The Returns to Attending a Prestigious Law School*

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Abstract

We measure the returns to attending a highly ranked law school using the "After the JD" survey of lawyers first passing the Bar Exam in 2000. In 2002, those lawyers that went to top 10 law schools made, on average, 25% more than those that went to schools ranked 11-20 and over 50% more than those that went to schools ranked 21-100. Graduates of Top 10 schools were also much more likely to work in large law firms in leading law markets. We use two methods to assess the degree of selection in the law school prestige premium – the methods developed by Altonji, Elder, and Taber (2005), who focus on the relationship between observable and unobservable variables, and propensity score matching. Our analysis suggests that only a small portion of the large returns to law school reputation are due to selection. We use NLSY data to contrast our law school results with the effect of undergraduate prestige on income and conclude that the selection issue is more important in explaining the undergraduate premium than the law school premium. We conclude that either there is a large causal effect of going to a top law school or selection based on unobservables is much more important for law schools than for undergraduate schools.

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1 Introduction

Graduate students comprise an increasingly large share of overall U.S. higher education attendance.[1] Despite this, the large economics literature on the returns to education and educational quality has paid relatively little attention to graduate education. One particularly large graduate sector that has grown considerably since 1970 is law school.[2] While the market for lawyers has grown, competition for admission to top US law schools has become very intense. For example, 7,168 people paid $75 each to apply to the Harvard Law School class of 2011 (as well as $132 each time they took the Law School Admission Test [LSAT]). Only 11% were accepted. Aspiring lawyers work very hard to get good grades as undergraduates and take other actions to make their applications more attractive. A substantial fraction of aspiring lawyers invest in LSAT preparation classes and materials that typically cost $1,000 or more and require over 50 hours in classroom time alone.[3]

Is it worth it? Does it matter what law school an aspiring lawyer attends? Conventional wisdom suggests that top lawyers come exclusively from top law schools and that entry to a top law school is a ticket to partnership at a top firm. However, there are reasons to question the belief that going to a top law school will make a lawyer successful. First, there are many highly successful lawyers from less prestigious schools. For example, Sullivan & Cromwell LLP and Skadden, Arps, Slate, Meagher, and Flom LLP rank as the third and fourth most prestigious firms, according to Vault.com. Not surprisingly, both firms have many graduates of Harvard, Yale, Columbia, and other top law schools in their associate and partner ranks. But Sullivan & Cromwell also has at least two associates and at least two partners from each of Brooklyn, Catholic, and Ohio State Law Schools. Skadden has, for example, ten associates and three partners from Villanova, eight associates and three partners from the University of Connecticut, and eight associates and nine

[1] The National Center for Education Statistics publishes tables on the U.S. Department of Education website indicating that full time undergraduate attendance grew by 58% between 1976 and 2006 and is expected to grow 12% from 2006 to 2017. Full time graduate school attendance grew by 103% from 1976 to 2006 and is expected to grow by 20% from 2006 to 2017.

[2] Lawyer growth was particularly intense in the 1970's when the lawyer population increased by 90% and from 1980 to 1988 when it increased 48% (see Sander and Williams (1989) and Figure 1 in Rosen (1992)). In 1996, lawyers made up approximately 5% of all graduate students. We have not been able to find more recent growth statistics. It appears that the largest firms have continued to grow substantially while the number of lawyers entering the market (at least from elite schools) has leveled off. For example, Stanford Law School's class of 2005 had 166 graduates. This is similar to the class of 1980 (164) but much larger than the class of 1970. Similarly, University of Utah's classes of 2005, 1980, and 1970 were 140, 133, and 92, respectively. Some schools have continued to grow since 1980, but Stanford and Utah are quite typical.

[3] A 1989 study of law school applicants (Wightman (1990)) found that about half took an LSAT preparation class. We suspect this number has, if anything, grown since the time of that study. 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.

partners from St. Johns.[4]

Apart from these examples of very successful lawyers from less prestigious schools, the fact that the most prestigious law schools are able to attract candidates with the most impressive backgrounds suggests that a large part of the association between law school prestige and lawyer success could be due to selection effects. As we will discuss, there is a large literature indicating that much of the relationship between undergraduate school selectivity and labor market outcomes is due to better schools attracting more talented students. The causal effect of undergraduate school prestige on wages is not generally considered to be large. The selection component of the school ranking/wage relationship might be particularly important for lawyers relative to undergraduates and other graduate students, leading to a small causal effect of school prestige on labor market outcomes, because prospective employers have relatively good information about law students. Law school grades are generally disclosed (unlike at many business schools, for example) and other honors, such as Order of the Coif and law review positions, are publicized. This may make it easier for employers to identify student ability levels independent of what school they attend.

