a decade. As we noted in our article, this seems to be why more white than minority graduates of the 1980s reported being partners at the time of our study. A much higher proportion of minorities than whites graduated late in the decade, and so at the time of our survey would not have been in practice long enough for the partnership decision to have been reached. Finally, most of the tables that report significant relationships not confirmed by the regressions break down a continuous variable into categories. The regressions treat the variables as continuous, which may account for differences in significance, and may explain the difference between the significance of the chi square Sander calculated for overall satisfaction with law school and the insignificant *t* statistic in our regression results.



minority status alone. In the first regression, minority status was insignificant, and the adjusted variance explained by the model actually diminished by .1% when minority status was added to the equation. In the second regression the correlation between minority status and log of income was significant, but it was only .108, which means that although no variables are competing with it, minority status can explain only 1.2% of the variance in logged income.<sup>7</sup> These results are consistent with our suggestion that any correlation of income with minority status might plausibly be explained by a time since graduation. Thus, we do not think that Nelson and Payne's conclusion that Michigan's minority graduates are less well paid than its white graduates is a fair one based on our data.

Nelson and Payne also believe we should have paid more attention to gender and that a comprehensive analysis of minority-majority differences must look at race-by-gender relations. We agree. But we also believe that our decision to deemphasize gender in this article does not affect the validity of our findings regarding minority status. In addition to including gender and a gender/minority-status interaction term in our income, satisfaction, and service regressions (with the interaction term dropped from the models we presented because of its insignificance)(2000, n.56), we also checked to see whether gender effects might distort minority status effects in our tables by doing regression analyses of 84 variables, including most of the variables the tables highlight. Somewhat to our surprise, when gender was entered into our equations immediately after minority status, in no case did it render a previously significant minority-status variable insignificant or a previously insignificant minority-status variable significant. On a few occasions, controlling for the interaction of gender and minority status did change the significance of the minority-status variable. These instances are noted in the text or footnotes of our article. From this we conclude that although gender is important to understanding the career situations of white and minority lawyers, our decision to largely ignore gender in this article provides no reason to question our findings regarding minority-white differences or the lack thereof.

We also agree with Nelson and Payne on the desirability of attending to social class in considering the practice situations of law school graduates. Unfortunately, the data we collected contain no good measure of social class. Thus, we cannot do much to control for or illuminate social-class effects with further analysis of these data.<sup>8</sup> Yet the robustness of our results

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7. With weighted data, there was no change in the adjusted explained variance when we added minority status to a model containing time since graduation, and minority status by itself correlated with logged income at .079, which means it explained only 0.6% of the variance.

8. Preliminary analysis of Michigan's annual alumni survey data indicates, as Nelson and Payne suspect, that a greater proportion of minority respondents than of whites, in the classes

to what we can control leads us to believe that attention to social class would not change the picture our data paint.

Nelson and Payne and Sander would all like to know what our results would look like if we had excluded from our minority sample minority graduates who would have been admitted to Michigan without a boost from affirmative action. Their concern is that the success of these graduates explains why minority status and admissions credentials seem not to explain current income or career satisfaction. We understand why they are curious and concerned, but there is a good argument that the groups should not be separated. The success of minorities who would have been admitted to Michigan without affirmative action may be due in considerable measure to the existence of the program. Without an affirmative action program, minority students might be present at Michigan in numbers as small as in the post-*Hopwood* Texas Law School that Russell describes. Standing out, as they would if they were present in such small numbers, and without enough fellow ethnics to constitute a community or support group, they might have felt stresses manifested in poorer law school performance. They also would have had fewer opportunities to develop ties to fellow ethnics while in law school. Lower grades and sparser networks could be expected to have career costs after graduation. Thus, we think it is reasonable to include all minorities eligible for affirmative action not only when our concern is with how Michigan's minority graduates fare in practice, but also in looking at the implications of the school's affirmative action program for the success of all minorities, whatever their qualifications, that the school admitted.

Moreover, if we turn from theory to practice, it is impossible to identify with certainty most of those minority students at Michigan who would have been admitted had the school not had an affirmative action program. Many minority students with admissions indexes in the range of white admittees nevertheless benefited at the admissions stage from Michigan's affirmative action program. This is because, like most of their white counterparts, most minority students with admissions indexes sufficient for admission to Michigan without affirmative action nonetheless do not have quantitative credentials so strong as to guarantee their admission. Their admissions indexes are in a range where some similarly credentialed white applicants are admitted and some (often the greater number of applicants) are rejected. Without diversity-based affirmative action—that is, with a truly race-blind admissions procedure—some of these minority applicants would have succeeded in the competition for places on the basis of their letters of recommendation, extracurricular activities, and other indicators of accomplishment, but others would not. Michigan's concern for diversity meant that all these students presented very strong cases for admission, and we have no way of

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of 1970–91, report having blue-collar, pink-collar, or clerical/sales parents, and a greater proportion of whites report having professional, executive, or business-owner parents.

distinguishing most of those students who would have made it had a concern for diversity not existed from those who would not have been admitted. Thus, it is reasonable to include in the group of minority graduates, most minority graduates with admissions indexes in the range of white graduates, if the goal is to compare the careers of minority students who benefited from diversity-based affirmative action at admission with the careers of Michigan's white alumni who did not benefit from the school's concern for ethnic diversity.

