Economics of Education Review 89 (2022) 102282

Contents lists available at ScienceDirect

Economics of Education Review

journal homepage: www.elsevier.com/locate/econedurev

The signaling value of university rankings: Evidence from top 14 law schools

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ARTICLE INFO

JEL classification:
I26
I23
J24
J23
J44

Keywords:
Human capital
Signaling
Law school
Returns to education
Asymmetric information
Value added
Regression discontinuity

ABSTRACT

This paper measures the impact of signaling on labor-market outcomes by estimating the labor-market effects of attending a U.S. News & World Report Top 14 (T14) law school. Utilizing data from the American Bar Association on class profiles, we use the value added with drift methodology to estimate the causal impact of attending a particular law school and then use a regression discontinuity methodology to estimate the difference in value added between T14 and non-T14 law schools that is attributable to T14 status. We find that T14 law schools confer no signaling effect on the Bar exam, which is graded blindly, but a substantial signaling effect on employment at "Big Law" firms with more than 250 attorneys, which pay some of the highest salaries in the law profession. The lowest-ranked T14 university increases the likelihood of Big Law employment by 30 percentage points (96%) more than the highest-ranked non-T14 university. This likely reflects asymmetric information in the labor market for lawyers, and thus graduating from a T14 law school serves as a signal of a lawyer's ability.

1. Introduction

The extent to which human capital accumulation and signaling play a role in the causal impact of education remains one of the most fundamental questions in the economics of education, as the answer to that question largely determines the optimal market structure and governmental policies for the educational system. Given that the per capita cost of postsecondary education exceeds that of primary and secondary education (National Center for Education Statistics, 2017, 2018), determining the efficiency of our postsecondary education system is particularly important. Oreopoulos and Petronijevic (2013) find that the earnings premium associated with a college degree has risen substantially in the past three decades, but whether investing in a postsecondary education system is efficient is determined by whether the education system imparts true, causal increases in human capital (Becker, 1962; Mincer, 1974) or whether educational attainment is a signal and the education system merely acts as a sorting mechanism for agents who have varying innate ability (Spence, 1973).

Many other studies have attempted to decompose the returns to education, as returns to both human capital accumulation and signaling value may simultaneously contribute to the causal effect of additional education. Despite the fact that identifying the exact contributions of human capital and signaling may be empirically unidentifiable when both have a causal effect (Huntington-Klein, 2021), prior studies have shown that there is some combination of a human capital and signaling effect in additional years of education (Kaymak, 2012; Lange, 2007) and college (Bingley, Christensen, & Markwardt, 2015; Fang, 2006). Research has also shown that there is substantial heterogeneity in the return to a bachelor's degree, as different majors or institutions may provide a higher return than other majors or institutions due to differences of both human capital accumulation and signaling (Dale & Krueger, 2002; Hastings, Neilson, & Zimmerman, 2013; Kirkeboen, Leuven, & Mogstad, 2016).[2]

On the other hand, some studies rule out the presence of one of the two potential mechanisms. Lang and Kropp (1986) and Bedard (2001) show that differences in educational attainment in response to compulsory schooling or the supply of postsecondary education institutions are not consistent with the human capital model. Tyler, Murnane, and Willett (2000) provide evidence that the General Educational Development credential provides a signaling effect for some individuals, while Clark

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[1] We are grateful to Martin Davies, Julie Youngman, and one anonymous referee for helpful comments and suggestions. This research did not receive any specific grant from funding agencies in the public, commercial, or not-for-profit sectors.

[2] This is despite the fact that students would all technically be earning bachelor's degrees.

https://doi.org/10.1016/j.econedurev.2022.102282
Received 30 October 2021; Received in revised form 7 June 2022; Accepted 21 June 2022
Available online 20 July 2022

and Martorell (2014) show that a high school diploma confers no signaling value, and Eble and Hu (2016) show that an additional year of schooling, holding highest credential constant, provides very little labor market return. Given the scope and complexity of the United States education system, it is not surprising that different contexts, which involve differences in the prestige and rigor of the credential, the labor market awaiting those obtaining credentials, and the ability of firms to identify direct measures of human capital, will often result in different relative contributions of the human capital and signaling effects.

This paper studies the labor market returns to education in the context of attending an elite law school, and then estimates whether this return is driven by an increase in human capital accumulation or whether there is a signaling value to attending a selective group of universities that the law profession deems particularly prestigious. Specifically, we utilize the consistency of the top 14 places of U.S. News & World Report's (USNWR) law school rankings to answer the following: do Top 14 ranked law schools add value (in terms of employment at a law firm larger than 250 attorneys and other labor-market outcomes) greater than implied by the increase in the human capital of their students (as measured by the percent who pass the Bar exam)?

In 1987, USNWR released its first ranking of accredited American law schools. Following a two-year hiatus, in 1990 USNWR started publishing law school rankings annually. Since the beginning of the annual rankings, the same fourteen schools[3] have occupied the top positions in almost[4] every year's ranking, allowing them to be widely recognized as the most prestigious schools. Due to the consistency in the top 14 ranked positions, these schools have been informally named the Top 14 (or T14). This label, while not an official designation of USNWR, has become widely used, including in academic studies.[5] These schools, being among the most coveted by prospective law students, are among the most selective in their admission criteria and essentially have their choice of students. The graduates of T14 universities go on to prestigious careers (large law firms and federal clerkships) at rates higher than most other schools[6] and earn higher salaries on average[7].

Figs. 1(a), 1(b), and 1(c) plot the average percentage of students who pass the Bar exam, obtain employment at Big Law firms, and obtain federal clerkships against the USNWR rank of the school. For all outcomes, law schools with a USNWR rank closer to one have better outcomes. However, the percentage of students passing the Bar exam trends smoothly throughout the domain of USNWR rank, while the percentage of students employed at Big Law firms or obtaining federal clerkships change discontinuously at some ranks. While the discontinuity occurs at a very high (i.e. top three) rank for federal clerkships, the discontinuity for Big Law employment occurs noticeably at the cutoff between T14 and non-T14 universities.

It is unclear if the success of T14 alumni are due to the causal effect of attending a T14 university or whether the distribution of pre-law-school ability changes discontinuously between T14 and non-T14 graduates. In order to estimate the causal impact of attending a particular university, we estimate school value added on Bar passage and labor-market outcomes, such as Big Law placement, using the value added with drift methodology (Chetty, Friedman, & Rockoff, 2014). The value added with drift methodology uses a selection-on-observables, most notably undergraduate grade point average (GPA) and Law School Admission Test (LSAT) score, approach to account for the fact that students do not randomly sort to law schools. The value-added methodology attributes any unexplained difference between the labor-market outcomes of graduating students and the predicted outcomes of those students based on observable pre-law-school characteristics to the causal impact of the school. We then utilize the fact that T14 status varies discontinuously across the threshold between the 14th- and 15th-ranked universities to estimate the causal difference in value added from the T14 designation using a regression discontinuity methodology.

Our results show that attending a law school with T14 status confers a substantial increase in the likelihood that a graduate will obtain a job at Big Law firm while providing little to no benefit in terms of likelihood of passing the Bar exam. We find that, on average, T14 law schools increase the likelihood that their graduates pass the Bar exam by 10 percentage points more than their non-T14 peers. However, this difference is driven almost entirely by differences in human capital accumulation between T14 schools and non-T14 schools both in theory, because the Bar exam is graded blindly, and in practice, because we find little to no discontinuous difference between the value added provided by the 14th-ranked school and the value added provided by the 15th-ranked school. For employment at Big Law firms, T14 law schools increase the likelihood of Big Law employment by 63 percentage points more than their non-T14 peers. We estimate that a large portion of this effect is due to the signaling[8] effect of attending a T14 university, as the 14th-ranked law school increases the likelihood of Big Law employment by 30 percentage points more than the 15th-ranked law school. Given that only about 31% of students obtain employment at a Big Law firm, this represents a 96% increase in the likelihood of obtaining a Big Law offer if one attends a T14 university relative to a comparable non-T14 university.

These findings have various labor market implications. Law school applicants have a large incentive to attend T14 schools instead of similar non-T14 schools because of the substantial potential private benefit. This will likely increase the demand for T14 law schools. T14 law schools likely respond by increasing tuition, as the supply of law school admission slots is likely relatively inelastic in the short run. Big Law firms, on the other hand, appear to be operating inefficiently. Our results suggest that Big Law firms could hire lawyers of similar quality for lower wages if they recruited students at law schools ranked just outside of the T14. This in turn may decrease relative demand for T14 universities, which would put downward pressure on T14 tuition until the difference in tuition between T14 and non-T14 law schools roughly reflects the difference in human capital accumulation between those universities.

[3] Columbia Law School, Cornell Law School, Duke University School of Law, Georgetown University Law Center, Harvard Law School, New York University School of Law, Northwestern University School of Law, Stanford Law School, UC-Berkeley School of Law, University of Chicago Law School, University of Michigan Law School, University of Pennsylvania Law School, University of Virginia School of Law, and Yale Law School.

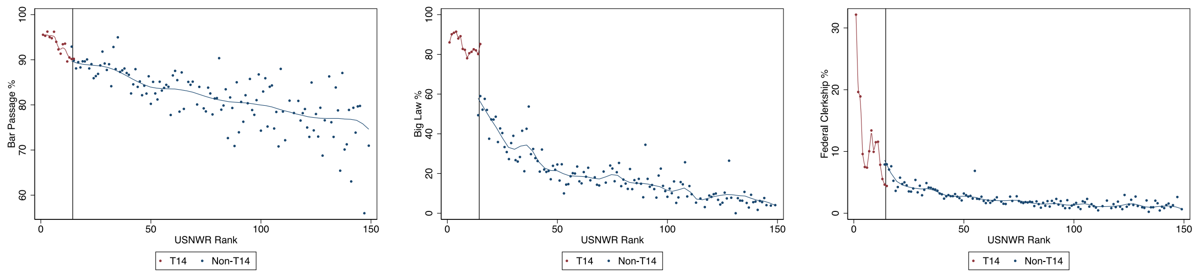
[4] For the 2018 rankings, University of Texas School of Law ranked 14th while Georgetown University fell to 15th. The universities previously tied for 14th in 2012. The 2022 rankings (published in 2021) placed UCLA School of Law at 14th and Georgetown University at 15th.

[5] See, for example, Bonica, Chilton, Goldin, Rozema, and Sen (2017), Bonica, Chilton, Rozema, and Sen (2018), Bonica, Chilton, and Sen (2016).

[6] For 2011–2019, T14 schools averaged 85% of students entering Big Law, compared to 42% for schools ranked 15–28. For federal clerkships, the rates were 13% and 5%, respectively.

[7] See USNWR's profile for each school for salary information.

[8] Feng and Graetz (2017) argue that regression discontinuity designs do not necessarily test for the presence of a *signaling* effect but instead test for the presence of *statistical discrimination*. This is due to the fact that a pure signaling effect (Spence, 1973) implies statistical discrimination (Arrow, 1973; Phelps, 1972), but the presence of statistical discrimination does not necessarily imply the existence of a signaling effect. A key assumption of the signaling model is that the acquisition of the (costly) signal does not change the underlying ability of the person acquiring the signal, thus the signal merely serves to distinguish people with different levels of *innate* ability. In our setting, it is possible that graduating from a T14 university results in more human capital accumulation (particularly human capital that is valued by Big Law firms but uncorrelated with Bar-exam outcomes) than graduating from a non-T14 university. We use the term "signaling effect" throughout this paper but note that "informational effect" may be a more accurate term, in the sense that information frictions in the labor market cause statistical discrimination but that graduating from a T14 university may have a positive causal impact on human capital.



(a) Bar Passage (b) Big Law (c) Federal Clerkships

Fig. 1. Law Outcomes by Law School USNWR Rank.
Figs. 1(a), 1(b), and 1(c) plot the average value of the variable on the *y*-axis against USNWR rank. The figures also include local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year.

This paper contributes to various literatures on human capital, signaling, and the impacts of law school. Our findings provide additional evidence that signaling plays a large and important role in the return to education. Our study is unique in that we can measure the impact of a signal (T14 status) on an outcome (Bar passage) for which it is impossible for a signal to have an effect (due to anonymity) and then compare this to the effect of the signal on outcomes where signaling may play a role. Furthermore, because Bar passage is a *post-graduation* outcome (i.e. post-acquisition of the signal), Bar passage gives us some measure of the human capital of graduates after completing their law school education. While imperfect, in the fact that the skills required for Bar passage may be highly correlated with but not exactly identical to the skills necessary to succeed at a Big Law firm, our setting provides a placebo test that gives an indication as to what extent human capital may vary with the signal.