In this paper, we use a large, representative dataset of lawyers that first passed the bar in 2000 to measure the relationship between law school prestige and lawyers' success in the labor market. We first show that law school prestige is, on average, associated with a large wage premium and a much higher probability of holding a "prestigious" position (which we define as working at a firm with 100 or more lawyers in one of the top four geographic law markets.) Graduates of a top 10 law school (using *U.S. News and World Report* rankings) earn an average of 25% more than graduates of schools ranked between eleven and twenty and over 50% more than graduates of top 100 schools not in the top 20. We then spend the rest of the paper taking various approaches to separate the portion of these wage premia that is due to selection from the causal effect of going to a top school. While we find that selection into top schools is associated with variables that may independently affect a lawyers' success, we find relatively little evidence that this drives the school selectivity premium. Using the methods in Altonji, Elder and Taber (2005) and propensity score matching, we show that the evidence is consistent with there being a large causal effect of attending a top law school and that investments by a marginal candidate in obtaining admission to a top school are generally good *ex ante* investments. For comparison, we perform similar analyses of the effect of undergraduate school prestige using the National Longitudinal Survey of Youth. We find that the law school prestige effects on labor market outcomes are larger and more robust to inclusion of control variables and matching than similar effects for undergraduate institution prestige. We

[4]The Sullivan & Cromwell and Skadden lawyer information comes from the firms' web pages and is based on the data used in Oyer and Schaefer (2009).

conclude that either there is a large causal effect to going to a prestigious law school and/or that selection based on unobservables that predict labor market success is more important for law school admissions than it is for undergraduate admissions.

Despite the large literature on the effects of college quality measures on earnings, we do not know of any prior study of the effects of law school reputation on earnings.[5] Given that the number of people going to law school or other graduate schools has increased substantially in recent years, it seems worthwhile to understand how the prestige of these schools relates to labor market outcomes. In fact, the prestige effects of law and other trade schools may be of more interest to education consumers and policy makers than the returns to college quality measures because the primary goal of professional schools is to advance their students' careers. If there is little or no financial return to going to Harvard instead of University of Massachusetts as an undergraduate, one may still justify attending Harvard based on the non-pecuniary value of an elite liberal arts education. While there are similar non-pecuniary advantages of attending law school at Harvard instead of University of Massachusetts, we would expect these to be a less important consideration.

Our paper is related to several other literatures in labor economics, the economics of education, and studies of the legal profession. We discuss the relevance of prior studies on the effects of undergraduate school quality in detail in the next section. See Galanter and Palay (1991) and Galanter and Henderson (2008) for 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 greater 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 only other study that relates labor market outcomes to graduate school quality, Arcidiacono et al. (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

[5] The paper by Arcidiacono, Cooley and Hussey (2008) that we discuss below is the only paper we know of that relates labor market success to the reputation of any type of graduate school. Their primary focus is on the overall return to getting an MBA rather than the effect of going to programs of varying reputation.

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.

In the next section, we provide background on the challenge in measuring the effect of going to a highly ranked school on labor market outcomes and how contributors to the literature on the returns to college selectivity have dealt with this. Section 3 describes the data we use and then shows our empirical analysis using standard regression methods that control for as many indicators of unobserved skill as we can. We attempt to assess the importance of unobserved variables by measuring the degree to which observed and unobserved variables would have to interact if there were no causal effect of law school reputation on labor market outcomes in Section 4 and by using propensity score matching methods in Section 5. We interpret the implications of our results for aspiring lawyers and speculate on possible sources of the law school prestige premium in Section 6. Section 7 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

$$y_i = \alpha_i + \beta_1 x_i + \beta_2 c_i + \varepsilon_i \tag{1}$$

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

$$c_i = \theta_i + \gamma_1 x_i + \gamma_2 z_i + \eta_i. \tag{2}$$

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 β_2.

Table 1 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 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 et al. (1996). However, the results in Brewer et al. (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.[6]

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.

3 Data and Simple Regressions

3.1 Data

Our lawyer data comes from the first wave of the "After the JD" survey, conducted in May 2002. The survey was conducted primarily by mail and phone (with a few responses done over the internet) and had a 70% response rate. We only used the 2,621 eligible 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 that were 40 or older when they first passed the bar 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. Our wage regressions are limited to the 2,037 respondents for whom we have fairly complete data. The sample size is slightly larger (2,208) when we run probits on whether the person works at a large firm in a major legal market because some people provided detail on the type of job they hold but not on their incomes.

Panel A of Table 2 provides details on the sample used in our wage regressions as a whole, as well as for those 201 respondents that went to law schools defined as being in the Top 10 by *US News*

[6] Studying 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 World Report in 2003 and the 270 who went to other Top 20 schools.[7] The sample as a whole and each subgroup splits roughly evenly between men and women and averages about thirty years old. Those attending top schools appear to come from somewhat 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 that continued their education after high school. Those attending more selective schools are, not surprisingly, more likely to have had undergraduate grade point averages above 3.5.

