

PROPOSAL NO.: 0720907

INSTITUTION: U of Cal Los Angeles

NSF PROGRAM: LAW AND SOCIAL SCIENCES

PRINCIPAL INVESTIGATOR: Sander, Richard H.

TITLE: Effects of Law School Admissions Preferences on Minority Bar Passage Rates

RATING: Multiple Rating (/Good/Fair)

REVIEW:

What is the intellectual merit of the proposed activity?

This paper proposes to test the effects of "mismatching" law student abilities and eliteness of law schools attended on students' scores on the law school bar exam. The hypothesis is that minority students who are accepted at better law schools than their credentials support will perform poorly, decreasing students' successes in school and their ability to pass the bar because of this educational mismatch. Professor Sander published a controversial 2004 law review article on this topic and has assembled a board of highly qualified law professors and social scientists to address the issue in two potential new data sets. The P.I. and the 4 co-P.I.s together possess more than enough resources to complete the proposed work with or without NSF funding. In fact, one of the limitations of this research is that it is not clear if the research team needs NSF support to complete the proposed analyses on the existing data bases. Once the data bases are acquired I am not so sure what will be the continued role of the research team or graduate assistants beyond conducting the statistical analyses and writing review articles.

The research team is in the process of negotiating with the State Bar of California (SBC) access to its data base of students who have taken the California bar. Assuming that the SBC grants access to the research team, they will have an adequate data base to examine some of the mismatch relationships that they propose to study in 3 separate SBC data sets. At the time of the writing of this proposal, the agreement between the SBC and the P.I. was not complete. This is a liability for this proposal. Assuming that the data is forthcoming the P.I. proposes to test whether the negative effects of mismatch between credentials and attending an elite law school is offset by the act of attending the more elite school. Accepting students into elite schools when they do not possess commensurate qualifications is the product of by affirmative action preference systems. The P.I. and colleagues offer a set of 4 analyses that they purport will overcome the selection biases that plagued past tests of the mismatch hypothesis. The mismatch hypothesis is well-developed in this proposal, if not well connected to social science theory, and it is supported in prior Sanders publications (2004, 2006), albeit in non-peer reviewed outlets. This is a limitation because the selection problems identified might have prevented publication of these results in reviewed journals in social science and law.

Analysis #1 will examine cohorts of minority law students admitted to school in California before and after the passage of Prop 209, which prohibits the use of race as a factor in admission decisions. The P.I. proposes that if the mismatch hypothesis is correct minorities at elite institutions should have better scores on the bar exam after the passage of Prop 209 than did their pre 209 cohorts. While such a finding would be consistent with this conclusion it is not clear to me if the active ingredient is the mismatch or simply the higher quality of candidates post Prop 209. After 209, there will be fewer minorities accepted, but those that do go to more elite schools will have better credentials than those prior to the passage of law. While the actual measured credentials would likely be confounded with the passage of 209, there are an almost limitless set of additional unmeasured factors that might correlate with higher credentials such as better home environment, more motivation, and higher economic standing. Further, a longer time series analysis with multiple data points (say; 30 plus years) would be much more powerful in detecting differences and attributing them to the passage of Prop 209.

The second analysis would examine the relationship between a measured gap between credentials and elite school status and the P.I. predicts that regardless of race, larger gaps would predict poorer bar scores. Selection is controlled by pairing students on credentials and examining differences between members of pairs who come from schools associated with low or high mismatch for the paired students. Once again, unmeasured variables (e.g., highly motivated students go to elite schools and less motivated students go to less elite schools) cloud a simple interpretation of these findings. Similarly, analysis 3 uses regression statistics rather than matching to control for factors other than mismatch to predict bar scores. While this analysis, which uses gender, race, year, and the credentials gap as predictors, does a better job of controlling selection it leaves many potential confounds (motivation, social status, maturity, and so on) unmeasured. Finally, the fourth analysis would also use regression approaches but would include a series of otherwise unobserved factors to help predict bar scores. It is not clear to this reviewer why analysis 3 and 4 are treated separately except that they will be conducted on different year samples. Can the samples be collapsed into one analysis?