This paper also contributes to the literature estimating the returns to professional degrees (Hussey, 2012), the wage premium for law school quality (Ehrenberg, 1989; Oyer & Schaefer, 2019; Sander, 2004), and college quality more generally (Andrews, Li, & Lovenheim, 2016; Anelli, 2020; Dale & Krueger, 2002; Ge, Isaac, & Miller, 2018; Hastings et al., 2013; Kurlaender, Carrell, & Jackson, 2016; Zimmerman, 2019). We find that highly-ranked schools do in fact have a larger causal impact on labor-market outcomes than lower-ranked schools, but that the beliefs of employers account for much of the causal impact and only some of this impact can be attributed to the actual skills acquired while attending an elite university.

2. Data

Because USNWR does not maintain an archive of their previous rankings, we rely on several legal blogs (Above the Law, LawSchooli, Blue Print Prep, and Spivey Consulting) for the previous decade's rankings. USNWR does not list the rankings for the bottom quartile of universities (schools ranked approximately 146–192) so we drop these universities from this study. Since the bottom quartile is not completely consistent throughout the years in our sample, there are several schools that, being in the bottom quartile some years but not others, have only a few observations. Similarly, in 2011 the third quartile (schools ranked approximately 100–150) are listed as T3 (tier 3) without an individual rank. Therefore, only the top 97 schools are used from 2011.

For employment outcomes and student characteristics data, we utilize the American Bar Association's (ABA's) Standard 509 Disclosures.[9] The reports contain data that all law schools accredited by the ABA are mandated to publicly release. This dataset goes back to 2011, and reports several key variables of the student body that may affect student outcomes such as median LSAT and UGPA, the gender ratio of the entire school, and the ethnicity ratio of the first-year class. Importantly, the reports also give employment statistics and Bar passage rates ten months after graduation.

Table 1 gives the summary statistics for our outcome variables (Bar passage rate, employment rate, employment rate for jobs requiring the passage of the Bar exam, Big Law placement rate, and placement into federal clerkships) and value-added controls for the years 2011 to 2019. In total the data represent 159 law schools which comprise 1201 observations. Approximately half of the law students are female. About two thirds of law students are white, 9% are Hispanic, 7% are Asian, 6% are black, and less than 1% are some other ethnicity. There are few demographic differences between T14 and non-T14 schools, with the exception that T14 schools have more Asian students and fewer white students.

There are, however, substantial differences in the academic qualifications of T14 and non-T14 students. The median LSAT score of T14 students is 12 points, or slightly less than two standard deviations, higher than the medan LSAT score of their non-T14 peers, and the median undergraduate GPA of T14 students is 0.32 points, or about one and a half standard deviations, higher. Because of the difference in incoming academic qualifications between T14 and non-T14 students, a simple comparison between the employment outcomes of T14 and non-T14 graduates would not accurately estimate the causal impact that attending a T14 university has on law students' outcomes. T14 students would most likely have better employment prospects no matter where they attended law school due to the fact that they have higher academic abilities. For this reason, it is necessary to account for systematic differences in student ability when determining the causal effect of attending a particular law school. We discuss the value-added methodology, which accounts for these systematic differences in order to obtain unbiased estimates of the causal impact of attending one school versus another, in Section 3.

Most law students (84.5%) pass the Bar exam and the vast majority (92.9%) are employed after graduation. Finding employment that requires Bar passage is more difficult, as only about two thirds of lawyers are employed in such a job. Receiving an offer from a Big Law firm is even less likely, as only about 30% of law students are employed in Big Law firms after graduation. Obtaining a federal clerkship is quite rare, as only 3.9% of law students become clerks for federal judges. We also note large differences in the Bar passage and employment outcomes between T14 and non-T14 students. T14 students are 10.7 percentage points (13%) more likely to pass the Bar, 6.1 percentage points (7%) more likely to be employed, 24.7 percentage points (38%) more likely to be employed in a job that requires Bar passage, 63.9 percentage points (204%) more likely to be employed with a Big Law firm, and 9.13 percentage points (234%) more likely to obtain a federal clerkship.

[9] The ABA is the accrediting body for American Law Schools and codifies its accreditation standards in the "Standards and Rules of Procedure for Approval of Law Schools."

Table 1
Summary statistics.

	All	T14	Non-T14
Median Percentile LSAT	160	170	158
	[6.06]	[2.05]	[4.64]
Median Percentile Undergraduate GPA	3.53	3.8	3.48
	[.198]	[.0707]	[.169]
% Male	51.6	52	51.6
	[5.14]	[3.98]	[5.33]
% Female	48.3	47.9	48.4
	[5.12]	[3.94]	[5.31]
% White	64.7	55.7	66.4
	[12.4]	[7.91]	[12.4]
% Hispanic	9.17	8.41	9.31
	[6.1]	[3.11]	[6.5]
% Asian	7.28	10.9	6.59
	[5.2]	[4.11]	[5.1]
% Black	6.32	6.9	6.21
	[5.76]	[1.97]	[6.21]
% Other Ethnicity	.747	.387	.814
	[1.22]	[.475]	[1.3]
Bar Passage %	84.5	93.5	82.8
	[8.87]	[3.27]	[8.57]
Employed %	92.9	98.1	92
	[5.16]	[1.48]	[5.02]
Employed Requiring Bar Passage %	65.4	86.3	61.6
	[14.1]	[7.1]	[11.4]
Big Law %	31.3	85.1	21.2
	[28.2]	[6.75]	[17.1]
Federal Clerkship %	3.91	11.6	2.47
	[4.85]	[7.31]	[2.2]
Observations	1,201	122	1,079
# of Schools	159	14	145

Values are means and standard deviations [in brackets] of the dependent and independent variables used in the value added estimation. Observations are weighted by the total number of graduates in each year. Data comes from the American Bar Association's Standard 509 Disclosures. T14 refers to the traditional Top 14 universities.

We therefore treat any employment, employment requiring Bar passage, employment at a Big Law firm, and federal clerkships as increasing in their level of prestige and competition. Under this assumption, the signaling value of a T14 law school would most likely have the biggest impact at Big Law firms and federal clerkships and the smallest impact on finding any employment. Given that the Bar exam is graded anonymously, we assume that there is no signaling value of a T14 law school on passing the Bar exam and treat this outcome separately given that Bar passage operates outside of the labor market.

Fig. 2 gives scatter plots of the average value of the control variables used in the estimation of school value added in Section 3 against USNWR rank. Given that we will be using a regression discontinuity design in Section 4 in order to estimate the causal impact of T14 status on school value added, it is important to confirm that the student body does not change discontinuously between non-T14 and T14 schools. Fig. 2 gives visual evidence that discontinuities in the characteristics of the student body are unlikely to be driving our results. For all variables used to control for innate differences in ability in the estimation of value added, there is no visible discontinuous difference between the 14th- and 15th-ranked schools, which suggests that any discontinuous difference in value added between T14 and non-T14 schools is unlikely to be due to unobserved discontinuous differences in the students, and therefore potential outcomes, of those who attend those schools.

Appendix Table A.1 gives the fuzzy regression discontinuity results that correspond to the outcomes in Fig. 2. Using Eicker–Huber–White HC3 heteroskedasticity-robust standard errors, we find that students at the 14th-ranked university have slightly *lower* median LSAT scores, are slightly more likely to be Asian or Other Ethnicity, and are slightly less likely to be black relative to students at the 15th-ranked university. Only the increase in percent Other Ethnicity is statistically significant using robust bias-corrected confidence intervals. Given the magnitude of our coefficient estimates for the discontinuity in the value-added controls, we find it unlikely that discontinuities in the characteristics of the student body are driving our results.

3. School value added

3.1. Methodology

In order to determine whether T14 status provides a signaling effect to employers after graduation, it is first necessary to obtain unbiased estimates of the causal impact of attending a specific law school on post-graduation employment outcomes. To do so, we extend the value added with drift methodology, as described for teachers in Chetty et al. (2014), to the law-school level. Intuitively, the value-added methodology measures the difference between the average performance of students who attend a particular school and the predicted performance of those students based on student characteristics that were determined prior to entering law school, in particular prior academic achievement[10].

To estimate each school's value added, we first residualize the outcome variables y_{st} with respect to student demographic characteristics X_{st}, as in Eq. (1). The demographic characteristics include median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, and percent other ethnicity. We include a school fixed effect, γ_{st}, in order to estimate the impact of demographic characteristics based off within-school variation due to the fact that students may sort to various qualities of schools in ways that are correlated with demographic characteristics.

$$y_{st} = \alpha + \beta_1 \boldsymbol{X}_{st} + \underbrace{\gamma_{st} + \varepsilon_{st}}_{r_{st}} \tag{1}$$

Appendix Table B.1 gives the coefficients from the residualization process. We calculate the residual, r_{st}, for each school by adding γ_{st} and ε_{st} so that we do not remove the causal impact of the school.

One advantage of the value added with drift methodology is that it accounts for common shocks that may impact all students at a particular law school in a particular year by using information on the value added for the same school in neighboring years. Our value added estimates for year t, $\hat{\mu}_{st}$, are the projection of the residual estimates in all years *except* year t, $\boldsymbol{r}_{st'}$, onto the residual in year t, r_{st}, as in Eq. (2). We therefore retain the variation that is consistent from year to year while discarding variation that is the result of an anomaly in a particular year.

$$\begin{aligned} r_{st} &= \kappa + \boldsymbol{\delta r}_{st'} + \epsilon_{st} \\ \hat{\mu}_{st} &= \hat{\kappa} + \hat{\boldsymbol{\delta}}\boldsymbol{r}_{st'} \end{aligned} \tag{2}$$

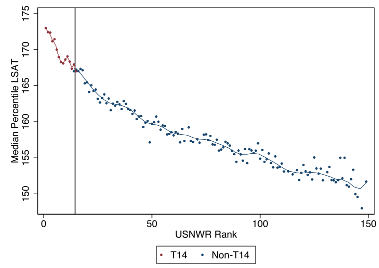
3.2. Results

3.2.1. Bar passage

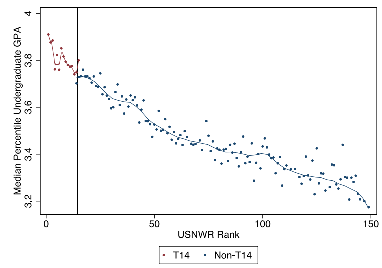
First, we look at each law school's value added in terms of the percentage of students who pass the Bar exam within ten months of graduating from law school. The Bar exam is an exam administered twice a year by each state and jurisdiction in order to test the competency of each lawyer attempting to practice in the state.[11] Because

[10] Kane and Staiger (2008) show that demographic characteristics are largely irrelevant in estimating *teacher* value added after conditioning on prior test scores, and Deming (2014) shows that demographic characteristics are largely irrelevant in estimating school value added after conditioning on prior test scores.

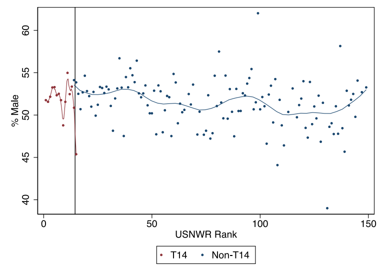
[11] See "Bar Admissions Basic Overview" on American Bar Association's website at https://www.americanbar.org/groups/legal_education/resources/bar_admissions/basic_overview/.