Our analysis below will focus on two dependent variables. The first of these is the log of the person's annual earnings and the second is an indicator variable that takes the value one if the person works 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). The pay differences suggest that those going to Top 10 schools earn more than 40% more than the sample as a whole and 25% more than those going to Top 11-20 schools. Figure 1, which displays kernel densitity estimates of pay differences between Top 10, Top 11-20, and Top 21-100 graduates, provides more detail on pay differences across law school tiers. 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.

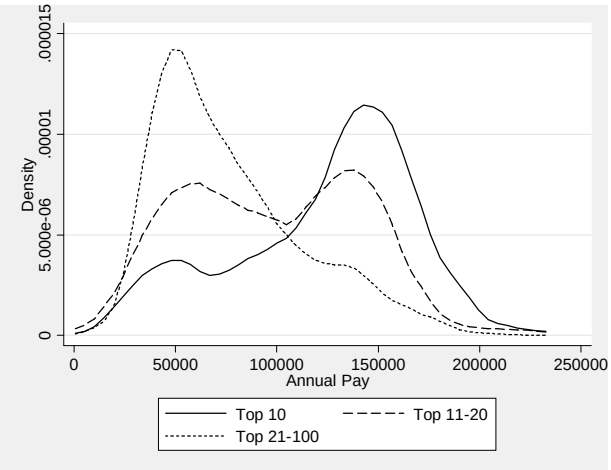

Figure 1: Pay of AJD respondents by Law School Tier

[7] Because 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" throughout the paper includes the following schools (ranked 12-20): Duke and Northwestern (tied), Georgetown, Texas, UCLA, Vanderbilt, USC, and Minnesota and Washington and Lee (tied).

The table also shows that Top 10 graduates are also much more likely to work for a large law firm in a top law market. These raw differences in pay, type of job, GPA, and background make two things clear – those who go to more selective schools are more successful (at least early in their careers) and had advantages of various kinds even before law school that could be affecting these post-law-school outcomes.[8] The rest of the paper attempts to give a sense for how much of the raw differences in pay and type of job shown in Table 2 are due to selection of the most promising lawyers by the best schools and how much is caused by the law schools people attend.

For comparison purposes, we use 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.[9] 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.[10]

Panel B of Table 2 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

[8] Because we only have the first wave of the AJD at this point, we primarily focus on lawyers shortly after graduation. The AJD's second wave is scheduled for release very soon and we will then expand our analysis to include lawyers seven to eight years after earning JDs.

[9] We also used the Baccaleureate 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.

[10] Schools 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.

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.

Table 3 explores the potential importance of selection through analyses where measures of school quality are the dependent variables. Panel A uses the AJD lawyer sample. The first two columns 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 sample while Column 2 is limited to lawyers from Top 20 schools. The results show that selection is likely to be very important. For example, lawyers with at least one parent that graduated from college have a 5.3 percentage point higher probability of going to a Top 10 school when looking at the whole sample (column 1) and more than a twelve percentage point higher probability when focusing on those that went to Top 20 schools (column 2). 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. The third column shows a regression where the dependent variable is 1 if the person went to an unaccredited school, 2 if she went to a *US News* Tier 4 school, and so on up to 6 for lawyers from Top 10 schools. Having a parent that graduated from college is associated with going to a law school that is 0.355 levels higher on this scale and a high GPA is associated with about two thirds of a level higher school. Holding other factors constant, minorities attend higher ranked schools, which could be the result of affirmative action.

Panel B shows similar analyses for undergraduate schools using the NLSY sample. Each additional year of mother's education is associated with an increase of 0.7 percentage points in the probability of the respondent going to a school in the highest tier (column 1) and this increase is 2.6 percentage points when limiting the sample to those in the top two college tiers. High rank within high school class and SAT scores are, not surprisingly, also closely related to the quality of the college attended. Column 3 shows results of an OLS regression where the dependent variable is 1 for those that went to schools with average SATs of 770 and increases in steps to 5 for those in the top tier. This analysis shows that being in the top 10% of a respondent's high school class leads, on average, to going to a school that is 0.07 levels higher on this scale.[11]

[11] In addition to the OLS regressions in column 3 of both panels, we ran ordered probits using the same dependent variable. The results are stronger in terms of statistical significance. However, because the coefficients are quite

3.2 OLS Regressions of Income

We begin by estimating equation (1) for the lawyers in the AJD sample. This includes 2,037 that first passed the bar exam in 2000 and that were under 40 years old at the time. The dependent variable is the log of the lawyer's salary in 2002.[12] 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 Table 4.

Column 1 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 in the Top 10, so all other ranks are relative to this top 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 25% or so less. Lawyers from Top 10 schools average pay of almost $123K, while those from Top 11-20 schools earn about $98K. Lawyers at "Tier 4" schools (for example, North Carolina Central University, Ohio Northern University, Texas Wesleyan University, and Whittier Law School) earn an average of about $63K. We know, therefore, that there is a large wage premium associated with going to a higher ranked law school. The goal for the rest of the paper is to decompose this premium into a part attributable to observable characteristics, part due to unobservable characteristics that people have before attending law school, and a causal effect provided by attending a top 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-square. Women in the sample earn approximately 14% 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. If we thought these control variables were sufficient to control for human capital differences among the lawyers (which we do not), we could conclude that the average differences in lawyer pay across law schools measured in Column 2 is entirely caused by the law schools.

