

**Educational credentials, hiring, and intra-occupational inequality:
Evidence from law firm dissolutions.**

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ABSTRACT

Motivated by the relationship between educational credentials and intra-occupational inequality, this study examines how human capital and social capital mechanisms influence organizational hiring. Treating six U.S. law firm dissolutions as mobility quasi-experiments, I analyze 1,426 lawyers' post-dissolution labor market outcomes and establish two key findings. First, the most rewarding employers (i.e., high prestige and high profitability) hired lawyers who graduated from the most prestigious law schools but this tendency weakened with one's experience. Second, individuals were typically hired by organizations that employed more former classmates but the most rewarding employers were least likely to embed employment relationships in these alumni networks. These results imply that educational credentials influence inequality not only by distributing individuals across employers based on signals but, also, by reproducing the distribution through networks.

Occupations are central to sociological accounts of inequality (e.g., Blau and Duncan, 1967; Parkin, 1971; Spilerman, 1977). Recent research (Mouw and Kalleberg, 2010) attributes rising inequality to just a few occupations and advocates more intra-occupational studies to complement recent inter-occupational work that establishes occupational boundaries as barriers to socioeconomic mobility (e.g., Massey and Hirst, 1998; Weeden, 2002; Wright and Dwyer, 2003; Autor, Katz, and Kearney, 2006). Given recent growth in the right tail of the income distribution (Piketty and Saez, 2003, 2006; McCall and Percheski, 2010), studies of high-income occupations are particularly timely.

Within occupations, a large body of work in organizational sociology examines how employers structure inequality by matching individuals to jobs (Pfeffer, 1977; Baron and Bielby, 1980; Granovetter, 1981; Stewman and Konda, 1983; Barnett, Baron, and Stuart, 2000; Petersen and Saporta, 2004; Reskin and Bielby, 2005; Kalev, 2009; Bidwell, Briscoe, Fernandez-Mateo, and Sterling, 2013; Cohen and Broschak, 2013). Generally, this work demonstrates that organizational hiring criteria govern the provision of employment opportunities tied to unequal socioeconomic rewards like earnings and prestige.

One particularly important organizational criterion for employment in high-income occupations is educational credentials – quality indicators associated with educational institution attended (Ishida, Spilerman, and Su, 1997). Although sociological research documents that institutions of higher education serve a variety of purposes (see Stevens, Armstrong, and Arum, 2008), two primary functions are particularly relevant to the relationship between credentials and inequality. First, because educational attainment is often considered an indicator of technical competence, educational credentials are often the basis for claims to prestigious positions (Weber, 1968). Second, because educational institutions socialize individuals, credentials are

also a basis for membership in social groups that regulate access to positions of varying socioeconomic status (e.g., Durkheim, 1922; Collins, 1971, 1979). In short, educational institutions influence socioeconomic attainment by providing individuals with both human capital and social capital (Coleman, 1988). Motivated by these insights, this study specifically examines how organizational hiring based on educational credentials influences inequality within a single, high-paying occupation: lawyer. Two empirical observations frame the inquiry.

First, prior work documents a strong positive association between educational prestige and socioeconomic attainment (Solmon, 1975; Tinto, 1980; Trusheim and Crouse, 1981; Karabell and McClelland, 1987; James, Alsalam, Conaty, and To, 1989; Kingston and Smart, 1990; Ishida, et al., 1997). Graduates of prestigious institutions have long populated the corporate elite (Warner and Abegglen, 1955; Klitgaard, 1985; Useem and Karabel, 1986; Rivera, 2011). Moreover, because organizational leaders generally employ socially similar individuals (Collins, 1971; Kingston and Clawson, 1990; Rivera, 2012), educational prestige often governs opportunities to labor for the most rewarding organizations. This link between educational prestige and attainment is, therefore, central to a growing literature that implicates the horizontal stratification of postsecondary institutions (i.e., institutional quality distinctions) in the production of socioeconomic inequality (see Gerber and Cheung, 2008 for a review).

Second, many cast higher education institutions as social “sieves” that regulate access to socioeconomic opportunity (Sorokin, 1959[1927]; Blau and Duncan, 1967; Jencks and Riesman, 1968). This metaphor suggests that socioeconomic rewards are allocated not only on the basis of educational prestige but, more specifically, with institution attended. Generally, individuals systematically “sort into” (or are “matched to”) employers based not on prestige but, rather, on degrees granted by specific educational institutions. For example, analyses of both lawyers and

investors demonstrate that two employees of the same organization are substantially more likely to be graduates of the same school than two employees randomly sampled from similar organizations (Parkin, 2006; Oyer and Schaefer, 2010; Rider, 2012). These studies imply an institution-specific element of occupational stratification.

Why do individuals sort into organizations based on educational credentials? Generally, selective hiring or selective retention of employees based on human capital and/or social capital can sort individuals into employers by school attended. But, isolating these mechanisms is challenging for many reasons (see Gerber and Cheung, 2008: 301). Focusing on hiring and reserving selective retention for future research, this study employs a research design that disentangles sorting mechanisms that clearly relate hiring tendencies to intra-occupational inequality. Importantly, this design can account for both the intra-occupational tendency of prestigious institutions' graduates to be employed at the most rewarding employers and for the tendency of two graduates of the same institution to be employed by the same organization.

Human capital may sort individuals into employers based on two mechanisms related to educational prestige. First, if individual skill is positively correlated with educational prestige and skill-based productivity is increasing with employer prestige then the most skilled individuals will probably be most rewarded by the most prestigious employers. This "skill" mechanism implies positive assortative matching of individuals and employers based on prestige (Becker, 1973). Second,), independent of the actual correlation between skill and prestige, sorting might also be observed if both supply and demand sides of the labor market treat prestige as reliable indicators of human capital (the "signaling" mechanism (Spence, 1973). Importantly, as Ishida, et al. (1997) suggest, signals should exert the strongest influences in the labor market

for inexperienced individuals. Evaluating how experience moderates the effect of credentials on hiring can, therefore, reconcile these two human capital mechanisms (i.e., skill versus signal).

Alternatively, social capital might also sort individuals into employers based on institution-specific educational credentials. First, because labor market categories reflect prevailing beliefs about skills (e.g., Zuckerman, Kim, Ukanwa, and von Rittman, 2003) employers might view some institutions' graduates as categorically more suitable for employment than other institutions' graduates (the "identity" mechanism). Second, organizations often embed hiring in employees' social networks (Granovetter, 1973; Fernandez and Weinberg, 1997). This "network" mechanism implies that employers might tend to hire employees' former classmates. To disentangle these possibilities, I leverage time of institutional attendance because, presumably, the formation of social ties depends more upon inter-personal interaction than does the sharing of an identity (Feld, 1982; Mael and Ashforth, 1992).

To be clear, the two human capital mechanisms imply relative prestige distinctions (e.g., Ivy League schools and public universities) but the two social capital mechanisms imply institution-specific distinctions (e.g., Yale and Ohio State). Ishida, et al. (1997: 868) emphasize that "it is important to separate the effect of a particular school from the effect of the school's position in a ranking of institutions." By elaborating the implications of skill, signal, identity, and network mechanisms for hiring within a single occupation, I aim to produce clear inferences about how educational credentials influence intra-occupational equality.

Inequality in Context: U.S. Corporate Legal Services.

