# Evaluating Affirmative Action in American Law Schools: Does Attending a Better Law School Cause Black Students to Fail the Bar?*

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**Abstract**

In the current issue of the *Stanford Law Review*, Richard H. Sander has published an already widely-acclaimed study of affirmative action at American law schools. The article proclaims boldly, and contrary to received wisdom, that affirmative action hurts black lawyers by causing them to fail the bar. I show that this is incorrect. Sander's conclusions are based on internally inconsistent (and statistically invalid) estimates of the causal effect of attending a higher-tier school. Correcting his assumptions and testing his hypothesis directly shows that for similarly qualified blacks, attending a higher-tier law school has *no detectable effect* on bar passage rates.

## 1 Introduction

In his widely-acclaimed study, Sander (2004) makes two central claims. First, he argues that affirmative action has a cascade effect, namely each tier of law schools admits, on average, less-qualified black students. Second, and more controversially, the article claims that this results in a mismatch effect. Since black students are on average less qualified than non-blacks at a given school, blacks receive worse grades in law school and disproportionately fail the bar.

In this Comment, I investigate the scientific validity of the causal effect of attending a higher-tier school on bar passage. At the outset, it is important to note that Sander has *no evidence* as to the direct causal effect of affirmative action, since he has no data resembling a counterfactual world without affirmative action. Instead, Sander investigates the causal effect of attending a better school. The primary finding rests on regressing bar passage on law school GPA, law school tier, and a number of other covariates. From that regression, Sander concludes that since the standardized coefficient of law school GPA is substantially

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higher than that of law school tier, “one is better off attending a less elite school and getting decent grades” (Sander, 2004, p.445).

I find that conclusion invalid for two primary reasons. First, Sander controls for an effect of the cause. In trying to assess the causal effect of attending a higher-tier school, he controls for law school GPA, which, by his own account, is affected by law school tier. As a result, the estimates of the effect of law school tier are invalid. This bias is akin to a clinical trial studying the effect of aspirin on heart health in which the researcher controls for blood pressure (which is itself affected by aspirin) after the treatment of aspirin has been assigned. Second, Sander compares the incomparable. Estimating how students would have performed in a counterfactual tier is an onerous empirical task precisely because students differ in all sorts of respects across tiers. In his dataset where students and schools choose each other, Sander is trying to find something akin to a randomized experiment assigning students to different tiers. By reducing the role of unwarranted assumptions I show that his conclusion holds no ground: *bar passage for similarly-qualified students is invariant to which tier of law school one attends.* This study differs from Ayres and Brooks (2005) and Chambers et al. (2005) in reaching the conclusion that law schools have no causal effect on student bar passage. It is also the first to point out the basic issue of controlling for a consequence of tier (lack of identification), to formally evaluate and analyze the study in a statistical framework of causation, and to reassess the hypothesis by controlling for more covariates.

## 2 Clarifying the Assumptions

First, we should clarify the assumptions of Sander’s bar passage regression in substantive terms, to enable those unfamiliar with statistics to understand the basis on which the findings rest. The task is to estimate how students would have performed in a counterfactual law school tier. Using the standard potential outcomes framework (Rubin, 1974; Holland, 1986), let $T_i$ be a treatment indicator which equals the tier of school that student $i = \{1, \ldots, n\}$ attended. Although I later estimate the effects of each tier, for ease of discussion, suppose we were interested in the causal effect of attending a top-tier school ($T_i = 1$) compared to some other school ($T_i = 0$). Let $Y_{1i}$ and $Y_{0i}$ represent potential outcomes of bar passage if student attends a top-tier school ($Y_{1i}$) or does not attend a top-tier school ($Y_{0i}$). Let $X_i$ represent pretreatment covariates for student $i$, most importantly LSAT scores and undergraduate GPA, but not law school GPA, as explained below.

Our quantity of interest is the causal effect of attending the top school on the probability of passing the bar: $\tau = E(Y_1 - Y_0)$, where $Y_1$ and $Y_0$ are simply vectors of all potential outcomes for all $n$ students and the expectation is across students. Since we never jointly observe the potential outcomes (e.g., we can never know how one particular Stanford law student would have performed in the counterfactual world of having attended University of Kentucky), we identify the causal effect pursuant to the following assumptions. First, covariates are pretreatment in the sense that attendance at a top-tier school does not affect covariate values:

ASSUMPTION 1 (NO POST-TREATMENT BIAS) $X_{0i} = X_{1i} = X_i$, *for all i.*

However, Sander violates this assumption because he controls for law school grades, which he shows in the first part of the paper are themselves a consequence of attending a higher-tier school. The assumption means that *we cannot estimate the causal effect of a top-tier school by controlling for factors that are themselves a result of school tier* (King, Keohane and Verba, 1994; Rosenbaum, 2002, p. 73). To understand this intuitively, suppose that attending a top-tier school affects bar passage solely through ones grades. By controlling for grades, we would not estimate anything close to the causal effect of school tier (see Appendix A for further discussion). I correct for post-treatment bias in omitting law school grades.[1]

Second,

ASSUMPTION 2 (NO INTERFERENCE AMONG UNITS) $Y_{1i}, Y_{0i} \perp\!\!\!\perp T_{i'}$, *where* $i' \neq i$,

which is implied by the usual independence assumption in a generalized linear model. This will hold if we believe student performance on the bar is independent of the other students. Whether diversity has benefits beyond the student admitted, of course, is one of the key issues in affirmative action, but Sander assumes this possibility away. I do not address this in our analysis, but to the degree that interference exists, estimates are both biased and falsely precise.[2]

Third, the crucial assumption is that treatment assignment is random conditional on covariates:

ASSUMPTION 3 (CONDITIONAL EXOGENEITY) $\left\{Y_{1i}, Y_{0i} \perp\!\!\!\perp T_i\right\} | X_i$ *for all i.*

In other words, whether a student attends a top-tier school or not is determined solely by the observed covariates. Even granting this crucial assumption of the Sander study yields no detectable tier effect because of post-treatment bias. But, of course, the assumption is not met when there are unobserved pretreatment covariates, and the only way to make estimates believable is to control for as many pretreatment covariates as possible. Do we truly believe that the student's choice of school is random even if LSAT and undergraduate GPA are the same? This would be violated, for example, if students who feel more comfortable in

[1] This does not mean that the analysis excludes the effect that law school tier may have on grades, but rather the analysis estimates the aggregate causal effect of tier which includes this mechanism, as well as many others (e.g., quality of teaching, peer group, etc.).

