

The Returns to Elite Degrees: The Case of American Lawyers*

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Abstract

We study the market for young attorneys to illuminate the role that elite universities play in human capital development. We find that attorneys who graduate from law schools ranked in the top ten nationally earn considerably more than those without such a qualification, even compared to attorneys who graduate from schools ranked 11-20. The premium to an elite education carries over to an attorney's undergraduate institution as well, and we find that elite bachelors and elite law degrees appear to be close substitutes in terms of their effects on salaries. We compare our findings to the broader literature on the returns to attending a selective college, and find that the elite-law-school premium is more robust to various methods for correcting for selection on ability than the premium to attending a selective undergraduate institution. We discuss several potential reasons for why elite-school premia may exist in this labor market.

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1 Introduction

Competition for seats in elite U.S. graduate school programs has intensified dramatically over the past 40 years. For example, despite the long odds of an 11% acceptance rate, more than 7,000 people paid \$75 each to apply to the Harvard Law School class of 2011. Aspiring lawyers work very hard to get good grades as undergraduates to improve their applications to elite law schools, and a substantial fraction invest in Law School Admission Test (LSAT) preparation classes and materials that typically cost \$1,000 or more and require over 50 hours in classroom time alone.¹

Despite this intense competition, there are at least two reasons to question the existence of a causal effect of elite law schools on career success for attorneys. First, there are many highly successful lawyers from less prestigious schools. Sullivan & Cromwell and Skadden Arps, for example, rank as the third and fourth most prestigious firms (according to Vault.com), and both employ many attorneys who graduated from elite law schools such as Harvard, Yale, and Columbia. However, Sullivan & Cromwell also has at least two associates and at least two partners from each of Brooklyn, Catholic, and Ohio State Law Schools. Skadden Arps has ten associates and three partners from Villanova, eight associates and three partners from the University of Connecticut, and eight associates and nine partners from St. Johns.² Thus, it is possible to reach the pinnacle of this field without attending an elite law school. Second, any association between elite law school attendance and career success could simply be due to selection. There is a large literature suggesting that much of the observed relation between undergraduate school selectivity and labor market outcomes is due to better schools attracting more talented students. While identification of this effect can be a challenge, the causal effect of undergraduate school prestige on wages is not generally considered to be large.

In this paper, we study the market for young attorneys to illuminate the role that elite graduate programs play in human capital development. We motivate our analysis and provide a preview of our main findings in Figure 1. The figure shows a non-parametric kernel density estimate of the annual pay earned in 2002 by attorneys who first passed the bar exam in 2000, where attorneys are placed into four categories based on their educational backgrounds. First, we categorize attorneys

¹A 1989 study of law school applicants (Wightman (1990)) found that about half took an LSAT preparation class. Current LSAT preparation offerings from Kaplan, an industry leader, include classes that range in price from \$1,300 to \$1,500 and involve 51 to 109 classroom hours. The company also offers an intensive summer course with 300+ hours in class at a cost of \$8,000 and private training packages ranging from \$2,300 to \$4,500. A Kaplan online self-study class costs \$1,150.

²This information is derived from lawyer biographies posted on firms' web pages during 2008 and 2009, and is based on the data used in Oyer and Schaefer (2012).

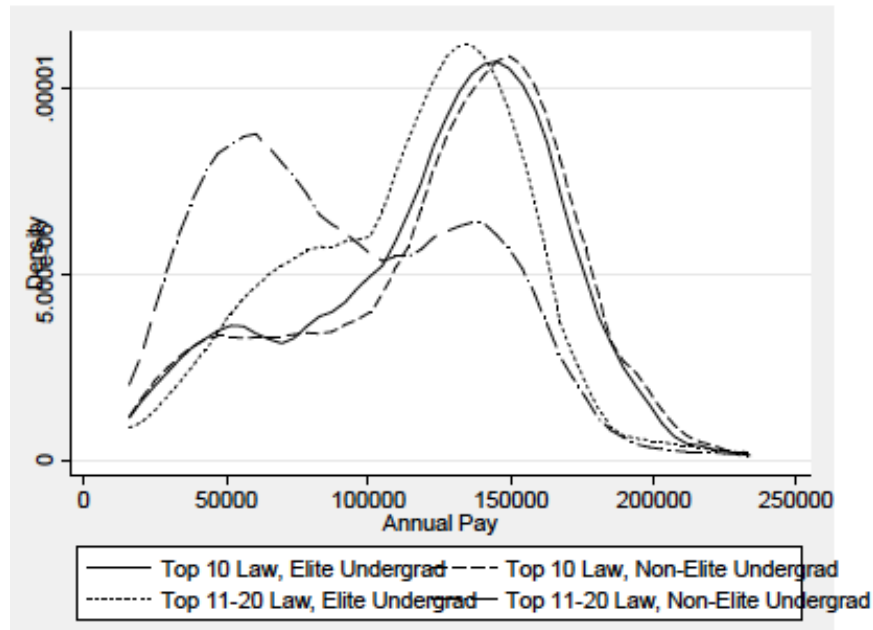


Figure 1: Non-parametric kernel density estimates of the annual pay earned in 2002 by attorneys who first passed the bar exam in 2000. Attorneys are placed into four categories based on their educational backgrounds.

based on the *US News* ranking of the law school they attended. For this figure, we consider only attorneys who attended a law school ranked in the top 20 U.S. Law Schools by *US News*, and split the sample into those who attended a top-10 law school vs. those who attended schools ranked 11-20. We further split the sample based on whether the attorney attended an elite undergraduate institution (where elite is defined as a school that is among the top 25 national universities or liberal arts colleges, again as ranked by *US News*.) The graph depicts the well known bi-modal distribution of young lawyer earnings, with one mode centered around \$140,000 and another near \$70,000.

For three of our four categories of attorneys, the kernel density estimates are strikingly similar. For attorneys who attended a top-10 law school, an elite undergraduate institution, or both, the major mode (that is, the mode with greatest probability density) is the \$140,000 mode. However, for attorneys who did not attend an elite undergraduate institution and attended an 11-20 law school, the major mode is near \$70,000. This pattern is borne out in a cross-tabulation of the average 2002 annual salaries across these two groups, as shown in Table 1. This preliminary evidence is suggestive of two potential findings, which we examine in our analysis. First, it appears that there is, on average, a substantial wage premium associated with attending a top-ten-ranked law school,

	Elite Undergraduate	Other Undergraduate
Top 10 Law School	\$122,200	\$123,800
Law School Rank 11-20	\$113,100	\$91,800

Table 1: Lawyer Pay By School Prestige. Average Salary in 2002 for lawyers who first passed the bar in 2000. Law School rankings based on US News and World Report in 2003. “Elite Undergraduate” schools include 1996 US News Top 25 National Universities and Top 25 Liberal Arts Colleges.

even compared to law schools ranked 11 through 20. Second, it appears that undergraduate and law degrees from elite institutions are close substitutes; the wage impact of attending a top ten law school is negligible for graduates of elite undergraduate institutions.

We examine these patterns in greater detail using a large, representative dataset of lawyers collected by the American Bar Association. Our first main finding is that attending an elite law school is, on average, associated with a large wage premium and a much higher probability of holding a “Big Law” job (which we define as working at a firm with 100 or more lawyers in one of the top four geographic law markets). Graduates of top-10 law schools earn an average of 25% more than graduates of schools ranked eleven through twenty and are far more likely to hold a Big Law position. Graduates of top-10 schools command a wage premium of more than 50% compared to graduates of schools ranked 21 through 100, and are 39 percentage points more likely to hold a Big Law position. These differences are comparable when considering lawyers two years after they pass the bar exam and seven years after, and are robust to conditioning on (most) demographic and background variables.

Our second main finding is that there is one background variable that greatly weakens the premium to attending an elite law school: whether the lawyer attended an elite undergraduate institution. For graduates of elite undergraduate institutions, the wage premium associated with attending a top-10 versus an 11-20 law school is not significantly different from zero, and top-10 law grads are not significantly more likely to hold a prestigious position. For lawyers who did not attend an elite undergraduate institution, the wage premium associated with attending a top-10 versus an 11-20 law school is 31%, and top-10 law grads are more likely to hold a Big Law position. Again these findings are robust to conditioning on available demographic and background information.

We then apply various methods to examine the extent to which the elite law school premium is attributable to selection on ability rather than a causal effect of law school on earnings. As with much of the literature on undergraduate school quality, we lack an ideal experiment in our

data that would allow us to isolate the causal effect. We are, however, able to compare the elite law school premium to the wage premium associated with attending a selective undergraduate institution (estimated using data on lawyers and non-lawyers alike from the National Longitudinal Survey of Youth). Our approach here is to examine how both premia vary as apply various methods of correcting for selection on ability.

Broadly, we find that the elite law school premium is larger and more robust to the inclusion of demographic variables than the selective undergraduate school premium. For example, we show that selection into elite law schools is associated with a variety of demographic and background variables that are also likely to independently affect a lawyer's career success. Controlling for these factors in a wage regression does not, however, affect our estimates of the elite law school premium. Notably, demographic and background characteristics drive selection into selective undergraduate institutions as well, and inclusion of these characteristics in a standard wage regression greatly reduces estimates of the elite undergraduate school wage premium.

These findings are consistent with two possible explanations, which need not be mutually exclusive. First, it is possible that there is a direct causal effect on lawyers' earnings of an elite undergraduate or law degree. Such a causal effect could operate through a variety of channels; perhaps attending an elite school (law or undergraduate) gives young lawyers access to high-value networks that lead to good career opportunities, or that elite schools are particularly good at teaching skills that the legal labor market values. This conclusion, if true, would suggest that prospective attorneys are wise to make substantial investments to improve the odds of being admitted to an elite law school. We present some calculations to this effect, but we caution that the figures do rely considerably on the assumptions one is willing to make about unobservables.

Second, it is possible that the findings are driven by selection on information that is not observable to us; elite schools may simply be good at identifying applicants with unobservable skills that make them successful attorneys. It is notable, however, that elite undergraduate institutions do not appear to be good at selecting on unobservable dimensions of ability for non-lawyers.

These findings and possible explanations raise important questions for future research on the role of elite universities in human capital development. For example, is the large premium to an elite graduate degree unique to law, or is this present for other fields (medicine or economics PhD programs) as well? How general is the finding of substitutability between elite undergraduate and graduate degrees? Finally, what is the precise mechanism leading to the large premium for elite degrees in the law?

Our paper is related to several other literatures in labor economics, the economics of education, and studies of the legal profession. We discuss prior studies on the effects of undergraduate school

quality in detail in Section 2. We are aware of just two other papers that relate individuals' labor market outcomes to graduate school quality. In one of these, Arcidiacono, Cooley, and Hussey (2008) study the effects of getting an MBA on wages. In specifications similar to ours, they find a large premium (20-25%) for going to a Top 25 MBA program relative to other schools but a very small difference between Top 10 and Top 11-25 schools. The premiums that they find for Top 25 programs are cut roughly in half when they control for individual fixed effects using pre-MBA salary. We cannot use a similar strategy because we do not have pre-law salary data for our sample and because, unlike MBAs, many lawyers have limited or no work experience before law school. Most of our sample went straight from undergraduate school to law school or waited just one year in between. Another paper in this stream is the highly controversial study of affirmative action "mismatch" in law schools by Sander (2004). In fact, Sander (2004) uses the same dataset that we use and runs similar regressions. He also finds a substantial premium to attending a top law school, controlling for other factors. However, this is not the focus of his analysis and he makes no inquiry into the causal effect of law school quality on career success. He further controls for several variables in his analysis that would be inappropriate to include as explanatory variables in our analysis (such as the geographic area where the lawyer works and the average LSAT of the law school).

There is a substantial literature on the economics of the legal profession. Galanter and Palay (1991) and Galanter and Henderson (2008) offer background on the traditional partner track at large and prestigious law firms. Ehrenberg (1989) looks at the relationship between pay and law school prestige, but his analysis is at the school level and makes no attempt to separate selection and value-added. Spurr (1987) shows that lawyers from better law schools work for more prestigious firms, on average, and handle legal issues with larger stakes. Rosen (1992) describes many facets of the lawyer labor market, including determinants of pay, variation in pay, and growth in the overall market. Henderson and Morriss (2006) analyze law schools' attempts to appear prestigious and how students respond to these in terms of attendance choices.