The specification in column 3 adds several controls for family background and the way the

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difficult to interpret, we report the OLS results instead.

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

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 whether the person went to a public law school, undergraduate GPA (indicators for 3.75-4, 3.5-3.74, etc.), 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), and an indicator variable for other graduate degrees. Once again, the additional control variables matter and help explain the variation in lawyer pay. For example, lawyers in the top decile of their undergraduate class earn 8% more, on average, than other lawyers. This set of control variables is not ideal, as we would also like to know information such as the quality of the undergraduate school and the lawyer's LSAT scores. Nonetheless, it is noteworthy that, even with all these control variables, lawyers from Top 10 schools still earn 20% more than those at schools ranked 11-20. The control variables have even a smaller effect at lower law school tiers, where we might have expected the characteristics to be very different than those of lawyers from top schools. While the results in Table 4 do not necessarily mean that there is a large causal effect of attending a selective law school, they certainly do not rule out this possibility.

Table 5 limits the analysis to lawyers from Top 20 schools in order to focus the analysis 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 4 (that the control variables have linear and constant effects for the whole AJD sample) 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 5 are almost identical to those in Table 4. Controls for family background and how lawyers paid for law school lower the top school premium by about one tenth while, in this limited sample, prior academic success does not affect the results. This is further evidence consistent with school prestige having a causal effect on pay, though still certainly not definitive proof.

Figure 2 graphically represents the relation between income and class rank in law school. Class

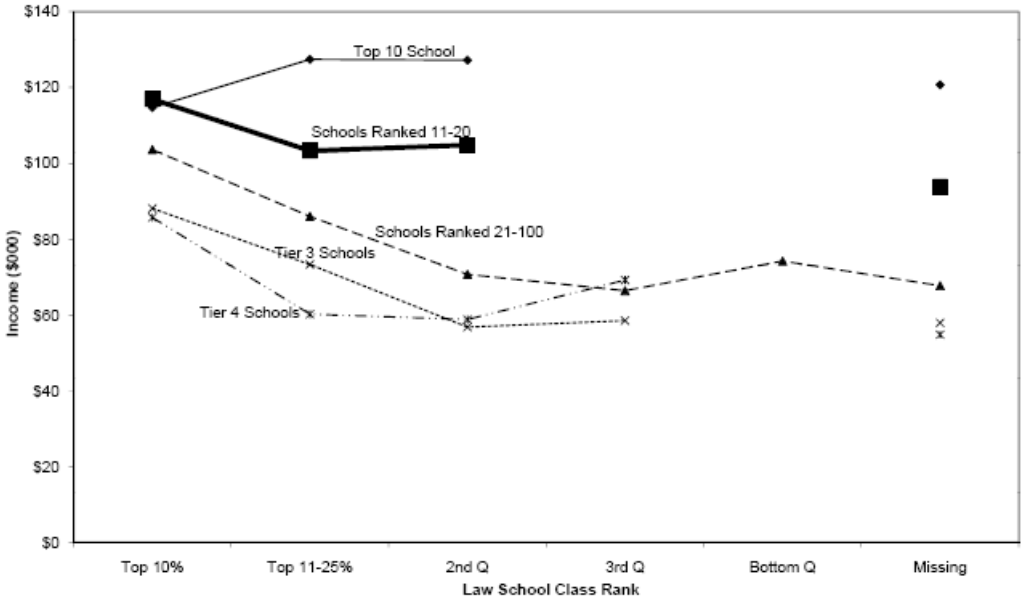

Figure 2: **Income by School Quality and Class Rank.** Class Rank and income are both self-reported in AJD survey.

rank is self-reported, and may be subject to considerable recall bias.[13] 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 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.[14] 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, Table 6 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. Black and Smith (2004) use the same data, though they use the 1998

[13] In 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.

[14] The 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.

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 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.[15] 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.

As column 1 of Table 6 shows, college quality does not appear to be an important determinant of pay in this sample. The average person in the highest tier of colleges (which is the excluded category in the regression) 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. Adding controls for gender, race (black and Hispanic indicator variables), age, and marital status in column 2 actually raises the premium for going to a top school relative to a second tier school, though the difference is not significant.[16] These controls have a bigger effect on the coefficients for lower-ranked schools. When all the controls in column 4 are included, which makes the NLSY specification comparable to the full set of controls used in column 4 of Table 4 for lawyers, the school quality/income relationship is much smaller than the specification in column 1 without controls and the controls added in columns 2-4 have a noticeably larger effect on the undergraduate school quality coefficients than they have on the law school quality coefficients.

Tables 4 and 6 provide at least circumstantial evidence consistent with law school quality having a much 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

[15] The 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.

[16] The "female" coefficient 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 about -0.12.