At the organizational level, data from *The American Lawyer's* annual report on financial performance for the 100 highest-grossing U.S. law firms reveal intra-occupational inequality for

lawyers. These firms primarily represent large, corporate clients and distribute profits to senior lawyers (Heinz, Nelson, and Laumann, 2001). Adjusting for price effects using the U.S. Bureau of Labor Statistics' Consumer Price Index (base year = 1984), *Figure 1* summarizes trends in firm profitability (i.e., profits per equity partner in real dollars) over the past two decades for the 10th, 50th, and 90th percentile of the 100 most profitable U.S. law firms. If one assumes that socioeconomic rewards are positively correlated with employer profitability based on the fact that law firm partners are typically residual claimants on firm profits and tendency of more profitable firms to pay large associate bonuses than less profitable firms, then inequality in socioeconomic rewards increased substantially between 1987 and 2010; the ratio of the 90th percentile firm's profitability to that of the 10th percentile firm increased from 2.5 to 3.5 (the 50th to 10th ratio increased from 1.3 to 1.8). Moreover, these figures probably understate inequality; disparities in prestige and in income for lawyers employed by these 100 highest-grossing law firms versus other lawyers is even greater (e.g., Heinz and Laumann, 1982; Heinz, Nelson, and Laumann, 2001).

Insert Figure 1About Here

At the lawyer level, the relationship between educational credentials and intra-occupational inequality is also clear. For example, one analysis of a large, representative dataset of junior lawyers indicates that graduates of the ten law schools ranked as most prestigious by *U.S. News & World Report* earn, on average, 25 percent more than graduates of the schools ranked 11th through 20th and 50 percent more than graduates of the schools ranked 21st to 100th (Oyer and Schaefer, 2012). The authors also found that educational prestige was positively correlated with the likelihood that a lawyer worked for one of the most rewarding law firms – large firms in New York, Washington D.C., Chicago, and Los Angeles. This study suggests that

intra-occupational inequality may be largely attributable to the sorting of law school graduates into employers on the basis of educational credentials.

Integrating firm-level performance data with *Martindale-Hubbell* directory data completes the logical loop on the relationship between educational credentials and the allocation of socioeconomic rewards. *Figure 2* depicts predicted values of firm profitability and firm-level educational prestige (i.e., mean numeric *U.S. News & World Report* law school rank for all firm lawyers), obtained from ordinary least squares regressions of each metric, respectively, on law firm prestige (i.e., *Vault* prestige rating) in 2008.¹ More prestigious employers are generally more profitable than less prestigious ones and graduates of the most prestigious law schools are disproportionately employed by the occupation's most rewarding employers.

Together, the figures derived from these data illustrate that socioeconomic rewards like earnings and intra-occupational prestige accrue disproportionately to the most prestigious law schools' graduates. Moreover, the inter-organizational distribution of rewards has become increasingly inequitable over time, favoring the partners of prestigious firms that employ graduates of prestigious law schools. One can reasonably infer that the horizontal stratification of law schools contributes to inequality by sorting lawyers into employment at firms that offer unequal socioeconomic rewards (Abbott, 1981; Phillips and Zuckerman, 2001). But, *why* are educational credentials so central to the allocation of earnings and prestige?

Insert Figure 2About Here

One possible explanation for the inequality observed in these data is that firms restrict entry-level recruiting to certain law schools. For example, an analysis of recruiting data from the National Association of Legal Placement (NALP) indicates that on average, the 1,611 legal

¹ Alternative years produce similar predicted values.

employers listed in the 2009 NALP directory recruited associates from only 12.1 law schools each.² But, several empirical observations run counter to the intuitively appealing explanation that the relationship between educational credentials and socioeconomic rewards is solely attributable to firm-level, campus recruiting practices for entry-level lawyers and/or the selective retention of lawyers based on law school attended.

First, the most prestigious firms – those ranked in the *Vault Top 100 Law Firms* (the “Vault 100”) – recruit from more law schools than firms that were not ranked in the Vault 100 (16.4 schools versus 6.1 schools; $p < 0.01$). So, the occupation’s more rewarding employers draw employees from a larger number of law schools than do less rewarding employers. Second, although it is true that the most prestigious law firms tend to recruit from the most prestigious law schools, Oyer and Schaefer’s (2012) analysis of law firms and lawyers reveals that many prestigious firms’ partners graduated from law schools not considered elite. Consequently, they observe that “it is possible to reach the pinnacle of this field without attending an elite law school.”

Third, the average associate-to-partner ratio at *The American Lawyer*’s 200 highest-grossing U.S. law firms in 2010 was 3.0. Because law firms typically promote only a fraction of associates to the partner level (Galanter and Palay, 1991), most junior lawyers change employers early in their legal careers. Fourth, analyses of lateral partner hiring reveal that the rate at which U.S. law firms hire senior lawyers from other firms also increased dramatically in the past decade (Rider and Tan, 2013). As in other occupations (e.g., Bidwell and Briscoe, 2010), the increasingly inter-organizational legal career implies that the distribution of educational credentials across employers is increasingly less sensitive to sorting into employers at occupational entry based on law school attended. A proper sorting explanation should, therefore,

² Results of this analysis are available for the author.

account for both entry-level hiring and subsequent inter-organizational employment transitions. But, for reasons described below, analyses of inter-organizational are very challenging.

Identifying Causal Mechanisms

Given this study's theoretical aims, it is imperative to study a sample that is representative of an occupation's human and social capital, as opposed to focusing exclusively on new entrants or on experienced incumbents. Inter-organizational career transitions provide one opportunity to examine how human capital and social capital mechanisms influence organizational hiring for an occupational sample that is diverse with regards to experience and education. But, the primary challenge to analyzing such employment transitions is that one must account for the possibility that both human capital and social capital jointly influence the likelihood that an individual changes employers at any given time as well as the socioeconomic rewards attained by doing so.

To illustrate this point, consider two very different possibilities supported by prior social capital research. On the one hand, if high quality networks provide individuals with access to rewarding job opportunities that elevate their hazard of voluntary turnover (Granovetter, 1973, 1974[1995]; McPherson, Popielarz, and Drobnic, 1992), then individuals with high quality networks may be over-represented in labor markets. On the other hand, if networks also influence individual performance evaluations (Podolny and Baron, 1997; Mizruchi and Stearns, 2001; Burt, 1992) and the likelihood of organizational exit (Krackhardt and Porter, 1986; McPherson, et al., 1992), then individuals with high quality networks may be retained by employers at higher rates than those with low quality networks. These two possibilities imply

that labor markets are heavily populated by individuals with high and low quality networks but few of moderate quality. Such samples, of course, are not representative of an occupation.

Similar empirical challenges render credible inferences on how human capital influences attainment elusive. If individuals accumulate human capital through work experience and employers hire and retain individuals based on their accumulated human capital, then it seems implausible that job-switcher samples will be representative of an occupation. But, extant research offers limited insight into the distribution of either human capital or social capital for sub-samples of individuals who do and do not change employers. Consequently, conventional mobility studies of job-switchers are unlikely to clearly disentangle how human capital and social capital mechanisms sort individuals into employers based on educational credentials.

A mobility field experiment could address these inferential issues. For example, a representative sample of individuals within an occupation could be randomly selected for dismissal from their current positions. One could then follow the dismissed group's transitions to subsequent employers and evaluate their labor market outcomes, relative to those not selected for dismissal, based on their educational credentials. But, such experiments are neither practical nor desirable, given the lasting negative effects of unemployment (e.g., Gangl, 2006).

Equally advantageous from an analytical perspective, and certainly superior from a social welfare perspective, is a quasi-experiment in which many individuals are simultaneously displaced from their employers for reasons independent of their human or social capital. In this scenario, one could capitalize on the expectation that, from the perspective of a hiring organization, displacement does not convey negative information about an individual's expected productivity in the same way as dismissal (Gibbons and Katz, 1991). In this way, holding constant the cause of inter-organizational mobility for a representative sample of individuals

within a single occupation, one could reasonably draw credible inferences about attainment mechanisms because there is not a strong *a priori* reason to expect a strong correlation between the timing of displacement and either human or social capital.