In the next section, we provide background on the challenges associated with measuring the causal effect of school quality on labor market outcomes, discuss and how the literature on the returns to college selectivity have addressed this. Section 3 describes the data we use and then presents our empirical analysis. We interpret the implications of our results for aspiring lawyers and assess possible sources of the law school prestige premium in Section 4. Section 5 concludes.

2 Background on Returns to Selective Schools

As has been widely studied in the undergraduate context (see below for details), measuring the causal effect of school reputation on labor market outcomes is difficult when unobservable factors such as intelligence and parental investments affect both the school someone attends and her eventual productivity in the workplace. Suppose that person i 's productivity (and, in equilibrium, her pay) is

$$y_i = \alpha_i + \beta x_i + \delta c_i + \varepsilon_i \quad (1)$$

where y is output or pay, x is a set of control variables such as age and family background, c is a measure of the reputation of the school she attended, α is person-specific ability, and ε is a random shock to productivity or to the measurement of productivity. If c were determined randomly conditional on x , traditional wage regressions would provide unbiased estimates of β_2 , the causal effect of college reputation on income. However, a more reasonable model would suggest that

$$c_i = \theta_i + \gamma_1 x_i + \gamma_2 z_i + \eta_i. \quad (2)$$

That is, the college the person chooses is likely to be a function of her taste for particular types of schools (θ), the characteristics that affect her productivity (x), and other characteristics that are observed by school admission officers but not by employers (z). The fact that the college choice is determined endogenously would not cause any problems in interpreting wage regressions using the specification in equation (1) if α were independent of c , controlling for the variables in the vector x . This condition seems unlikely to be satisfied, though. For example, if person i has a positive work ethic, this is likely to affect productivity through α and make the person's school admission application more attractive through z . In this case, a wage regression that did not have individual fixed effects would attribute some of the effects of α to c through an upwardly biased δ .

Table 2 summarizes several papers that, in the context of undergraduate institution prestige, have taken different approaches to solving the selection issue. That is, different researchers have chosen different methods to get an unbiased estimate of β_2 . Behrman, Rosenzweig, and Taubman (1996), who look only at female twins born in Minnesota between 1936 and 1955, use the common background of twins to separate innate ability from the effects of schooling. They find that, at least for this group, there is a substantial wage premium associated with attending an undergraduate school that grants PhDs, small private colleges, and higher faculty salaries. The magnitude of their estimates is quite large, as they suggest that if a given person receives her undergraduate degree from Wellesley College or the University of Pennsylvania instead of Mankato State University in

Paper	Comparison	Result
Behrman, Rosenzweig, and Taubman (1996)	Female twin pairs.	Attending Private and PhD-granting universities leads to 10-25% higher earnings.
Brewer, Eide, and Ehrenberg (1999)	Model college selection.	Attending elite schools increase earnings up to 40% relative to low-ranked public schools.
Dale and Krueger (2002)	Uses students admitted to same schools but attending different ones.	Little or no effect of school SAT scores, but higher tuition leads to higher earnings.
Black and Smith (2004)	Propensity score matching.	Attending a top quartile school increases earnings by up to 15% relative to a bottom quartile school.
Hoekstra (2009)	RDD between state university campuses.	Those attending campus with +75 SAT points earn 20% more.

Table 2: Previous Findings on the Returns to Attending a Selective College

Minnesota, she can expect approximately a 20% or 36% wage premium, respectively. Brewer, Eide, and Ehrenberg (1999) use a more representative sample and take a more structural approach by specifying a model for selection of college and subsequent earnings. They identify the causal effect of college quality on wages by instrumenting for college choice through the costs of the school attended and through the functional form of the school choice and wage equations. They find results generally in line with those in Behrman, Rosenzweig, and Taubman (1996). However, the results in Brewer, Eide, and Ehrenberg (1999) are somewhat problematic because, unlike other research in this area and counter to most researchers' intuition, they find that selection correction is not important in measuring the effect of college quality.

Dale and Krueger (2002) and Black and Smith (2004) find much smaller effects of college reputation on earnings. Dale and Krueger (2002) identify the effects of college reputation by comparing earnings of people that were accepted to similar colleges but made different choices

about which one to attend. They find essentially no effect of college prestige on earnings. Black and Smith (2004) use propensity score matching techniques to control for school selection. They find that, in most specifications and most subgroups, selection is important. Their estimated causal wage premiums are generally not large, with a maximum of about 15% for a student that attends a top quartile school relative to if she attended a bottom quartile school. Finally, Hoekstra (2009) uses a regression discontinuity approach by comparing students near the margin for getting into the top state university campus in the state. He finds that getting into this campus, where the average SAT score is 65-90 points higher than the other campuses, leads to a zero to twenty percent increase in earnings at ages 28-33.³

The variety in the estimated effects of college quality suggests that this effect can be quite heterogeneous and/or that it is difficult to specify the proper selection correction to separate the selection and value-added effects of school quality measures on earnings. But we generally read the results as suggesting that selection is an important component in the correlation between undergraduate school quality and labor market outcomes and think the estimates of the causal effect of college quality are generally small on the margins that most students consider. We suspect that few students that attend Wellesley College seriously consider Mankato State University, for example, or that many students that end up at top schools were ever seriously at risk for attending a school in the bottom quartile.

Below, we produce our own (fairly small) estimates of the causal effect of undergraduate college quality on earnings. But the real innovation in our work is to look at the effect of law school quality so our undergraduate estimates are primarily for comparison and the details are relegated to appendix tables.

3 Empirical Analysis

3.1 Data

Our lawyer data comes from the first two waves of the “After the JD” (AJD) survey, collected in May 2002 and 2007. The surveys were conducted primarily by mail and phone (with a few responses done over the internet) and had a 70% response rate. We used only responses from the mail and internet surveys, however, as the phone survey did not gather the background characteristics that are critical to our analysis. We dropped people who were 40 or older when they first passed the bar

³Studying Colombian students and workers, Saavedra (2008) also uses a regression discontinuity approach. He finds the highest returns to college quality that we know of, indicating the returns may be higher outside the United States.

and anyone who failed to report her law school, age, gender, whether her mother was born in the United States, how she paid for law school, and whether she lived near her mother at the time of the survey. We also limit our analysis to graduates of top 100 law schools (defined below) for two reasons. First, the AJD sample imposes some potential selection bias into our analysis because it only included law graduates that pass a bar exam. This problem is less severe when focusing on highly ranked schools, since bar passage rates for these schools are high. Second, we believe the labor market for lawyers from low-ranked schools is markedly different from that for graduates of more prestigious schools.

Our wage regressions contain 1,425 Wave 1 and 1,646 Wave 2 respondents. Some lawyers reported where they work but not their wage, so our sample size is slightly larger (1,531 in Wave 1) when we analyze a lawyer’s likelihood of holding a prestigious job. We do not have geographic information for Wave 2, and therefore confine our prestigious job analysis to Wave 1.

We measure the quality of the lawyers’ law and undergraduate schools using *US News and World Report* rankings. The AJD data place each lawyer in one of six law-school categories varying from Top 10 to Unaccredited. We use the top three categories: Top 10, other Top 20, and other Top 100.⁴ For undergraduate schools, we use twenty-four categories based on the *US News* 1996 rankings. We created an indicator variable, “Top Undergraduate,” which equals one for lawyers whose undergraduate institution ranks as one of the top 25 “National Universities” or top 25 “Liberal Arts Colleges.”⁵

Panel A of Table 3 provides summary statistics on the Wave 1 sample. The average age (both for the full sample and for the various law-school-category subsamples) is around 30, and about half of respondents are women. Those attending top-ten schools appear to come from more privileged backgrounds, as their friends and family paid for a higher fraction of their law school expenses and they are more likely to have mothers who continued education after high school. Those attending more selective law schools are, not surprisingly, more likely to have had undergraduate grade point averages above 3.5 and are more likely to have graduated from an elite undergraduate institution. Panel B provides similar information for Wave 2 respondents. These respondents are about five years older and earn significantly more money than Wave 1 respondents. We do not

⁴Because of a tie for number ten, the Top 10 includes the following 11 schools (in order of rank): Yale, Stanford, Harvard, Columbia, NYU, Chicago, Pennsylvania and Michigan (tied), Virginia, and Cornell and Berkeley (tied). “Top 11-20” includes the following schools (ranked 12-20): Duke and Northwestern (tied), Georgetown, Texas, UCLA, Vanderbilt, USC, and Minnesota and Washington and Lee (tied).

⁵We use 1996 rankings because that is the modal undergraduate completion year for our AJD survey respondents. We also created an alternative categorization of undergraduate school quality based on average standardized test scores. This led to similar insights, so we use the *US News* variable throughout the paper.

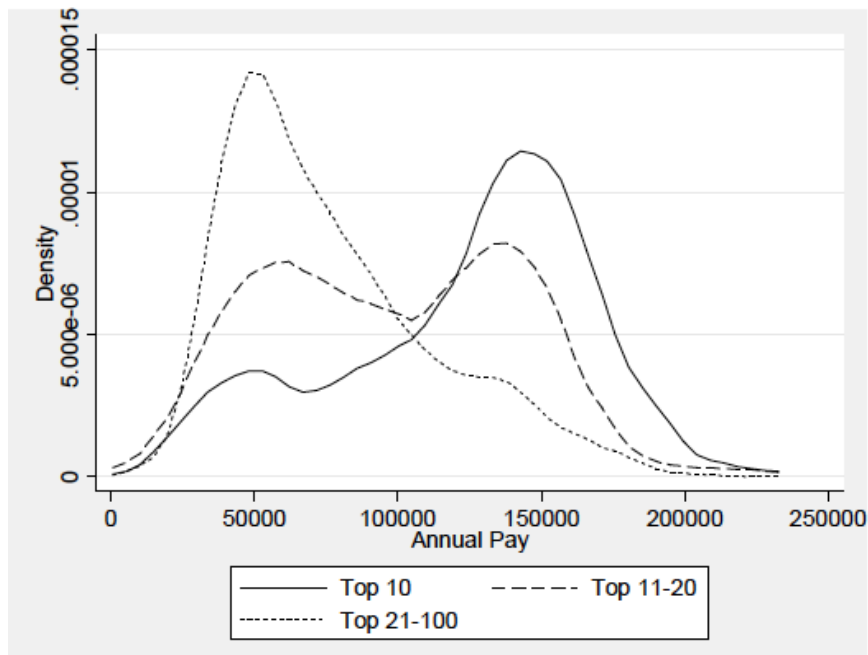


Figure 2: Non-parametric kernel density estimates of the annual pay earned in 2002 by attorneys who first passed the bar exam in 2000. Attorneys are placed into three categories based on law school ranking.

have information on Wave 2 undergraduate GPA and the second wave survey asked about total debt rather than the source of funds for law school.

Our analysis below will focus on two dependent variables, the first of which is the log of an attorney’s annual earnings. Panel A of Table 3 shows that those attorneys who attend a Top 10 schools earn around 40% more than the sample as a whole and 25% more than those going to Top 11-20 schools. Figure 2 displays kernel density estimates of Wave 1 pay differences between Top 10, Top 11-20, and Top 21-100 graduates. The graph shows the well-known bi-modal nature of young lawyer earnings (see discussion of this on www.abovethelaw.com and www.elsblog.org) and large differences in what fraction are near the upper mode by law school tier. Figure 3, which shows the same graph for Wave 2, illustrates that while the bi-modal distribution of earnings is gone, the substantial differences by law-school category are not.