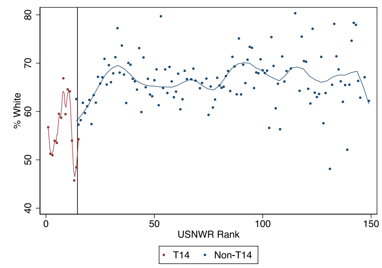
(a) Median LSAT Score



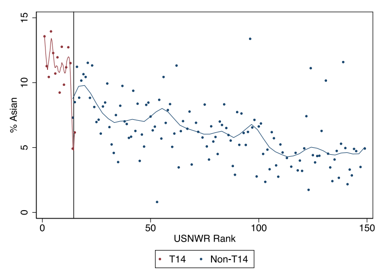
(b) Median Undergraduate GPA



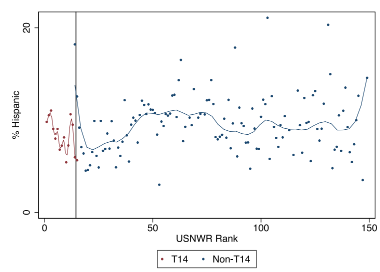
(c) Percent Male



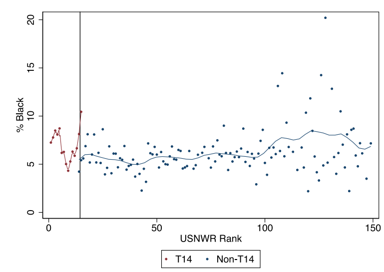
(d) Percent White



(e) Percent Asian



(f) Percent Hispanic



(g) Percent Black

Fig. 2. Value Added Controls by Law School USNWR Rank.
Figs. 2(a), 2(b), 2(c), 2(d), 2(e), 2(f), and 2(g) plot the average value of the variable on the *y*-axis against USNWR rank. The figures also include local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year.

each state takes measures to assure that the exam is graded fairly and consistently[12], we take the Bar exam as an objective measure of the ability of law students and therefore use Bar passage as a measure of graduating students' human capital.

Because each jurisdiction determines the standard for the Bar, and some jurisdictions may have more difficult Bar exams than other jurisdictions, we control for state fixed effects using the jurisdiction where the largest number of students work after graduation[13] for each school when estimating Bar passage value added. Appendix Fig. B.1 plots the average Bar passage rate for each state of largest employment.[14] While we cannot rule out top-coding or bunching because we lack Bar exam *scores*[15], we find that the Bar passage rates for states to which T14 universities send the plurality of their graduates[16] are not systematically different than the Bar passage rates of other states. Our state fixed effects account for the fact that the Bar passage rate differs drastically between states of largest employment, thus with the inclusion of state fixed effects universities cannot increase their value

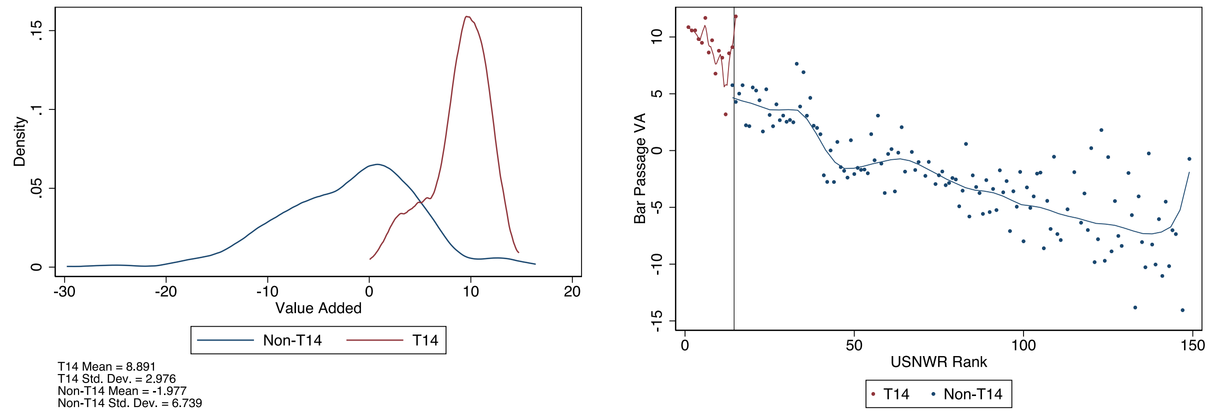
[12] See "Know Your Audience - Who is Grading Your Bar Exam?" at https://barexamtoolbox.com/know-your-audience-who-is-grading-your-bar-exam/.

[13] We assume that students are most likely to take the Bar exam in the state where they are first employed after graduation.

[14] The state of largest employment does not necessarily correspond to the state where a university is located.

[15] One potential issue with the value-added methodology is that upper bounds may mechanically limit how much a university can improve a student's performance. For example, students who would obtain a perfect score on the Bar exam without attending a university cannot improve their Bar exam score by attending a university, thus it would be impossible for a university to provide value added to that specific student. For this reason, one might worry that our value added estimates are biased if there are many students who obtain perfect scores on the Bar exam in some states but not others. Data on individual Bar exam scores are unavailable at the state or university level so we are unable to test this hypothesis, but national data for the Multistate Bar Exam show that the maximum national score for 2019 was 189.4 out of 200 (National Conference of Bar Examiners, 2019) so achieving a perfect score is not a common outcome. We also note that, for our purposes, a perfect Bar exam score results in the same outcome as a Bar exam score at the passing threshold, because we only observe average university Bar exam passage rates and not average university Bar exam scores. Thus we believe that potential top-coding or bunching of Bar exam scores is unlikely to be a source of significant bias in our value added estimates, as our value added estimates are based on the difference between a university's predicted Bar passage rate and the university's actual Bar passage rate. Nevertheless, our state fixed effects do not address the limitation that a university may have a higher (observed) value added on Bar passage than another university that has a higher (unobserved) value added on Bar exam scores.

[16] New York, California, Illinois, and the District of Columbia.



(a) Bar Passage VA Distribution (b) Bar Passage VA by Rank

Fig. 3. Bar Passage Value Added.
Fig. 3(a) gives the kernel density estimates of value added on Bar passage and includes the mean and standard deviation of value added on Bar passage for T14 and non-T14 universities. Fig. 3(b) plots the average value added on Bar passage against USNWR rank and includes local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

added on Bar passage simply by sending more of their students to states with high Bar passage rates.

Fig. 3(a) gives the kernel density estimates of school value added on Bar passage, and Fig. 3(b) shows the average Bar passage value added by USNWR rank of the school. The kernel density estimates show that the T14 schools have both a much higher average value added on Bar passage than non-T14 schools (by about 11 percentage points) and a much smaller standard deviation. The scatter plot of school value added versus USNWR rank shows that, with the exception of some outliers, a school's value added on Bar passage tends to increase as its USNWR rank decreases (lower ranks are better). Given that value added is an estimate of the skills imparted by a school independent of a student's incoming ability, the estimates suggest that higher-ranked schools increase human capital, as measured by the Bar exam, more than lower-ranked schools. This may be surprising given both the diminishing returns to education, such that high-ability students enrolled at the highest-ranked schools might be expected to gain less from high-quality instruction, and potential for mean reversion, such that students who performed unexpectedly well on the LSAT and attend a higher-ranked university may obtain worse outcomes than students who performed unexpectedly poorly on the LSAT and attend a lower-ranked university.

Nevertheless, the negative correlation between school value added on Bar passage and USNWR rank can be potentially explained in two ways. First, some other estimates of college value added have found similar results, so it may simply be the case that higher-ranked universities have a larger causal impact on Bar passage than lower-ranked universities. Mountjoy and Hickman (2020) find that Texas universities with higher Bachelor's completion rates generally have higher value added on Bachelor's completion rates (although the opposite is true for earnings), Kurlaender et al. (2016) find that community colleges at the bottom and very top end of the distribution for average first-year units tend to stay at the bottom and top of the value added rankings, and Grosz (2020) finds that community college nursing programs with selective admission programs have larger causal impacts on earnings than those that use waitlists or lotteries which do not evaluate students according to merit.

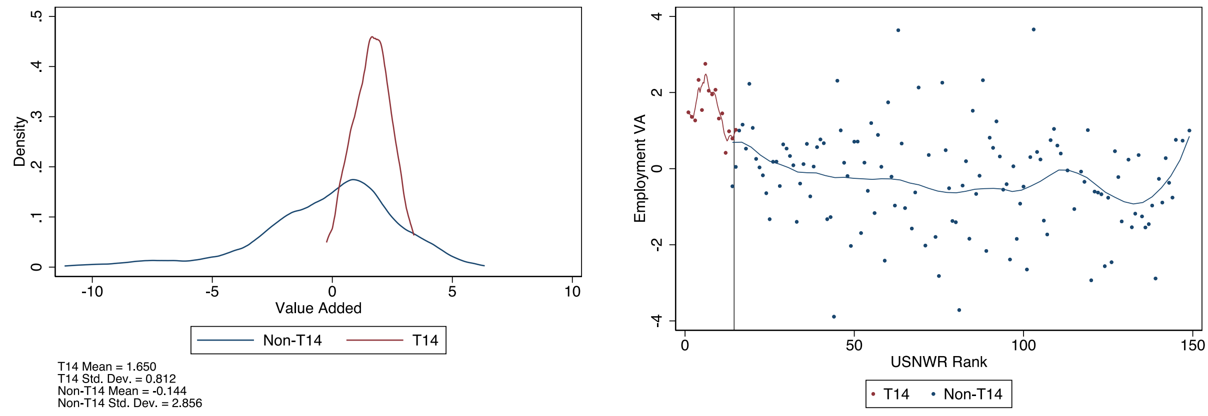
Second, the binary nature of the outcome (i.e. pass/fail) could result in large increases in a university's Bar-passage rate from small increases in human capital accumulation if the incoming students are expected to perform just below the passing threshold upon enrollment. Similarly, large increases in human capital accumulation could result in small increases in a university's Bar-passage rate if the incoming students are expected to perform far below the passing threshold upon enrollment. For this reason, Bar exam *scores*, the underlying latent variable that determines the pass/fail outcome, would provide a more accurate measure of the true value added of each university on human capital. Unfortunately, Bar exam scores at the university level are not available, so we note this potential downside to our measure of value added on human capital.

While higher-ranked schools indeed have higher value added on Bar passage, there is no discontinuity in the trend as we go from non-T14 to T14 schools, which suggests that there is no signaling value from attending a T14 university on Bar passage. This is expected given the fact that the Bar exam is graded anonymously, so there should be no benefit to attending an elite school above and beyond the human capital accumulation that took place at that school, and we would not expect human capital accumulation to vary discontinuously between the 14th- and 15th-ranked schools.

3.2.2. Employment rate

Next, we look at the employment rate of law school graduates ten months after graduation. We define the employment rate as one minus the number of students unemployed and seeking employment divided by the total number of graduates from that school. Because employment offers are not given blindly, so that the employer knows the law school that a student graduated from, we assume that employment offers are given in response to both the human capital of the applicant but also any signaling value provided by their law school pedigree.

Fig. 4(a) gives the kernel density estimates of school value added on employment, and Fig. 4(b) plots average school value added on employment against USNWR rank. While the average T14 school increases the likelihood of any employment by about two percentage points more than the average non-T14 school, the variance of value added on employment for non-T14 schools is much larger and there are schools not ranked in the top 14 that have a higher value added on employment than any of the T14 schools. However, there are also many non-T14 schools that provide very low value added on employment, while all T14 schools are above average. Fig. 4(b) shows that there is little correlation between USNWR rank and value added on employment, which perhaps is not very surprising given that 93% of graduates are employed in some form after graduation. We also see that there is no visible discontinuity in the value added on employment between the T14 and non-T14 schools, suggesting that T14 status does not confer signaling status for obtaining employment more broadly.



(a) Employment VA Distribution

(b) Employment VA by Rank

Fig. 4. Employment Value Added.
Fig. 4(a) gives the kernel density estimates of value added on employment and includes the mean and standard deviation of value added on employment for T14 and non-T14 universities. Fig. 4(b) plots the average value added on employment against USNWR rank and includes local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

However, there is still the possibility of underemployment, meaning that a school could appear to have better employment outcomes if a large number of its graduates are able to gain employment but in jobs for which they are overqualified. To account for underemployment, we also look at the percent of each class employed in position that requires them to pass the Bar exam. Data measuring employment requiring Bar passage did not exist prior to 2012, so we drop 2011 data for the following results.

Fig. 5(a) gives the kernel density estimates of school value added on employment requiring Bar passage, and Fig. 5(b) plots average school value added on employment requiring bar passage against USNWR rank. The figures show that while higher-ranked schools may have little additional benefit when it comes to obtaining any form of employment, higher-ranked schools are better at increasing the likelihood of a student obtaining a job that requires Bar passage. The average T14 school increases the likelihood that one of their graduates obtains employment requiring Bar passage by 22 percentage points more than the average non-T14 school. We also see a more distinct negative relationship between USNWR rank and value added on employment requiring Bar passage (i.e. schools with rankings closer to one have higher value added), suggesting that higher-ranked schools impart more human capital on their graduates. There is also some evidence of a small discontinuity between the T14 and non-T14 trend, which suggests that there may be some positive signaling value from attending a T14 university to employers for jobs requiring Bar passage.