This study's research design approximates this scenario for over 1,400 lawyers who lost their jobs due to the surprising failures of six large U.S. law firms in 2008 and 2009. The sample is probably representative of lawyers employed by large, corporate-oriented law firms, as the lawyers range from first-year associates to partners with decades of experience. Their firms represented clients whose primary lines of business were hurt most by the economic downturn: mortgage-backed securities, real estate, construction, and other financial services. Their efforts to regain post-failure employment probably depended largely on their human and social capital. Most importantly, their labor market participation was largely independent of their networks, ability, or job performance. For the most part, they were simply employed by the wrong organization at the wrong time. Below, I develop precise theoretical implications of skill, signaling, identity, and network mechanisms on these lawyers' labor market outcomes.³

Educational Credentials and Human Capital

Socioeconomic attainment may vary with educational credentials for several reasons (Useem and Karabel, 1986; Ishida, et al., 1997). For example, some researchers consider the educational prestige of an organization's employees to be a clear indicator of organizational prestige (Phillips and Zuckerman, 2001). But, such relationships are the outcome of a labor market sorting process. For example, many employers screen candidates on the basis of educational credentials because of implicit beliefs about ability, prestige, and cultural fit between

³ These mechanisms could also sort individual via rates of exit out of – not entry into – organizations (“selective retention”). Although this study's research design isolates selective hiring effects, a research agenda for retention effects is discussed later.

employee and employer (e.g., Brown, 2001; Rivera, 2012b). But, educational institutions also screen applicants on many of the same characteristics that employers consider indicators of cultural fit (see Stevens, et al., 2008: 129-131). Consequently, despite extensive research on the relationship between education and labor market outcomes (see Gerber and Cheung, 2008: 301-305), ambiguity underlies the relationships among credentials, hiring, and attainment.

That socioeconomic attainment is positively correlated with educational prestige is widely acknowledged (e.g., Becker, 1964; Karabel and Astin, 1975; Kanter, 1977; Collins, 1979). But, the relative contributions of human capital and social capital to that correlation remain unclear. Focusing specifically on how human capital and social capital mechanisms influence organizational hiring can help clarify the relationship. Why are graduates of the most prestigious schools disproportionately employed by prestigious employers?

Two human capital mechanisms can account for influences of organizational hiring on the relationship between credentials and attainment: skills and signals. Ishida, et al. (1997) note that human capital may be positively correlated with educational prestige because cognitive and/or non-cognitive skills are acquired more effectively by students of more prestigious higher education institutions than by students of less prestigious ones (Becker, 1964; Mincer, 1974; Wise, 1975). Both pecuniary and non-pecuniary benefits may be increasing with employer prestige because research on organizational status suggests that both cost and price advantages in product markets provide prestigious employers with advantages in labor markets (e.g., Podolny, 1993; Benjamin and Podolny, 1999; Podolny, 2001). If the most rewarding employers tend to hire the most skilled individuals, then this skill mechanism can produce a positive correlation between educational prestige and socioeconomic attainment.

The signal mechanism implies that employers operate according to the belief that educational prestige is a reliable correlate of one's otherwise difficult or costly to observe human capital (Spence, 1973). Valuing skill but lacking a superior indicator of skill, employers might believe that the cost of acquiring prestige is decreasing with skill and, therefore, treat educational prestige as a reliable signal of skill. If the most rewarding employers aim to hire the most skilled individuals then this signal mechanism can also account for a positive correlation between prestigious credentials and socioeconomic attainment.

Both the skill and signal mechanisms imply that each skill-seeking employer will hire the candidate with the most educational prestige that is not hired by more rewarding employers. If so, then the most rewarding employers will hire the most prestigious candidates while less rewarding employers will hire the most prestigious candidates from the lower prestige tiers of the candidate pool. In economic terms, this line of reasoning implies positive assortative matching of candidate educational prestige and employer prestige (Becker, 1973). This leads to a testable prediction.

Hypothesis 1: The more similar the employer's prestige and the candidate's educational prestige, the more likely the employer hires the candidate.

Both human capital mechanisms motivate the same baseline prediction, but the two can be disentangled. The signal mechanism implies that educational prestige is less relevant to hiring the more work experience a candidate possesses. As Ishida, et al. (1997: 868) state, the signaling effect of educational credentials "...should diminish over time as other measures of productivity and performance become available." If so, then signaling effects should be weaker the more experienced an individual is.

The implications of experience for the skill mechanism are less clear. Some suggest that human capital accelerates the rate of skill acquisition so that assortative matching tendencies will

be strongest for the most experienced candidates (e.g., Mincer, 1974; Wise, 1975). To disentangle skill and signal mechanisms, it is sufficient to propose more simply that signaling effects are attenuated by experience but that skill effects are not. This logic implies a testable prediction to adjudicate the skill-based and signal-based variations of the human capital account. If signaling effects account for the positive assortative matching of educational prestige and employer prestige, then the individual-organizational differential should influence hiring most strongly for the hiring of the least experienced candidates.

Hypothesis 2: The more experienced the individual is the weaker is the negative relationship between the prestige differential and the likelihood of hiring.

Educational Credentials and Social Capital

There are two social capital mechanisms that might account for the relationship between educational credentials and socioeconomic attainment: identities and networks. The “identity” mechanism implies that employers screen candidates based on expectations of person-organization “fit” (e.g., Chatman, 1991; Rivera, 2011, 2012). Although institution-specific training may account for differences in fit, prior work also indicates that institutional admission criteria like class or ethnicity may also render some institutions’ graduates better “fits” than others (e.g., Karabel and Astin, 1975; Collins, 1979; Karabel, 2005). Regardless of whether the key mechanism is institutional selection or treatment, some institutions’ graduates may be considered better “fits” with a given employer than other institutions’ graduates.

Presumably, retained employees “fit” better with the organization than do departed employees. If institution attended indicates suitability for employment then an employer considering two candidates with equivalent prestige and observable skill is likely to hire the

candidate who attended the same institution as more of the employer's current employees. Therefore, institution-specific identities may also sort individuals into employers.⁴

Alternatively, social capital might also operate through networks of social relationships. Ishida, et al. (1997: 868) suggest that some institutions' graduates are disproportionately represented in the corporate elite because of advantages derived "...directly from informal personal ties among graduates from the same school." In evaluating candidates, many employers alleviate information asymmetries by relying upon employees for referrals (e.g., Fernandez, Castilla, and Moore, 2000; Petersen, Saporta, and Seidel, 2001). Because education is a focused activity (Feld, 1982), "people experience schooling as a thick web of relationships" (Stevens, et al., 2008: 142) that can persist long after graduation and influence one's career (e.g., Suitor and Keeton, 1987; Burt, 2001).

This "networks" mechanism implies that an employer considering two candidates who graduated from the same education institution is likely to hire the candidate who is more socially connected to the organization's employees. The more former classmates at a focal employer, the more likely it is that a focal candidate is socially connected to the organization. This reasoning leads to a testable prediction.

Hypothesis 3: The more employees who graduated from the same institution as a candidate the more likely the employer hires the candidate.

As with human capital, it is important to differentiate these two social capital mechanisms. Time of attendance provides a convenient way to disentangle identity and network effects. Presumably, independent of co-attendance, all of an institution's graduates will share an

⁴ Because identity is based on a social group (e.g., alumni of a specific institution) I treat identity as a social capital. Although others might consider institution attended a form of cultural capital, the distinction between social and cultural capital is less important than the implication that some institutions' graduates are categorically considered better-fitting candidates than other institutions' graduates.

identity. But, it seems likely that social relationships form more often between two people who attended an institution at the same time than between two people who attended at different times. Therefore, the network contacts argument implies that hiring likelihoods vary primarily with those who attended the institution at the same time as the focal individual. Conversely, an identity-based, school-specific preference implies that employees from all attendance periods influence hiring.

Hypothesis 4a: The more employees who attended the same institution at the same time as a candidate the more likely the employer hires the candidate.

Hypothesis 4b: The more employees who attended the same institution at a different time than a candidate the more likely the employer hires the candidate.