Our second dependent variable is an indicator that takes the value one if the person works at a “Big Law job.” We define this to be employment at a private law firm with more than 100 lawyers and in one of the top four legal markets (New York, Washington DC, Chicago, and Los Angeles). Panel A of Table 3 also shows that Top 10 graduates are twenty-one percentage points more likely

Panel A: AJD Lawyers in 2002 (Wave 1)			
	All	Top 10 Law School	11-20 Law School
Female	0.4996	0.5174	0.51111
Age	31.183 (3.336)	29.542 (2.789)	29.719 (2.960)
% from Fam/Friends	0.183 (0.305)	0.270 (0.365)	0.183 (0.305)
Mother > HS Educ	0.7614	0.8259	0.8000
Live near Mother	0.3579	0.3184	0.3630
Undergrad GPA>3.5	0.5193	0.8101	0.5630
Top Undergrad	0.2400	0.5771	0.3074
Annual Pay	\$88.4K (46.2K)	\$122.9K (44.0K)	\$98.4K (43.6K)
Large Firm/Big Mkt	0.1818	0.4577	0.2704
N	1,531	201	270
Panel B: Lawyers in 2007 (Wave 2)			
	All	Top 10 Law School	11-20 Law School
Female	0.4708	0.4419	0.4831
Age	35.072 (3.268)	34.577 (2.606)	34.704 (2.944)
Debt at L.S. Graduation	\$65.2K (40.5K)	\$76.8K (47.2K)	\$66.6K (39.9K)
Mother > HS Educ	0.7526	0.7778	0.7572
Live near Mother	0.3663	0.3140	0.3345
Top Undergrad	0.2503	0.5543	0.2939
Annual Pay	\$110.6K (55.7K)	\$137.8K (61.6K)	\$116.1K (58.5K)
N	1,646	258	296

Table 3: Summary Statistics

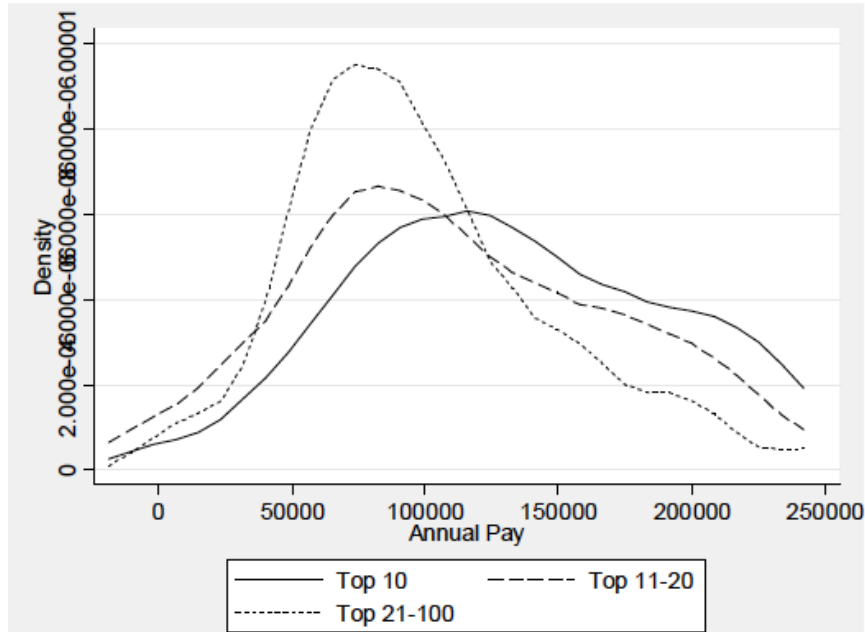


Figure 3: Non-parametric kernel density estimates of the annual pay earned in 2002 by attorneys who first passed the bar exam in 2000. Attorneys are placed into three categories based on law school ranking.

to hold a Big Law job compared to an 11-20 graduate.

For comparison purposes, we gathered data from two sources on earnings and undergraduate school quality from a broad samples of college graduates (which presumably include a few lawyers but many non-lawyers as well). We present results using the 1990 wave of the National Longitudinal Survey of Youth (NLSY). We divided the colleges attended into quintiles such that the top and second group are similar proportions of the NLSY sample as the Top 10 and Top 20 groups are of the AJD sample. We also performed analyses using the Baccalaureate and Beyond (B&B) survey. This survey has both advantages and disadvantages relative to the NLSY for our purposes and all our analysis led to similar conclusions to those we draw using the NLSY. More detail on both the NLSY and B&B can be found in the appendix, which is also where we present details of our NLSY analysis. In the main body of the paper, we briefly discuss how the NLSY results compare to the AJD results, as well as the implications of those comparisons. Appendix Table 1 displays summary information for the NLSY comparable to the AJD summary in Table 3. As with the lawyers, the NLSY respondents from better schools come from families with more education and they are more likely to live somewhere different from where they grew up. Again, those going to better schools both make more after school and show more skill before school (as measured by SAT scores), so

it is not entirely clear whether the school quality/wage correlation is due to selection or a causal effect of school quality on earnings.

Table 4 shows the importance of considering selection issues through analyses where measures of school quality are the dependent variables. Panels A and B display results for Waves 1 and 2, respectively. The first two columns of each panel show the results of probits where the dependent variable equals one if the person went to a Top 10 law school. Column 1 uses the whole Top 100 law schools sample while Column 2 is limited to lawyers from Top 20 schools. The results show that selection may be very important. For example, lawyers with at least one parent that graduated from college have a 3 percentage point higher probability of going to a Top 10 school when looking at the whole sample (Column 1). Having an undergraduate GPA above 3.5 also has a highly significant (statistically and economically) effect on whether the person attends a Top 10 law school. Most dramatically, graduating from an elite undergraduate institution is associated with at least a 27 percentage point higher probability of attending a top law school. The third column shows a regression where the dependent variable is 1 if the person went to a *US News* Top 100 school, 2 if she went to a Top 20 school, and 3 if she went to a Top 10 school. Having a parent that graduated from college is associated with going to a law school that is 0.1 levels higher on this scale and graduating from an elite undergraduate school is associated with more than half a level higher law school. Holding other factors constant, minorities attend higher ranked schools, which could be the result of affirmative action. Reassuringly, given that all the variables in the table do not change once the person goes to law school, the Panel B results for Wave 2 are very similar. The table highlights the potential importance of selection into a top law school and points out the particularly important role of undergraduate institutions. When we run regressions similar to those in Table 4 without controlling for undergraduate school, the coefficients and significance on the other background variables increase substantially indicating (not surprisingly) that the same factors affect selection into undergraduate and law schools.

Appendix table 2 shows similar analyses for undergraduate schools using the NLSY sample. Going to a better undergraduate school is strongly associated with such factors as mother's education, high school rank, and SAT scores. This initial look at who attends top undergraduate and law schools indicates that the potential selection problems are similar in these two distinct environments.

3.2 OLS Regressions of Income

We begin by estimating equation (1) for the lawyers in the Wave 1 AJD sample who attended a top 100 school. This includes 1,425 that first passed the bar exam in 2000 and that were under 40 years

Panel A: AJD Lawyers in 2002			
	Top 10 Probit	Top 10 Probit	OLS: <i>US News</i> category
Female	-0.020 (0.014)	-0.057 (0.048)	0.051 (0.033)
Minority	0.063 (0.018)	0.110 (0.051)	-0.162 (0.036)
Parent with College Degree	0.032 (0.015)	0.062 (0.059)	-0.100 (0.038)
Undergrad GPA>3.5	0.184 (0.016)	0.351 (0.044)	-0.424 (0.033)
Undergrad Science/Business Major	0.007 (0.014)	0.052 (0.048)	0.012 (0.033)
Top Undergrad	0.277 (0.027)	0.332 (0.046)	-0.607 (0.039)
N	1,531	497	1,531

Panel B: AJD Lawyers in 2007			
	Top Tier Probit	Top Tier Probit	OLS: Tier
Female	-0.027 (0.017)	-0.060 (0.044)	-0.096 (0.035)
Minority	0.038 (0.019)	0.046 (0.046)	-0.096 (0.038)
Parent with College Degree	0.012 (0.018)	0.004 (0.045)	-0.033 (0.036)
Undergrad Science/Business Major	0.016 (0.018)	0.024 (0.045)	-0.030 (0.036)
Top Undergrad	0.249 (0.025)	0.268 (0.042)	-0.540 (0.041)
N	1,646	554	1,646

Table 4: Selection into Top Schools. Panel A uses first wave of AJD and Panel B uses second wave of AJD. Columns 1 and 2 are probits where the dependent variable is an indicator variable for graduating from a Top 10 law school. The sample in column 2 is limited to those who went to Top 20 schools. Column 3 shows results from an OLS regression where the dependent variable is 1 if the person went to a top 10 school, 2 if he/she went to a school ranked 11-20, and 3 if he/she went to a school ranked 21-100. Coefficients displayed in logits are marginal effect of a one unit change in the explanatory variable.

old at the time. The dependent variable is the log of the lawyer's salary in 2002.⁶ It is common to use the log of a person's hourly wage as the dependent variable in wage regressions such as these, but about a fifth of AJD respondents did not provide hours. Our results are similar, but a bit less precise, if we use hourly wages. Regression results are reported in the first four columns of Table 5.

Column 1 of Panel A reports results with no control variables, so it provides an indication of the average differences in lawyer pay across six levels of *US News and World Report* school rankings. The omitted category in each regression is schools ranked 11-20, so the other ranks are relative to this group. Column 1 makes it clear that there are very substantial differences in pay based on where lawyers went to school. Lawyers in schools ranked 11-20 earn approximately 25% less, on average than those in Top 10 schools. Those in schools ranked 21-100 earn another 23% or so less. Lawyers from Top 10 schools average pay of almost \$123K, while those from Top 11-20 schools earn about \$98K. We know, therefore, that there is a large wage premium associated with going to a higher ranked law school.

Column 2 adds controls for gender, marital status, age (indicators for 25-29, 30-34, etc.), and race (indicators for Black, Hispanic, Native American, Asian, and Other). Some of these control variables are important and they add considerable explanatory power to the regression, as measured by the R^2 . Women in the sample earn approximately 12% less than men (though this difference disappears when we look at hourly pay.) However, adding these controls does not have any effect on the relationship between law school rank and pay.

The specification in Column 3 adds several controls for family background and the way the lawyer paid for law school, including whether the lawyer lives near her mother, whether her mother was born in the United States, mother's education, father's education, whether any of her parents or grandparents are lawyers, and the fraction of law school expenses paid through savings and by parents. These variables add some explanatory power and some are significant predictors of lawyer income. However, once again the additional controls have no effect on the law school prestige relationship with pay.

Finally, Column 4 includes our fullest set of controls where we try to capture ability through measures of prior academic success and the cost of law school. Added control variables now include indicator variables for 24 categories of undergraduate school quality, undergraduate GPA (indicators for 3.75-4, 3.5-3.74, etc.), a full set of interactions between these undergraduate quality and GPA variables, an indicator variable for being in the top 10% of her undergraduate class, undergraduate major (indicators for science, business, social science, humanities, and other/missing), whether the

⁶The question in the AJD survey is "What is your total annual salary (before taxes) including estimated bonus, if applicable, at your current job?"

	(1)	(2)	(3)	(4)	(5)
Panel A					
Top 10	0.250 (0.046)	0.255 (0.045)	0.241 (0.046)	0.158 (0.049)	0.120 (0.065)
Rank 21-100	-0.227 (0.038)	-0.228 (0.038)	-0.228 (0.039)	-0.173 (0.046)	-0.077 (0.059)
Female		-0.115 (0.027)	-0.118 (0.027)	-0.117 (0.028)	-0.342 (0.037)
Near Mother			-0.034 (0.027)	-0.027 (0.028)	0.013 (0.037)
Undergrad Top 10%				0.083 (0.042)	
R-square	0.110	0.132	0.146	0.287	0.213
Panel B					
Top 10	0.314 (0.060)	0.312 (0.061)	0.304 (0.062)	0.234 (0.062)	0.213 (0.0791)
Elite Undergrad	0.210 (0.036)	0.211 (0.036)	0.208 (0.037)	0.224 (0.038)	0.1095 (0.0490)
Top 10 * Elite Undergrad	-0.209 (0.078)	-0.196 (0.078)	-0.196 (0.078)	-0.159 (0.078)	-0.138 (0.100)
R-square	0.131	0.152	0.165	0.207	0.123
Controls					
Demographic	no	yes	yes	yes	yes
Family Background	no	no	yes	yes	yes
School Funding	no	no	yes	yes	yes
Academic History	no	no	no	yes	yes
AJD Sample	wave 1	wave 1	wave 1	wave 1	wave 2
N	1,425	1,425	1,425	1,425	1,558

Table 5: Lawyer Pay Regressions. OLS – Dependent Variable is Log of annual pay. Sample for columns 1-4 is cross-sectional AJD sample in 2002 of lawyers who first passed Bar Exam in 2000. “Top 10” and “Rank 21-100” are based on 2003 US News and World Report rankings. The excluded category is schools ranked between 11 and 20. “Elite Undergrad” indicates the lawyer graduated from an undergraduate school ranked as a Top 50 National University or Liberal Arts College using 1996 US News rankings. The specifications in the two panels are identical except for the inclusion of Elite Undergrad and its interaction with Top 10 law school in Panel B and that Panel B column 4 does not control for all the categories of undergraduate college quality (and their interaction with GPA.)

person went to a public law school, and an indicator variable for other graduate degrees. The additional control variables make much more of a difference now and, specifically, this is driven by the undergraduate quality measures. As the table shows, lawyers in the top decile of their undergraduate class earn 8% more, on average, than other lawyers. Looking at the college quality indicators carefully shows that, with all the Column 4 controls included, lawyers that went to a Top Undergraduate school (as defined above) earn 20% more than those that went to other schools. So this regression indicates that Top 10 law school graduates earn about 16% more than Top 11-20 graduates *and* that lawyers that went to elite undergraduate schools earn an additional 20% regardless of their law school. Thus, the regression in Column 4 leads to two conclusions. First, there is still a substantial premium for going to a top law school, even including all the controls we can. Second, the fact that undergraduate school matters so much means that selection on unobservables is important for lawyers. *If* all this selection is captured by the undergraduate school categories, then Column 4 captures the causal effect of going to a top law school. But that seems unlikely to be an entirely valid assumption and we will use other methods to explore the role of unobservables.