We note the low value added for the school ranked number one (Yale). This is likely partially explained by the high number of graduates who are pursuing other graduate degrees ten months after graduation. The mean percentage of students pursuing graduate degrees for T14 schools ranked below Yale is .97% with a median of .92%, ranging from 0% to 3.21%. However, for Yale, the mean is 2.81%, the median is 2.93%, and the range is .93% to 4.35%. Since graduate students are not included in the count of graduates employed in a Bar passage required occupation, Yale's value added is lower than other T14 schools.

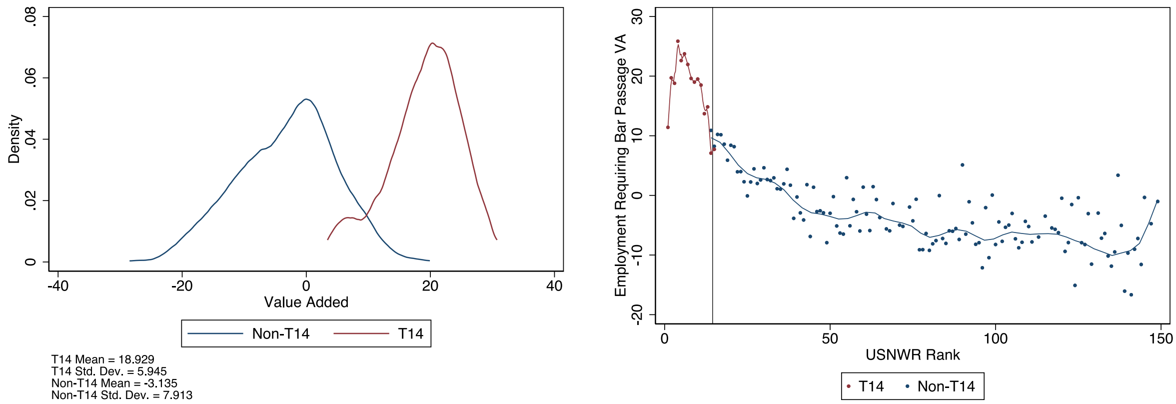
3.2.3. *Big law*

In this section, we seek to calculate the value added by law schools in terms of placement into "Big Law" firms. The National Association of Law Placement defines Big Law as law firms employing more than 250 attorneys.[17] While different definitions of Big Law have been used by those in or covering the legal profession (including definitions regarding the starting salary of associates, Vault's rankings, etc.), the 250-attorneys cut-off is not an arbitrary measurement. Firms larger than 250 attorneys see a sizable benefit in wages. The median starting salary in 2019 for firms between 101 to 250 attorneys was $115,00 while firms with 251 to 500 attorneys had a median starting salary of $160,000 (National Association for Law Placement, 2019). Additionally, in 2019, 38.2% of firms with 251 to 500 attorneys paid $190,000 as their first-year base salary (the rate paid by Vault's top firms) compared to only 6.4% of firms with 101 to 250 attorneys (National Association for Law Placement, 2019). Securing a position in Big Law has thus become a mark of prestige for lawyers.

To account for differences between class sizes (the number of graduates in a law school class range from 33 to 625), we look at Big Law placement as a percentage. Specifically, we look at the percentage of graduates who enter Big Law out of the graduates who enter any law firm or solo practice. This is to avoid biasing against schools with a high rate of graduates who enter jobs that may be more preferable to work in than a law firm or the result of different innate preferences of their students. For example, Yale in 2019 may seem to have a low placement rate, with only 77 out of 217 graduates (approximately 35%) in Big Law, than UCLA, where 135 out of 317 graduates (43%) are in Big Law. However, one must consider the differences in preferences among Yale and UCLA students. Yale and many other T14 schools offer incentives for graduates who enter law jobs in the government and the public interest, including loan forgiveness and fellowship opportunities, which potentially attract students who are not interested in Big Law.[18] Also, Yale is consistently among the top schools in clerkship placement – an average of 32% for our 9-year period – which is potentially a more desirable and more competitive position than Big Law. Therefore, we seek to only compare the amount of those who work in Big Law with those from the same school who work in any firm or solo practice, assuming that most students who go to work for a law firm would prefer to work in Big Law (due to prestige or financial incentives). We thus

[17] See, for example, "The Stories Behind the Numbers: Jobs for New Grads Over More Than Two Decades" on the National Association for Law Placement's website at https://www.nalp.org/1216research.

[18] See, for example, "Financial Support for Public Interest" at https://law.yale.edu/studying-law-yale/areas-interest/public-interest-law/about-public-interest-law/financial-support-public-interest.



(a) Employment Requiring Bar Passage VA Distribution (b) Employment Requiring Bar Passage VA by Rank

Fig. 5. Employment Requiring Bar Passage Value Added.
Fig. 5(a) gives the kernel density estimates of value added on employment requiring Bar passage and includes the mean and standard deviation of value added on employment requiring Bar passage for T14 and non-T14 universities. Fig. 5(b) plots the average value added on employment requiring Bar passage against USNWR rank and includes local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data. Observations are weighted by the total number of graduates in each year. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

take public interest, clerkship, and government work on one hand and work in a law firm on the other hand as non-substitutable preferences. We see that, among Yale graduates who work in law firms or solo practice, an average of 86% work in Big Law compared to just 54% at UCLA.

Fig. 1(b) showed that there was a discontinuous increase in the likelihood of accepting a Big Law offer for the 14th-ranked law school relative to the 15th-ranked law school. This increase could be a result of a discontinuous increase in human capital of the students between the 14th- and 15th-ranked schools. Conversely, the discontinuity could be the result of Big Law firms having a strong demand for T14 schools even over comparable schools just below the rank of 14. Using our value added estimates instead of the raw probability of Big Law placement, Figs. 6(a) and 6(b) show that there are striking differences between the value added on Big Law placement between T14 and non-T14 schools. On average, T14 schools increase the likelihood that their students place at a Big Law firm by 63 percentage points more than non-T14 firms. We also see that the raw discontinuity in Big Law placement between T14 and non-T14 schools seen in Fig. 1(b) is not fully due to a discontinuity in the incoming ability of students at the 14th-ranked school versus the 15th-ranked school, as Fig. 6(b) shows that there is also a discontinuity in the value added the schools provide on Big Law placement.

Combined with the fact that there is no visible discontinuity in value added on Bar passage in Fig. 3(b), Fig. 6(b) gives suggestive evidence that the increase in value added on Big Law placement for T14 schools is driven by the signaling value that a T14 degree confers, as T14 schools do not appear to increase the likelihood that their students pass the Bar exam more than similarly ranked non-T14 schools but do increase the likelihood that their students obtain Big Law placements by much more than their similarly ranked non-T14 peers. In order to determine the size and significance of these discontinuities, we implement a regression discontinuity design in Section 4.

3.2.4. Federal clerkships

Finally, we look at law schools' effects on the placement into federal judicial clerkships, one of the most prestigious career steps for recent law school graduates[19] — in our sample only 3.91% of students obtain a federal clerkship upon graduating. Judicial clerkships typically last one to two years, and essentially allow recent law graduates to act as apprentices for federal judges. Clerks assist their judge in completing the judge's obligations through research, verifying citations, and possibly even drafting opinions, as well as the completion of administrative duties. This allows for clerks to see the inner workings of the U.S. judicial system first-hand, which gives them hands-on experience. As a result of this opportunity's prestige, federal clerkships are also known to be highly competitive (Kozinski, 1990).

In addition to the prestige and educational opportunity of obtaining a federal clerkship, there are also significant labor-market incentives for obtaining a clerkship. While law clerks only have a median salary of $54,000[20] during the clerkship (Indiana University Robert H. McKinney School of Law, 2021), they are highly sought by law firms when their tenure as a clerk is complete. As such, there have been several firms that have announced signing bonuses of over $100,000 for former federal clerks[21], including the California-based firm Dovel & Luner LLP which offers a $140,000 bonus[22]. Thus, the value added by schools in terms of placement into clerkships is a factor of great importance for many law students who hope for this opportunity.

Fig. 7(a) gives the kernel density estimates of school value added on clerkship placement, and Fig. 7(b) plots average school value added on clerkship placement against USNWR rank. Given that clerkships are so rare, it is not particularly surprising that the vast majority of schools have relatively little impact on the likelihood that their students obtain a federal clerkship (i.e. a value added close to zero), nor that the value added on clerkship placement increases exponentially as USNWR rank approaches one. Only three universities outside of the top five[23] have a value added greater than 10, and the top 3 universities have an average value added of 19.3 while the remaining 11 universities in the T14 have an average value added of 4.7. Correspondingly, we see little difference in the value added on clerkships between the 14th- and 15th-ranked schools as they both have relatively little impact on the likelihood that

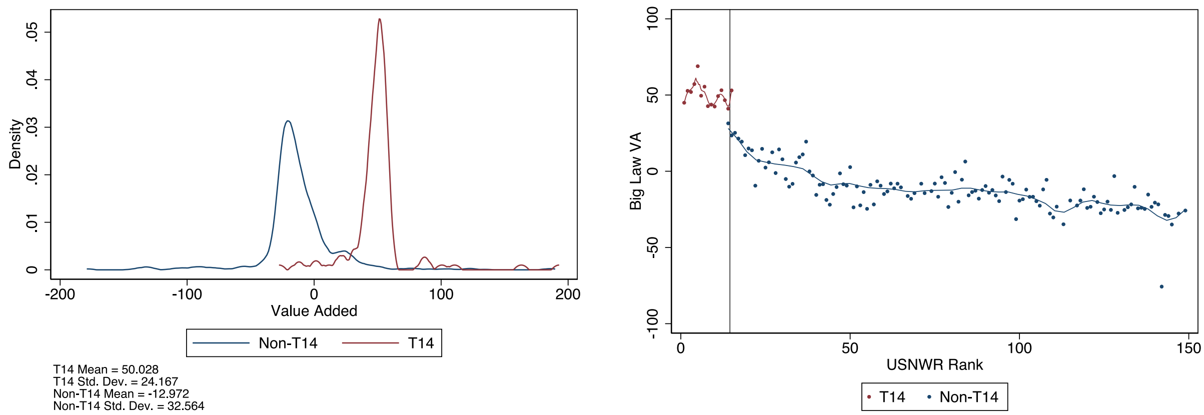
[19] See, for example, Indiana University's Judicial Clerkship Guide at https://mckinneylaw.iu.edu/careers/judicical-clerkships-guide.html.

[20] Salaries are determined by the Judiciary Salary Plan at https://www.uscourts.gov/sites/default/files/jsp_2021/jsp_base_pay_rates_-_table_00_2021.pdf.

[21] See, for example, "April Brings $115K Bonus Showers for Federal Clerks" at https://abovethelaw.com/2018/04/april-brings-showers-of-115k-bonuses-for-federal-clerks/.

[22] See "Top Salary and Benefits" at https://www.dovel.com/join-us/top-salary-and-benefits/.

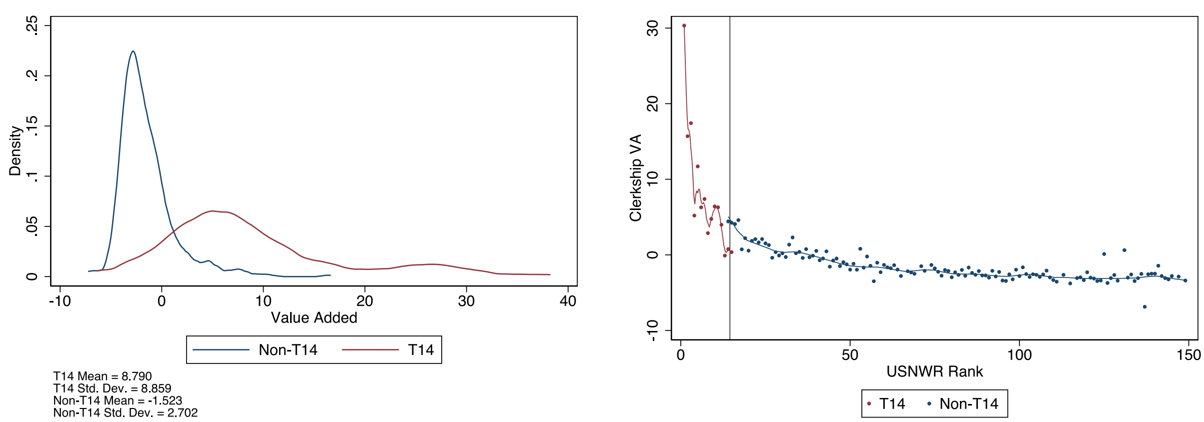
[23] Washington and Lee University, University of Virginia, and New York University.