EMPIRICAL SETTING AND ANALYSES

The context for testing these arguments is the U.S. legal services industry and, in particular, the large, prestigious law firms that provide legal services to large corporations (Sandefur, 2001; Heinz, Nelson, Sandefur, and Laumann, 2005). Typically, these firms are organized as partnerships where partners generate business, share profits (or losses) and supervise junior lawyers (e.g., associates). A partnership grows as associates are promoted from within the firm or partners are hired laterally from other firms.

Generally, the greater a firm's profits-per-partner the greater is the compensation and intraprofessional prestige of firm employees. *Figure 2* depicts these metrics for the 2008 *Vault Top 100 Law Firms* (the Vault 100), an industry ranking of firm prestige based on annual surveys of thousands of legal professionals. This figure illustrates a positive correlation between educational prestige (i.e., the inverse of school rank) and expected rewards for one's labor (as indicated by firm profitability). Intra-occupational inequality based on educational credentials is evidenced by the lower mean numeric law school ranks of lawyers employed by more

prestigious firms. Because this tendency has been observed over several decades (e.g., Phillips and Zuckerman, 2001) and because *Figure 1* indicates that intra-occupational inequality has increased over the past two decades, this occupation is well-suited for studying the relationship between educational credentials and inequality.

Sample

To examine human capital and social capital influences on intra-occupational inequality, I constructed a sample of lawyers who were displaced by six large law firm dissolutions. In 2008 and 2009, these firms dissolved rather unexpectedly and left over 1,400 lawyers searching for new jobs; each firm's dissolution is detailed in *Appendix 1*. Of course, these dissolutions have idiosyncratic elements but each firm's survival chances probably would have been greater were it not for abnormally poor economic conditions during this time period. The six dissolved firms are Dreier LLP, Heller Ehrman LLP, Morgan & Finnegan LLP; Thacher Proffitt Wood LLP, Thelen LLP, and WolfBlock LLP. These firms represented clients adversely affected by the economic downturn due to their primary lines of business: mortgage-backed securities, real estate, construction, and other financial services.

Importantly, these firms dissolved unexpectedly and fairly quickly. Dissolutions of large law firms are rare (Heinz, 2009), so few employees would have expected their firm to dissolve; mergers are much more common than dissolutions. These firms varied in terms of size, prestige, practice areas, geographic locations, and other key dimensions. So, collectively, these six firms are fairly representative of the U.S. legal services industry.

From the six firms' websites, I identified all 1,459 lawyers employed at time of dissolution to construct the sample summarized in *Table 1*. For 1,426 (97.7 percent) of these

lawyers, I obtained data suitable for analysis from firm website biographies, the Martindale-Hubbell Law Directory (“Martindale-Hubbell”), the West Law Legal Directory (“West Law”), and the Internet Archive. Specifically, I obtained each lawyer’s level (e.g., associate, partner), area(s) of practice, office location, law school attended, and, if available, year in which they passed the bar. I excluded 33 lawyers (2.3 percent) from the analysis because I could not identify the law school they attended and/or the year in which they were admitted to the bar.

Insert Table 1 About Here

I then searched other firms’ website directories, the online version of Martindale-Hubbell, individuals’ LinkedIn profiles, ZoomInfo, and other internet resources to identify post-dissolution employers for 1,248 of the 1,426 lawyers (88 percent). The lawyers in the sample graduated from 120 law schools that vary in terms of prestige and in the geographic distribution of their alumni; nearly 80 percent, though, graduated from one of 35 law schools (see *Table 2a* for details). They regained employment at over 400 organizations in almost 80 cities following their employers’ dissolutions, but almost 80 percent were employed in one of four U.S. Metropolitan Statistical Areas centered on New York City, San Francisco, Philadelphia, or Washington, DC (see *Table 2b*). The data includes information on each individual’s education, title, gender, race, practice area, geographic office location, and experience.

Insert Tables 2a and 2b About Here

Analyses and Dependent Variables

First, to gauge sample representativeness, I use probit models to estimate the likelihood that a lawyer obtains employment and is located by my sampling methods (“employment

analyses”). In these analyses, the dependent variable is coded as 1 for the 1,248 lawyers for whom I could find subsequent employment data and 0 for the 178 lawyers for whom I could not. More specifically, I also model the likelihood that a lawyer is employed by a NLJ 250 firm. This more restrictive dependent variable is coded as 1 for the 933 lawyers who I found to be re-employed by a NLJ 250 firm-office and 0 for all others. Of the 1,426 lawyers in the sample, 933 (or 65 percent) regain employment within the NLJ 250 (75 percent of the “re-employed and located” sub-sample). This more restrictive sub-sample is the basis for the hypothesis tests. The sampling restriction (i.e., NLJ 250 firms) is necessary in order to obtain sufficient covariates on each lawyer’s subsequent employer and the at-risk set of potential employers. Therefore, I report results of these “employment analyses” only for external validity purposes.

Second, the individual-organization hiring analyses focus on the subsequent destination for each displaced lawyer (“hiring analyses”). For all lawyers who regain employment at a NLJ 250 firm, I model the likelihood that a firm-office hires a displaced lawyer. Conditional logit models compare each lawyer’s subsequent employer to other NLJ 250 firm-offices within the same metropolitan area that might have hired the lawyer. The dependent variable takes a value of 1 if a given firm-office hires a focal lawyer and 0 for all other firm offices in the “at-risk” set of potential employing firms.

I formed case-control sample by including all firm-offices that hire one of the sample lawyers and matching each of those observations to up to 10 offices of NLJ 250 firms within the same Core Based Statistical Area (CBSA), as defined by the U.S. Office of Management and Budget. Due to an inadequate number of at-risk offices in CBSAs with few NLJ 250 offices, I excluded 21 of the 933 NLJ 250 lawyers in this analysis. This produced a sample of 9,817 lawyer-firm-office dyads for 912 lawyers in 21 metropolitan areas that could have been hired by

one of the 875 offices operated by the 188 law firms in the sample.⁵ The hiring likelihood hypotheses are tested with this sample.

Independent Variables

The hypotheses require a measure of the prestige differential between employer and candidate as well as a measure of same-school connections between employees and candidates that can be disaggregated by time of attendance. The hypotheses also require an individual measure of work experience.

To measure employer prestige, I obtained the average prestige score in the 2009 Vault 100 rankings of U.S. law firms for every firm included in the potential employer sample. Reported on a scale of 1 to 10, this score is assigned by thousands of attorneys asked to evaluate over 300 law firms based on their perceived prestige in 2009. Although Vault only publishes the top 100 firms' mean ratings, I obtained prestige scores for the top 167 firms identified by Vault survey nominations. For the 16 NLJ 250 firms included in the hiring analyses but not in the Vault data, I assigned the lowest prestige score (2.247) of the 167 firms that were included in the 2009 Vault ratings. To measure educational prestige for each candidate, I obtained the numeric rank of each lawyer's law school in the 2008 *U.S. News & World Report "Best Law School"* (USN&WR) rankings.⁶ All unranked law schools were assigned a rank of 120, the lowest ranked school in the rankings.

The employer prestige measure ranges from 2.25 to 8.73 while the educational prestige measure ranges from 1 to 120. To measure the differential between a potential employer's

⁵ Not all lawyers may "choose" from up to 10 firm offices within the focal CBSA because not all CBSAs contain ten NLJ 250 firm-offices. Therefore, matching does not produce a precise 10-to-1 unrealized-to-realized sample. Reported results are insensitive to including or excluding all observations for the lawyers in such CBSAs.

⁶ This data is discussed extensively in Espeland and Sauder (2007) and Sauder and Espeland (2009).

prestige and a candidate's educational prestige, I first transformed each firm's prestige score and each law school's numeric rank into percentile ranks within the distribution of all firms assigned prestige scores by Vault and all schools in the *USN&WR* rankings. I created a variable hiring firm prestige percentile rank that is each employer's percentile rank and a variable educational prestige percentile rank that is the percentile rank of each lawyer's law school. The absolute difference in percentile ranks is, therefore, the key prestige sorting variable: hiring firm-law school prestige differential. Hypothesis 1 predicts a negative coefficient on this variable in the hiring analyses because both human capital mechanisms imply that hiring likelihoods are decreasing with the magnitude of this differential.