In any event, it seems to me that the mismatch hypothesis is not well tested by the association between measured mismatch and bar score. Instead, it is best tested with an interaction between measured credentials and status of the school attended on bar scores, after controlling for all other possible factors that might influence bar scores. (Some more general theory would be helpful in identifying these potential confounds.) One would predict that the interaction term of credentials by school status would contribute to bar scores after controlling for the main effects of credentials and the main effects of school status. I do not see a test of this interaction in this proposal. Further, many of the potential unobserved confounds are not included in the SBC data (e.g, motivation, family support, presence of children, study environment, and so on). Many of these would be correlated with credentials and could be the active factors predicting bar scores. To be fair, the research team proposes to develop a richer data set with additional states to test further their hypotheses, but the creation of this data set is a promise and not a reality. Further, there is no claim that the data set will include more control factors.

In the end, the P.I. proposes a causal hypothesis, namely that educational mismatch lowers performance in law school and bar scores. It may be that an experimental test of the hypothesis is not possible, but there are a number of alternative quasi-experimental procedures (regression discontinuity designs, time series designs, selection modeling designs) that can be brought to bear on this issue. Some combination of these approaches coupled with a complete model regression analysis using more than a handful of control factors would be most effective in ruling out plausible rival hypotheses (such as selection).

What are the broader impacts of the proposed activity?

The Sanders papers on affirmative action have already had a broad impact on the affirmative action debate and it is certain that additional analyses of similar data will enhance that debate and contribute law review publications to the literature. However, social scientists may find the next round of data analyses wanting for controls of selection and other plausible rival hypotheses. Still, the implications of additional "tests" of the mismatch hypothesis despite the fact that they do not rigorously control other confounds will likely intensify the policy debate about these issues.

Summary Statement

The P.I. and his research team propose to analyze California State Bar data examining the effects of school status, student credentials, and race on the likelihood of passing the California bar.

They add some additional controls to prior modeling studies of this issue and promise to collect even more data from additional state bars. This reviewer remains unconvinced that these analyses adequately control selection (and other plausible hypotheses). Further, it is not certain that these new data are forthcoming and even if they are, I am not sure why the research team requires funding to carry out the planned analyses.

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RATING: Poor

REVIEW:

What is the intellectual merit of the proposed activity?

An Appendix to this proposal makes it clear that the PIs view this as a re-submission (I was unaware that NSF actually used a "revise and resubmit" category); the Appendix provides detail about the modifications made in response to (presumably a subset) of the reviewers. I was one of those reviewers and I am not particularly satisfied with the response to my comments, and some of the response from the PIs heightens my concern about the scientific value of supporting this work.

The PIs propose the use of an interesting database arising from the California Bar Exam, wherein they will have access to raw scores as well as a variety of demographic information on the takers, so as to contribute further to their analysis of what is termed the "mismatch" debate. The use of the raw data from the California exam is a possible improvement over using BPS longitudinal data. The fact that this is a single-state exam does not control for who is taking this or who may have selected to take exams elsewhere, and the PIs' response using national statistics to assert that there is no selection effect to control for is not useful and I think quite beside the point: how do you intend to control for this problem in this particular analysis? This is equally true for some of the other selection issues raised, which are brushed aside. And new ones appear, which have been raised about the original work. Consider the supposed control of using students who apply to two differently-ranked schools and are accepted at both, but one chooses to go to the lower-ranked school. From what I can see, the PIs persist in attributing this choice to differences in credentials, but it might be better attributed to differences in financial offers made to the two students.

What bothers me most about this research is that it is played out in law reviews and not in high-quality, peer-reviewed scholarly journals where the reader can expect that papers are subjected to review by referees with expertise in the types of analysis being applied. Student editors at law reviews simply are not a substitute for referees with real expertise, and this probably contributes to the concern expressed by some of the previous reviewers about NSF support crossing the line between supporting science and advocacy (the "objectivity" issue). In the Appendix the PIs argue that they have added someone to the team as a means of response to this criticism, Professor Amar. With all due respect to Professor Amar, I see no evidence whatsoever that "... Amar will undoubtedly help insure that the widest possible range of tests are used in our analyses." There is no evidence that this added PI is particularly qualified to knowledgeably review and critique statistical work, which has been a major bone of contention in the debate (and I emphasize the word debate, given the fact that the work only shows up in a law review) over the previous work. With the array of top-notch statisticians, econometricians, or sociologists specializing in labor markets located in California, this "response" is bizarre. This is not what we expect of science (rather, it does have an adversarial-contest flavor), and I do not see any commitment (and it is a little late in the game to suddenly argue the alternative) that this work will be subjected to the sort of refereeing process that is the hallmark of science. Contrast what has gone on here with the paper cited briefly by the authors (Epple, *J App Econom.*, 2003) which develops a distinct theory, uses it to inform the econometric analysis, and has been subjected to review-process scrutiny. To me the response from this team to the criticism concerning

objectivity of the proposed work, by adding someone who has no apparent expertise to review such technically-intensive analysis, adds to rather than reduces my concerns. The issues involved in this research are serious ones and deserve serious, considered analysis subjected to review. This subject deserves no less, but simply having a continuing debate in the pages of the Stanford Law Review does not serve the purposes for which I believe NSF funding is intended.