(a) Big Law VA Distribution

(b) Big Law VA by Rank

Fig. 6. Big Law Value Added.
Fig. 6(a) gives the kernel density estimates of value added on Big Law and includes the mean and standard deviation of value added on Big Law for T14 and non-T14 universities. Fig. 6(b) plots the average value added on Big Law against USNWR rank and includes local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year. Due to three outliers with value added estimates greater in absolute value than 200 (University of Pittsburgh in 2012 with a value added of −204, Howard University in 2013 with a value added of 218, and Boston University in 2012 with a value added of −318), we restrict the graphs' ranges to −200 to 200. We do not impose this restriction when estimating the mean and standard deviation nor in any of the subsequent analyses. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.



(a) Federal Clerkship VA Distribution

(b) Federal Clerkship VA by Rank

Fig. 7. Federal Clerkship Value Added.
Fig. 7(a) gives the kernel density estimates of value added on federal clerkships and includes the mean and standard deviation of value added on federal clerkships for T14 and non-T14 universities. Fig. 7(b) plots the average value added on federal clerkships against USNWR rank and includes local linear regressions estimated separately for T14 and non-T14 universities. The local linear regressions are estimated on the binned data, not the underlying microdata used in the regression analyses. Observations are weighted by the total number of graduates in each year. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

one obtains a federal clerkship, and, if anything, the difference appears to be negative.

4. Regression discontinuity

Fig. 1 showed that students attending schools with higher USNWR rankings are more likely to pass the Bar exam, more likely to obtain a Big Law firm offer, and more likely to obtain a federal clerkship. Section 3 isolated the extent to which these differences in outcomes are attributable to the quality of the school as opposed to the incoming ability of the students enrolled in a school using the value added with drift methodology. While the value added estimates give us an estimate of the causal impact of a particular school, they do not, however, decompose the causal impact down into causal increases in human capital versus the causal signaling value of attending a particular university. This section attempts to isolate the signaling component of being labeled as a T14 university by using a regression discontinuity (RD) design to determine the change in a school's value added as a school goes from rank 15 to rank 14.

4.1. Methodology

The RD design is based upon the assumption that potential outcomes, in this case school value added, are smooth around the cutoff where law schools go from being non-T14 to T14. This assumption essentially implies that treatment (T14 status) is randomly assigned at the limit of the cutoff between T14 and non-T14 law schools. We use a local linear specification (Gelman & Imbens, 2019; Hahn, Todd, & Van der Klaauw, 2001; Porter, 2003) that takes the form

$$\hat{\mu}_{st} = \alpha_2 + \delta_2^- R_{st} \cdot \mathbb{1}[R_{st} \leq 14] + \delta_2^+ R_{st} + \gamma_2 T14_s + \beta_2 \boldsymbol{X}_{st} + \varepsilon_{st} \quad (3)$$

where $T14_s$ is a binary variable for T14 status, $\hat{\mu}_{st}$ denotes the estimated value added of a law school s in year t, R_{st} is the USNWR rank of the law school, $\boldsymbol{X}_{st}$ is a vector of covariates that includes all of the controls included in the value added residualization process, and ε_{st} is an idiosyncratic error term. We do not constrain the slope of the lines to be equal on both sides of the cutoff, hence the separate coefficients δ_2^- and δ_2^+. Therefore $(\hat{\delta}_2^- + \hat{\delta}_2^+)$ is the estimated slope for observations to the left of the cutoff, $\hat{\delta}_2^+$ is the estimated slope for observations to the right of the cutoff, and $\hat{\gamma}_2$ gives the estimated increase in school value added for schools in the T14 relative to schools not in the T14. Observations are weighted using a triangular kernel that puts more weight on observations closer to the cutoff, as this provides the optimal boundary correction (Cheng, Fan, & Marron, 1997). Observations are also weighted by the total number of graduates from the university in that year. We calculate standard errors using Eicker–Huber–White HC3 heteroskedasticity-robust standard errors following (Kolesár & Rothe, 2018).[24]

When defining the T14 variable, we look only at the traditional T14 law schools. By traditional, we mean the schools that have been in the top 14 places of the rankings in almost every single year.[25] This definition excludes The University of Texas at Austin since it has been ranked in the top 14 only twice while the other universities have had 30 years to gain the reputation of being a T14. Therefore, we would not expect UT-Austin and UCLA to have the same potential signaling effect as the other schools that have placed in the top 14. Similarly, despite Georgetown ranking 15th in 2018, we still include Georgetown in the "traditional" T14 because one year at the 15th-ranked placement most likely would not end a decades-long reputation. Our assumptions appear to be justified when comparing UT-Austin's Big Law placement with Georgetown's.[26] Placing outside the top 14 in 2018 does not appear to have had a negative effect on Georgetown. While UT-Austin did see a rise in the percent of its class going into Big Law following its 2012 tie for 14th place, the most UT-Austin ever placed in Big Law was 67%, which is in the bottom 5% of T14 placements. Thus, we feel there is a necessity of differentiating non-traditional top 14 schools from the T14.

Since our data lacks perfect compliance at the observed cutoff point (meaning that our defined T14 schools do not correspond perfectly to being within the top 14 ranks given that the T14 Georgetown ranks 15th in one year and the non-T14 UT-Austin ranks 14th in two years), we implement a fuzzy RD by using the RD design to instrument for T14 status. We then use predicted T14 status to estimate the effect of T14 status on school value added. The first stage regression of the fuzzy RD design gives the change in probability of the treatment variable (T14) as the running variable (USNWR rank) crosses the threshold (as rank goes from 14 to 15) (Lee & Lemieux, 2010). The equation for the first-stage specification is therefore

$$T14_s = \alpha_1 + \delta_1^- R_{st} \cdot \mathbb{1}[R_{st} \leq 14] + \delta_1^+ R_{st} + \gamma_1 \cdot \mathbb{1}[R_{st} \leq 14] + \beta_1 \boldsymbol{X}_{st} + \nu_{st} \quad (4)$$

where $\mathbb{1}[R_{st} \leq 14]$ is a binary variable indicating whether a university's rank is less than or equal to 14, R_{st} is the USNWR rank of the law school, $\boldsymbol{X}_{st}$ is a vector of covariates that includes all of the controls included in the value added residualization process, and ν_{st} is an idiosyncratic error term. Therefore $(\hat{\delta}_1^- + \hat{\delta}_1^+)$ is the estimated slope for observations to the left of the cutoff, $\hat{\delta}_1^+$ is the estimated slope for observations to the right of the cutoff, and $\hat{\gamma}_1$ gives the estimated increase in the likelihood of T14 status as you cross the threshold from 15th-ranked university to 14th-ranked university.

Evidence of manipulation to a university's USNWR rank would violate the assumption that law schools are essentially randomly assigned treatment status at the T14 cutoff. Because law schools have no incentive to purposefully lower their ranking and there are a fixed number of T14 schools, there is little concern of manipulation of the running variable. Statistically testing for the difference in densities at the cutoff (McCrary, 2008) will provide little useful information given that there is only one 14th-ranked university and one 15th-ranked university. For this reason, we do not report results from the McCrary test.

That being said, one might worry that the types of students who decide to enroll in the 14th-ranked university may be unobservably different (with respect to the value-added controls) than students who decide to enroll in the 15th-ranked university, particularly because T14 status is well-known in the legal profession. For example, a student who is absolutely determined to obtain an offer from a Big Law firm may work harder to secure admission to a T14 university or be more likely to accept an admission offer from a T14 university than a student who is less interested in Big Law. To the extent that unobserved differences in effort manifest as differences in GPA or LSAT score, particularly given that a high GPA and LSAT score are necessary for admission to a T14 university, we control for these differences in GPA and LSAT score both when estimating school value added and in our RD analyses. Thus these differences are unlikely to bias our value added or RD estimates.

If, however, unobserved differences do not manifest as differences in our control variables, then our value added estimates would be biased and our RD estimates would be biased upwards. This is because T14 universities would appear to have a larger causal impact on outcomes than they truly do, which would increase the gap between T14 and non-T14 value added. Therefore our RD estimates should be considered an upper bar of the true causal effect of T14 status. Nevertheless, even a student uninterested in Big Law would have an incentive to attend a T14 university if given the opportunity, as the prestige given by graduating from a T14 university would be beneficial even if applying to jobs with less prestige than Big Law firms. Fig. 4(a) shows that, on average, T14 universities have higher value added on any employment than non-T14 universities. Thus the potential bias introduced by the selection of students to the 14th- and 15th-ranked universities may be small.

4.2. Results

Table 2 gives the second stage coefficient estimates for $\hat{\gamma}_2$, the estimated effect of T14 status on school value added. Columns (1) and (2) give the effect on Bar passage value added. Column (1) excludes a state fixed effect in the estimation of value added, while column (2) includes a state fixed effect to account for the fact that, all else equal, the Bar exam may be more difficult to pass in certain states. Columns (3), (4), (5), and (6) give the effect of T14 status on employment value added, employment requiring Bar passage value added, Big Law value added, and federal clerkship value added, respectively. Eicker–Huber–White heteroskedasticity-robust HC3 standard errors are presented in parenthesis, and robust bias-corrected confidence intervals (Calonico, Cattaneo, & Farrell, 2018, 2020; Calonico, Cattaneo, Farrell, & Titiunik, 2019; Calonico, Cattaneo, & Titiunik, 2014) are presented in brackets. The first row beneath the T14 coefficient estimate gives the mean of the outcome for which value added was estimated (e.g. percent passing the Bar, percent entering Big Law, etc.). The second row presents the

[24] Ideally, we would block bootstrap our standard errors at the school level by block bootstrapping the sample prior to the estimation of value added. This is to account for the fact that our dependent variable is estimated (not a parameter) and therefore also has a corresponding standard error. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors calculated via the sandwich estimator assume that value added is measured without error, so the standard errors will likely be inaccurately precise. However, bootstrapped standard errors are infeasible for two reasons. First, block bootstrapping will inherently omit some ranks, which makes it difficult to estimate the conditional expectation function on both sides of the cutoff, particularly for T14 schools given that there are only 14 values of the independent variable to create variation in the dependent variable. Second, the value-added methodology assumes stationarity, so the expected value of value added is 0 in all years. The value-added methodology thus estimates a *relative* value added, based on the average value added of the other schools in the sample. Therefore value added estimates from different bootstrap samples are incomparable.

[25] See footnote 1.

[26] See appendix Table C.1.

Table 2
Regression discontinuity on law school value added.

	(1) Bar Passage VA	(2) Bar Passage VA	(3) Employment VA	(4) Employment Requiring Bar Passage VA	(5) Big Law VA	(6) Clerkship VA
T14	1.617*	0.406	0.075	6.701***	30.136***	−1.365
	(0.891)	(0.452)	(0.301)	(2.033)	(7.988)	(0.994)
	[−1.283, 3.954]	[−0.939, 1.608]	[−1.214, 0.278]	[−3.057, 8.152]	[10.194, 51.772]	[−4.448, 1.354]
Outcome Mean	84.5	84.5	92.9	65.4	31.3	3.91
Polynomial Degree	1	1	1	1	1	1
Demographic Variation	Within-School	Within-School	Within-School	Within-School	Within-School	Within-School
Demographic Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	–	Y	–	–	–	–
T14 Bandwidth	13	13	13	13	13	13
Non-T14 Bandwidth	20	25.5	24.1	32.4	40.5	51.8
Effective T14 Observations	105	105	105	105	105	105
Effective Non-T14 Observations	181	233	223	287	362	472

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates on the variable T14 from a fuzzy regression discontinuity of school value added on T14 status, where T14 status is instrumented with the running variable USNWR rank. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors are reported in parenthesis and robust bias-corrected 95% confidence intervals are presented in brackets. Observations are weighted by the total number of graduates in each year. The first row beneath the T14 coefficient estimate gives the mean of the outcome for which value added was estimated. The second row presents the local polynomial degree used in Eqs. (3) and (4). All regressions include the controls median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, percent other ethnicity, and year fixed effects. Rows five and six give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eqs. (1), (3), and (4). Rows seven and eight give the bandwidth for the T14 and non-T14 observations, respectively, which are calculated by minimizing the mean squared error (Imbens & Kalyanaraman, 2012) of the regression. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows nine and ten give the corresponding effective number of observations in the dataset after imposing the bandwidth condition. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

local polynomial degree[27] used in Eqs. (3) and (4). Rows five and six give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eqs. (1), (3), and (4).[28] Rows seven and eight give the bandwidths for the T14 and non-T14 observations, respectively, which are calculated by minimizing the mean squared error (Imbens & Kalyanaraman, 2012) of the regression. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows nine and ten give the corresponding effective number of observations in the dataset after imposing the bandwidth condition.