I obtained prior education and bar admission data from the Martindale-Hubbell Law Directory for all lawyers in the sample and for over 107,000 lawyers employed in all offices of the NLJ 250 firms. For firms or offices not listed in Martindale-Hubbell and for those with missing data, I obtained data from West Law to characterize each lawyer's experience and educational prestige as well as the office-level presence of law school alumni networks at all potential employers. I computed each lawyer's years of work experience by subtracting the year in which the lawyer received their undergraduate degree from 2008; I added one and transformed the sum by the natural logarithm to adjust for the skewness of the legal experience variable (long right tail). Hypothesis 2 predicts a positive coefficient on the interaction term of this variable and the prestige differential; the reduction in hiring likelihood associated with equivalent prestige differentials should be lesser the more experienced a candidate is.

For each firm-office, I constructed a measure of alumni network contacts that is the number of firm-office j lawyers that graduated from lawyer i 's law school (i.e., firm-office-school counts). Hypothesis 3 predicts a positive coefficient for this variable in the hiring

analyses. To test Hypotheses 4a and 4b, I disaggregate this variable into two sub-component variables in order to examine the independent effects of overlapping attendance and shared affiliation.⁷ Again, in interpreting the effects of these variables, I assume that one's network contacts are more likely to have attended law school at approximately the same time as the focal lawyer than are non-contacts. If so, then the effects of overlapping attendance on hiring should be stronger than the effects of non-overlapping attendance.

First, the count of firm-office j lawyers whose same law school attendance overlapped with lawyer i 's attendance counts all firm-office j lawyers who entered the bar within a three-year window of the year in which the focal lawyer i entered the bar (i.e., same year, prior year, or following year). Hypothesis 4a predicts a positive coefficient for this variable in the hiring analyses. Second, the count of firm-office j lawyers whose same law school attendance did not overlap with the focal lawyer counts all firm-office lawyers who entered the bar in other years. Hypothesis 4b predicts a positive coefficient for this variable in the hiring analyses. For all count variables, I added one to the count and then transformed the resulting sum by its natural log in order to desensitize coefficient estimates to extreme values.

Control Variables

In the employment analyses, I utilize fixed effects specifications to account for lawyer-level heterogeneity by dissolved firm, geographic location, and practice area. All models include unreported fixed effects for the six dissolved firms (i.e., Heller, Thelen, Thacher, WolfBlock, Dreier, and Morgan & Finnegan). Office location fixed effects include Los Angeles, Northern New Jersey, New York, Philadelphia (including suburban areas in Southern New Jersey), San

⁷ Using a variable that is the percentage of all firm-office lawyers who attended the same law school as the focal lawyer instead of a count variable produces results similar to those reported here. I use the count variable because for consistency with the co-worker count variable.

Francisco, Seattle, Silicon Valley, Washington, and “Other” (Anchorage, Boston, Harrisburg, Hartford, Madison, San Diego, Stamford, and Wilmington). Approximately 80 percent of the sample lawyers were employed in offices in the greater New York City area, the San Francisco Bay Area, Philadelphia, or Washington, DC. Practice area fixed effects include Litigation, Bankruptcy and Restructuring, Corporate Law, Corporate Finance, Intellectual Property, Securities, Real Estate, Government Law, International Law, Labor and Employment, Technology, and “All Other.” *Tables 2a* and *2b* summarize office locations and practice areas.

Additional control variables are included in the analyses. To account for geographic variance in the prevalence of alumni networks, I included a variable for each lawyer that is the percentage of all NLJ 250 lawyers within the lawyer’s CBSA that graduated from the focal lawyer’s law school. A partner indicator variable is coded as 1 if a lawyer was a partner at the dissolved firm and 0 otherwise (e.g., associate, counsel). I coded gender by having myself and four trained research assistants review lawyer names, photos, and/or biographical information like membership in a women’s bar association. I created a “Female” variable that takes a value of 1 if the majority of the five coders identified the lawyer as female and 0 otherwise; 31.0 percent of the 1,426 lawyers were identified as female.⁸ Consistent with prior research on law firms (Gorman, 2005; Gorman and Kmec, 2009), females constitute 42.7 percent of the 653 associates in the full sample but only 20.2 percent of the 592 partners.

Using the same information, the five coders also classified each lawyer’s race and/or ethnicity according to the U.S. Census Bureau’s racial and ethnic classifications. Given that over 86 percent of the lawyers in the full sample were identified as “White” and “Black” was the next most common category (3.5 percent) I simply coded two variables that equal 1 if the majority of the five coders coded an individual as “White” or “Black,” respectively and 0 otherwise. The

⁸ Reported results are insensitive to using a percentage of coders who identified the lawyer as female instead.

omitted category includes lawyers classified primarily as Arab, Asian, Indian, Hispanic, Latino, or Middle Eastern. Because 86 percent of all lawyers are coded as “White” there are not enough observations to employ a broader coding scheme.

Lawyers commonly transition from one employer to another in groups. To control for multi-lawyer moves to the same employer, I included a control variable that is the count of other lawyers formerly employed by the same dissolved firm as the focal lawyer i who were hired into the focal firm-office j . For example, if the New York office of Arnold & Porter hired two former Heller Ehrman lawyers then this variable would take a value of 1 for both lawyers in the analyses. Only lawyers employed by the same firm at the time of dissolution as the focal lawyer (i.e., co-workers) are included in these counts.

I partitioned this count into two variables to account for the effects of lawyers within the same dissolved firm practice (e.g., Securities) and those in different practices (e.g., Real Estate, Technology). First, the count of firm-office j lawyers in the same practice counts all of focal lawyer i ’s co-workers whose dissolved firm biography listed at least one practice in common with focal lawyer i . Second, the count of firm-office j lawyers not in the same practice counts all of focal lawyer i ’s former co-workers whose biography did not list at least one practice in common with the focal lawyer. Presumably, those who worked in the same practice will exert different influences on one’s destination than those who worked in different practices. Again, I added one to all counts and then transformed the sum by its natural log to desensitize coefficient estimates to the count variables’ skewed distributions (i.e., long right tails).⁹

I included additional control variables to account for otherwise unobserved heterogeneity among potential hiring firm-offices. Coefficients on the firm-office count variables for law

⁹ Skewness is largely attributable to the fact that 94 former Thacher lawyers joined the New York office of Sonnenschein, Nath, and Rosenthal. In several robustness checks (e.g., dropping these observations, recoding the count at the second-highest value), I verified that the results reported here are largely insensitive to this outlier.

school alumni and former co-workers hired might be biased by potential hiring firm scale, so I also include a count of firm-office lawyers that did *not* attend the focal lawyer's law school. I also included two variables that are counts of prior employment transitions of law firm partners (1) from the dissolved firm to the potential hiring firm and (2) from the potential hiring firm to the dissolved firm. Using data obtained from Incisive Legal Intelligence's Lateral Partner Moves Database (American Lawyer, 2010), I summed the counts of partner moves for the previous four years based on the time lag that produced the greatest improvement in model fit (reported results are insensitive to time lags ranging from 1 to 8 years).

To account for the potential hiring firm's recent financial performance, I computed a firm-level variable that is the percentage change in firm headcount between 2008 and 2009, according to the *NLJ 250*. For firms listed in the American Lawyer 200, I obtained variables that are the firm's average revenues per lawyer, average profits per equity partner, and leverage ratio (number of associates per partner) in 2008. The lateral hire and financial variables are only available for the 200 U.S. law firms with the highest revenues, which excludes some firms listed in the *NLJ 250* (a headcount ranking).