[2] To relax this assumption, and potentially test the diversity hypothesis, one would need to permit for additional potential outcomes for diversity level or peer-group covariates.

environments where they can be "big fish" go to lower-tier schools to obtain higher grades, or if students who are motivated by having challenging peers attend higher-tier schools when presented the option (Manski, 1990). Moreover, Sander has omitted key covariates, such as undergraduate institution, family income, student ability, and geography, which are all likely correlated with school tier and affect the likelihood of bar passage. The key is to control for as many factors as possible to make conditional exogeneity believable.[3] I show that accounting for some 180 pretreatment covariates from the Bar Passage Study (compared to the 9 covariates that Sander controls for) yields no detectable causal effect of law school tier (see 3.4).

The logistic model, employed by Sander, makes particular functional assumption to identify a causal effect. Chiefly:

ASSUMPTION 4 (LINEAR ADDITIVE LOGISTIC LINK)

1. *$E(Y_{0i}|X_i) = logit^{-1}(X_i'\beta)$, and*
2. *$E(Y_{1i}|X_i) = logit^{-1}(X_i'\beta + \tau)$ for all i.*

where $\text{logit}^{-1}$ is the logistic transformation. This implies that the average treatment effect and covariates enter the logistic link linearly and additively. If observable covariates are unbalanced between treatment and control cases, these functional form assumptions may loom large, extrapolating from the bounds of the data. In fact, this is very likely in the Sander data: students that are admitted to top-tier schools, for example, are vastly different from students in low-tier schools, so estimating how these students would perform in a counterfactual school relies on questionable modeling assumptions. This is the case particularly since the treatment indicator is very rough (school tiers, and not individual schools). Our framework relaxes the role of these assumptions.

# 3 Empirical Analysis

## 3.1 Confounding Covariates

Figure 1 plots the outcome, bar passage, conditional on race and tier. There is a strong positive correlation between higher tiers for black and white students. Naturally, in large part due to the admissions system, different types of students attend different tiers. Figure 2 plots main confounding covariates, showing the strong correlation between undergraduate GPA and LSAT score by race and tier. Recall that Sander's central claim is that accounting for covariates, the tier effect becomes smaller than the reduction in grades. One

[3] Of course, in a setting where both students and schools determine who attends which school, the assumption remains highly questionable.

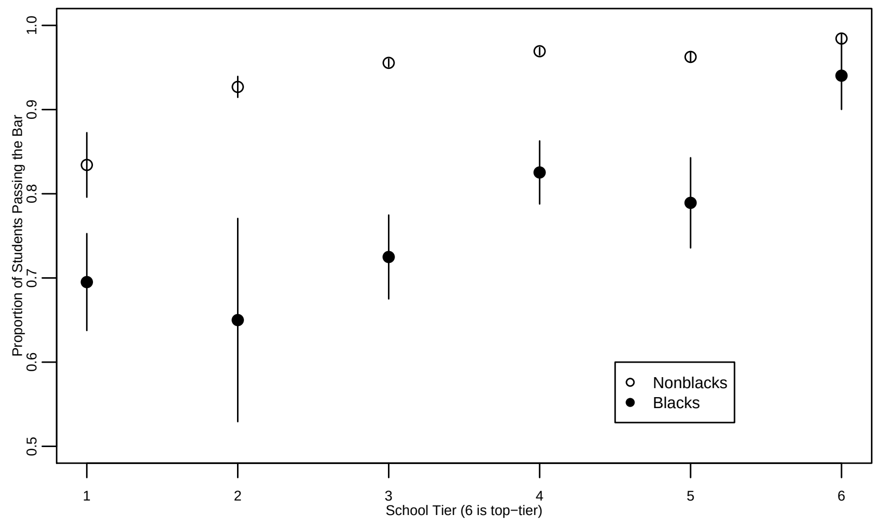


Figure 1: Proportion of Black and Nonblack Students Passing the Bar for Each School Tier. This Figure demonstrates that attendance at a higher tier is associated with a higher probability of passing the bar for both black and white students.

crucial point, however, is that the relative magnitude of law school grade and tier coefficients is *uninformative* about the causal effect of school tier when jointly estimated because of post-treatment bias. I account for this below by omitting law school grades, since these are precisely part of the effect of tier that Sander purports to investigate. Note also that since law school grades are scaled for each school, these appear to be largely independent of law school tier.

To visually anticipate our main findings, note that the primary confounding covariates appear to be LSAT and undergraduate GPA. Figure 3 shows that rates of bar passage are comparable once we examine comparably qualified students. I now turn to a more rigorous analysis to control for other possible confounding covariates.

## 3.2 Methods

Having replicated Sander's findings exactly, I estimate the causal effect of attending a higher-tier school by preprocessing the data via matching (Ho et al., 2004). The central idea is to identify students that are similar in observable covariates, since the only way to identify the causal effect of attending a high-tier school is by examining comparable students in lower-tier schools. These are the students which provide information about how black students would have performed in lower schools (i.e., which could potentially have gone to either school). This is not necessarily the same as the causal effect of affirmative action, because in a world without affirmative action grade curves and attendance patterns would have differed more systematically.