Looking at columns (1) through (3), there is little evidence that T14 schools increase the likelihood that their students pass the Bar exam or obtain employment by any more than their USNWR rank would imply, as there is no discontinuous change in value added between the 14th- and 15th-ranked schools for these outcomes after controlling for potential differences in the difficulty of the Bar exam between states. We do, however, see a large discontinuity in the value added that T14 schools provide on employment requiring bar passage (7 percentage points) and Big Law (30 percentage points). These discontinuous increases in the value added that T14 schools provide, which are not mirrored by discontinuous increases in value added on Bar passage, represent a 10% and 96% increase in the likelihood of obtaining employment requiring Bar passage or at a Big Law firm, respectively. Given that the causal impact of attending a T14 school is only higher for more prestigious outcomes where labor demand is more selective and not for outcomes measuring human capital, the T14 effect is almost surely due to the signaling effect of attending a university that the law profession identifies as elite.

Interestingly, we estimate no signaling effect for T14 law firms on our most prestigious outcome, federal clerkships. In fact, the point estimate is negative, albeit statistically insignificant. This is likely due to the laws of supply and demand in the labor market for federal clerkships. Federal clerkships are prestigious because they are rare. In our data, there are an average of 1136 federal clerkship placements per year and an average of 29,049 total graduates per year, so only 4% of students obtain a federal clerkship. Given that the top five schools on average contribute 1624 graduates per year, it is entirely possible that the totality of federal clerkships is filled by top-*five* law schools such that the remaining nine schools in the T14 have little opportunity to add value to the likelihood that their students obtain a federal clerkship. While it is not the case that federal clerkships are exclusively filled by top-five law school students (2.5% of non-T14 law school students obtain federal clerkships on average), federal clerkships are dominated by the top three law schools, which send 32%, 20%, and 19%, of their students, respectively, into federal clerkships. The rest of the T14 schools (ranks 4–14) send only 9% of their students on average.

Fig. 8 gives placebo coefficient estimates and empirical p-values by estimating an RD coefficient estimate for every single possible rank cutoff, from 5 to 145. We estimate sharp RD estimates to account for the fact that T14 status would not correspond to the various placebo cutoffs. We calculate empirical p-values by calculating the proportion of the placebo coefficient estimates that are greater in absolute value than the absolute value of our sharp RD coefficient estimate from a cutoff of 14.[29] As can be seen from Figs. 8(d) and 8(e), our estimates for the impact of T14 status on value added on employment requiring Bar passage and Big Law placement are in the far right tail of the placebo RD coefficient estimate distribution. Six estimates are larger in absolute value than the coefficient estimate we estimate for value added on employment requiring Bar passage, although two of the six placebo estimates are negative, and only one estimate is greater in absolute value than the coefficient estimate for value added on Big Law, although this placebo coefficient estimate is negative instead of positive. The RD estimates for value added on Bar passage (excluding and including state fixed effects) and employment remain statistically insignificant at conventional levels using empirical p-values, and the coefficient estimate for value added on federal clerkships is significantly negative. As seen in Fig. 7(b), it appears that the primary signaling

[27] We use local linear regression for all analyses, so the local polynomial degree is always equal to one.

[28] All regressions include the controls median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, and percent other ethnicity.

[29] Our sharp RD coefficient estimates are smaller in magnitude than our fuzzy RD estimates because of the fact that the fuzzy RD coefficient estimate is scaled by the inverse of the increase in probability of T14 status, which is less than one.

Table 3
Regression discontinuity on employment requiring bar passage value added.

	(1) Employment Requiring Bar Passage VA	(2) Employment Requiring Bar Passage VA	(3) Employment Requiring Bar Passage VA	(4) Employment Requiring Bar Passage VA	(5) Employment Requiring Bar Passage VA	(6) Employment Requiring Bar Passage VA
T14	−0.889	4.636*	7.095***	6.701***	7.398***	8.630***
	(4.488)	(2.482)	(2.554)	(2.033)	(1.841)	(1.755)
	[−16.042, 18.319]	[−7.046, 7.397]	[−5.156, 7.576]	[−3.057, 8.152]	[−2.010, 8.519]	[−0.986, 9.161]
Outcome Mean	65.4	65.4	65.4	65.4	65.4	65.4
Polynomial Degree	1	1	1	1	1	1
Demographic Variation	Within-School	Within-School	Within-School	Within-School	Within-School	Within-School
Demographic Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	-	-	-	-	-	-
T14 Bandwidth	5	10	13	13	13	13
Non-T14 Bandwidth	5	10	15	32.4	60	100
Effective T14 Observations	31	74	105	105	105	105
Effective Non-T14 Observations	44	93	138	287	537	871

* ($p < 0.1$), ** ($p < 0.05$), *** ($p < 0.01$). This table contains the coefficient estimates on the variable T14 from a fuzzy regression discontinuity of school value added on employment requiring Bar passage on T14 status, where T14 status is instrumented with the running variable USNWR rank. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors are reported in parenthesis and robust bias-corrected 95% confidence intervals are presented in brackets. The bias bandwidths for each column are calculated by setting the main bandwidth on each side of the cutoff and maintaining the ratio of main bandwidth to bias bandwidth from Table 2 column (4) on each side of the cutoff. Observations are weighted by the total number of graduates in each year. The first row beneath the T14 coefficient estimate gives the mean of the outcome for which value added was estimated. The second row presents the local polynomial degree used in Eqs. (3) and (4). All regressions include the controls median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, percent other ethnicity, and year fixed effects. Rows five and six give the fixed effect controls included in X_{st} in Eqs. (1), (3), and (4). Rows seven and eight give the bandwidth for the T14 and non-T14 observations, respectively. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows nine and ten give the corresponding effective number of observations in the dataset after imposing the bandwidth condition. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

effect for value added on federal clerkships occurs only for the top three universities.[30]

Tables 3 and 4 vary the bandwidth of the kernel of the local linear regression. For a bandwidth of five ranks on each side of rank 14, the RD coefficient estimate on value added on employment requiring Bar passage is small in magnitude and statistically insignificant. The RD coefficient estimate increases to 5 percentage points (7%) at a bandwidth of ten, and then remains relatively stable and similar in magnitude to our main estimate of 7 percentage points (10%) for bandwidths larger than 15. For value added on Big Law employment, the RD coefficient estimate is relatively stable regardless of the bandwidth. The coefficient estimate is slightly larger with a tight bandwidth of five ranks (37 percentage points, or 119%) and slightly smaller with bandwidth of ten ranks (21 percentage points, or 68%), but for all larger bandwidths the coefficient estimate is remarkably similar to our main estimate of 30 percentage points (96%).

Thus our results suggest that there is an economically and statistically significant signaling effect of attending a T14 law school on obtaining a prestigious job. However, there are other possible explanations for our findings. Bar-exam passage may not be a sufficiently rigorous measure to determine differences in human capital at the right tail of the law-student-ability distribution. Because differences in a latent variable that varies somewhat continuously are mapped onto a binary outcome of pass or fail, we may be missing important variation in human capital that would show that differences in value added on human capital do in fact vary discontinuously from the 14th- to 15th-ranked law schools. If this were the case, then the discontinuous difference in value added on Big Law employment would simply reflect the fact that T14 schools are doing that much better teaching their students and would be an example of why Feng and Graetz (2017) argue against interpreting regression discontinuity results in education as pure signaling effects. Nevertheless, we find it likely that some of our measured signaling effect is in fact a true effect that is independent of human capital accumulation. We discuss the policy implications of such an effect in the following section.

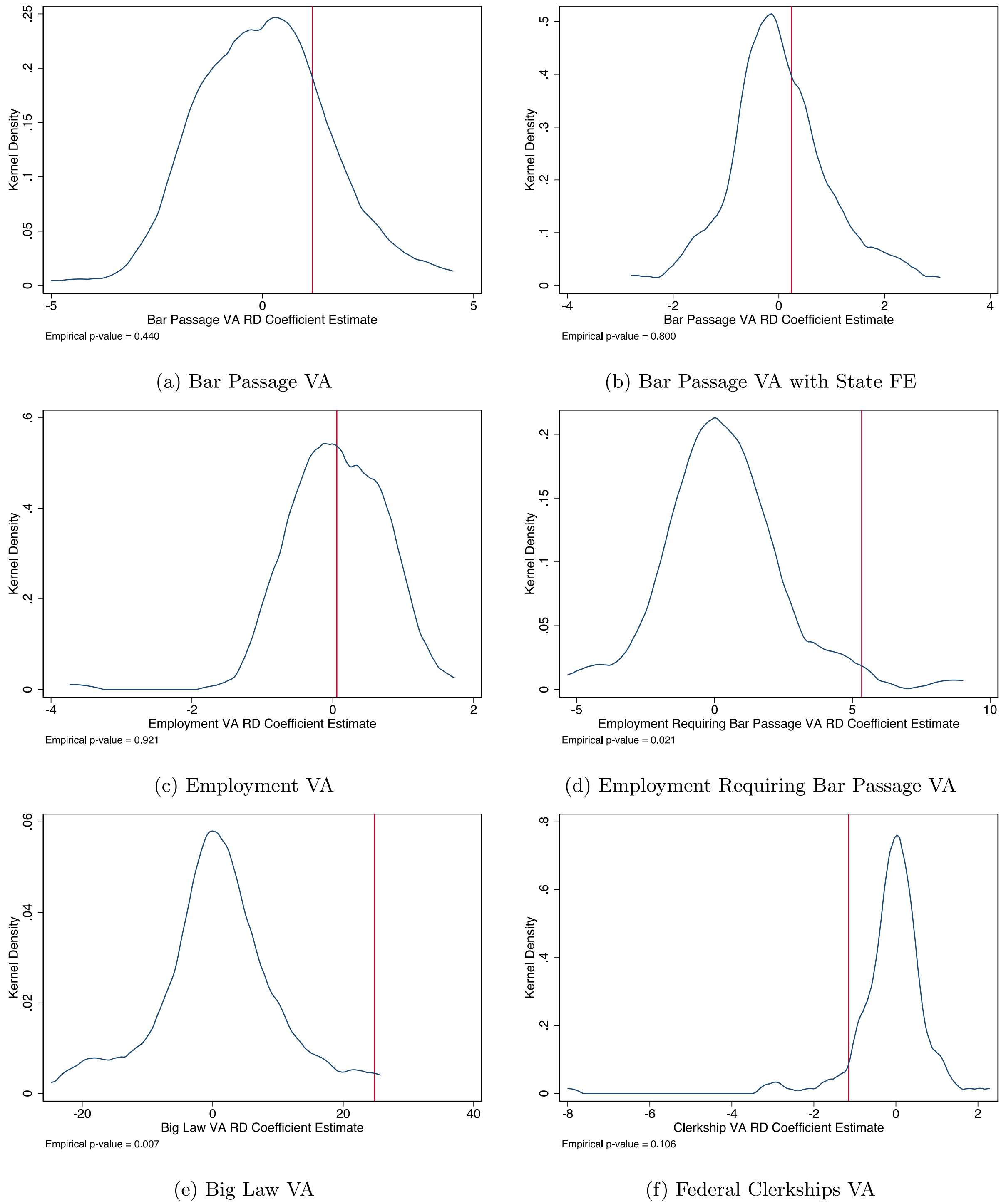
[30] The coefficient estimate from a RD of value added on federal clerkships on rank using a cutoff of 5 gives a coefficient estimate of 37.615.