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 cannot draw too strong a conclusion at this point given differences in composition of the NLSY and AJD samples. Also, column 5 of Table 6 adds controls for ability that are much better than those available for our lawyer sample. Specifically, we add controls for SATs and for a basic intelligence test (the AFQT) and find that this cuts the school quality coefficients from column 4 roughly in half. It could well be that finer controls for lawyer intelligence, such as LSAT scores and measures of where the lawyer went to undergraduate school, would lower our estimates of the law school quality/income relationship in Table 4.

Our analysis thus far is limited by the fact that the AJD covers only new attorneys and by the lack of information about LSATs and undergraduate grades. 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. 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 (2009) 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 partners at these firms. The Tier 3 and 4 fractions were 1.6% and 1.1%, respectively.

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 4. 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 4. 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, undergraduate quality, and undergraduate GPA simultaneously with the other datasets.[17] 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-square 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 rather than within law schools. However, these results provide at least a bit of evidence that LSAT scores, undergraduate school quality, and undergraduate success 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

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

sector, as in-house counsel, or other jobs law graduates take.[18] 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. The coefficients in the table are the marginal effect of a one unit change in the explanatory variable, so the coefficient of -0.071 on "Rank 11-20" in column 1 means that graduates of Top 11-20 schools are 7 percentage points less likely to hold one of these top jobs than a Top 10 graduate when we do not include any controls. That is a very large difference, given that approximately 13% 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 third of the top job/top school relationship can be explained with controls for pre-law-school academic history. Top 10 graduates have a 4.6 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 4 which supports the hypothesis that the effect of school selectivity on earnings comes through sorting lawyers into higher paying firms.[19]

4 Using Observables to Assess Selection Bias

We now follow Altonji et al. (2005) and consider how the relationship between unobservable factors, the schools lawyers attend, and their future careers may affect the interpretation of our results.[20] 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:

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

[18] UCLA 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.

[19] In unreported probits, we find similar effects if the dependent variable equals one for lawyers working at 100+ lawyer firms in any geographic market except that the addition of control variables has less of an effect on the school quality measures.

[20] We know of no other work that uses the methods in Altonji et al. (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.

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

and

$$\begin{bmatrix} \eta \\ \varepsilon \end{bmatrix} \tilde{} N(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}). \tag{5}$$

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 α. As a substitute for that, we now estimate the three equation system above, making various assumptions about the value of ρ. We start with $\rho = 0$, which is what we have been doing to this point and tells us the value of α if the variables in x fully control for the endogeneity of school quality. Then we use bivariate probits 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.

Table 8 presents our results. 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 this coefficient is hardly changed at all by including the controls in column 4 of Table 4. The coefficients remain strongly positive and statistically significant for ρ's of up to 0.2. When we assume ρ is 0.3, 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 8 is 0.2381, which means that the correlation of the disturbances with the full set of controls we have available would have to be just as great as the correlation of the variables themselves if selection fully explains the school prestige relationship with pay. In other words, the controls we have would have to be essentially uncorrelated with the unobservables that are driving both selection and 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 the controls in column 4 of Table 6 (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. In later drafts, we hope to make further use of the methods in Altonji et al. (2005) and include analyses that will allow us to make informed estimates of ρ.

Panel B of Table 8 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.

5 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 spirt to Black and Smith (2004), but the sample and variables are completely different. 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 will analyze each tier of law school (and, when using the NLSY sample, college) separately relative to Top 10 (Top Tier) schools.

As we have shown above, and especially in Table 3, assignment of c is not random and it is difficult to estimate the treatment effect of going to a top school, which, in the notation of equations (3) and (4), can be expressed as $E(y_{c=1} - y_{c=0}|c = 1)$. 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 implement matching using the nearest neighbor method and the Stata programs described in Becker and Ichino (2002).[21] Panel A of Table 9 presents our results for the AJD sample of lawyers. Lawyers from unaccredited schools are sufficiently different in observable characteristics from lawyers at top schools that we do not include unaccredited lawyers in the analysis. The results for the other types are clear – matching does nothing to reduce the estimated effect of going to a more selective law school relative to our OLS estimates with the full set of controls (Column 4 of Table 4). The propensity score matching estimate of the causal effect of going to a Top 10 school on income is roughly 30% and estimates relative to other school tiers are also quite large. These matching results do nothing to suggest that selection drove our earlier large estimates of the causal effect of law school reputation.