The fifth column performs an analysis similar to the one Column 4, using the Wave 2 sample of lawyers (that is, those who had passed the bar seven years before the time of the survey). Column 5 does not include controls for undergraduate GPA (which we do not have for Wave 2) or undergrad GPA/undergraduate school quality interactions, but is otherwise the same as Column 4. The results are quite similar to those for Wave 1 in terms of the magnitude of the law school quality coefficients. Note that the female wage discount is higher, as many women have pulled back on their hours. Also note that the R^2 is much lower (by much more than the difference in explanatory variables would justify), which is typical as workers age and pay becomes more a function of individual ability than of observed proxies (Altonji and Pierret (2001).)

Panel B repeats the analysis in Panel A of Table 5, but shows the association between going to a top undergraduate school and lawyer pay. Going to a top undergraduate school leads to about 20% higher pay in each specification and Top 10 law school graduates continue to show a large premium relative to the next tier. However, the large negative interaction effect indicates that there is a large premium to going to a top undergraduate school OR to a top 10 law school, but the combination of the two adds little. The message is similar, though the coefficients are a bit smaller and less precise, when looking at Wave 2.

Table 6 limits the analysis to lawyers from Top 20 schools in order to focus on a somewhat more homogeneous sample. If the effects of some of the control variables are much different for lawyers that go to lower tier schools than for those that go to top schools, for example, then the

restriction imposed in Table 5 (that the control variables have linear and constant effects for the all Top 100 graduates) would limit the ability of these variables to properly control for factors that could affect our school selectivity estimates. This does not appear to be a major concern, however, as the results for the differences between lawyers from Top 10 schools and those ranked 11-20 in Table 6 are almost identical to those in Table 5. Also, comparing the two panels of the table again shows the results are similar for the two different waves of AJD respondents.

Figure 4 graphically represents the relation between income and class rank in law school. Class rank is self-reported, and may be subject to considerable recall bias.⁷ Due to these data problems, we do not use class rank in our formal analysis, but two patterns in the graph are instructive nonetheless. First, pay shows no evidence of declining with law school class rank at Top 10 schools but does drop off appreciably at the other levels. Second, pay at Top 10 schools is higher at all but the highest class ranks (as well as for the large group of Top 10 graduates that did not provide a class rank) than pay at any class rank of any other school.⁸ While certainly not proof of the causal effect of going to a top school, the figure is consistent with the hypothesis that top students at schools outside the Top 10 could expect to earn more if they went to a Top 10 school, even if they were further down in the quality distribution at that school.

For comparison purposes, Appendix Table 3 shows the results of similar regressions on the NLSY cross-section in 1990. To make the sample comparable to our lawyer sample, we include only people with at least two years of college. The results show that college quality does not appear to be an important determinant of pay in this sample. The average person in the highest tier of colleges earns an average of 6% more than a person in the second tier. But this effect is not statistically significant. The top tier premium grows as college quality drops but the difference only becomes statistically and economically significant when reaching schools with average SAT scores below 840. When all the controls are included, which makes the NLSY specification comparable to the full set of controls used in Column 4 of Table 5 for lawyers, the school quality/income relationship is very small and the controls have a noticeably larger effect on the undergraduate school quality coefficients than they have on the law school quality coefficients.

The regressions so far provide at least circumstantial evidence consistent with law school quality

⁷In our sample, 25% of AJD respondents did not provide a class rank and those who did seem to have been, on average, remembering their law school grades somewhat favorably. 15% of the AJD sample (and 20% of those who answered the class rank question) said they were in the top 10% of their class and 38% (51% of those who answered the question) said they were in the top quartile.

⁸The slightly lower pay for top decile students at Top 10 schools is based on a small sample and is not significantly smaller than other Top 10 students.

Panel A					
	(1)	(2)	(3)	(4)	(5)
Top 10	0.250	0.251	0.227	0.166	0.114
	(0.048)	(0.048)	(0.049)	(0.058)	(0.090)
Female		-0.182	-0.192	-0.209	-0.325
		(0.049)	(0.050)	(0.053)	(0.076)
Near Mother			-0.080	-0.078	-0.025
			(0.052)	(0.055)	(0.082)
Undergrad Top 10%				0.038	
				(0.074)	
R-Square	0.055	0.088	0.131	0.370	0.220
Panel B					
Top 10	0.328	0.326	0.314	0.278	0.282
	(0.066)	(0.066)	(0.068)	(0.071)	(0.097)
Elite Undergrad	0.257	0.270	0.298	0.308	0.247
	(0.067)	(0.066)	(0.068)	(0.070)	(0.108)
Top 10 *	-0.255	-0.255	-0.278	-0.234	-0.268
Elite Undergrad	(0.098)	(0.099)	(0.100)	(0.100)	(0.145)
R-Square	0.084	0.120	0.168	0.234	0.165
AJD Sample	wave 1	wave 1	wave 1	wave 1	wave 2
N	471	471	471	471	523

Table 6: Top School Lawyer Pay Regressions. Same analysis as Table 4, except limited to graduates of top 20 law schools. Column 2 includes demographic controls. Column 3 adds family background and school funding controls. Column 4 adds academic history controls.

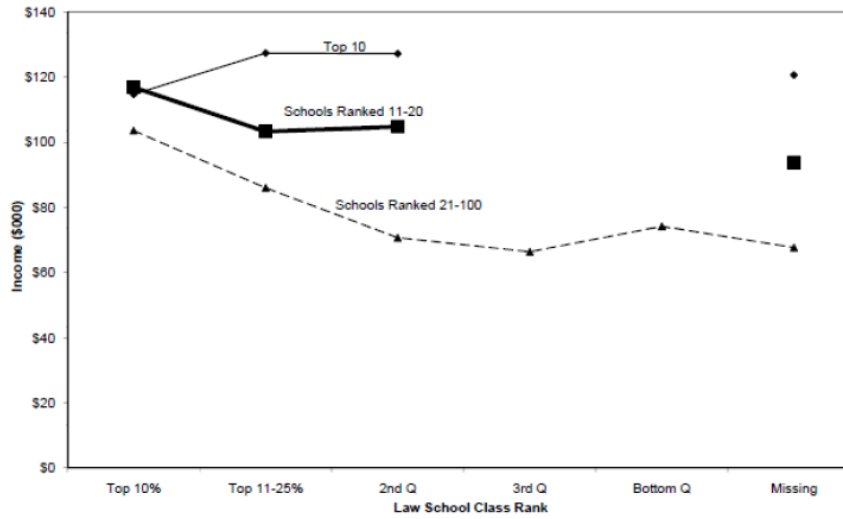


Figure 4: Income and class rank by law school ranking.

having a larger effect on lawyer income than undergraduate school quality has on income. Further, they suggest that selection is a larger component of the undergraduate selectivity effect than of the law school ranking effect. The evidence is consistent with law school quality having a substantial causal effect on lawyer income and a bigger effect than undergraduate quality has on income. However, we did find that undergraduate school quality is related to lawyers' incomes in a way that substantially lowers the implied effect of law school quality.

Our analysis thus far is limited by the fact that the AJD covers only attorneys with seven years or less of experience and by the lack of information about LSATs.⁹ We can partially address these issues by examining data from other sources. We first examine whether the relation (whether causal or not) between law school quality and career success continues as lawyers gain experience beyond the level captured by the AJD second wave. One indication that this relationship is long-term is that lawyers from top law schools are highly over-represented in the partnership of top law firms. The data used in Oyer and Schaefer (2012) include background information for the partners of 285 of the 300 largest law firms in the U.S. Using this data and data on the number of people that graduated from each U.S. law school, we calculated that, as of the Summer of 2007, 13.4% of graduates of Top 10 law schools between 1970 and 2005 were partners at one of these 285 firms. 8.9% of graduates of Top 11-20 schools and 3.5% of graduates of other Top 100 schools were

⁹Note that, while the creators of the AJD have a measure of LSAT success, they have chosen not to make this variable available to insure confidentiality and because they do not believe it is reliable for inference in this sample.

partners at these firms.

The 1994-1995 Chicago Lawyers Survey provides another dataset we can use to examine how the school-quality/career-success relation changes as lawyers gain experience. This survey of lawyers based in Chicago has some important limitations for our purposes. To the extent that part of the effect of law school quality is sorting lawyers into more productive locations, that will be lost when looking at lawyers in a single location. Also, the survey gathered significant demographic data, but did not ask for undergraduate GPA or major. The survey does include the name of the person's undergraduate school, so we can control for quality of undergraduate school. Using the 848 lawyers in this sample for which we have sufficient data, we ran regressions of log annual pay on the same law school quality variables we used in Table 5. One specification includes no controls, another controls for years since law school graduation (linear and squared), indicators for female, minority, married, mother was/is a professional, father was/is a professional, either parent was/is a lawyer, grew up in Chicago, and grew up in Illinois, while the third controls for all those variables plus indicators for undergraduate school average SAT score (in 100 point increments). The results of all these specifications are similar (though somewhat less precisely estimated) to those in Table 5. The Top 10 premium relative to Top 11-20 is approximately 20%.

We used another dataset, The University of Michigan Law School Alumni Survey, to try to more directly assess the importance of not being able to control for LSAT scores.¹⁰ This comes at the rather steep cost of taking away our variable of interest, as there is no variation in law school quality among this group. We used data from surveys done five years after graduation for the classes of 1991-2000 and ran regressions of log annual pay (which is measured four years after graduation) on undergraduate GPA, LSAT percentile, fraction of law school paid for by family and savings, and indicators for female, minority, married, each graduating class, 5-year age groupings, whether the person lives in the same state as his/her parents, parent occupations (lawyer, professional, business owner), non-law graduate school, four undergraduate major categories, University of Michigan undergraduate degree, and Ivy League/Seven Sisters undergraduate degree. Within this one school, very little of the variation in wages can be explained — even with all these controls, the R^2 of the regression is only 0.0677. The noteworthy finding for our purposes is that LSAT score is not significantly related to pay in this regression. Also, whether LSAT score, undergraduate school, and undergraduate GPA are included or not has no effect on the other variables in the regression. Of course, whether including these variables would have an effect on a school quality measure is not known and it seems plausible that, to the extent LSAT scores predict earnings, they do so across

¹⁰For details on the data and examples of other studies that use it, see Lempert, Chambers, and Adams (2000) and Sauer (1998).

rather than within law schools. However, these results provide at least a bit of evidence that LSAT scores do not explain lawyer pay accurately and to the exclusion of other variables.

3.3 Job Quality Probits

It seems unlikely that any given employer of lawyers would vary pay for new lawyers based on where they went to school, so we expect the return to selective law schools to operate largely by sorting lawyers from more selective schools into higher paying firms. We now look for evidence of this sorting by analyzing the relationship between law school prestige and the propensity of lawyers to work in the highest-paying segment of the law sector. Private law firms are the highest paying jobs, on average, for new lawyers, with compensation considerably greater than pay in the public sector, as in-house counsel, or other jobs law graduates take.¹¹ Opportunity to work on the most interesting and lucrative work is generally considered to be greatest in the largest legal markets (see Garicano and Hubbard (2009)), so we focus on the four largest legal markets — New York, Washington, Los Angeles, and Chicago.

Table 7 contains the results of probit regressions where the dependent variable equals one if the AJD respondent works at a private law firm with at least 100 lawyers in one of the four largest legal markets.¹² Note that all our analysis in this section is limited to Wave 1 of the AJD because we do not know where the Wave 2 lawyers live. The coefficients in the table are the marginal effect of a one unit change in the explanatory variable, so the coefficient of 0.240 on “Top 10” in Column 1 means that graduates of Top 10 schools are 24 percentage points more likely to hold one of these top jobs than a Top 10 graduate when we do not include any controls except the indicator variable for a prestigious undergraduate degree. That is a very large difference, given that approximately 18% of the AJD sample works in one of these top jobs.