5. Labor-market implications

We consider policy implications from four perspectives: the law student, the law school, the Big Law firm, and a benevolent social planner attempting to maximize social surplus. From the law student's perspective, there is an immense benefit to attending a T14 university if the student is deciding between T14 and non-T14 offers. This is consistent with the literature showing that elite universities confer benefits above and beyond the quality of the institution in the form of prestige (Bordón & Braga, 2020; Rivera, 2011) or markers of fit (Rivera, 2012), as elite firms may exclusively recruit from certain prestigious universities regardless if this is the profit-maximizing strategy for the firm (Rivera, 2015, 2020). This confers an advantage to the graduates of those universities, thus, *Ceteris paribus*, a student is better off attending a T14 university over a non-T14 university, although preferences for other factors such as location or campus amenities do not make this unambiguously true for all students deciding between T14 and non-T14 schools. Also, T14 schools may be more expensive (depending on the particular aid package received by the applicant), although the student may still receive more consumer surplus attending the higher-priced T14 school given the increase in payoff. This will likely increase the demand for T14 law schools, potentially leading to tuition increases at T14 law schools.

From the law school's perspective, T14 schools ranked near the bottom of the top 14 have a large incentive to advertise the causal benefit of attending their school. For example, the traditionally-14th-ranked school, Georgetown University, could advertise that while the University of Texas at Austin's law school was ranked 14th by USNWR recently, UT Austin would nevertheless be unable to provide the signaling effect to Big Law firms that attending Georgetown would provide. Georgetown could trade on its prestige, which would in turn likely change the preferences of law school applicants and lead to the increase in demand mentioned above. Given that the supply of law school admission slots is likely relatively inelastic in the short run, this would allow Georgetown to increase tuition.

From the Big Law firm's perspective, firms are likely overpaying for T14 students and under-recruiting highly-ranked non-T14 students. Given our evidence of a T14 signaling effect, firms could likely maintain similar productivity levels if, at the margin, they replaced employees



(a) Bar Passage VA

(b) Bar Passage VA with State FE

(c) Employment VA

(d) Employment Requiring Bar Passage VA

(e) Big Law VA

(f) Federal Clerkships VA

Fig. 8. Placebo Regression Discontinuity Coefficient Estimate Distributions.
Figs. 8(a), 8(b), 8(c), 8(d), 8(e), and 8(f) give the kernel density estimates of the placebo coefficient estimates from a regression discontinuity of value added on USNWR rank, where the USNWR rank cutoff varies throughout the range of USNWR rank so that each placebo cutoff results in one placebo regression discontinuity coefficient estimate. The figures also include empirical p-values, which are estimated by calculating the proportion of the placebo coefficient estimates that are greater in absolute value than the absolute value of our coefficient estimate from Table 2. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

that graduated from the 14th-ranked law school with an employee that graduated from the 15th-ranked law school. Given that many Big Law firms place a value on T14 graduates, the demand, and therefore equilibrium wage, for non-T14 graduates will likely be lower than for T14 graduates. If firms can replace, at the margin, 14th-ranked graduates with 15th-ranked graduates and therefore maintain constant productivity while lowering wages, they will increase their profits and gain a competitive advantage. This should lead to a decrease in demand for T14 graduates and an increase in demand for highly-ranked non-T14 graduates, which should close the wage gap between T14 and non-T14 graduates.

Alternatively, Big Law firms may not place a value on T14 graduates because the firm itself believes T14 graduates have a higher productivity but instead because those that demand legal services from Big Law firms are willing to pay a premium for services provided by T14 graduates. Rivera (2015) provides evidence from interviews with

Table 4
Regression discontinuity on big law value added.

	(1) Big Law VA	(2) Big Law VA	(3) Big Law VA	(4) Big Law VA	(5) Big Law VA	(6) Big Law VA
T14	37.215	21.386*	29.256***	30.136***	30.711***	31.618***
	(23.689)	(12.044)	(9.079)	(7.988)	(7.228)	(7.024)
	[−84.895, 118.824]	[−8.129, 58.738]	[3.969, 50.887]	[10.194, 51.772]	[11.989, 51.191]	[13.728, 52.314]
Outcome Mean	31.3	31.3	31.3	31.3	31.3	31.3
Polynomial Degree	1	1	1	1	1	1
Demographic Variation	Within-School	Within-School	Within-School	Within-School	Within-School	Within-School
Demographic Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	–	–	–	–	–	–
T14 Bandwidth	5	10	13	13	13	13
Non-T14 Bandwidth	5	10	20	40.5	80	100
Effective T14 Observations	31	74	105	105	105	105
Effective Non-T14 Observations	44	93	181	362	699	871

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates on the variable T14 from a fuzzy regression discontinuity of school value added on Big Law on T14 status, where T14 status is instrumented with the running variable USNWR rank. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors are reported in parenthesis and robust bias-corrected 95% confidence intervals are presented in brackets. The bias bandwidths for each column are calculated by setting the main bandwidth on each side of the cutoff and maintaining the ratio of main bandwidth to bias bandwidth from Table 2 column (5) on each side of the cutoff. Observations are weighted by the total number of graduates in each year. The first row beneath the T14 coefficient estimate gives the mean of the outcome for which value added was estimated. The second row presents the local polynomial degree used in Eqs. (3) and (4). All regressions include the controls median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, percent other ethnicity, and year fixed effects. Rows five and six give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eqs. (1), (3), and (4). Rows seven and eight give the bandwidth for the T14 and non-T14 observations, respectively. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows nine and ten give the corresponding effective number of observations in the dataset after imposing the bandwidth condition. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology.

lawyers that hiring graduates with prestigious academic credentials is a means of increasing clients' confidence in a law firm and the perceived value of a law firm's services. If the demand for Big Law services is partially determined by the number of T14 graduates employed by the firm, then Big Law firms may be acting rationally in paying T14 graduates more than equivalently-skilled non-T14 graduates, as the former have a higher marginal revenue product than the latter. In this case, it is the Big Law firm's clients who are overpaying for services provided by T14 graduates, and the clients would likely receive similar legal services at a reduced cost if they purchased legal services from firms that employed, at the margin, more graduates of the 15th-ranked law school than the 14th-ranked law school. This should also lead to a decrease in demand for T14 graduates and an increase in demand for highly-ranked non-T14 graduates, which should close the wage gap between T14 and non-T14 graduates.

From a benevolent social planner's perspective, the wage difference between T14 and non-T14 graduates should reflect differences in the marginal product of labor between T14 and non-T14 graduates. In other words, the signaling effect of attending a T14 university should be equal to zero if the Big Law labor market is efficient. However, asymmetric information between labor supply and labor demand likely lead to the equilibrium outcome where firms desire signals from applicants to show that they have high levels of human capital. Furthermore, efficient markets would suggest that, *ceteris paribus*, the cost of attending any two law schools that have the same value added should be equal. Again, location differences and other preferences lead to universities operating in a monopolistically competitive market instead of a perfectly competitive one. However, the tuitions of two universities that differ only in the human capital that they impart upon their graduates should differ only by the amount that the human capital they impart is valued in the marketplace.

6. Conclusion

This paper studies to what extent human capital and signaling play a role in the labor market for law students. We investigate this question by examining whether the causal effect of Top 14 law schools on the likelihood that their graduates obtain offers from Big Law firms, which are the most competitive and offer the highest starting salaries, is more than one would expect given the causal effect those law schools have on the likelihood that their students pass the Bar exam, which is graded blindly.

In order to estimate the causal effect of each law school, we implement the Chetty et al. (2014) value added with drift methodology at the school level. The value-added methodology controls for observable student characteristics, most importantly undergraduate GPAs and LSAT scores, that proxy for a student's ability prior to entering law school and then attributes the difference between a student's expected outcome and their actual outcome to the school. The drift methodology allows for a school's value added to change from year to year, which likely accurately reflects the true causal effect of a law school as professors are hired, retire, go on sabbatical, and vary in their own teaching abilities from year to year.

We then utilize the fact that the 14 universities that have continuously comprised the top 14 spots of the USNWR's law school rankings are known within the profession as the Top 14 universities, and therefore have potential signaling value. We use a regression discontinuity design in order to estimate the causal difference in value added due to obtaining T14 status by estimating the difference in value added between the 14th- and 15th-ranked universities.

We find that there is a discontinuous increase in the value added a school provides on both employment requiring Bar passage and Big Law employment. This increase does not follow the natural increase in value added as USNWR rank approaches one, as value added trends smoothly to the left of rank 14 and the right of rank 15 but not from rank 14 to 15. This discontinuous increase could indicate either a discontinuous increase in human capital accumulation at T14 universities or the signaling effect of T14 designation. We attempt to decompose these two effects by measuring the difference in value added between T14 and non-T14 law schools on Bar passage, which measures the pure human capital effect due to the fact that students are graded anonymously on the Bar exam. We find little to no causal impact of T14 status on the value added that a law school provides on Bar passage. We therefore conclude that T14 status provides a large signaling benefit to both T14 universities and their graduates.

This large signaling effect represents a market failure, and the incentives differ between the various agents. If provided with this information, students will likely increase their demand for T14 universities due to the large potential private benefit that T14 universities provide. T14 schools will likely take it upon themselves to disseminate this information in order to increase demand for law school admissions at their university and correspondingly increase tuition. Big Law firms could recruit fewer 14th-ranked graduates and more 15th-ranked graduates

Table A.1
Regression discontinuity on student demographic characteristics.

	(1) Median Percentile LSAT	(2) Median Percentile Undergraduate GPA	(3) % Male	(4) % Female	(5) % White	(6) % Hispanic	(7) % Asian	(8) % Black	(9) % Other Ethnicity
T14	−1.400**	−0.006	1.389	−1.404	1.487	0.302	3.406*	−1.913*	0.831***
	(0.646)	(0.018)	(1.311)	(1.293)	(5.426)	(1.896)	(1.974)	(0.999)	(0.278)
	[−1.669, 1.503]	[−0.082, 0.021]	[0.420, 6.723]	[−6.641, −0.387]	[−28.461, −9.037]	[−1.704, 9.658]	[−2.321, 11.161]	[−1.833, 2.529]	[0.707, 2.393]
Outcome Mean	159	3.5	52	48	67	8.7	6.37	6.26	.921
Polynomial Degree	1	1	1	1	1	1	1	1	1
T14 Bandwidth	13	13	13	13	12.9	13	13	13	13
Non-T14 Bandwidth	14.5	27.2	28.2	28.2	15.7	18.8	55.9	27.1	31.9
Effective T14 Observations	105	105	105	105	105	105	105	105	105
Effective Non-T14 Observations	138	250	265	265	147	173	509	250	281

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates on the variable T14 from a fuzzy regression discontinuity of demographic characteristics on T14 status, where T14 status is instrumented with the running variable USNWR rank. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors are reported in parenthesis and robust bias-corrected 95% confidence intervals are presented in brackets. Observations are weighted by the total number of graduates in each year. The first row beneath the T14 coefficient estimate gives the mean of the dependent variable. The second row presents the local polynomial degree used in Eqs. (3) and (4). Rows three and four give the bandwidth for the T14 and non-T14 observations, respectively, which are calculated by minimizing the mean squared error (Imbens & Kalyanaraman, 2012) of the regression. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows five and six give the corresponding effective number of observations in the dataset after imposing the bandwidth condition.

Table B.1
Value added residualization coefficient estimates.

	(1) Bar Passage %	(2) Bar Passage %	(3) Employed %	(4) Employed Requiring Bar Passage %	(5) Big Law %	(6) Federal Clerkship %
Median Percentile LSAT	−.0583	−.0499	.169	.0000944	.18	.00595
	(.29)	(.29)	(.285)	(.32)	(.216)	(.059)
Median Percentile Undergraduate GPA	3.01	2.98	5.85*	8.26	5.71	−1.63
	(5.06)	(5.07)	(3.01)	(5.51)	(4.06)	(1.1)
% Female	−.054	−.0438	−.114**	−.00857	.0484	−.00221
	(.1)	(.102)	(.0576)	(.14)	(.0928)	(.0301)
% Hispanic	−.065	−.0653	−.0228	−.026	−.0657	−.00327
	(.0628)	(.0625)	(.0459)	(.0947)	(.0538)	(.0187)
% Asian	.0371	.028	.0265	−.115	.0198	.00505
	(.0802)	(.0813)	(.0554)	(.0923)	(.066)	(.0153)
% Black	.142	.143	−.0213	−.146	−.159*	−.0138
	(.116)	(.116)	(.0663)	(.134)	(.0883)	(.0252)
% Other Ethnicity	−.0421	−.0389	.428**	.512**	.305*	−.0221
	(.209)	(.21)	(.18)	(.217)	(.18)	(.0484)
Observations	1,201	1,201	1,201	1,104	1,201	1,201
R^2	.781	.783	.702	.881	.977	.936
Demographic Variation	Within-School	Within-School	Within-School	Within-School	Within-School	Within-School
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	–	Y	–	–	–	–

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates for the covariates included as controls in the value added residualization regressions from Eq. (1). Standard errors are reported in parenthesis. Observations are weighted by the total number of graduates in each year. Rows four and five beneath the coefficient estimates give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eq. (1).

if they can afford to pay non-T14 graduates a lower starting salary. This would put downward pressure on T14-graduate wages and upward pressure on non-T14-graduate wages. As the general equilibrium effects played out, students would eventually start increasing their demand for highly-ranked non-T14 law schools and decreasing their demand for low-ranked T14 law schools until the differences in tuition roughly reflects the differences in human capital accumulation (and amenities) those schools provide.