Panel B of the same table presents propensity score matching estimates of the returns to college quality measures for the NLSY sample. This serves two purposes. First, comparing the estimates in column 1 of Panel B to those in Column 4 of Table 6 (which includes similar control variables) shows that propensity score matching and OLS lead to similar conclusions in the undergraduate context, as well. This set of control variables for the NLSY is most similar to our full set of control variables for the AJD sample, so it may be the most appropriate comparison for our purposes. These results are consistent with the findings of Black and Smith (2004), at least for men and they provide further confirmation that undergraduate college prestige has relatively little effect on earnings. Also, they provide some level of reassurance about our law school findings because the relationship between the OLS and propensity score matching results are similar for the college and

[21] We have performed similar analyses using alternative matching methods (radius matching, multiple neighbors, 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.

law school samples. Second, column 2 of Table 9, Panel B adds two "intelligence" measures, AFQT and SAT scores, to the NLSY matching analysis. A comparison of these results to those in column 1 shows that the addition of these intelligence measures has only a trivial effect on the coefficients except at the second tier (average SATs between 1041 and 1120) schools and, even at those schools, the change is insignificant. This comparability between column 1 and column 2 suggests that the lack of direct intelligence measures may not be driving our results for law schools and that matching on the types of variables we have in the AJD sample may be sufficient.

6 Interpretation

6.1 Investments by Aspiring Lawyers

We now use these estimates of the financial returns to attending a selective law school for simple thought experiments that show the implications for law school applicants and as an opportunity to consider the plausibility of our estimates. Consider a law school applicant that, with probability one, can get into a law school ranked approximately fiftieth.[22] 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,

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

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 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, that her earnings are 81.8% of these figures if she goes to UCLA (which is based on the -0.201 coefficient in Column 4 of Table 4), and that her earnings are 66.8% of the Stanford level if she goes to a school ranked fiftieth (based on the -0.403 in Column 4 of Table 4).

Column 1 of Table 10 shows that, given these assumptions, our lawyer's expected income with her LSAT of 163 is just over \$4 million if she does not discount future income streams, \$2.4 million if she discounts at 5%, and \$1.54 million if she discounts at 10%.[23] Column 2 shows that our lawyer's chances of getting into a Top 10 or Top 20 school and, therefore, her income are enhanced significantly if she can increase her LSAT scores by 5 points. If she does not discount, five LSAT points are worth over \$700,000 in expectation and nearly \$300,000 if she discounts by the full 10%. The increases are smaller (\$200,000 without discounting and \$80,000 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.

Another thought experiment is to consider an aspiring lawyer trying to maximize her income who has been admitted to Stanford, UCLA, and the fiftieth ranked law school. Is there ever an

[23] We 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.

argument to be had for attending UCLA or the lower ranked school? Without discounting, the premium for Stanford relative to UCLA totals over $1 million and, relative to the lower-ranked school, is nearly $2 million. At 10% discounting, these figures drop to approximately $400K and $750K, respectively. 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. 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. 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.

6.2 Sources of Selective School Premium

If the large premium to going to a selective law school can actually be interpreted as a causal effect, we can only speculate on the underlying drivers of the effect. One possibility is that the top schools actually produce, on average, more value added for their students. While this could be the case, it is hard to figure out why the law school effect appears to be larger than the effect of going to a selective undergraduate school. If top schools really add much more value, it must be the case that the quality of education drops off much more steeply as one drops down the law school distribution than as one drops down the college distribution. One contributing factor could be that teaching is generally taken much more seriously at top law schools (and other professional schools) than at top undergraduate programs. Alternatively, the network effects of learning with higher ability classmates may be more valuable in law school.

The large effect of law school reputation could also be due to an information problem on the part of employers that hire lawyers. Perhaps, even with such information as grades and honors, firms cannot easily distinguish the ability of law students and they rely on the reputation of the law school as the primary signal of the applicant's ability. This 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 future drafts of this paper, we will be able to test this idea using the second wave of the AJD survey which was conducted seven years after the lawyers first passed the bar.

7 Conclusions

We used a representative sample of lawyers that first passed the bar in 2000 to show that, as of 2002, 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 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. Also, propensity score matching did not provide evidence that selection was a large contributing factor to our estimates of the school prestige relationship to labor market outcomes. We cannot say with certainty that we have identified the causal effect of attending a highly ranked law school, but our results are all consistent with there being a large positive wage effect on going to a top school. At the least, we can safely conclude that either there is a large causal return to attending a highly ranked law school or the effect of selection on unobservables is much more important for law school admissions than it is for undergraduate admissions.

In addition to further refining our current analysis (especially by refining our analysis of the relationship between observables and unobservables in Section 4), we plan to use future waves of the AJD survey to make broader and stronger conclusions about the effect of law school prestige. Wave 2 of the AJD surveyed the lawyers from wave 1 in 2007 and 2008 and will be available soon. We will perform similar analyses to those in this paper to see if the law school prestige effects are similar for lawyers with seven years of experience to those of the lawyers with two years of experience. If the AJD is able to follow through on its hopes to launch a third wave, we hope to look at even more seasoned lawyers and to include some questions on the third questionnaire that will allow us to better correct for selection into top law schools.