Moving to the right of the table and adding controls, we find that about a half of the top-job/top-school relationship can be explained with controls for pre-law-school academic history. Top 10 graduates have a 16 percentage point advantage in these sought-after jobs when we add our full set of controls. The effects of the control variables in Table 7 on the effect of going to a top school mirrors that of the control variables in Table 5 which supports the hypothesis that the effect of

¹¹UCLA law school’s website provides relevant statistics for its 2007 graduates. The median starting salary for lawyers at firms with at least 250 lawyers was \$160,000 and dropped monotonically with the size of the firm. Pay at public sector or private sector jobs outside law firms was considerably less, on average.

¹²As Ai and Norton (2003) show, interactions of dummy variables can cause problems and inconsistencies in interpreting marginal effects of probit regressions. We redid the analysis in Table 7 using linear probability regressions and the results do not affect the conclusions.

	(1)	(2)	(3)	(4)
Top 10	0.240 (0.057)	0.240 (0.057)	0.223 (0.057)	0.159 (0.054)
Elite Undergrad	0.133 (0.030)	0.129 (0.030)	0.135 (0.031)	0.121 (0.032)
Top 10 *	-0.116 (0.022)	-0.116 (0.022)	-0.109 (0.022)	-0.094 (0.022)
Elite Undergrad				
Rank 21-100	-0.152 (0.027)	-0.149 (0.027)	-0.154 (0.027)	-0.140 (0.027)
Female		0.001 (0.019)	0.001 (0.019)	0.004 (0.019)
Near Mother			-0.033 (0.019)	-0.032 (0.018)
Undergrad Top 10%				0.023 (0.031)
Controls				
Demographic	no	yes	yes	yes
Family Background	no	no	yes	yes
School Funding	no	no	yes	yes
Academic History	no	no	no	yes
Pseudo R-Square	0.125	0.131	0.155	0.192
N	1,531	1,531	1,531	1,531

Table 7: Lawyer Placement at Top Firms. Each column is a probit where the dependent variable equals one if the person works for a 100+ lawyer firm in one of the top four law markets (NYC, DC, LA, or Chicago.) The dependent variable equals one for 18.2% of the sample. Displayed coefficients are marginal effects of a one unit change in the explanatory variable. Sample is the same as the wage regressions above, though a few lawyers are included here that did not provide wage information.

school selectivity on earnings comes through sorting lawyers into higher paying firms. As with the pay regressions, the coefficients on Elite Undergrad and its interaction with Top 10 law school are consistent with there being large returns for lawyers who go to a top undergraduate school OR a top 10 law school but no additional value to going to both.¹³

3.4 Heterogeneity of Effects

In order to generate further insight into the sources of the law school prestige effects we have measured so far and the degree to which they may be driven by unobserved variables, we now measure the degree to which these effects vary across various subsamples of the AJD sample. For simplicity, we limit the sample to graduates of Top 20 law schools and we compare Top 10 graduates to other Top 20 graduates.

Table 8 presents the results, with Wave 1 wage regressions in Panel A, Wave 2 wage regressions in Panel B, and Wave 1 job quality probits in Panel C. Columns 1 and 2 perform the analyses in Column 4 of Tables 6 and Table 7 separately for women and men. Point estimates for law school prestige effects on pay are higher for men than for women, though the gender effects are also not significantly different from one another or from the coefficients for the sample as a whole shown in earlier tables. The point estimates suggest that the larger effects of undergraduate school quality for men than for women (see Black and Smith (2004) and Hoekstra (2009)) may apply for law school quality, as well.¹⁴ But the sample size is not large enough to say this confidently.

Columns 3 and 4 split the sample based on whether a lawyer lives near his/her mother and is representative of several analyses we did splitting the sample based on background characteristics. In Panels A and B, we find that there is no significant difference between the financial rewards to going to a top law school for these groups. In Panel C, the school quality effects are bigger for lawyers who live near their mothers (though this seems to be a few people driving the result given those who live near their mothers are less likely to go to top law schools.). Based on the results in Columns 3 and 4, as well as other similar analyses, we do not find noteworthy differences in subsamples split by these characteristics.

Columns 5 and 6 show that law school prestige is much less closely related to labor market success for graduates of top undergraduate schools than for other lawyers. The estimates for Top Undergraduate lawyers (as defined above) in Column 5 provide no evidence at all of a link between

¹³In unreported probits, we find similar effects if the dependent variable equals one for lawyers working at 100+ lawyer firms in any geographic market.

¹⁴Arcidiacono, Cooley, and Hussey (2008) also find that school quality has a smaller effect on female wages than male wages for their MBA sample in their preferred specifications.

Panel A: Wage Regressions, Wave 1						
	Gender		Live Near Mother		Undergraduate	
	Women	Men	Within 50 Miles	More than 50 Miles	Top Undergrad	Other School
	(1)	(2)	(3)	(4)	(5)	(6)
Top 10	0.209	0.304	0.369	0.255	0.045	0.296
	(0.117)	(0.097)	(0.125)	(0.096)	(0.082)	(0.081)
Elite Undergrad	0.333	0.271	0.362	0.357		
	(0.110)	(0.097)	(0.112)	(0.096)		
Top 10 *	-0.262	-0.180	-0.459	-0.214		
Elite Undergrad	(0.158)	(0.138)	(0.169)	(0.134)		
R-Square	0.256	0.209	0.444	0.255	0.225	0.285
N	242	229	162	309	199	272
Panel B: Wage Regressions, Wave 2						
Top 10	0.191	0.345	0.116	0.370	-0.003	0.234
	(0.179)	(0.114)	(0.195)	(0.119)	(0.125)	(0.107)
Elite Undergrad	0.267	0.272	0.272	0.270		
	(0.184)	(0.133)	(0.217)	(0.136)		
Top 10 *	-0.198	-0.312	-0.154	-0.316		
Elite Undergrad	(0.253)	(0.176)	(0.296)	(0.179)		
Pseudo R-Square	0.154	0.270	0.268	0.233	0.253	0.191
N	246	277	169	354	213	310
Panel C: Top Job Probits, Wave 1						
Top 10	0.306	0.255	0.654	0.189	-0.045	0.266
	(0.107)	(0.098)	(0.125)	(0.090)	(0.088)	(0.078)
Elite Undergrad	0.434	0.041	0.469	0.282		
	(0.097)	(0.112)	(0.138)	(0.093)		
Top 10 *	-0.461	0.012	-0.434	-0.250		
Elite Undergrad	(0.084)	(0.144)	(0.079)	(0.052)		
Pseudo R-Square	0.218	0.156	0.393	0.135	0.144	0.178
N	256	241	169	328	213	284

Table 8: Top School Lawyer Analyses – Heterogeneous Effects. All specifications in Panel A are the same as column 4 of Table 5 with sample limited as indicated. All specifications in Panel B are the same as column 5 of Table 5 with sample limited as indicated. Specifications in Panel C are the same as column 4 of Table 7 with sample limited as indicated.

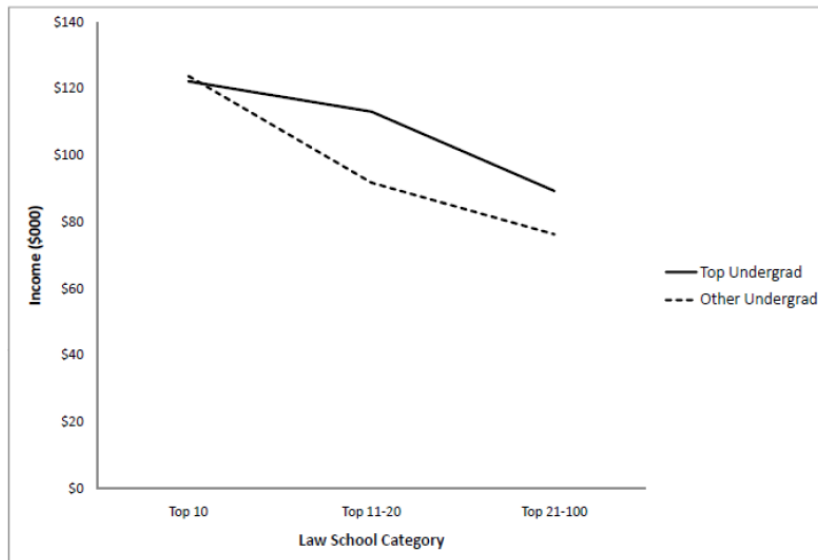


Figure 5: Relationship between income and the prestige of lawyers’ undergraduate and law schools.

law school prestige and labor market success. Going to a Top 10 law school does not appear to help the initial legal careers of graduates of elite undergraduate schools. Column 6, on the other hand, shows very large effects of going to a top law school for graduates of all other colleges. Using our full set of control variables (including indicators for undergraduate school quality within this sample and interactions of undergraduate quality with undergraduate GPA), we find that graduates of Top 10 schools make approximately 24-30% more than other Top 20 law school graduates. Top 10 graduates are twenty seven percentage points more likely to work at a large firm in a top market.

Figure 5 graphs the relationship between income and the prestige of lawyers’ undergraduate and law schools. The graph shows average incomes for lawyers grouped by law school tier and divided into Top Undergraduate school graduates and lawyers that attended other schools. Pay rises steadily with law school quality, with the exception of Top 10 vs. Other Top 20 law schools graduates who attended Top Undergraduate schools. This figure, as well as Columns 5 and 6 of Table 8, indicate that lawyers who graduate from an elite law school or an elite undergraduate school earn a substantial premium but that the marginal effect of going to a second elite school is very small. That is, we find no evidence that lawyers who go to Top Undergraduate schools earn a further premium for attending a top law school.

3.5 Using Observables to Assess Selection Bias

We now follow Altonji, Elder, and Taber (2005) and consider how the relationship between unobservable factors, the schools lawyers attend, and their future careers may affect the interpretation of our results.¹⁵ It is simplest to do this when looking at two endogenous indicator variables rather than an indicator variable (top school) and a linear variable (pay). Consider the following three equation system:

$$c = 1(x'\gamma + \eta > 0), \tag{3}$$

$$y = 1(x'\beta + \delta c + \varepsilon > 0), \tag{4}$$

and

$$\begin{bmatrix} \eta \\ \varepsilon \end{bmatrix} \sim N \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right) \tag{5}$$

Note the specification in (3) omits individual-specific characteristics that are observed by school admissions officers but not by the econometrician (z from Equation 2), while the specification in (4) omits individual-specific productivity (α in Equation 1) which affects wages but is not observable to us. These terms therefore fall into the residuals (η and ε), and the variable ρ captures the resulting correlation. Under the assumption that $\rho = 0$, our figures in Tables 5 through 7 are unbiased estimates of the causal effect of attending a top school.

We use two y indicator variables that capture success at the time of the interview, the indicator for working in a 100+ lawyer firm in a top four market that we analyzed in Table 7 and an indicator variable for having income greater than \$100K per year. The results in Table 7 are estimates of equation (4) under the assumption that $\rho = 0$. If we had a credible instrument for school quality or were willing to make strong functional form assumptions, we could estimate δ . As a substitute for that, we now estimate the three equation system above, making various assumptions about the value of ρ . We start with $\rho = 0$ and increase ρ in steps until it is no longer statistically significant and, eventually, changes sign. This tells us how large ρ has to be for selection to fully explain the relationship between school quality and our measures of y .

¹⁵We know of no other work that uses the methods in Altonji, Elder, and Taber (2005) to study the effects of school quality on labor market outcomes. See Hinrichs (2009) for an analysis of the effect of attending a diverse college which is very similar in spirit to our approach.

Table 9 presents our results. We limit our results in this sample to the Wave 1 sample, but the results for Wave 2 (which we can do for wages only) are similar to those presented for Wave 1. Columns 1-3 look at the indicator variable for lawyers making over \$100K per year for various treatment and control groups. In each case, there is a very large and significant coefficient on going to a top school when running a probit of the pay variable on the school prestige variable with no controls (the first row) and (as in Table 5) this coefficient drops by about one-third when including all available controls. The coefficients remain strongly positive and statistically significant for ρ 's of up to 0.1. When we assume ρ is 0.2, the estimated effect of law school prestige becomes small and insignificant when comparing Top 10 schools to schools ranked 11-20 or comparing Top 11-20 schools to Top 21-100 schools. It is very difficult to know what the correlation of the disturbances in equations (3) and (4) might be. We do know that the correlation between the school prestige and pay indicator variables for the sample and groups in Column 1 of Panel A of Table 9 is 0.2381, which means that the correlation of the disturbances with the full set of controls we have available would have to be nearly as great as the correlation of the variables themselves if selection fully explains the school prestige relationship with pay. That is not implausible but also does not strike us as likely.