Results

Summary statistics and correlations for the variables in the employment analyses are presented in *Table 3*; results are presented in *Table 4*. These analyses gauge the extent to which the sample may be biased by the search methods that yielded each lawyer's subsequent employer. In Models 1 through 5 the dependent variable equals 1 if the focal lawyer regained employment and was located via sample construction searches and 0 otherwise; in Model 6 the dependent variable equals 1 if the focal lawyer was found to be employed by a firm in the 2009

NLJ 250 and 0 otherwise (i.e., either not located, not employed, or not employed by a NLJ 250 firm). Of those who regained employment and were located, 75 percent regained employment within the NLJ 250 (i.e., 933 of 1,248 lawyers).

Insert Tables 3 and 4 About Here

Model 1 of Table 4 indicates that of the 1,426 lawyers in the full sample, those who regained employment and were also located are more likely to have been partners at the dissolved firm than associates or other types of lawyers (e.g., of counsel, contract attorneys). Level held constant (e.g., partner), the more experienced a lawyer the less likely they are included in the sample of 1,248 lawyers. White lawyers were more likely and black lawyers less likely than lawyers of other racial or ethnic categories (e.g., Asian, Indian, Hispanic/Latino) to regain employment and be located. The lesser the prestige of a lawyer's law school (i.e., the greater the numeric rank) the more likely they were to be identified as employed. Lawyers located in labor markets with disproportionately more fellow alumni were more likely to be identified as employed.

Models 2 through 5 maintain the baseline specification but also include unreported firm, office location, and practice area fixed effects. Model 2 indicates that there is substantial heterogeneity across lawyers from the six dissolved firms but that the coefficient estimates on the covariates are fairly stable when firm fixed effects are included. Model 3 indicates that the likelihood that a lawyer regained employment varies with local labor market conditions, as evidenced by the improved model fit when including office fixed effects to account for each lawyer's geographic location. Model 4 demonstrates that legal practice area also has a substantial influence on this outcome. Model 5 includes all of these controls and demonstrates

that the key baseline effects of partner level, experience, local alumni, and being white on a lawyer's re-employment prospects are robust to including firm, office, and practice fixed effects. Model 6 of Table 4 presents similar results when the dependent variable is coded more restrictively for only those 933 lawyers who were employed by NLJ 250 firms. The likelihood of being included in the NLJ 250 sample does not vary with law school prestige.

We now turn to the hypothesis tests. Summary statistics and correlations for the variables in the hiring analyses are presented in *Table 5*; results are presented in *Table 6*. For all lawyers that regained employment, I model the likelihood that a focal NLJ 250 firm-office hires a focal lawyer. The conditional logit specification parsimoniously accounts for lawyer-specific covariates by grouping observations on the focal lawyer. To account for non-independence of observations (e.g., co-mobility), I generate robust standard errors by clustering observations by the firm that hired each lawyer.

Insert Tables 5 and 6 About Here

Model 1 indicates that there is no straightforward empirical relationship between employer prestige and the likelihood that a firm-office hires one of the displaced lawyers; they are dispersed across employers of varying prestige. Model 2 indicates that, consistent with Hypothesis 1, a firm-office is less likely to hire a lawyer the greater is the prestige differential between organization and individual. This result is consistent with a human capital account of sorting based on educational credentials; increasing the differential by 1 standard deviation above its observed mean reduces the likelihood of hiring by 32 percent. Although the negative coefficient on the employer prestige main effect indicates that the likelihood of hiring is decreasing with employer prestige, the summary statistics indicate that the modal destination is

an employer ranked in the 74th percentile of the prestige distribution. This observation can be reconciled with Model 1 by considering that size and prestige are positively correlated. Therefore, more prestigious firms contribute disproportionately more firm-offices to the sample than less prestigious ones.

Model 3 is consistent with Hypothesis 3; a lawyer is more likely to be hired by a firm-office if more graduates of the lawyer's law school are employed by that office. At the mean, a one standard deviation increase in this variable increases the hiring likelihood by 23 percent. To reconcile the competing social capital explanations, Model 4 disaggregates this variable into counts of firm-office employees whose law school attendance did and did not overlap with the focal lawyer. Consistent with Hypothesis 4a, Model 4 demonstrates that the same-school effect is primarily attributable to the presence of employees whose law school attendance overlapped with the focal lawyer. Inconsistent with Hypothesis 4b, the coefficient on the non-overlapping count is not statistically significant. Model 5 demonstrates that the effects of law school alumni counts are not merely attributable to firm scale; including the count of lawyers that did not attend the focal lawyer's law school does not erode support for Hypothesis 4a. The coefficients in Model 5 indicate that a one standard deviation increase above the mean of the overlapping attendance variable increases the hiring likelihood by 58 percent.

Model 6 indicates that support for Hypothesis 1 is eroded when we account for the co-movements of former co-workers. A given lawyer is more likely to be hired by an employer the more of their former co-workers are also hired into that office. Model 7 disaggregates the co-worker count variables into "same practice" and "different practice" counts and demonstrates that both positively influence employer-individual matching but the effects of same-practice co-

workers are stronger than those of different-practice co-workers. These results were not theorized but are presented because lawyers often change employers together.

To account for variance in the availability of positions at the firm-offices in the sample and/or otherwise omitted variables, Model 8 includes additional control variables. The number of attorneys employed in the focal firm's office and the change in firm-level headcount between 2008 and 2009 is included. It is not clear whether shrinking or growing firms are more likely to hire displaced lawyers, but the prior year's headcount change variable accounts for either tendency. To account for omitted variable bias, prior partner transitions are also included in the models. If partners previously transitioned between the dissolved firm and the potential hiring firm, then the two firms' lawyers may be considered good "fits" for each other. Again, Hypotheses 4a is supported but Hypothesis 1 is not.

Model 9 includes additional firm-level performance measures and the leverage ratio (partners per associate) for the subset of 822 lawyers who regained employment within the *American Lawyer 200* (a subset of the NLJ 250 that represents the 200 highest-grossing firms in terms of revenue). Due to missing data (i.e., 200 firms instead of 250), inclusion of these variables reduces the sample from 9,861 dyads to 8,610 dyads. Again, support for Hypothesis 4a is robust to the inclusion of these controls. These results imply that the sorting of individuals into employers on the basis of educational credentials is primarily attributable to the networks mechanism – employees tend influence employer hiring decisions in favor of former classmates so that employment is embedded in alumni networks. A one standard deviation increase in the number of former classmates who are employed by a firm-office increases the likelihood that a lawyer is hired by 20 percent. But, before concluding that social capital mechanisms are

responsible for credential-based sorting, we revisit the human capital argument in Table 7 and, specifically, the signaling argument supporting Hypothesis 2.

Insert Table 7 About Here

Model 10 of Table 7 includes an interaction term that is the product of the employer-candidate prestige differential and the focal lawyer's work experience. Because this variable is missing for 44 lawyers, the sample size is reduced from 822 lawyers to 778 lawyers and from 8,589 dyads to 8,126 dyads. Without eroding support for Hypothesis 4a (the "network mechanism"), the results support Hypothesis 2 (the "signal" mechanism). The negative coefficient on the prestige differential variable indicates that the signaling mechanism is most relevant to the hiring of the least experienced lawyers (i.e., the coefficient represents the effect of the prestige differential on lawyers with no experience). For example, for two lawyers at the mean prestige differential level the hiring likelihood is approximately 8 percent higher for a lawyer with mean experience than a lawyer with no experience. Large differences between an employer's prestige and a candidate's educational prestige are least likely to prevent the most experienced lawyers from being hired (i.e., the positive coefficient on the interaction term). This attenuating effect of experience on prestige favors the signaling account to the skill-based account of sorting based on educational credentials.