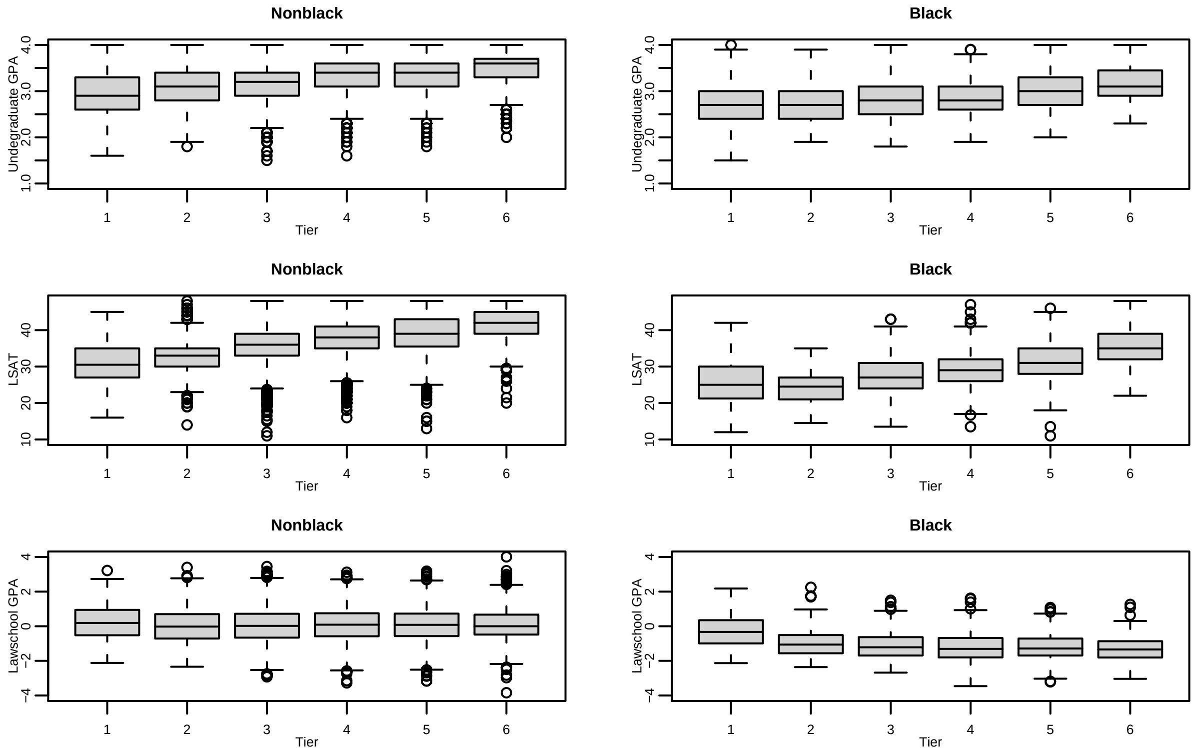


Figure 2: This Figure shows boxplots for undergraduate GPA (top panels), LSAT score (middle panels), and law school GPA (bottom panels) on the y-axis by race (nonblack in left column, and black in right column) and tier of law school (on x-axis). This Figure shows that to assess the effect of school tier, we must account for anything that is correlated with tier and might affect bar passage. I do not control, however, for law school GPA which is itself a consequence of tier.

The data is simply not informative about such a larger policy evaluation. Instead, our evaluation (as well as the one implicit in Sander) is about the average causal effect of law school tier, which is potentially estimable from the data.

In the raw data set students differ drastically from tier to tier. We thereby perform one-to-one matching on all pretreatment covariates (LSAT, undergraduate GPA, gender, and race), considering each tier of law school as a separate treatment.[4] I also match on race exactly to consider specifically the effect on black students. After matching, I achieve substantial balance in all covariates that Sander controls for as shown in Table 1. That Table shows, for example, that top-tier students have an average undergraduate GPA of 3.51, compared to 3.21 of non-top-tier students (t-stat=33.6). Matching balances each of these dimensions. As a result, our inferences will be much less suspect to arbitrary modeling assumptions (including several high leverage points, such as students with a GPA of zero, that would otherwise threaten regression analysis) (Ho et al., 2004).

[4]This relaxes a strong and implausible linearity assumption of tier treatment that average gains are constant across all tiers of law schools.