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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 1: Previous Findings on the Returns to Attending a Selective College

Panel A: AJD Lawyers in 2002			
	All	Top 10 Law School	11-20 Law School
Female	0.5076	0.5174	0.51111
Age	30.536	29.542	29.719
	(3.602)	(2.789)	(2.960)
% from Fam/Friends	0.163	0.270	0.183
	(0.289)	(0.365)	(0.305)
Mother > HS Educ	0.7369	0.82587	0.80597
Live near Mother	0.38586	0.3184	0.36296
Undergrad GPA>3.5	0.468	0.819	0.578
Annual Pay	$81.2K	$122.9K	$98.4K
	(46.2K)	(44.0K)	(43.6K)
Large Firm/Big Mkt	0.2724	0.7065	0.4741
N	2,037	201	270

Panel B: NLSY in 1990			
	All	Top Tier College	2nd Tier College
Female	0.518	0.500	0.436
Age	29.609	29.440	29.173
	(2.266)	(2.310)	(2.284)
Financial Aid	0.534	0.546	0.495
Mother's Education	12.696	14.030	12.771
	(2.896)	(2.978)	(2.354)
Moved since age 14	0.664	0.728	0.728
SAT %ile	0.576	0.803	0.652
	(0.271)	(0.208)	(0.223)
Years of Education	16.149	16.917	16.329
	(1.365)	(1.445)	(1.274)
Annual Pay	$25.8K	$29.6K	$26.9K
	(13.2K)	(16.2K)	(12.3K)
N	1,767	156	225

Table 2: Summary Statistics

Panel A: AJD Lawyers in 2002			
	Top 10 Logit	Top 10 Logit	OLS: *US News* category
Female	-0.013	-0.025	-0.077
	(0.011)	(0.047)	(0.048)
Minority	0.073	0.104	0.403
	(0.015)	(0.050)	(0.053)
Parent with College Degree	0.053	0.124	0.355
	(0.010)	(0.054)	(0.052)
Undergrad GPA>3.5	0.143	0.301	0.650
	(0.013)	(0.044)	(0.048)
Undergrad Science/Business	0.007	0.057	-0.023
Major	(0.010)	(0.046)	(0.048)
N	2,208	497	2,208

Panel B: NLSY in 1990			
	Top Tier Logit	Top Tier Logit	OLS: Tier
Female	0.003	0.080	0.006
	(0.012)	(0.052)	(0.016)
Minority	0.000	0.119	0.001
	(0.015)	(0.070)	(0.013)
Mother's Education	0.007	0.026	0.008
	(0.002)	(0.008)	(0.002)
Top 10% of HS Class	0.053	0.118	0.071
	(0.020)	(0.065)	(0.019)
SAT %ile	0.050	0.142	0.047
	(0.008)	(0.038)	(0.008)
N	1,767	381	1,767

Table 3: Selection into Top Schools. Panel A uses AJD sample. 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, 3 if he/she went to a school ranked 21-100, 4 if the school is a US News and World Report Tier 3 school, 5 if it is a US News and World Report Tier 4 school, and 6 if it is unaccredited. Panel B uses the NLSY sample. 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)
Rank 11-20	-0.250	-0.253	-0.244	-0.201
	(0.044)	(0.044)	(0.044)	(0.044)
Rank 21-100	-0.477	-0.481	-0.475	-0.403
	(0.037)	(0.037)	(0.037)	(0.039)
Tier 3	-0.630	-0.621	-0.611	-0.594
	(0.042)	(0.042)	(0.044)	(0.045)
Tier 4	-0.687	-0.681	-0.670	-0.659
	(0.045)	(0.045)	(0.046)	(0.048)
Unaccredited	-0.650	-0.692	-0.679	-0.661
	(0.097)	(0.098)	(0.099)	(0.098)
Female		-0.135	-0.135	-0.116
		(0.021)	(0.021)	(0.022)
Near Mother			-0.036	-0.040
			(0.022)	(0.021)
Undergrad Top 10%				0.080
				(0.036)
Controls				
Demographic	no	yes	yes	yes
Family Background	no	no	yes	yes
School Funding	no	no	yes	yes
Academic History	no	no	no	yes
R-square	0.146	0.171	0.182	0.218
N	2,037	2,037	2,037	2,037

Table 4: Lawyer Pay Regressions. OLS – Dependent Variable is Log of annual pay. Sample is cross-sectional AJD sample in 2002 of lawyers who first passed Bar Exam in 2000. "Rank 11-20" through "Unaccredited" are based on 2003 US News and World Report rankings. The excluded category is schools ranked in the Top 10. "Near Mother" is an indicator variable for living within 50 miles of respondent's mother. "Undergrad Top 10%" is a self-reported indicator variable of whether the person was in the top decile of her undergraduate class.

	(1)	(2)	(3)	(4)
Rank 11-20	-0.250	-0.251	-0.227	-0.228
	(0.048)	(0.048)	(0.049)	(0.053)
Female		-0.182	-0.192	-0.176
		(0.049)	(0.050)	(0.050)
Near Mother			-0.080	-0.080
			(0.052)	(0.051)
Undergrad Top 10%				0.022
				(0.069)
Controls				
Demographic	no	yes	yes	yes
Family Background	no	no	yes	yes
School Funding	no	no	yes	yes
Academic History	no	no	no	yes
R-Square	0.0553	0.0882	0.1307	0.1979
N	471	471	471	471

Table 5: Top School Lawyer Pay Regressions. Same analysis as Table 4, except limited to graduates of top 20 law schools.