Because our theoretical motivation is intra-occupational inequality, the final models explore differential hiring practices for the occupations most rewarding employers. support for the network mechanism continues to hold for Hypothesis 4a. Lawyers are most likely to be hired by firm-offices that employ many of their former classmates. Model 11, therefore, includes an interaction term that is the product of the number of same-school lawyers at the firm-office and

the potential employer's prestige. Note that the same-school count aggregates both the overlapping and non-overlapping attendance counts because prior models demonstrate that only the overlapping counts exert significant effects on the hiring likelihood. Model 11 indicates that the most prestigious employers are least likely to embed hiring decisions in law school alumni networks. The positive influence of same-law school lawyers on the hiring likelihood is smallest for the most prestigious employers. Model 12 offers a similar result when firm profitability is substituted for firm prestige as an indicator of socioeconomic rewards. These results suggest that alumni networks exert the least influence on hiring at the most rewarding employers.

DISCUSSION

Recent work on networks, labor markets, and careers calls for research designs that more clearly identify network mechanisms that contribute to inequality (Mouw, 2003, 2006; DiPrete and Eirich, 2006; Mouw and Kalleberg, 2010). This study answers these calls by utilizing a mobility quasi-experiment to identify how educational credentials influence organizational hiring and individual attainment. The results support both human capital and social capital accounts of the relationship between educational credentials and intra-occupational inequality.

This study found that the most rewarding employers (i.e., high prestige and high profitability) hired lawyers who graduated from the most prestigious law schools. But, consistent with signaling theory, this tendency weakened with one's experience. Theories of cumulative career advantage (e.g., Merton, 1968; Zuckerman, 1998) imply that human capital signals contribute strongly to intra-occupational inequality by influencing the distribution of inexperienced individuals. This study's prestige result is consistent with a sorting account of

inequality based on educational credentials, but additional mechanisms appear to reproduce the distribution of inexperienced individuals.

A second key finding pertains to the role of social capital in the reproduction of inequality based on educational credentials. In the dissolution context, individuals were typically hired by organizations that employed more former classmates but the most rewarding employers were least likely to embed employment relationships in these alumni networks. This finding is consistent with prior speculation about labor market sorting on the basis of prestige. Podolny (2001: 43) argued that the most prestigious employers would hold labor market advantages in the recruitment of clearly productive individuals and that, conversely, the least prestigious employers would be forced to rely on networks to identify high potential candidates with less observable productivity.

Together, these results imply that educational credentials influence inequality not only by distributing individuals across employers based on signals of human capital but, also, by reproducing the distribution through social capital embodied in alumni networks. The insight is consistent with work in both sociology (e.g., Montgomery, 1991; 1994; Smith, 2000) and economics (Calvo-Armengol and Jackson, 2004), as well as Ishida, et al.'s (1997) contention that prestigious institutions' graduates benefit "...directly from informal personal ties among graduates from the same school."

More broadly, it is taken-for-granted by most sociologists (Granovetter, 1974[1995]; Marsden and Gorman, 2001) and by many economists (e.g., Rees, 1966; Montgomery, 1991; Calvo-Armengol and Jackson, 2004) that network contacts may be utilized by both individuals and organizations to alleviate information asymmetries associated with hiring. But, a large sociological literature establishes mixed findings on the attainment effects of using networks in

the job search process (e.g., Lin, Edsel, and Vaughn, 1981; Bridges and Villemez, 1986; Wegener, 1991; Lin, 1999; Marsden and Gorman, 2001; Mouw, 2003). By focusing on hiring organizations and analyzing a large occupational sample of displaced individuals, this study offers important insights to this literature.

It may be fruitful to re-evaluate the network-attainment relationship from the demand side of the labor market. Little prior work investigates heterogeneity across employers in terms of their use of employee referrals – either formally or informally – in the hiring process. The results of this study suggest that the most prestigious employers are least likely to do so. A promising line of inquiry may, therefore, be examining which employers are most likely to embed employment relationships in employee networks. Changing the focus from the supply side to the demand side may help reconcile the large body of mixed evidence on how networks influence socioeconomic attainment (e.g., Fernandez and Galperin, 2012).

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Figure 1. Increasing intra-occupational inequality.

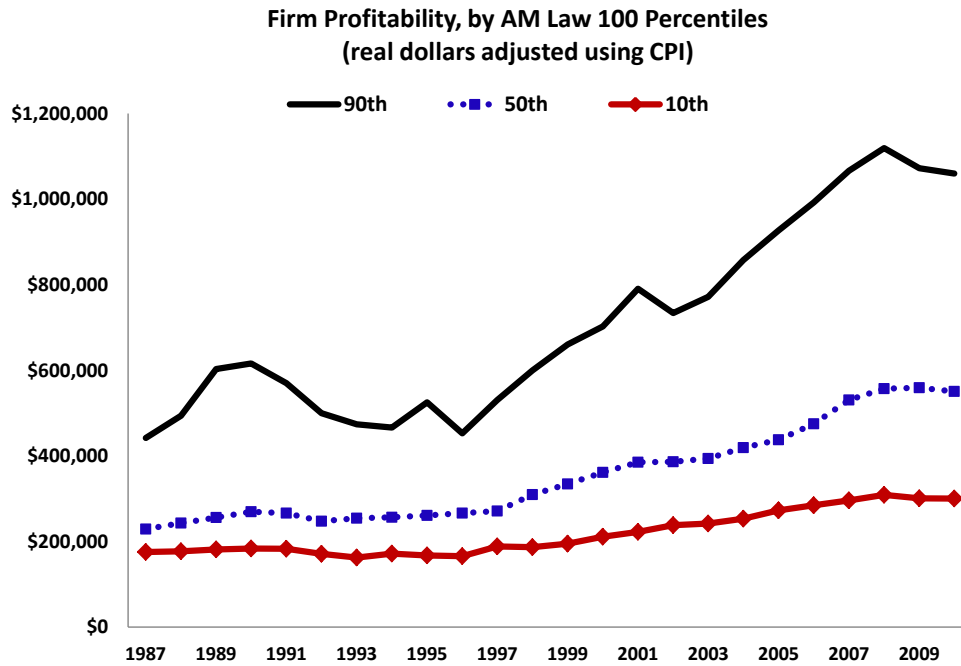


Figure 2. Sorting into inequality based on educational credentials (2008).

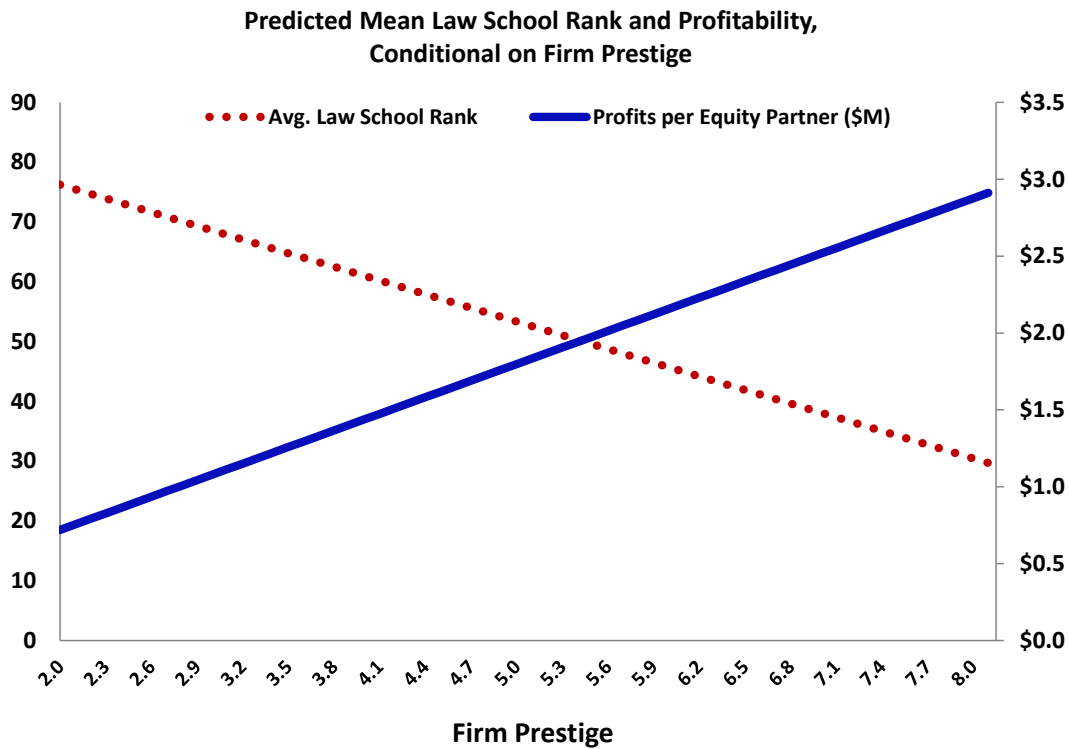


Table 1: Lawyers in sample, by dissolved firm.