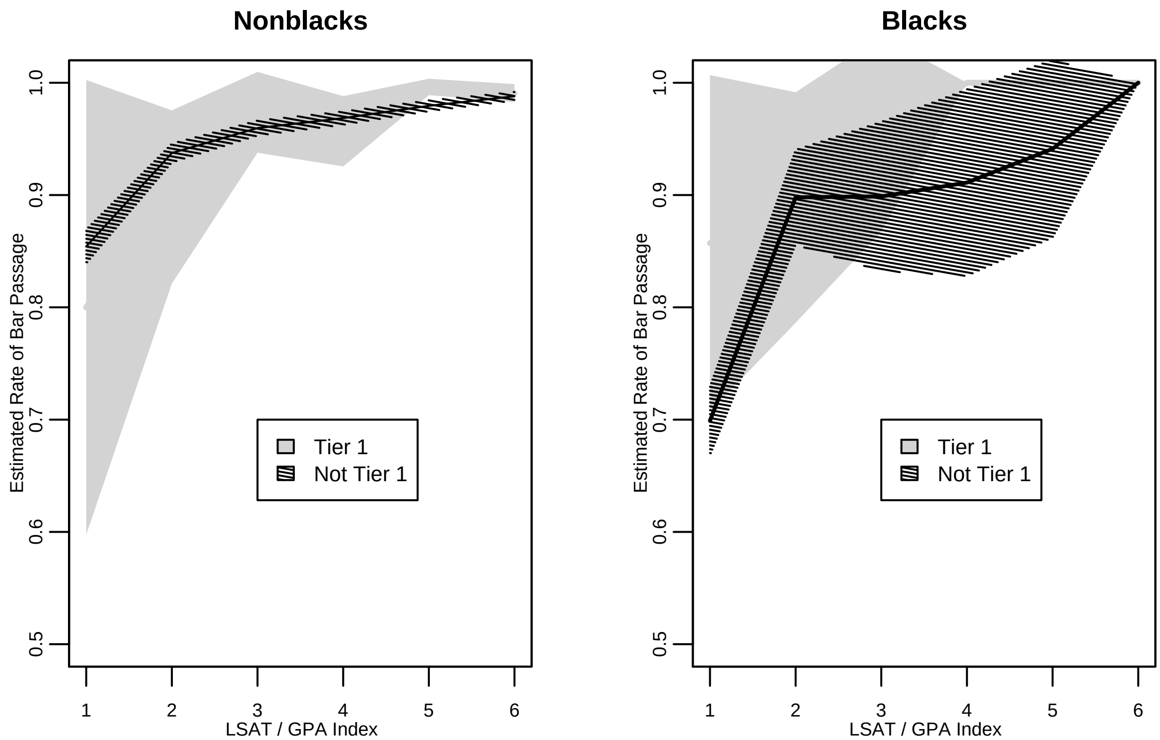


Figure 3: This Figure plots binned GPA/LSAT index on the x axis and the estimated proportion of bar passage on the y axis with 95% confidence intervals broken down by blacks / nonblacks (right and left panels, respectively) and tier 1 (grey shaded confidence bands) vs. not tier 1 (dashed bands). This figure suggests that once we look at comparably qualified students, the confidence intervals intersect, meaning that it remains difficult to statistically distinguish bar passage rates between comparable tier 1 and tier 2 blacks or nonblacks. While the mass of the bands on the right hand panel may indicate that there are gains (the reverse mismatch hypothesis (Ayres and Brooks, 2005, see)), the precision of the estimated proportion is insufficient to distinguish these.

### 3.3 Results with the Original Data

Figure 4 depicts the causal effect for attending each tier of schools with 95% confidence intervals estimated by logistic regression of bar passage on proper pretreatment covariates, both pooling races and for blacks only. For each tier, the confidence intervals intersect with the origin, and we can thereby conclude little about the causal effect of school tier. This is consistent with the notion that similarly-qualified students will perform similarly on the bar, irrespective of law school tier.

### 3.4 Reassessing the Hypothesis with More Data

The above results stem from the crucial assumption that all covariates that might be related to law school tier and that affect bar passage have been accounted for (conditional exogeneity). However, Sander controls for only a few covariates, which makes this assumption highly implausible. I thereby collect more data to

| | Raw Data | | | | Matched Data | | | |
|---|---|---|---|---|---|---|---|---|
| | Means Treated | Means Control | SD | T-stat | Means Treated | Means Control | SD | T-stat |
| Propensity Score | 0.22 | 0.06 | 0.10 | **38.86** | 0.22 | 0.22 | 0.16 | 0.10 |
| LSAT | 41.92 | 36.38 | 5.43 | **48.26** | 41.92 | 41.94 | 4.29 | −0.19 |
| UGPA | 3.51 | 3.21 | 0.41 | **33.60** | 3.51 | 3.51 | 0.34 | −0.47 |
| Male | 0.55 | 0.56 | 0.50 | −0.56 | 0.55 | 0.54 | 0.50 | 0.67 |
| Asian | 0.07 | 0.04 | 0.19 | **4.62** | 0.07 | 0.05 | 0.23 | 1.65 |
| Black | 0.06 | 0.06 | 0.23 | −0.34 | 0.06 | 0.05 | 0.23 | 0.54 |
| Hisp | 0.00 | 0.00 | 0.07 | −0.63 | 0.00 | 0.00 | 0.07 | −0.54 |
| Other | 0.02 | 0.02 | 0.13 | 0.73 | 0.02 | 0.02 | 0.15 | −0.48 |

Table 1: Example of Balance in Raw ($n = 20,827$) and Matched Data ($n = 3,212$) when comparing students in first-tier schools to others. SD denotes standard deviations. Bolded t-statistics indicate a statistically significant difference in top-tier and non-top-tier students, which are reduced in the matched sample. This Figure shows that preprocessing is likely to substantially reduce model sensitivity for major confounding covariates (LSAT score, undergraduate GPA, gender, and race). The propensity score is simply the probability of treatment given the covariates and provides a one-dimensional summary of covariate imbalance (Rosenbaum and Rubin, 1983).

reassess the effect of law school tier. Fortunately, the LSAC Bar Passage Study includes an "Entering Student Questionnaire," which was administered to entering law students *prior to the start of law school* during their orientation program (Wightman, 1999, p. 4). The Questionnaire contains a wealth of information (roughly 200 covariates) about the students in the dataset, including (a) personal and family background (e.g., student disabilities, whether the student's mother tongue is English, whether the student plans to attend law school full time, post-college full-time employment, prior legal employment, undergraduate work experience, female and male household head education and employment, family income, marital status, number of children, prior discrimination) (b) educational background (e.g., undergraduate major, prior degrees earned, year of graduation), and (c) financial status (e.g., financial dependents, prior loans, plans on working during law school). Since the Questionnaire was administered *before* law school began, most of these covariates are all plausibly pretreatment. I did not include any covariates from follow-up surveys taken after law school began such as whether a student graduated from law school, law school graduation date, first semester and overall law school grades, number of attempts at taking the bar, expected law school rank, educational debts, etc. I did include the region in which the bar was taken, since bar passage rates vary drastically across jurisdictions.[5]

We match on 180 of these pretreatment covariates to assess the effect of a top-tier school. For space

[5]One might argue that students strategically select jurisdictions conditional on law school performance, but here we believe this bias to be small in comparison to omitted variable bias due to variation in jurisdictions.