What are the broader impacts of the proposed activity?

Summary Statement

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RATING: Excellent

REVIEW:

What is the intellectual merit of the proposed activity?

Let me begin this review with three caveats. First, I may not be the best person to review this proposal, inasmuch as I know little about law schools. Second, while the proposal has a veneer of neutrality, the anti-affirmative action orientation of the authors is rather unmistakable, and I am ideologically at odds with this orientation. There are many reasons to support the policy that have little to do with the types of outcomes to be measured by the investigators. Moreover, such a study could just as easily be conducted on legacy admits into law programs. And third, I cannot possibly judge the relative merits of this proposal—i.e., whether it justifies the rather large amount requested. It seems to me that the study could be done for less.

Having issued these caveats, I also believe that this is, on intellectual grounds, one of the strongest proposals I have reviews—either for the NSF or for any funding agency. The authors do an excellent job of combing through prior research (being a novice in this field, I will have to take their word that the cited work is, in fact, the most important work) and, more importantly, defining the deficiencies of this research. The stated hypotheses are logically deduced from extant theory and findings. The research design is thoughtful, careful, and appropriate to address these hypotheses. And the data sources will offer a goldmine of information with which to answer the questions raised throughout the proposal.

What are the broader impacts of the proposed activity?

On its face, this proposal seems quite narrow, inasmuch as the study of legal education, and the students who are admitted to such programs, would have little appeal outside of that domain. However, assuming the authors are sufficiently attentive to extensions of this project, it should have a lot to tell us about affirmative action more generally, educational policy, and more. As such, it should appeal to scholars in numerous disciplines, including law, economics, sociology, political science, business, government, etc.

Summary Statement

I wish I could be more specific in terms of criticisms and comments. But, in my judgment, there is very little of a methodological nature that should be changed. Assuming: a) that the award is made contingent upon procurement of SBC data; and b) others on the review panel believe that the proposal justifies the requested expenditures, I have no problem endorsing this proposal, and, despite my ideological misgivings (frankly, I hope the investigators are wrong), with enthusiasm.

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TITLE: Effects of Law School Admissions Preferences on Minority Bar Passage Rates

RATING: Multiple Rating (/Very Good/Good)

REVIEW:

What is the intellectual merit of the proposed activity?

Sander

Summary

This proposal examines whether affirmative action alters bar passage rates. Previous work by Sander, using data from the Bar Passage Study (BPS) demonstrates a large racial gap in credentials among law students at all types of law schools. Sander also found that grades in law schools were a much better predictor of bar passage rates and graduation rates than school identity. Subsequently, the poor performance of Blacks relative to peers in schools was responsible for a large fraction of racial gap in bar exams pass rates. In a subsequent study, Sander also performed an analysis similar to Dale and Krueger that compared outcomes of students admitted to elite and less prestigious law schools, and then using the variation across these similar quality students, examined outcomes of minorities who matriculated to one or the other type of school. The authors propose to extend this previous work using a new data set drawn from CA bar exam records and establish the first stages of a multi-state bar exam data set. The CA bar exam data set would be different from the BPS in that the authors would have actual bar exam scores rather than just whether the person passed or not.

Comments

The authors do an excellent job of describing the debate and literature surrounding the research question.

The most exciting research in the proposal surrounds the efforts to examine the impact of Prop 209 that eliminated racial preferences in admissions to CA law schools. This policy change reduced the number of Black law students at elite CA schools by about 100 per year. Although the authors do not describe it in this way, they are proposing a two-stage least square model. The question is, does attending a more elite school help minorities who would not otherwise get on. The introduction of prop 209 reduced this propensity so if there is a mismatch between student skill and school quality, the match should have improved after passage of 209. This is a reasonable proposal but one that the authors need to flesh out in more detail. More importantly, the passage of 209n may have generated a ripple effect that changes the entire distribution of placements into law schools for minorities. Deciphering the treatment in this case is hard. There are only 100 students/year who were not admitted to the top schools and as a result, has to attend elite institutions.