Column 4 of Panel A shows that the message is quite different when looking at college quality and the NLSY sample. The bivariate probit in that column is limited to NLSY respondents in the two highest college quality groups and the pay indicator variable is defined at roughly the median for this sample. The effect of college prestige is small and insignificant even with no controls and gets noticeably smaller (though not by a statistically significant amount) when we add all the available control variables (that is, controls similar to those we have available for the lawyer sample.) The estimated effect of college quality turns negative at very low levels of ρ . While we do not know what level of ρ is reasonable for either sample, Panel A provides further evidence that selection in college prestige is more important than in law school prestige and is consistent with a larger causal effect of law school reputation than undergraduate school reputation.

Panel B of Table 9 redoes the analysis in Columns 1-3 of Panel A, but using working at a large firm in a top market as the labor market outcome variable. The effect of law school prestige becomes insignificant at a slightly smaller level of ρ for the Top 20 sample in Column 1, but the basic message is similar to that in Panel A. While we do not know what ρ is, there is little reason from these results to make us believe selection dominates the law school prestige effect on job placement.

Panel A: High Pay				
Sample	AJD	AJD	AJD	NLSY
Dependent Variable	Pay >\$100K	Pay > \$100K	Pay > \$100K	Pay > \$27.5K
Treatment	Top 10	Top 10	Top 11-20	Avg SAT > 1120
Comparison	Top 11-20	Top 21-100	Top 21-100	SAT 1040-1120
$\rho = 0$, no controls	0.629 (0.118)	1.170 (0.101)	0.541 (0.085)	0.101 (0.131)
$\rho = 0$, full controls	0.481 (0.139)	1.019 (0.121)	0.466 (0.089)	0.038 (0.158)
$\rho = 0.1$	0.314 (0.138)	0.853 (0.121)	0.292 (0.088)	-0.128 (0.157)
$\rho = 0.2$	0.144 (0.137)	0.682 (0.120)	0.117 (0.088)	-0.294 (0.156)
$\rho = 0.3$	-0.028 (0.135)	0.504 (0.118)	-0.060 (0.086)	-0.459 (0.153)

Panel B: Work at 100+ Lawyer Firm in Top 4 Market			
Treatment	Top 10	Top 10	Top 11-20
Comparison	Top 11-20	Top 21-100	Top 21-100
$\rho = 0$, no controls	0.517 (0.118)	1.184 (0.102)	0.667 (0.097)
$\rho = 0$, full controls	0.356 (0.136)	1.075 (0.120)	0.591 (0.105)
$\rho = 0.1$	0.188 (0.136)	0.901 (0.130)	0.418 (0.104)
$\rho = 0.2$	0.019 (0.134)	0.723 (0.128)	0.244 (0.103)
$\rho = 0.3$	-0.152 (0.132)	0.539 (0.126)	0.070 (0.101)

Table 9: Sensitivity Analysis: Estimates of School Quality Effects Given Various Estimates About Correlation of Errors. Each entry is the coefficient in a bivariate probit regression of the treatment variable on a high pay indicator (Panel A) or an indicator for holding a job in NYC, LA, DC, or Chicago at a firm with 100 or more lawyers, where the correlation between the error terms in the two probits within the bivariate probit are assumed to equal the value of rho listed. Sample in each analysis is limited to the treatment and comparison groups.

3.6 Propensity Score Matching

We now use propensity score matching as another means of controlling for selection into high prestige schools. Our NLSY analysis is quite similar to that of Black and Smith (2004), though we use slightly different samples and variables and we do not separate our analysis by gender. Our lawyer analysis is similar in spirit to Black and Smith (2004), but we study law school graduates whereas they focus on a broader sample of people that attend college. This allows us to interpret our propensity score matching results for lawyers relative to college students and to use the importance of selection in college choice as a benchmark. Black and Smith (2004) provide a very useful and intuitive discussion of the benefits and drawbacks of using propensity score matching in this context (see section 5 of their paper.) Basically, we observe a “treated” population, which is those who went to Top 10 law schools, and an “untreated” population, which is those who went to lower ranked schools. We analyze Top 11-20 and Top 21-100 schools separately relative to Top 10 schools.

As shown in Table 4, assignment of c is not random and it is difficult to estimate the true treatment effect (the so-called “treatment on the treated”) of going to a top school, which, in the notation of equations (3) and (4), can be expressed as $E(y_{c=1} - y_{c=0}|c = 1)$. That is, it is the effect of going to a top law school on the people that choose to (and are accepted by) a top law school. Our prior regressions measured $E(y_{c=1} - y_{c=0}|x)$, leaving open the possibility that some unobservable factors not captured by x affects both c and y . By matching each person for whom $c = 1$ to a person with very similar x , we can relax the assumption in our previous regressions that linear controls for x eliminate selection bias. However, matching requires that $\Pr(c = 1|X) < 1$ for all x – that is, for any given x , there must be some person that does not go to a top tier law school. Whereas in the regression context, we wanted to add as many controls as possible to the x vector to minimize selection bias, matching people based on a larger set of x variables can lead to what is commonly known as the “curse of dimensionality.” That is, if conditioning on enough x variables leads to a set of people that all attended top schools, there will be nobody in the untreated group with whom these people can be matched. As a result, in some of our comparisons between top tier schools and other tiers, we have to drop some covariates and hope that these are not the key variables that drive both school selectivity and ability. We reduce the most important selection covariates (undergraduate school quality) to a single indicator variable for Top Undergraduate school (as defined previously.)

We implement matching using the nearest neighbor method and the Stata programs described in Becker and Ichino (2002).¹⁶ Table 10 reports our results for the variable “Top 10 Law School” and

¹⁶We have performed similar analyses using alternative matching methods (radius matching, multiple neighbors,

	(1)	(2)	(3)	(4)
Wave 1	0.298	0.018	0.287	-0.072
	(0.076)	(0.077)	(0.110)	(0.103)
Wave 2	0.237	0.248	0.256	0.231
	(0.115)	(0.104)	(0.138)	(0.178)
Sample	All	All	Not Top Undergrad	Top Undergrad
Control for	No	Yes	N/A	N/A
Top Undergrad				

Table 10: Propensity Score Matching Results. Coefficient is for the “Top 10 Law School” variable. The sample and specification is similar to column 4 of Table 6 (columns 1 and 2) or columns 5 and 6 of Table 8 (columns 3 and 4), except some covariates had to be dropped to insure convergence. Sample is limited to graduates of Top 20 law schools.

limiting the sample to graduates of Top 20 law schools. The first two columns report specifications analogous to Column 4 of Table 6 without a control for “Top Undergraduate” (Column 1) and with such a control (Column 2.) Columns 3 and 4 show specifications similar to Columns 5 and 6 of Table 8 – that is, on the subsamples that did not and did go to Top Undergraduate institutions. In general, the propensity score matching estimates lead to similar conclusions to the OLS regressions. In Wave 1, we again find that the returns to going to a top 10 law school are strong for those who did not go to a prestigious undergraduate institution and not as strong for those that did not. The Wave 2 results, while not sensitive to undergraduate institution, suggest a large effect of going to a Top 10 law school. The matching estimates are all insignificantly different from the coefficients in the analogous OLS regressions. These matching results do not change our qualitative conclusions relative to the earlier analysis and continue to be consistent with a substantial causal effect of going to a top law school (especially for those that did not go to a prestigious undergraduate institution.)

Appendix Table 4 presents propensity score matching estimates of the returns to college quality measures for the NLSY sample. That exercise serves two purposes. First, it shows that propensity score matching and OLS lead to similar conclusions in the undergraduate context. Our results are consistent with the findings of Black and Smith (2004) and they provide further confirmation that undergraduate college prestige has relatively little effect on earnings. Second, we do the

and kernel matching) and using the Stata matching commands developed by Leuven and Sianesi (2003). Some of our results are sensitive to these choices, but the results we present are consistent with the majority of alternative specifications we considered.

propensity score matching with and without two “intelligence” measures, AFQT and SAT scores, to the NLSY matching analysis. The addition of these intelligence measures has only a trivial effect on the coefficients, suggesting that the lack of direct intelligence measures may not be driving our results for law schools.

4 Interpretation

4.1 Reconciling the Results

Our results so far have established the following:

- Graduates of Top 10 law schools earn about a 25% premium relative to graduates of other Top 20 law schools. Very little of this premium can be explained by reasonably extensive controls for demographics, background, school funding sources, and pre-law graduate degrees.
- When we control for quality of undergraduate institutions, the premium for going to a top law school drops substantially (and, using some methods, becomes negligible) to a degree that is consistent with the law school premium being largely driven by selection of high ability aspiring lawyers by high prestige schools.
- When looking at lawyers that went to the most elite undergraduate institutions, there is little evidence of a premium for going to a top law school.
- The premium for going to a top law school is much higher, and remains very high despite extensive controls, for graduates of less elite undergraduate institutions.
- Lawyers that went to elite undergraduate schools earn a substantial premium relative to other lawyers, even controlling for quality of law school and other characteristics. This premium is substantially larger than most estimates of the returns to undergraduate school prestige for broader (that is, non-lawyer) samples.

We believe this set of results is consistent with any or all of the following three explanations:

Top Schools Teach and Prepare Lawyers Better: One possibility is that the top law schools actually produce, on average, more value added for their students — or at least that they produce more value for those students that did not go to elite undergraduate institutions. If top schools really add much more value, it must be the case that the quality of education drops very steeply as one drops down the law school distribution. Given the effects are much smaller for elite undergraduate lawyers, it could be the case that top schools at either level provide some set of skills that is critical

to excelling as a lawyer (perhaps analytical thinking and/or writing skills) that most lawyers cannot get without obtaining at least one elite degree. While it is hard to imagine that there could be such a difference in the teaching at Top 10 and other Top 20 schools, perhaps the effects of being surrounded by the most capable peers at elite undergraduate or law schools is what sets the learning at these institutions apart.

Some of the more qualitative questions in the AJD survey can provide a bit of insight into lawyers' perceptions of the value of education at law schools. The AJD asked how helpful the respondent's first year law school curriculum and other specific elements of the law school curriculum were in making the transition to early lawyer work (Wave 1) and the person's current work assignments (Wave 2.) Respondents answered on a seven point scale with one indicating "Not at all helpful" and 7 indicating "Extremely helpful". Due to varying response rates and the very subjective interpretations that these questions may generate, we do not perform any formal statistical analysis of these answers. However, a simple perusal of the means and medians of responses by law school quality and by undergraduate quality lead to several clear and consistent conclusions.

First, lawyers that went to elite undergraduate schools report getting substantially less value out of the first-year law curriculum than those who went to less elite colleges. This difference by college quality is sizeable and similar for lawyers from Top 10 law schools and for lawyers from second tier (ranked 11-20) schools. This similarity by law school rank suggests that, at least based on lawyers' impressions, there is *not* a big difference in the value-added of elite law schools in terms of helping those from less rigorous colleges catch up.

Second, if elite schools did have a unique teaching ability of value to lawyers, it seems likely that this skill would involve writing. However, lawyers from elite and non-elite undergraduate institutions answered nearly equally when asked about the value of "law school training for legal writing."

The basic facts regarding lawyer salaries fits the "elite schools teach some unique skill" hypothesis. However, we think this is unlikely to be the primary driver of our results given the lawyers' responses to the value of their legal education and the implausibility that teaching quality would drop off so sharply from the Top 10 schools to the next group.