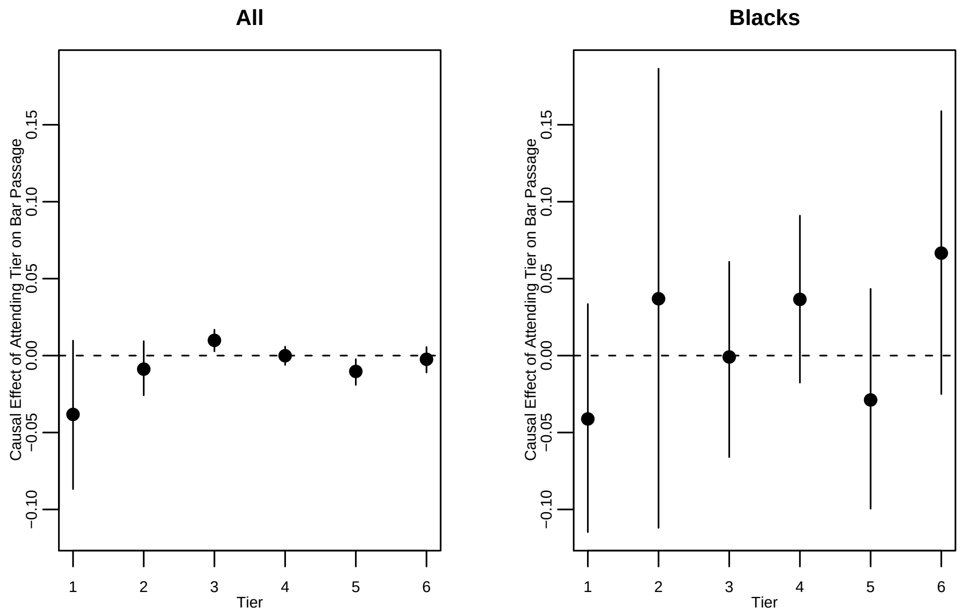


Figure 4: Estimated Average Treatment Effects of Attending Each Tier with 95% confidence interval (estimated by posterior simulation from logistic regression of bar passage on pretreatment covariates and treatment indicator with preprocessed data). The left panel plots the average causal effect for all students. The right panel plots the average causal effect for black students only. This Figure shows that the causal effect on bar passage is negligible or not detectable for similarly-qualified students.

limitations, Table 2 presents balance statistics on only a subset of these covariates for which the absolute t-statistic in the raw data exceeded 4. Clearly, top-tier students differ in a host of characteristics not accounted for by Sander: they are generally wealthier ("income5" t-stat=6.7), older ("DOB yr" t-stat=14.0), take the bar in different jurisdictions, such as in the far west, where underlying bar passage rates differ ("region1FW" t-stat=11.8), are more likely to report that English is their best language ("english.bestY" t-stat=10.2), are much more likely to attend law school full time and during the day ("fulltime" t-stat=-20.8 and "day.night" t-stat=-22.6), are less likely to have financial dependents ("dependentsN" t-stat=9.1), are much more likely to be single ("marital1" t-stat=11.4), and have fewer children ("children" t-stat=-10.9). Matching on these 180 covariates yields balance on all covariates, with absolute t-statistics less than 2 for all 180 covariates ($n = 3,128$). Figure 5 presents histograms of selected covariates for all non-top-tier (solid line) and top-tier students (grey). The dashed histogram overlays the distribution of covariates for matched non-top-tier students, showing that matching achieves substantial balance. Having achieved balance, I then run a logistic regression on the matched dataset to assess the effect of tier.[6] This yields an average treatment effect of attending a top-tier law school on the probability of bar passage of -0.005 (SE=0.004), statistically indistinguishable from 0. In short, there is no evidence that for similarly-situated students, attending a top-tier school has any effect on bar passage rates.

## 4 Conclusion

Assessing the impact of affirmative action is a difficult task. In light of the substantial impact on education, law, and policy, scholars should bear the burden to do so in a scientifically rigorous fashion.[7] I find that Sander's analysis is based on internally inconsistent assumptions leading to unwarranted conclusions about affirmative action. Although the job market analysis is outside the scope of this Comment, the problem of controlling for a consequence of the cause would also invalidate the effects of law school tier on labor market outcomes, since Sander also controls for law school grades in these analyses.[8] In addition to contradicting Sander's conclusion, our findings also are substantively interesting. For aspiring law students who are qualified to attend a higher tier, the marginal decision to do so is likely to have little impact on ultimately passing the bar. Much of the game appears to be in law school admissions. In addition, our findings suggest that at least with respect to bar passage, affirmative action does not appear to hurt black students. Our research leaves open considering the role of legal education given the finding that law school tier has no causal effect on bar passage.

[6] We report results from a logistic regression with top-tier indicator, propensity score, LSAT, UGPA, race, and gender covariates, which corresponds to the original Sander equation. Because balance has already been achieved by preprocessing, the result of the treatment effect is robust to covariate selection at the analysis stage (Ho et al., 2004). In other words, because treatment is already balanced along confounding covariates, the treatment effect estimate is not affected by controlling for covariates.

[7] While I have focused on one particular aspect of Sander's analysis, there are a host of other statistical issues that could threaten the validity of inferences. For example, the incomparability of the law school GPA, which is scaled separately for each school, makes the grade calculations highly suspect. Also, Sander assumes that non-response to the LSAC survey is completely random, which is very unlikely (16% of bar passage data alone is missing). Inferences are therefore likely biased and should be accounted for by imputation. Nonetheless, even staying within the framework provided by Sander, the conclusions fail.