Although our study's approach seems right, our commitment to giving empirical answers to empirical questions remains. Thus, we tried to do what strictly speaking cannot be done: identify a group of minority students who would have been admitted to Michigan in the absence of an affirmative action program so that we could exclude them from our minority sample, and with the minority group newly defined, redo our crucial analyses. To accomplish this we took four-year moving percentiles of the admissions indexes<sup>9</sup> of the whites in our sample. A minority graduate of a given year was considered to be someone who would have been admitted to Michigan absent affirmative action if that person had an admissions index above the moving 20th percentile of the index scores of whites in our sample for that year. This procedure must label some minorities incorrectly, but for reasons given above, the errors, on balance, should work to classify more minorities as admitted without attention to diversity than would have been the case if Michigan was unconcerned with securing ethnically diverse classes. In other words, without attention to diversity, a number of minorities with admissions indexes above the 20th percentile of the indexes of whites matriculating their year would not have been accepted. This number is likely to exceed the number of minorities with admissions indexes below the 20th percentile who would have gotten in.<sup>10</sup> This procedure identified 78 black,

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9. We remind our readers that the admissions indexes we used in our study, which are all we had available for this exercise, were indexes we constructed. We could not employ the admissions indexes the school used when evaluating the applications of those in our sample, since the formula for constructing the school's index changed from time to time, and we did not know what it had been at various times. Our indexes are essentially a linear combination of each respondent's LSAT score and UGPA.

10. We believe our procedures although crude are reasonable. We used moving percentiles to attain greater stability. We specified the 20th percentile cutoff a priori and did not explore any other cutoff points. We did not correct for the bias which results from the fact that we are looking only at students who matriculated at Michigan. Hence, the 20th percentile cutoff we identify is lower than we would have found had we looked at the index scores of all students admitted to Michigan, including those who chose to go elsewhere. We could not control for residence status because of cases without information on this variable, but the distribution of cases suggests this is unlikely to create important biases. Ideally, of course, we would have approached this task differently. Unfortunately we lacked data on the proportion of whites with different index scores who are accepted at Michigan. Had we had that data, we could have established probabilities for the admission of minorities across the range of index scores, and used bootstrap techniques to generate multiple plausible samples of admittees for further analysis.

TABLE 1  
OLS Regression Coefficients for Logged Income

Model 2 (n = 924)		
	b	Std. Error
Constant	9.819***	0.265
Years since graduation	0.090***	0.012
Years since graduation squared	-0.002***	0.0004
Sex (M = 0, F = 1)	-0.175***	0.052
Age entering law school	-0.017*	0.007
Minority or white (W = 0, M = 1)	0.122	0.077
LSAT/UGPA index	-0.002	0.0008
Undergraduate major (social sciences omitted)		
Humanities	0.135*	0.055
Natural Sciences	-0.079	0.115
Business	0.135*	0.062
Engineering	0.009	0.122
Other	-0.039	0.093
Final LSGPA	0.005***	0.0006
Current job sector (private practice omitted)		
Business/Finance	0.175**	0.062
Government	-0.342***	0.063
Legal Svc/Pub.Int.	-0.590***	0.142
Education	-0.624***	0.098
Other	-0.383***	0.103

\* $p < .05$   
\*\* $p < .01$   
\*\*\* $p < .001$

( $R^2 = .326$ ), (Adj.  $R^2 = .313$ )

Latino, and Native American graduates in our sample as people who (hypothetically) would have been admitted to Michigan even if the school had not been committed to obtaining ethnically diverse entering classes. With these people excluded from the analysis, we then reran the regressions that

Raudenbush, whose work we cite in our article, had access to data on the proportions of whites and minorities offered admission by LSAT and UGPA ranges for the two years he studied. Unfortunately, we had no similar data for our 27 graduation years. Thus, his estimates of the proportion of minorities who would be admitted without affirmative action are more reliable for the years he studied than ours would have been. These estimates, although higher than ours, are not inconsistent with ours, for the credentials of minorities admitted to Michigan have increased considerably over time, and Raudenbush's estimates are for classes that graduated from Michigan after the end of our time series. His estimates also apply to classes that were admitted under a policy that was different from the one under which all but the last two classes in our time series were admitted.

**TABLE 2**  
**Incremental Variance Explained by Logged Income Predictors**

Order of Entry	Change in R Square	F Change (DoF)
Years since graduation (years since graduation) <sup>2</sup>	.1616	88.835*** (2, 922)
Gender and age	.0237	27.37*** (2, 920)
Minority status	.0009	1.03 (1, 919)
LSAT/UGPA index	.0012	1.41 (1, 918)
Undergraduate major	.0094	2.13 (5, 917)
Final LSGPA	.0485	58.64*** (1, 912)
Job sector	.0798	21.50*** (5, 907)

\**p* < .05  
\*\**p* < .01  
\*\*\**p* < .001

we report in tables 31 through 36 of our article.<sup>11</sup> Tables 1 and 2 report results when log income is dependent and may be compared to model 2 in tables 31 and 32. We see that eliminating minority students who hypothetically would have been admitted to Michigan without an affirmative-action program hardly changes the results of the analysis reported in tables 31 and 32. In particular, neither minority status nor the admissions index has any significant relationship to future income. We don't present here tables with the satisfaction index or the service index as dependent variables, but the situation is similar in that the results are also like those we report in our article. Neither minority status nor the admissions index is significantly related to the career-satisfaction index, and minorities still score significantly higher than whites on the service index.

Nelson and Payne and Sander both conclude their comments pointing to the need for more research of the kind we have done, with special attention to law schools covering a wider spectrum of legal education. We can think of no better place to conclude our response to their comments than with the same call for research. We hope researchers of the caliber of Sander, Nelson and Payne, and our other commentators can be attracted to

11. Some of these students had not figured in the original analyses because of missing data on included variables.

this task. We can testify that, like Mark Twain, we have found that exploring the river is work, fun, an education, and the source of much that is worth writing about.

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