School-Based Networks: While the previous explanation involved elite schools differentiating themselves from other schools through their teaching, perhaps the top school effects we have documented are driven by the networks lawyers develop in school. If lawyers that go to elite schools learn more from their relatively talented peers while in school and develop social connections with their classmates that are valuable in the labor market, then we would see graduates of more prestigious schools making more money. Suppose that, in a very simplistic depiction of the lawyer market,

there are only two networks — a high-level network where the most talented workers do the most difficult and lucrative legal work and the low-level network where other lawyers work and transact. Further suppose that, regardless of innate ability, a lawyer that goes to either an elite law school or an elite undergraduate school is more likely to be admitted to the high-level network than any other lawyer but that there is no marginal return, in terms of admission to the high-level network, from going to elite undergraduate and law schools. That is, suppose a lawyer that goes to the University of Utah for both undergraduate and law degrees is less likely than a lawyer that went to Stanford for undergraduate or law school to be able to join the high-level network and that a lawyer that went to Stanford for undergraduate school and Harvard for law school is no more likely to be in the network than a lawyer that got one degree at Stanford and one at University of Utah. This would explain the pattern we showed in Figure 5. It is also consistent with analyses we have done elsewhere using a sample of lawyers at the largest and most prestigious firms. In Oyer and Schaefer (2012), we show that law school is an important factor determining which lawyers match with each other at these firms. We show evidence consistent with networks within these schools (that is, preferences for lawyers from the same school to work together to capture either productivity or non-pecuniary benefits) driving this matching. In Oyer and Schaefer (2010), we show that lawyers are less likely to leave their firms if there are lawyers with whom they share common schools (law school or undergraduate school) in their office. The evidence we have presented here is broadly consistent with this idea of elite-school-based networks being an important driver of the school prestige relationship to law labor market outcomes.

To get a bit more insight into the network hypothesis, we again turn (informally) to the AJD’s subjective questions. In both waves of the AJD, respondents were asked to rate various factors’ importance in their employers’ decision to offer them a job. Not surprisingly, graduates of Top 10 law schools generally rated “the reputation of the law school I attended” as very important and this did not vary noticeably by the quality of the respondents’ undergraduate institution. Also not surprisingly, graduates of elite colleges rank “the reputation of the undergraduate school I attended” noticeably more highly than did non-elite college graduates. There were not sizable and consistent differences across other dimensions such as law school grades and prior jobs.

Perhaps the most relevant questions regarding networking asked how important various factors were in helping the lawyer to obtain his or her job. Among graduates of Top 10 law schools, those who did not go to elite colleges reported making much *more* use of the law school’s placement office and much *less* use of friends than those who went to elite undergraduate schools. This suggests graduates of top undergraduate schools were more likely to have a useful network in place before they got to law school. Also, those who went to elite universities, when asked what was

important in leading their employer to make them an offer, were much more likely to say “Friends”, “Connections through colleagues”, and, of course, “The reputation of the undergraduate school I attended.” Though hardly firm evidence to support the network hypothesis, the answers to the qualitative questions suggest that going to a top law school provides access to resources and people that others already have due to attending a top undergraduate school.

Unobservables: Though we have done a great deal to control for as much as we can, we simply cannot rule out that highly talented potential lawyers have some innate skill that is observable to admissions officers of top undergraduate schools and top law schools, but not to us. This could be information provided in the form of letters of reference or essays, for example, that indicates work ethic or some other important skill that is not evident from grades and test scores. If these skills are particularly valuable for lawyers, then one could imagine that talented prospective lawyers would be admitted to elite schools but would have done almost as well had they attended less elite schools. We interpret our results using propensity score matching and looking at the potential effects of unobservables as indicating that unobservables are unlikely to be the primary driver of our results regarding the empirical relationship between elite schools and lawyer incomes. But, given the lack of exogenous variation in who attends which universities, we (like almost all papers in this literature) cannot rule out an important role for unobservable measures of lawyer ability.

In any case, we note that, while an important role for unobservables would indicate that elite schools are not having a substantial causal effect on lawyer earnings, *it would still mean there is something special about these elite schools*. Specifically, this would indicate that admissions officers of elite colleges and law schools have the ability to identify especially talented lawyers before the candidates ever practice law. In essence, admissions officers would have the ability to “learn” about applicants’ abilities as lawyers from reading an application in the way that typically takes months or years for firms to learn on-the-job (as in Farber and Gibbons (1996) and Lange (2007)).

Note that, given the size of the relationship we see between school quality and income is much higher for lawyers than prior work has found in the economy more generally, our results can only be explained by unobservables if admissions officers are particularly good at identifying things not available to the econometrician that are related to ability *as a lawyer*.

On the other hand, we do not think the set of results in this paper can be reconciled with the following models and we therefore do not think they are likely to be important determinants of wages in the market for lawyers.

Information Limitations: We do not think that the large association between university reputation and labor market success results from an information problem on the part of employers that hire lawyers. If, even with such information as grades and honors, firms cannot easily distinguish

the ability of law students, they might rely on the reputation of law and undergraduate schools as the primary signal of the applicant's ability. Going to (and, since we control for undergraduate grades, doing well at) an elite school at either level could be a sufficient signal of high ability to impress top firms. But this idea would suggest that high ability lawyers from lower ranked law schools would catch up with lawyers from higher ranked schools as their careers develop. In that case, we would expect the top school premium to dissipate over time. Given that all our results are similar for the Wave 1 and Wave 2 sample, we think this is unlikely.

Law School Admissions Preference for Less Prestigious Schools: If undergraduate school is a variable where law schools and their admissions offices value diversity, then we might expect that law schools provide the equivalent of affirmative action on behalf of students from less prestigious undergraduate schools. That is, top law schools may fear that, if they admit strictly by test scores and similar factors, they would fill their class with Ivy League (and equivalent) undergraduates. As a result, all else equal, an applicant to a top law school who received his undergraduate degree at the University of Utah may be more appealing to the admissions committee than an undergraduate from Stanford. If this is the case, then we would expect that graduates of top undergraduate schools would be of higher ability, holding quality of law school constant. This would explain the large measured return to attending an elite undergraduate school in our sample. However, this idea does not seem to fit with the pattern in Figure 5. The graph shows that pay is lower for graduates of less prestigious undergraduate schools that go to every tier of law school below the Top 10 but that Top 10 graduates make the same regardless of the prestige of their undergraduate institution. So there is no evidence that Top 10 students from less prestigious undergraduate schools are less skilled than Top 10 lawyers from elite undergraduate schools, which suggests law firm admissions preference does not have a large impact on our results.

Lawyer Preferences Correlated with School Quality: One idea that may help explain how selection effects are related to lawyer pay and school prestige is that lawyers who are more ambitious apply to more prestigious schools. So lawyers that go to a top law school or a top undergraduate school are simply the ones that are most ambitious and, therefore, most successful. This would lead to a large return to attending either a top undergraduate or law school and not (necessarily) a marginal return to attending both. However, while this could also be a contributing factor to our results, we would also expect the background characteristics to be more related to the law school prestige premium. For example, if preferences and ambition drove the selection into top law schools, we would expect the law school premium to be smaller when controlling for how close a lawyer lives to his/her family and how they paid for law schools. But, as we showed earlier, controlling for these variables has no effect on the law school premium.

4.2 Investments by Aspiring Lawyers

We now perform simple thought experiments to determine the implications for law school applicants of the possible interpretations of our results. Consider a law school applicant that, with probability one, can get into a law school ranked approximately fiftieth.¹⁷ She would like to attend Stanford Law School (the representative top 10 school for this analysis) if at all possible and, if not, she prefers UCLA (our representative school ranked in the second ten) to the options ranked around fiftieth.

To formulate estimates of the probability that a student will get into Stanford and UCLA, we use self-reported admissions outcomes from lawschoolnumbers.com of people that applied for Fall 2008 entry to law school. We cannot be sure that people report reliably, but the website is anonymous so there is little incentive to misreport. More problematically, we have no idea whether there is selection bias as to who participates in this voluntary website and whether people that report outcomes here are more or less likely than others to get in. So these admissions probabilities should be thought of as having wide standard errors. We approximate admissions probabilities for individual schools within 5-point LSAT ranges.

Suppose our hypothetical aspiring lawyer received a score of 163 on the LSAT. One out of a total of 25 people with LSAT scores between 160 and 164 report being admitted to Stanford so we assume our lawyer has a 4% probability of getting into Stanford. Five out of 65 UCLA applicants with LSAT scores in this range report being admitted, so she has a 7.6% chance of being admitted there. Assume maximum admissions correlation (that is, all applicants that are admitted to Stanford are also admitted to UCLA). If our lawyer does nothing to increase her LSAT score, she will go to Stanford with 4% probability, UCLA with 3.6% probability, and the school ranked around fiftieth with 92.4% probability.

Our estimates of her expected income are based on a survey of Stanford Law School alumni that was conducted in 2007 and included just over 1,000 alumni from the classes of 1987-2006. These respondents were asked, “What is your total annual income, before taxes, from your CURRENT job? Please include salary, bonuses, profit sharing, and any other direct financial compensation.” They were asked to check a box for no income, under \$50K, \$50K-\$99K, \$100K-\$199K, \$200K-\$399K, \$400K-\$599K, \$600K-\$2million, or over \$2 million. We assign the midpoint of each category and \$3 million for those earning over \$2 million. There were 790 valid responses. We took the average income by graduating class and, after discounting by zero (this would assume that discounting

¹⁷Southern Methodist University is ranked fiftieth in the US News 2003 rankings, just below Tulane University and the Universities of Alabama, Maryland, and Washington.

	LSAT = 163	LSAT = 168	LSAT = 173	Relative Wage
Prob(attend) Stanford	4.0%	14.5%	19.0%	100%
Prob(attend) UCLA	3.6%	60.5%	73.0%	85.4%
Prob(attend) #50 School	92.4%	25.0%	8.0%	71.8%
Exp. Income, No Discount	\$4.41 mm	\$5.05 mm	\$5.22 mm	
Exp. Income, 5% Discount	\$2.57 mm	\$2.94 mm	\$3.05 mm	
Exp. Income, 10% Discount	\$1.65 mm	\$1.89 mm	\$1.96 mm	

Table 11: Estimates of Relationship Between LSAT Scores and Income. Probabilities of attending each school based on self-reported admission outcomes for applicants to UCLA and Stanford for entry in Fall of 2008 on lawschoolnumbers.com. Potential assumed to be able to get into 50th ranked school with certainty. “Relative Wage” is based on column 4 of Table 4. Expected income over first twenty years after graduation for Stanford graduates based on averages from survey of Stanford classes of 1987-2006.

will be exactly outweighed by the increase in earnings for lawyers with a given number of years of experience in the time between graduation and when the person reaches that level of seniority), 5%, or 10%, added these up as an estimate of what an aspiring lawyer attending Stanford could expect to earn over the first twenty years after graduation. These averages are \$6 million for zero discounting, \$3.5 million when discounting by 5%, and \$2.25 million when applying a 10% discount rate. We assume that these are our lawyer’s expected earnings if she goes to Stanford.

We start by estimating the largest law school premium we believe to be at all plausible by assuming that the controls in Column 4 of Table 5 fully capture all relevant observable and unobservable characteristics. Under this assumption, the lawyer’s earnings would be 84.9% of the Stanford earnings if she goes to UCLA (which is based on the -0.158 coefficient in Column 4 of Table 5), and that her earnings are 71.5% of the Stanford level if she goes to a school ranked fiftieth (based on the -0.331 in Column 4 of Table 5).

Column 1 of Table 11 shows that, given these assumptions, our lawyer’s expected income with her LSAT of 163 is approximately \$4.4 million if she does not discount future income streams, \$2.6 million if she discounts at 5%, and \$1.6 million if she discounts at 10%.¹⁸ Column 2 shows that our lawyer’s chances of getting into a Top 10 or Top 20 school and, therefore, her income are

¹⁸We believe that 10% is probably too large a discount factor given the age/wage gradient for lawyers in the Stanford survey suggests that real wages would be dropping for most of the twenty years we measure.

enhanced significantly if she can increase her LSAT scores by 5 points. If she does not discount, five LSAT points are worth about \$600,000 in expectation and \$200,000 if she discounts by the full 10%. The increases are smaller (nearly \$200,000 without discounting and \$70,000 with 10% discounting) for a further increase in LSAT scores from 168 to 173. Even taking our lowest estimates and allowing for some noise in our estimates, these estimates suggest that such actions as LSAT preparation classes, spending time refining admissions essays, and even studying hard in relevant undergraduate classes in hopes of improving grades all have a significant positive return for aspiring lawyers *if our regressions fully control for unobservables*.