[8] For a critique of the job market findings on different grounds, see Dauber (2005).

| | Full Dataset | | | | | Matched Dataset | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Covariate | $\bar{x}_1$ | $\bar{x}_0$ | SD | t-stat | Bias | $\bar{x}_1$ | $\bar{x}_0$ | SD | t-stat | Bias | Reduction |
| pscore | 0.270 | 0.059 | 0.122 | 40.754 | 1.040 | 0.270 | 0.257 | 0.194 | 1.875 | 0.064 | Yes |
| asian | 0.066 | 0.036 | 0.191 | 4.707 | 0.122 | 0.066 | 0.061 | 0.244 | 0.587 | 0.021 | Yes |
| lsat | 41.931 | 36.406 | 5.418 | 47.620 | 1.275 | 41.931 | 41.897 | 4.292 | 0.225 | 0.008 | Yes |
| ugpa | 3.506 | 3.208 | 0.414 | 33.042 | 0.885 | 3.506 | 3.499 | 0.329 | 0.598 | 0.021 | Yes |
| DOB yr | 66.799 | 65.371 | 5.166 | 14.010 | 0.382 | 66.799 | 66.653 | 3.756 | 1.081 | 0.039 | Yes |
| region1FW | 0.254 | 0.121 | 0.337 | 11.843 | 0.306 | 0.254 | 0.281 | 0.443 | −1.656 | −0.060 | Yes |
| region1GL | 0.255 | 0.170 | 0.381 | 7.481 | 0.195 | 0.255 | 0.228 | 0.428 | 1.754 | 0.062 | Yes |
| region1MW | 0.010 | 0.054 | 0.219 | −14.345 | −0.430 | 0.010 | 0.008 | 0.094 | 0.759 | 0.025 | Yes |
| region1Mt | 0.024 | 0.055 | 0.224 | −7.365 | −0.202 | 0.024 | 0.025 | 0.155 | −0.115 | −0.004 | Yes |
| region1NW | 0.002 | 0.008 | 0.086 | −4.676 | −0.136 | 0.002 | 0.003 | 0.047 | −0.378 | −0.015 | Yes |
| region1SC | 0.036 | 0.111 | 0.307 | −14.394 | −0.404 | 0.036 | 0.033 | 0.182 | 0.492 | 0.017 | Yes |
| region1SE | 0.070 | 0.130 | 0.331 | −8.718 | −0.236 | 0.070 | 0.057 | 0.244 | 1.469 | 0.050 | Yes |
| region2FW | 0.254 | 0.120 | 0.336 | 11.899 | 0.308 | 0.254 | 0.281 | 0.443 | −1.697 | −0.062 | Yes |
| region2GL | 0.254 | 0.171 | 0.382 | 7.389 | 0.192 | 0.254 | 0.228 | 0.428 | 1.756 | 0.062 | Yes |
| region2MW | 0.010 | 0.054 | 0.219 | −14.452 | −0.434 | 0.010 | 0.008 | 0.094 | 0.759 | 0.025 | Yes |
| region2Mt | 0.024 | 0.056 | 0.225 | −7.435 | −0.204 | 0.024 | 0.024 | 0.154 | 0.000 | 0.000 | Yes |
| region2NW | 0.002 | 0.008 | 0.086 | −4.676 | −0.136 | 0.002 | 0.003 | 0.047 | −0.378 | −0.015 | Yes |
| region2SC | 0.036 | 0.110 | 0.306 | −14.272 | −0.400 | 0.036 | 0.033 | 0.182 | 0.492 | 0.017 | Yes |
| region2SE | 0.071 | 0.130 | 0.332 | −8.543 | −0.231 | 0.071 | 0.058 | 0.245 | 1.531 | 0.052 | Yes |
| bar1 yr | 94.109 | 94.175 | 0.419 | −7.027 | −0.188 | 94.109 | 94.105 | 0.345 | 0.362 | 0.013 | Yes |
| bar2 yr | 94.146 | 94.237 | 0.486 | −8.435 | −0.226 | 94.146 | 94.142 | 0.403 | 0.310 | 0.011 | Yes |
| englishN | 0.613 | 0.740 | 0.444 | −10.042 | −0.262 | 0.613 | 0.625 | 0.486 | −0.736 | −0.026 | Yes |
| englishY | 0.386 | 0.256 | 0.442 | 10.281 | 0.268 | 0.386 | 0.373 | 0.485 | 0.737 | 0.026 | Yes |
| english.bestY | 0.373 | 0.245 | 0.436 | 10.167 | 0.265 | 0.373 | 0.364 | 0.483 | 0.556 | 0.020 | Yes |
| fulltime | 1.011 | 1.079 | 0.261 | −20.765 | −0.653 | 1.011 | 1.009 | 0.099 | 0.541 | 0.018 | Yes |
| day.night | 1.012 | 1.088 | 0.275 | −22.595 | −0.716 | 1.012 | 1.010 | 0.102 | 0.525 | 0.018 | Yes |
| jointY | 0.043 | 0.022 | 0.151 | 4.157 | 0.107 | 0.043 | 0.042 | 0.203 | 0.177 | 0.006 | Yes |
| grd.testsN | 0.043 | 0.078 | 0.264 | −6.299 | −0.170 | 0.043 | 0.042 | 0.202 | 0.266 | 0.009 | Yes |
| grd.testsY | 0.950 | 0.916 | 0.274 | 5.843 | 0.157 | 0.950 | 0.956 | 0.212 | −0.760 | −0.026 | Yes |
| fulltime.college2 | 0.005 | 0.017 | 0.127 | −5.984 | −0.171 | 0.005 | 0.005 | 0.071 | 0.000 | 0.000 | Yes |