While our evidence suggests a large signaling effect, there are other possible explanations for our findings. Bar exam passage may not be a sufficiently rigorous measure to determine differences in human capital at the right tail of the law-student-ability distribution. Because differences in a latent variable that varies somewhat continuously are mapped onto a binary outcome of pass or fail, we may be missing important variation in human capital that would show that differences in value added on human capital do in fact vary discontinuously from the 14th- to 15th-ranked law schools. Further research should investigate the factors, over which schools have control, that can increase schools' abilities to place their students in competitive legal careers.

CRediT authorship contribution statement

Matthew Naven: Conceptualization, Methodology, Software, Formal analysis, Investigation, Writing – original draft, Writing – review & editing, Visualization, Supervision, Project administration. **Daniel Whalen:** Conceptualization, Software, Formal analysis, Investigation, Data curation, Writing – original draft, Visualization.

Appendix A. Data

Table A.1 gives the regression discontinuity results that correspond to the outcomes in Fig. 2.

Appendix B. School value added

B.1. Methodology

Table B.1 gives the coefficient estimates from the value added residualization process described in Section 3.1. To estimate each school's

Table B.2
Value added residualization coefficient estimates.

	(1) Bar Passage %	(2) Bar Passage %	(3) Employed %	(4) Employed Requiring Bar Passage %	(5) Big Law %	(6) Federal Clerkship %
Median Percentile LSAT	.727***	.641***	.27***	1.43***	4.62***	.515***
	(.142)	(.143)	(.0886)	(.242)	(.397)	(.13)
Median Percentile Undergraduate GPA	8.93**	10.2***	7.77***	10	−22.6**	1.95
	(3.8)	(3.63)	(2.67)	(6.76)	(10)	(2.49)
% Female	−.244***	−.12	−.198***	−.57***	.389*	−.0624
	(.0859)	(.0793)	(.0466)	(.117)	(.206)	(.0431)
% Hispanic	−.0604	−.0959	−.000879	.0289	−.194**	.00166
	(.0596)	(.07)	(.0375)	(.0627)	(.0965)	(.0339)
% Asian	−.326***	−.0778	−.142**	−.258**	.463**	−.0116
	(.0855)	(.0613)	(.0623)	(.113)	(.207)	(.043)
% Black	−.0527	−.0589	.0702***	.174***	.747***	.0841***
	(.0655)	(.052)	(.0214)	(.0489)	(.0911)	(.0309)
% Other Ethnicity	−.315	−.128	.309**	.559	−1.08**	.0365
	(.285)	(.209)	(.12)	(.349)	(.445)	(.104)
Observations	1,201	1,201	1,201	1,104	1,201	1,201
R^2	.531	.678	.461	.686	.815	.501
Demographic Variation	Across-School	Across-School	Across-School	Across-School	Across-School	Across-School
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	–	Y	–	–	–	–

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates for the covariates included as controls in the value added residualization regressions from Eq. (1). Standard errors are reported in parenthesis. Observations are weighted by the total number of graduates in each year. Rows four and five beneath the coefficient estimates give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eq. (1).

value added, we first residualize the outcome variables y_{st} with respect to student demographic characteristics X_{st}, as in Eq. (1). The demographic characteristics include median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, and percent other ethnicity. We include a school fixed effect, γ_{st}, in order to estimate the impact of demographic characteristics based off within-school variation due to the fact that students may sort to various qualities of schools in ways that are correlated with demographic characteristics. Somewhat surprisingly, few of the coefficient estimates are statistically significant despite the fact that the independent variables explain a large proportion of the variation in the dependent variables. This is likely due to the fact that the regressions also include a school fixed effect which is the strongest predictor of outcomes.

Table B.2 gives the coefficient estimates from the value added residualization process described in Section 3.1, but differs from Table B.1 in that we *exclude* the school fixed effects, γ_{st}, in order to estimate the impact of demographic characteristics based off *across*-school variation. Our main estimates use *within*-school variation in demographic characteristics due to the fact that students may sort to various qualities of schools in ways that are correlated with demographic characteristics. We see coefficient estimates that are more in line with the expected effect of the independent variables, as LSAT scores and undergraduate GPAs become strongly predictive of Bar passage and labor-market outcomes if we do not account for the fact that students with different LSAT scores and undergraduate GPAs systematically sort to universities of differing quality.

B.2. Results

B.2.1. Bar passage

Fig. B.1 plots the average Bar passage rate against state of largest employment, weighted by the total number of graduates employed in the state of largest employment. States are ranked by the average Bar passage rate for the years in our sample. The plurality of T14 graduates obtain employment in New York, California, Illinois, and the District of Columbia, which are labeled with maroon diamonds in the figure. We find that the Bar passage rates for these states are not systematically different than the Bar passage rates of other states, providing evidence that differences in Bar exam difficulty are unlikely to be driving our results.

Table C.1
Value added estimates.

	Employment Requiring Bar Passage VA		Big Law VA	
	Georgetown	UT Austin	Georgetown	UT Austin
2011	4.21	8.79	40.7	24.8
2012	10	11.8	2.97	36.6
2013	7.37	10.7	60.3	19.7
2014	7.88	10.7	51.2	25.2
2015	7.93	10.8	48	22
2016	8.74	10.3	46.9	21.3
2017	4.44	7.82	53	27.8
2018	7.72	9.89	53	25.4
Total	7.29	10.1	44.5	25.4

Values are the value added estimates in each year. The total average across all years is weighted by the total number of graduates in each year.

Appendix C. Regression discontinuity

C.1. Methodology

Table C.1 gives the value added estimates on employment requiring Bar passage and Big Law employment for the lowest-ranked T14 university, Georgetown University, and University of Texas at Austin, which was ranked 14th in both 2012 and 2018. In all years UT Austin has a higher value added on employment requiring Bar passage than Georgetown, but Georgetown has higher value added on Big Law employment in all years except 2012, which happens to be the year that Georgetown and UT Austin tied for rank 14th.

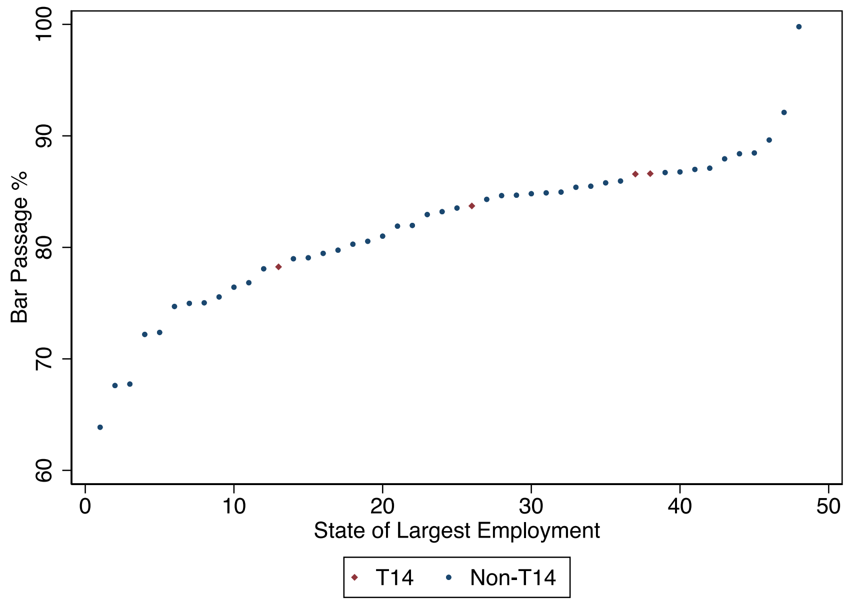


Fig. B.1. Bar Passage by State of Largest Employment.
Fig. B.1 plots the average Bar passage rate against state of largest employment. Observations are weighted by the total number of graduates employed in the state of largest employment. States are ranked by the average Bar passage rate for the years 2011–2019. The plurality of T14 graduates obtain employment in New York, California, Illinois, and the District of Columbia, which are labeled with maroon diamonds in the figure.

Table C.2
Regression discontinuity on law school value added.

	(1) Bar Passage VA	(2) Bar Passage VA	(3) Employment VA	(4) Employment Requiring Bar Passage VA	(5) Big Law VA	(6) Clerkship VA
T14	1.537	0.250	0.091	6.262***	24.977***	1.800
	(1.033)	(0.425)	(0.380)	(1.875)	(1.345)	(2.000)
	[−2.066, 4.415]	[−0.997, 1.445]	[−1.666, 0.453]	[−2.871, 7.769]	[20.519, 28.283]	[−4.712, 5.751]
Outcome Mean	84.5	84.5	92.9	65.4	31.3	3.91
Polynomial Degree	1	1	1	1	1	1
Demographic Variation	Across-School	Across-School	Across-School	Across-School	Across-School	Across-School
Demographic Controls	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Bar State FE	–	Y	–	–	–	–
T14 Bandwidth	13	13	13	13	13	13
Non-T14 Bandwidth	19.9	30.6	27	37	26.4	43.9
Effective T14 Observations	105	105	105	105	105	105
Effective Non-T14 Observations	181	275	250	341	242	385

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This table contains the coefficient estimates on the variable T14 from a fuzzy regression discontinuity of school value added on T14 status, where T14 status is instrumented with the running variable USNWR rank. Eicker–Huber–White HC3 heteroskedasticity-robust standard errors are reported in parenthesis and robust bias-corrected 95% confidence intervals are presented in brackets. Observations are weighted by the total number of graduates in each year. The first row beneath the T14 coefficient estimate gives the mean of the outcome for which value added was estimated. The second row presents the local polynomial degree used in Eqs. (3) and (4). All regressions include the controls median LSAT score, median undergraduate GPA, percent female, percent Hispanic, percent Asian, percent black, percent other ethnicity, and year fixed effects. Rows five and six give the fixed effect controls included in $\boldsymbol{X}_{st}$ in Eqs. (1), (3), and (4). Rows seven and eight give the bandwidth for the T14 and non-T14 observations, respectively, which are calculated by minimizing the mean squared error (Imbens & Kalyanaraman, 2012) of the regression. We select the bandwidth separately on each side of the cutoff to account for the fact that there are 14 schools to the left of the cutoff by construction but many more schools to the right of the cutoff. Rows nine and ten give the corresponding effective number of observations in the dataset after imposing the bandwidth condition. The value added estimates are estimated using the Chetty et al. (2014) value added with drift methodology. Value added estimates used in this table do not include a school fixed effect in the residualization regression from Eq. (1), so the coefficient estimates on the demographic characteristics are identified using *across*-school variation in demographic characteristics instead of *within*-school variation as in our main estimates.

C.2. Results

Table C.2 gives the regression discontinuity coefficient estimates when excluding a school fixed effect from the value added residualization process. These value added estimates correspond to the value added residualization regression presented in appendix Table B.2 and identify the coefficient estimates on the demographic characteristics using across-school variation in demographic characteristics as opposed to within-school variation as used in our main estimates in Table 2 and appendix Table B.1. We find that the coefficient estimates on T14 status for value added on employment requiring Bar passage and Big Law are qualitatively similar to our main estimates, although they are slightly more conservative. Using these alternative value added estimates, we find that T14 status increases the likelihood of obtaining employment requiring Bar passage by 6 percentage points (10%) and increases the likelihood of obtaining employment at a Big Law firm by 25 percentage points (80%).

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