Analysis 2 is all screwed up. The authors want to pair students based on their propensity to pass the bar based on pre-existing conditions. This is backwards. Why not use the propensity score method and pair people with predicted probabilities of attending an elite school (or a school of a certain quality level) and use as the basis of estimation? The propensity score has been used to measure the effects of single treatments but in recent years the models have been expanded to consider multiple controls.

Analysis 3 is a simple analysis of cross sectional regression models. The authors do a good job earlier in the paper arguing these type of models are subject to important selection biases so why continue with this line of inquiry?

The research plan for the proposed plan to establish a national data base is no more than "we are working on it." No grant money should be distributed for this effort until a more detailed research plan is offered.

Conclusion:

Analysis 1 is good, 2-4 are weak and there really is no research plan about starting a nationwide data set.

What are the broader impacts of the proposed activity?

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RATING: Fair

REVIEW:

What is the intellectual merit of the proposed activity?

The author's initial work on this topic was interesting and important, although more appropriate for the law review in which it was published than for a top economics journal. I am sure that the further research sketched here will further add to our knowledge of this important question. Yet it doesn't seem to be different enough from the previous work to really be appropriate for an NSF grant.

What are the broader impacts of the proposed activity?

I think that the broader impacts of this debate are significant, but I also think that our knowledge remains sufficiently limited that the NSF needs to tread warily in this area.

Summary Statement

Good work, but probably not for the NSF.

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TITLE: Effects of Law School Admissions Preferences on Minority Bar Passage Rates

RATING: Excellent

REVIEW:

What is the intellectual merit of the proposed activity?

This study takes up a question that is highly charged: the effect of racial preferences on academic performance. Its major contribution will be to advance research on the "mismatch" question, namely, the question of whether beneficiaries of affirmative action exhibit lower performance as a result of a credentials gap. While the existence of mismatch effects has been a subject of heated dispute, and while a lead researcher in the proposed study is a proponent of mismatch effects (Sander), all participants in the debate agree on the need for better data. This project promises to supply that better data with regard to law school education and performance on the bar exam.

An original and continuing strength of the study is its capacity to take advantage of the natural experiment created by Proposition 209, as well as the cooperation of California law schools and the California Committee of Bar Examiners. While the transferability of the results to undergraduate education will be unclear, this is a tremendous opportunity to generate better data for legal education.

The revised proposal, which includes a summary of changes reflected in the resubmission, has been strengthened in multiple ways. The addition of Professor Vikram Amar to the research team is especially noteworthy, as he is a known supporter of affirmative action. Amar's expertise, coupled with that of the other researchers, will insure that the study will raise the bar on empirical knowledge about the effects of affirmative action on beneficiaries, at least in law schools. I am also happy to see the authors acknowledge the potential interaction of race and gender in their analyses, as well as discuss the potential effects of socioeconomic status and bar preparation activities. The authors, of course, discuss SES and bar prep activities to defend their view that such factors are less significant than might be imagined. I suspect that these discussions will not settle the matter, but providing a definitive settlement should not be the criterion for funding the study. These issues have now been given explicit recognition and a plausible position has been staked out.

In sum, the main contribution of this study will be the creation of a new database, which will be of enormous value in advancing knowledge on a controversial subject. The shortcomings in the Bar Passage Study are quite clear, as outlined in the proposal, and this study will provide much-improved data. It is a carefully crafted proposal and highly deserving of funding.

What are the broader impacts of the proposed activity?

see above

Summary Statement

see above

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RATING: Excellent

REVIEW:

What is the intellectual merit of the proposed activity?

Understanding the factors that contribute to or mitigate racial disparities in educational and occupational outcomes is of obvious social and intellectual importance. The proposed project has great potential for shedding light on this an important part of this process through a focused study of law school admissions policies and outcomes.

The project also promises to contribute to future scholarship in this area by assembling data from a variety of state and national sources. One minor revision to the existing proposal should be to make explicit the plans for making the "National Database on Legal Education Outcomes" which the authors describe available to other scholars (through a web site, the ICPSR, etc.). If necessary, the budget should be supplemented to ensure that the necessary resources exist in order to make these data available in an accessible format for future research.

What are the broader impacts of the proposed activity?

The impact of this study is potentially quite broad. The findings may well influence race-related aspects of both law school admissions policies and law schools' pedagogical practices. Indeed, while this study is focused on law schools, the findings will have important implications for higher education policy more broadly. Efforts to redress race-based (and other) inequalities will benefit from a firmer understanding of the way educational policies influence occupational outcomes.

Summary Statement

An excellent project of great social significance.