| fulltime.college3 | 0.012 | 0.032 | 0.172 | −6.916 | −0.193 | 0.012 | 0.017 | 0.118 | −1.215 | −0.048 | Yes |
| BA.year | 89.186 | 88.376 | 4.317 | 9.344 | 0.254 | 89.186 | 89.068 | 3.235 | 1.023 | 0.037 | Yes |
| post.BA.dN | 0.951 | 0.916 | 0.274 | 6.171 | 0.166 | 0.951 | 0.956 | 0.210 | −0.595 | −0.021 | Yes |
| post.BA.dY | 0.028 | 0.060 | 0.233 | −6.999 | −0.191 | 0.028 | 0.029 | 0.167 | −0.214 | −0.008 | Yes |
| law.help.aN | 0.992 | 0.981 | 0.133 | 4.238 | 0.117 | 0.992 | 0.994 | 0.084 | −0.856 | −0.028 | Yes |
| law.help.aY | 0.000 | 0.005 | 0.067 | −9.667 | −Inf | 0.000 | 0.000 | 0.000 | | | Yes |
| law.help.eN | 0.968 | 0.942 | 0.229 | 5.383 | 0.145 | 0.968 | 0.973 | 0.169 | −0.846 | −0.029 | Yes |
| law.help.eY | 0.013 | 0.030 | 0.168 | −5.341 | −0.147 | 0.013 | 0.013 | 0.115 | 0.000 | 0.000 | Yes |
| grad.applyN | 0.492 | 0.425 | 0.495 | 5.079 | 0.133 | 0.492 | 0.501 | 0.500 | −0.536 | −0.019 | Yes |
| ftjobN | 0.705 | 0.633 | 0.480 | 5.939 | 0.157 | 0.705 | 0.706 | 0.456 | −0.078 | −0.003 | Yes |
| ftjobY | 0.295 | 0.365 | 0.480 | −5.841 | −0.154 | 0.295 | 0.293 | 0.456 | 0.118 | 0.004 | Yes |
| dependentsN | 0.965 | 0.920 | 0.266 | 9.127 | 0.251 | 0.965 | 0.965 | 0.183 | 0.000 | 0.000 | Yes |
| dependentsY | 0.026 | 0.073 | 0.254 | −10.782 | −0.301 | 0.026 | 0.029 | 0.164 | −0.656 | −0.024 | Yes |
| abroadN | 0.928 | 0.961 | 0.199 | −4.892 | −0.126 | 0.928 | 0.935 | 0.252 | −0.781 | −0.027 | Yes |
| abroadY | 0.063 | 0.031 | 0.180 | 5.020 | 0.130 | 0.063 | 0.059 | 0.239 | 0.373 | 0.013 | Yes |
| policeY | 0.001 | 0.008 | 0.085 | −5.937 | −0.184 | 0.001 | 0.001 | 0.036 | 0.000 | 0.000 | Yes |
| f.head.work5 | 0.286 | 0.236 | 0.427 | 4.232 | 0.111 | 0.286 | 0.305 | 0.456 | −1.176 | −0.042 | Yes |
| m.head.work5 | 0.527 | 0.425 | 0.495 | 7.777 | 0.204 | 0.527 | 0.522 | 0.499 | 0.251 | 0.009 | Yes |
| m.head.work9 | 0.054 | 0.089 | 0.282 | −5.762 | −0.155 | 0.054 | 0.055 | 0.227 | −0.079 | −0.003 | Yes |
| f.head.educ5 | 0.123 | 0.219 | 0.408 | −10.792 | −0.290 | 0.123 | 0.131 | 0.333 | −0.644 | −0.023 | Yes |
| f.head.educ11 | 0.268 | 0.184 | 0.393 | 7.251 | 0.189 | 0.268 | 0.268 | 0.443 | 0.000 | 0.000 | Yes |
| m.head.educ5 | 0.070 | 0.120 | 0.321 | −7.268 | −0.195 | 0.070 | 0.073 | 0.258 | −0.277 | −0.010 | Yes |
| m.head.educ7 | 0.066 | 0.098 | 0.294 | −4.843 | −0.129 | 0.066 | 0.065 | 0.247 | 0.145 | 0.005 | Yes |
| m.head.educ11 | 0.481 | 0.379 | 0.487 | 7.768 | 0.204 | 0.481 | 0.466 | 0.499 | 0.823 | 0.029 | Yes |
| income3 | 0.270 | 0.362 | 0.479 | −7.811 | −0.207 | 0.270 | 0.275 | 0.445 | −0.281 | −0.010 | Yes |
| income4 | 0.489 | 0.432 | 0.496 | 4.327 | 0.114 | 0.489 | 0.497 | 0.500 | −0.429 | −0.015 | Yes |
| income5 | 0.136 | 0.076 | 0.272 | 6.746 | 0.175 | 0.136 | 0.130 | 0.340 | 0.473 | 0.017 | Yes |
| marital1 | 0.868 | 0.764 | 0.420 | 11.382 | 0.306 | 0.868 | 0.853 | 0.347 | 1.186 | 0.043 | Yes |
| marital2 | 0.120 | 0.199 | 0.395 | −9.011 | −0.241 | 0.120 | 0.132 | 0.332 | −0.970 | −0.035 | Yes |
| marital3 | 0.012 | 0.035 | 0.178 | −7.683 | −0.216 | 0.012 | 0.015 | 0.114 | −0.786 | −0.030 | Yes |
| marital4 | 0.000 | 0.001 | 0.032 | −4.693 | −Inf | 0.000 | 0.000 | 0.000 | | | Yes |
| children | 0.058 | 0.184 | 0.635 | −10.890 | −0.301 | 0.058 | 0.065 | 0.390 | −0.504 | −0.017 | Yes |