	(1)	(2)	(3)	(4)	(5)
SAT 1041-1120	-0.061	-0.087	-0.072	-0.052	-0.032
	(0.079)	(0.078)	(0.078)	(0.079)	(0.079)
SAT 841-1040	-0.113	-0.095	-0.068	-0.047	-0.011
	(0.067)	(0.065)	(0.066)	(0.068)	(0.069)
SAT 771-840	-0.240	-0.217	-0.184	-0.160	-0.107
	(0.075)	(0.073)	(0.075)	(0.078)	(0.080)
SAT <=770	-0.214	-0.174	-0.132	-0.108	-0.046
	(0.089)	(0.089)	(0.091)	(0.094)	(0.095)
Female		-0.350	-0.347	-0.343	-0.326
		(0.036)	(0.036)	(0.037)	(0.037)
Move since Age 14			0.046	0.052	0.050
			(0.039)	(0.039)	(0.039)
Financial Aid			-0.041	-0.038	-0.040
			(0.038)	(0.039)	(0.039)
GPA				0.045	0.032
				(0.034)	(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

Table 6: 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)	(3)	(4)
Rank 11-20	-0.071	-0.069	-0.064	-0.046
	(0.013)	(0.013)	(0.012)	(0.014)
Rank 21-100	-0.209	-0.203	-0.196	-0.159
	(0.020)	(0.020)	(0.020)	(0.020)
Tier 3	-0.152	-0.148	-0.140	-0.131
	(0.010)	(0.010)	(0.010)	(0.011)
Tier 4	-0.148	-0.144	-0.136	-0.127
	(0.009)	(0.009)	(0.009)	(0.009)
Female		0.001	0.001	0.004
		(0.013)	(0.013)	(0.013)
Near Mother			-0.029	-0.030
			(0.013)	(0.012)
Undergrad Top 10%				0.023
				(0.022)
Controls				
Demographic	no	yes	yes	yes
Family Background	no	no	yes	yes
School Funding	no	no	yes	yes
Academic History	no	no	no	yes
	0.148	0.156	0.179	0.214
	2,208	2,208	2,208	2,208

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 12.9% of the sample. Unaccredited schools are dropped because none in the sample work work at a 100+ lawyer firm in one of the relevant markets. 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.

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	1.170	0.541	0.101
	(0.118)	(0.101)	(0.085)	(0.131)
$\rho = 0$, full controls	0.600	1.119	0.527	0.038
	(0.133)	(0.113)	(0.087)	(0.158)
$\rho = 0.1$	0.433	0.946	0.353	-0.128
	(0.132)	(0.113)	(0.087)	(0.157)
$\rho = 0.2$	0.263	0.767	0.178	-0.294
	(0.131)	(0.112)	(0.086)	(0.156)
$\rho = 0.3$	0.089	0.581	0.001	-0.459
	(0.129)	(0.110)	(0.084)	(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	1.188	0.671
	(0.118)	(0.102)	(0.097)
$\rho = 0$, full controls	0.409	1.135	0.665
	(0.130)	(0.120)	(0.102)
$\rho = 0.1$	0.242	0.957	0.491
	(0.130)	(0.119)	(0.102)
$\rho = 0.2$	0.074	0.775	0.317
	(0.129)	(0.118)	(0.101)
$\rho = 0.3$	-0.097	0.587	0.141
	(0.126)	(0.116)	(0.099)

Table 8: 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. Control variables in all but the first row of each panel include age (five-year indicators), fraction of law school paid for by savings and family, and indicator variables for minority, female, married, living within 50 miles of lawyer's mother, mother born outside U.S., one of parents is a lawyer, public law school, in top 10GPA was 3.75 or higher, and undergraduate major was humanities or missing.

Panel A: AJD	
Rank 11-20	-0.298
	(0.076)
Rank 21-100	-0.308
	(0.057)
Tier 3	-0.671
	(0.078)
Tier 4	-0.611
	(0.083)

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

Table 9: Propensity Score Matching Results. Panel A matches lawyers from Top 10 schools with lawyers from each of the other categories in separate analyses. The sample and specification is similar to column 4 of Table 4, except some covariates had to be dropped to insure convergence. Panel B 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 Table 5, with covariates dropped as needed.

	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%	81.8%
Prob(attend) #50 School	92.4%	25.0%	8.0%%	66.8%
Exp. Income, No Discount	$4.12 mm	$4.84 mm	$5.04 mm	
Exp. Income, 5% Discount	$2.40 mm	$2.82 mm	$2.94 mm	
Exp. Income, 10% Discount	$1.54 mm	$1.81 mm	$1.89 mm	

Table 10: 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.