We could do a similar exercise where we consider two aspiring lawyers facing this same decision, where one went to a prestigious undergraduate school and the other did not. Looking at the far right columns in Table 8, it would at first seem that the value of 5 LSAT points is much higher for the non-Top Undergraduate law applicant than for the “representative” student analyzed in Table 11. However, this depends on what the LSATs would be in the absence of taking the course. We would estimate a bigger discount for going to UCLA relative to Stanford for the Top Undergraduate but only a marginal advantage in terms of getting into Stanford. So the value of 5 LSAT points is very high for this group, but not necessarily greater than indicated in Table 11. Looking at Top Undergraduates, the premium of 0.045 from Table 8 still suggests the investment in improved LSAT scores are worthwhile, but the aspiring lawyer would also have to consider the fact that a zero effect cannot be ruled out by the underlying regression.

Another thought experiment is to consider an aspiring lawyer trying to maximize the net present value of her income less tuition who has been admitted to Stanford and UCLA. Assuming room and board costs would be roughly the same at each school (they could be lower outside Silicon Valley and Los Angeles, but not by enough to affect this decision), the key difference is tuition. Stanford tuition costs \$40,000 for a total of about \$120,000 over the time in law school, while UCLA’s tuition is \$81,000 (\$27,000 per year for three years). The total difference is \$39,000, which is about 1-2% of the discounted expected income over the first 20 years of the lawyer’s career. This suggests that, as long as the causal portion is at least one eighth of the measured 16% premium for going to Stanford instead of UCLA, the lawyer is better off paying to go to the more prestigious school. If she were offered a scholarship such that she paid no tuition to go to UCLA, the marginal cost of going to Stanford (\$120,000) would be as much as about 6% of her expected future income. For a lawyer facing this choice, making the right decision requires taking a stand on the degree to which she thinks unobservables contribute to the 16% premium in Table 5. But it seems likely that, even if UCLA or a lower ranked school offered a full tuition scholarship, the aspiring lawyer would be better off in terms of expected wealth by attending Stanford (especially if she did not

attend a top-tier undergraduate institution). If she did not get into Stanford, it would also be worth paying UCLA's \$81,000 tuition (\$27,000 per year for three years) instead of taking free tuition at a lower-ranked school.

5 Conclusions

We used a representative sample of lawyers that first passed the bar in 2000 to show that, as of 2002 and 2007, there was a large wage premium associated with having gone to a highly ranked law school. Lawyers from Top 10 schools made about 25% more than those from the next ten schools and much more than those from schools ranked further below. Lawyers from top schools were also considerably more likely to hold jobs at large firms in top legal markets. Adding controls for various demographic factors that might affect both where a lawyer went to school and her later success did surprisingly little to lower the strong effect of going to a top law school. However, controlling for the quality of a lawyer's undergraduate school made a significant difference in our estimates and suggested that unobservables are an important issue. As a result, we are left with evidence that there may be a substantial causal effect of going to a top school, but this conclusion depends on the assumptions one is willing to make about the role of unobservables.

While the exact causal effect of going to a top law school depends on one's interpretation of the potential role of unobservables in our analysis, we believe that, at a minimum, we have been able to rule out some explanations of drivers of the relationship between law school prestige and labor market outcomes. Our results are hard to reconcile with law schools affecting the labor market by providing "affirmative action" on behalf of graduates of less prestigious undergraduate institutions or firms statistically discriminating in favor of top law school graduates. However, our results are consistent with top academic institutions providing skills that are valuable to lawyers and with these schools generating networks that are valuable to their graduates.

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6 Appendix: Undergraduate School Quality

We analyzed the relationship between undergraduate college quality and earnings after college using two different datasets. We present detailed results in this appendix using data from the National Longitudinal Survey of Youth (NLSY). This is a panel survey of over 12,000 people that were between the ages of 14 and 21 when the survey began in 1979. To make our analysis comparable to the AJD survey, in terms of the age of the population, we use a cross-section of respondents in 1990 (though many of the background variables are gathered from earlier survey years.) Because our goal is to look at the effect of college quality measures, we limit the sample to people that have completed at least two years of college, those that report the college they attended, and those who provided compensation data in 1990. We divided the colleges attended into quintiles such that the top and second group are similar proportions of the NLSY sample as the Top 10 and Top 20 groups are of the AJD sample. We define the Top Tier of colleges as those where the average combined SAT score (according to *US News and World Report*, as of 1991) is above 1120 and the second tier includes schools with an average SAT score above 1040 and not greater than 1120.¹⁹

Panel B of Table 3 displays summary information for the sample we use in our wage regressions below and for those portions of the sample that went to the highest and second highest school tiers. As with the AJD sample, the NLSY sample is about half male and averages about thirty years of age (though the age variation is smaller because NLSY ages only vary by up to six years whereas our AJD sample includes people between 25 and 39.) As with the lawyers, the NLSY respondents at better schools come from families with more education and they are more likely to live somewhere different from where they grew up. Again, those going to better schools both make more after school and show more skill before school (as measured by SAT scores), so it is not entirely clear whether the school quality/wage correlation is due to selection or a causal effect of school quality on earnings. The rest of the tables in this appendix are meant to parallel tables in our AJD analysis and are discussed briefly in the text.

Black and Smith (2004) use the same data, though they use the 1998 cross-section. They point out that, when trying to identify the causal effect of school reputation on income, it is somewhat debatable whether to include years of schooling as an explanatory variable. People that go to

¹⁹Schools just making the cutoff into the top group include American University, Saint Olaf College, and UCLA. Those with average SATs just above 1040, and therefore making the second tier cutoff, include University of Central Florida, University of San Diego, and Evergreen State College in Washington. Our results are similar when we use other measures of school quality, such as freshman retention rate or expenditure per student, or an average of several measures. Also, note that our definition of an elite undergraduate school is quite different (and much less exclusive) when looking at the NLSY sample than when looking at the AJD sample.

better schools are more likely to complete more education. So, if one of the ways going to a better school increases earnings is by increasing completed education, controlling for education will lead to understating the effect of school quality on pay. We therefore think estimates without years of education would be the best indication of the effect of college quality on earnings and would leave it out of our regressions if this were our primary goal. However, we are only using the NLSY for the purposes of comparing the effect of undergraduate school prestige to law school prestige. Given that the lawyers in our sample are homogeneous in terms of their education level, we include years of education in our NLSY regression so that, in both samples, we are measuring the effect of school quality independent of its effect on how much schooling the person gets.²⁰ Similarly, tenure on their current job is very homogeneous for the AJD sample but quite variable for the NLSY sample, so we control for months of tenure on the current job throughout the analysis below. Also note that the female coefficient in our NLSY regressions indicates a very large gender gap because we use annual earnings and women work fewer hours, on average. When we look at hourly wage, the female coefficient is cut dramatically (to about -0.12.)

The second dataset we used to study undergraduate school quality is the Baccalaureate and Beyond (B&B) survey. This survey, which focuses on people that received undergraduate degrees in 1992-1993, has some advantages relative to the NLSY. The total sample size is similar to the NLSY, but the focus on college graduates makes the relevant sample size for our purposes much larger. We can therefore look at finer levels of college quality and, in particular, focus on undergraduates that are more similar to our lawyer sample. However, the B&B's cost relative to the NLSY is that it has less background, demographic, and high school data. As a result, we could not include as many of the controls that are the key to our attempts to separate selection effects from value added by more selective schools. We did all the analysis we do with the NLSY using B&B incomes in the 2003 follow-up survey. We defined school quality the way we define it in the NLSY and we also did it using higher average SAT cut-offs for the highest quality school groups. We found that the college quality effects are quite similar to those in the NLSY and they respond similarly to the demographic controls we do have in the B&B. Overall, our conclusions are very similar whether we use the B&B or the NLSY for our undergraduate analysis. Details of our B&B analysis are available on request, but we do not present them here.

²⁰The coefficient on years of education is approximately 0.08 for the regressions we run, though it drops to about 0.063 when we control for test scores.

	All	Top Tier College	2nd Tier College
Female	0.518	0.500	0.436
Age	29.609 (2.266)	29.440 (2.310)	29.173 (2.284)
Financial Aid	0.534	0.546	0.495
Mother's Education	12.696 (2.896)	14.030 (2.978)	12.771 (2.354)
Moved since age 14	0.664	0.728	0.728
SAT %ile	0.576 (0.271)	0.803 (0.208)	0.652 (0.223)
Years of Education	16.149 (1.365)	16.917 (1.445)	16.329 (1.274)
Annual Pay	\$25.8K (13.2K)	\$29.6K (16.2K)	\$26.9K (12.3K)
N	1,767	156	225

Appendix Table 1: Summary Statistics, NLSY Sample interviewed in 1990

	Top Tier Logit	Top Tier Logit	OLS: Tier
Female	0.003 (0.012)	0.080 (0.052)	0.006 (0.016)
Minority	0.000 (0.015)	0.119 (0.070)	0.001 (0.013)
Mother's Education	0.007 (0.002)	0.026 (0.008)	0.008 (0.002)
Top 10% of HS Class	0.053 (0.020)	0.118 (0.065)	0.071 (0.019)
SAT %ile	0.050 (0.008)	0.142 (0.038)	0.047 (0.008)
N	1,767	381	1,767

Appendix Table 2: Undergraduate Selection into Top Schools

Selection into Top Undergraduate schools. Sample is from 1990 NLSY interviews of people with two or more years of college education. Columns 1 and 2 are probits where the dependent variable is an indicator variable for graduating from a school where the average SAT score is above 1120. The sample in Column 2 is limited to those who went to schools with average SAT scores above 1040. Column 3 shows results from an OLS regression where the dependent variable is 1 if the person went to a school with average SAT scores above 1120, 2 if he/she went to a school with average SATs between 1041 and 1120, 3 if he/she went to a school with average SATs between 841 and 1040, 4 if he/she went to a school with average SATs between 771 and 840, and 5 if he/she went to a school with average SATs of 770 or below. Coefficients displayed in logits are marginal effect of a one unit change in the explanatory variable.

	(1)	(2)	(3)	(4)	(5)
SAT 1041-1120	-0.061 (0.079)	-0.087 (0.078)	-0.072 (0.078)	-0.052 (0.079)	-0.032 (0.079)
SAT 841-1040	-0.113 (0.067)	-0.095 (0.065)	-0.068 (0.066)	-0.047 (0.068)	-0.011 (0.069)
SAT 771-840	-0.240 (0.075)	-0.217 (0.073)	-0.184 (0.075)	-0.160 (0.078)	-0.107 (0.080)
SAT <=770	-0.214 (0.089)	-0.174 (0.089)	-0.132 (0.091)	-0.108 (0.094)	-0.046 (0.095)
Female		-0.350 (0.036)	-0.347 (0.036)	-0.343 (0.037)	-0.326 (0.037)
Move since Age 14			0.046 (0.039)	0.052 (0.039)	0.050 (0.039)
Financial Aid			-0.041 (0.038)	-0.038 (0.039)	-0.040 (0.039)
GPA				0.045 (0.034)	0.032 (0.035)
AFQT					0.288 (0.114)
Controls					
Education and Tenure	yes	yes	yes	yes	yes
Demographic	no	yes	yes	yes	yes
Family Background	no	no	yes	yes	yes
School Funding	no	no	yes	yes	yes
Academic History	no	no	no	yes	yes
Intelligence	no	no	no	no	yes
R-Square	0.064	0.124	0.133	0.140	0.149
N	1,767	1,767	1,767	1,767	1,767

Appendix Table 3: NLSY Pay Regressions.

OLS – Dependent Variable is Log of annual pay. Sample is cross-sectional NLSY sample in 1990, when ages are similar to ADJ sample, and is limited to people with at least two years of college. Average SAT scores, based on US News and World Report 1991 rankings. The excluded category is schools with average SAT scores over 1120. “Move since Age 14” is an indicator variable for living in a different count in 1990 than at age 14. “Financial Aid” is an indicator variable for whether received financial aid (not including loans) to help pay for undergraduate school. “GPA” is high school GPA and “AFQT” is percentile rank on the Armed Forces Qualifying Test. All regressions include linear controls for years of education and tenure in current job.

	(1)	(2)
SAT 1041-1120	-0.116 (0.172)	0.052 (0.177)
SAT 841-1040	-0.060 (0.103)	-0.052 (0.110)
SAT 771-840	-0.242 (0.111)	-0.228 (0.215)
SAT <=770	-0.302 (0.224)	-0.291 (0.402)
Intelligence Measures	No	Yes

Appendix Table 4: Propensity Score Matching Results for NLSY sample.

Matches NLSY respondents that attend top ranked colleges with those from each of the other categories in separate analyses. The sample and specification is similar to Column 5 of the previous table, with covariates dropped to insure convergence.