Table 2: Balance of 62 observable covariates for which the absolute t-statistic exceeded 4 in full data, between first-tier and non-first-tier schools for full ($n = 20,827$) and matched dataset ($n = 3,128$), where units were matched by one-to-one nearest neighbor matching on the propensity score (estimated using 180 covariates in a logistic model). After matching, balance is achieved on all 180 covariates, with no absolute t-statistics below 2.

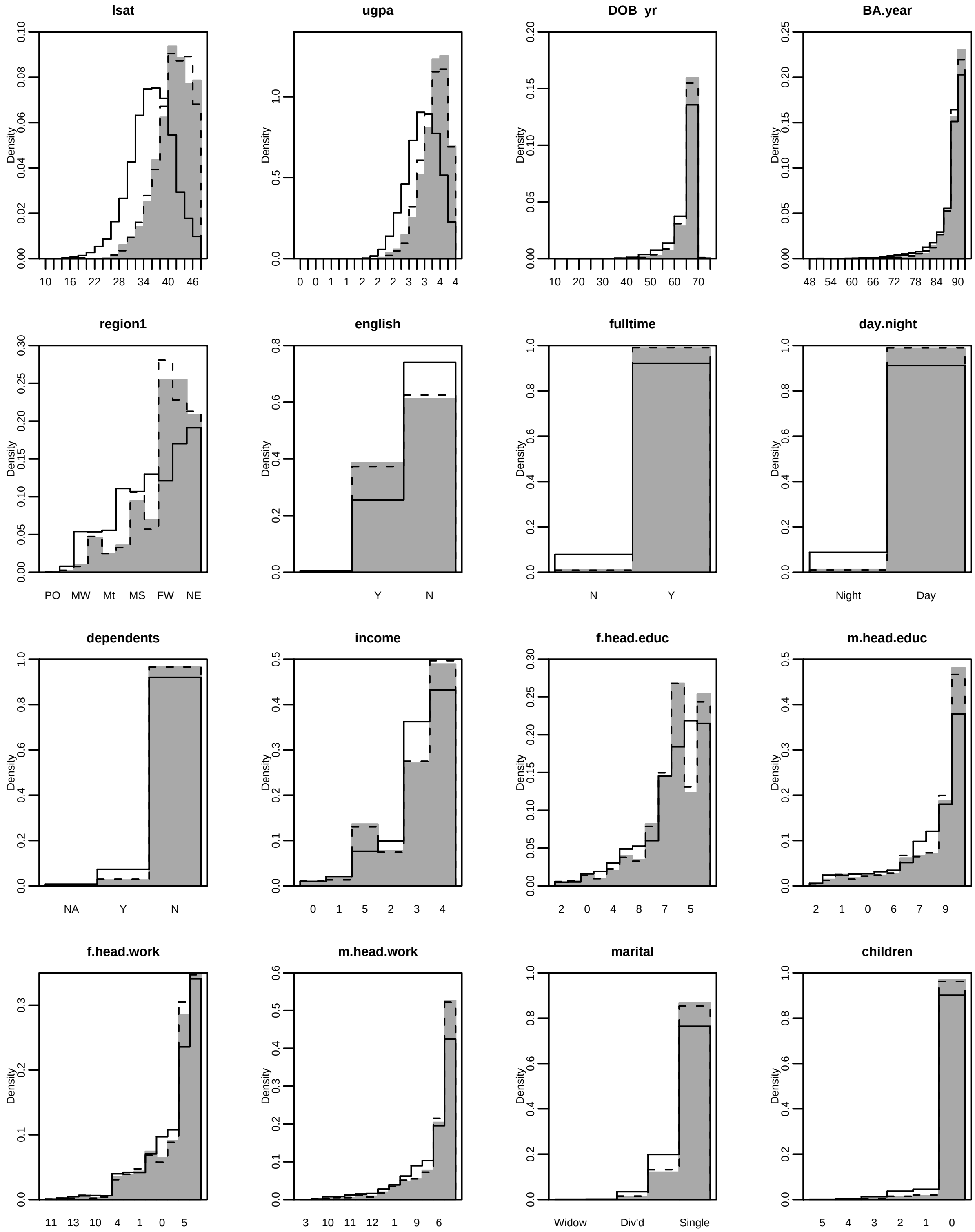


Figure 5: Histograms of Selected Covariates for Top-Tier (grey) and Non-Top-Tier Students (solid line) and Matched Non-Top-Tier Students (dashed line). This figure illustrates substantial differences of the raw data and that matching achieves balance on covariates since the dashed histogram of matched non-top-tier students substantially overlaps with the grey histogram of top-tier students

# A Appendix: The Pathology of Post-Treatment Bias

The danger of post-treatment bias is perhaps one of the most overlooked problems in observational studies, so it is not entirely surprising that no researcher has to date uncovered this in the Sander analysis. To illustrate just how pathological the problem can be, consider a stylized example in Table 3.[9] If smoking causes both low birth weight and increases the mortality rate, controlling for birth weight leads to a *reversal* of the causal effect. For example in the Table, the overall mortality rate is higher for parents who are smokers (0.55) than for non-smokers (0.35). However, by controlling for birth weight, infant mortality rate is actually lower for *both* high-birth-weight and low-birth-weight smokers than for non-smokers! How can this be? This is the reverse of what statisticians call "Simpson's paradox," namely that aggregate proportions can be reversed by controlling for some covariate. By wrongly controlling for birth weight (which is itself a consequence of smoking), we may estimate the exact opposite causal effect. To get a sense of just how problematic this is, even if smoking had been *randomly assigned* in a classic experiment, post-treatment bias would be induced by controlling for birth weight, yielding the wrong estimate. This is the issue in the Sander analysis: the mismatch effect posits that black students getting admitted into a higher-tier school will cause grades to drop and to fail the bar. Yet in estimating the causal effect on bar passage, Sander cannot control for law school grades. I show that as a result, his findings are largely an artifact of post-treatment bias. For a more formal treatment, see Rosenbaum (1984).

| | **Low Birth Weight** | | | | **High Birth Weight** | | | | **Overall** | |
|---|---|---|---|---|---|---|---|---|---|---|
| Infant | Smoker | | Non-smoker | | Smoker | | Non-smoker | | Smoker | Non-smoker |
| Death | *n* | Proportion | *n* | Proportion | *n* | Proportion | *n* | Proportion | Proportion | |
| Yes | 60 | 0.60 | 9 | 0.90 | 1 | 0.10 | 30 | 0.30 | 0.55 | 0.35 |
| No | 40 | 0.40 | 1 | 0.10 | 9 | 0.90 | 70 | 0.70 | 0.45 | 0.65 |

Table 3: Example of Post-Treatment Bias. This Table provides a numerical example showing that controlling for a covariate (birth weight) that is itself a consequence of a cause (smoking) can seriously deceive. While the overall infant mortality rate for smokers is 0.55 and for non-smokers is 0.35, the infant mortality rate is (wrongly!) lower for both low-birth-weight and high-birth-weight children of smokers than non-smokers (0.60 vs. 0.90 and 0.10 vs. 0.30, respectively). *n* represents the number of children and proportion represents the fraction of children dying at birth.

[9] This example is often known as the Wilcox-Russel hypothesis.

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