Firm	Partners	Associates	Other	Total	Employed	% Employed
Dreier LLP	49	52	19	120	92	77%
Heller Ehrman	113	200	39	352	320	91%
Morgan Finnegan	32	32	8	72	62	86%
Thacher Proffitt & Wood	55	106	14	175	135	77%
Thelen LLP	188	152	52	392	367	94%
Wolf Block	155	111	49	315	272	86%
Totals	592	653	181	1,426	1,248	88%

Note: Employment only verified if a lawyer is located; some employed lawyers may not have been located.

Table 2a: Lawyers in sample, by law school attended.

Rank	Law School	Lawyers	Rank	Law School	Lawyers
1	Harvard University	82	21	Boston University	22
2	University of Pennsylvania	75	22	University of California, Los Angeles	20
3	Fordham University	71	22	Villanova University	20
4	University of California, Hastings	63	24	Santa Clara University	16
5	University of California, Berkeley	58	25	Duke University	15
6	Georgetown University	53	25	Yale University	15
7	New York University	52	27	Boston College	14
8	George Washington University	50	27	Rutgers University, Camden	14
9	Columbia University	46	27	University of Chicago	14
10	University of Michigan	41	27	University of Southern California	14
11	Brooklyn College	40	31	Hofstra University	13
12	New York Law School	38	32	University of Connecticut	12
13	St. John's University	37	33	University of San Francisco	11
13	Yeshiva University	37	33	Widener University	11
15	University of Virginia	33	35	American University	10
16	Rutgers University	31	35	Northwestern University	10
17	Cornell University	30	35	Tulane University	10
18	Temple University	29	35	University of California, Davis	10
19	Seton Hall University	27	35	University of San Diego	10
20	Stanford University	25	35	University of Washington	10

Table 2b: Lawyers in sample, by geographic labor market and legal practice area.

City	Lawyers	% Total	Practice area	Lawyers	% Total
New York	567	40%	Litigation	496	35%
San Francisco	230	16%	Corporate law	414	29%
Philadelphia	179	12%	Corporate finance	316	22%
Washington, DC	96	7%	Intellectual property	249	17%
Silicon Valley	72	5%	Securities	229	16%
Northern New Jersey	69	5%	Real estate	196	14%
Los Angeles	64	4%	International	192	13%
Seattle	48	3%	Labor	191	13%
Hartford	28	2%	Government	129	9%
San Diego	25	2%	Technology	98	7%
Boston	13	1%	Emerging companies	91	6%
Harrisburg	13	1%	Energy	90	6%
Stamford	13	1%	Construction	86	6%
Wilmington	11	1%	Appellate	84	6%
Anchorage	4	0%	Antitrust	74	5%
Madison	2	0%	Bankruptcy/restructuring	51	4%

Note: Some lawyers are assigned to more than one office and/or practice area.

Table 3: Summary statistics and correlations of variables in employment analyses..

	Mean	St. Dev.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1) Lawyer employed and located (0/1)	0.88	0.33	1.00								
(2) Lawyer employed by NLJ 250 firm and located (0/1)	0.65	0.48	0.51	1.00							
(3) Female (0/1)	0.31	0.46	-0.05	-0.05	1.00						
(4) Partner (0/1)	0.42	0.49	0.17	0.18	-0.19	1.00					
(5) In (years of work experience)	2.91	0.65	0.03	0.04	-0.26	0.60	1.00				
(6) Rank of law school attended	40.6	37.9	-0.03	-0.06	0.03	-0.08	-0.09	1.00			
(7) % of local attorneys from lawyer <i>i</i> 's law school	0.08	0.06	0.17	0.12	-0.03	0.03	0.08	-0.13	1.00		
(8) Black (0/1)	0.04	0.18	-0.13	-0.08	0.07	-0.01	-0.05	-0.01	0.02	1.00	
(9) White (0/1)	0.86	0.34	0.17	0.11	-0.16	0.19	0.24	-0.02	0.07	-0.40	1.00

Table 4

Probit models of the likelihood that a lawyer is employed and located ($Y_i = 1$ if "Yes"; 0 if "No").

	(1)	(2)	(3)	(4)	(5)	(6)
<i>[dependent variable]</i>	<i>[employed]</i>	<i>[employed]</i>	<i>[employed]</i>	<i>[employed]</i>	<i>[employed]</i>	<i>[NJ 250]</i>
Female (0/1)	-0.055 (0.106)	-0.039 (0.109)	-0.024 (0.111)	-0.058 (0.109)	-0.042 (0.112)	-0.057 (0.086)
Partner (0/1)	0.902 ** (0.130)	0.979 ** (0.132)	1.06 ** (0.139)	0.981 ** (0.136)	1.05 ** (0.143)	0.744 ** (0.103)
ln (years of work experience)	-0.411 ** (0.105)	-0.450 ** (0.105)	-0.479 ** (0.110)	-0.453 ** (0.105)	-0.481 ** (0.109)	-0.376 ** (0.080)
Rank of law school attended	0.001 (0.001)	0.003 ** (0.001)	0.002 * (0.001)	0.003 * (0.001)	0.002 (0.001)	0.000 (0.001)
% of local attorneys from lawyer's law school	5.38 ** (1.25)	6.10 ** (1.27)	7.45 ** (1.34)	6.12 ** (1.27)	7.48 ** (1.34)	2.51 ** (0.747)
Black (0/1)	-0.627 ** (0.219)	-0.646 ** (0.231)	-0.614 * (0.250)	-0.657 * (0.242)	-0.625 * (0.261)	-0.358 (0.234)
White (0/1)	0.429 ** (0.131)	0.468 * (0.135)	0.448 ** (0.138)	0.475 ** (0.137)	0.446 ** (0.141)	0.315 ** (0.126)
Constant	1.39 ** (0.302)	1.39 * (0.360)	1.53 ** (0.419)	1.02 ** (0.402)	1.12 * (0.467)	1.83 ** (0.364)
N (lawyers)	1,426	1,426	1,426	1,426	1,426	1,426
Firm fixed effects	No	Yes	Yes	Yes	Yes	Yes
Office city fixed effects	No	No	Yes	No	Yes	Yes
Practice area fixed effects	No	No	No	Yes	Yes	Yes
Log pseudolikelihood	-414.83	-392.13	-367.93	-384.40	-360.63	-710.17
Wald Chi-square (d.f.)	89.11 (7)	134.3 (12)	162.8 (20)	157.1 (23)	193.0 (31)	228.1 (31)

Robust standard errors in parentheses.

** p < 0.01; * p < 0.05; † p < 0.10; two-tailed tests.

Table 5. Summary statistics and correlations of variables in firm-office/lawyer matching analyses.

	Mean	St. Dev.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(1) Lawyer <i>i</i> hired by firm-office <i>j</i> (0/1)	0.09	0.29	--															
(2) Hiring firm prestige percentile rank	0.64	0.28	0.01	--														
(3) hiring firm-law school prestige differential	0.28	0.24	-0.05	-0.77	--													
(4) ln (# of firm-office <i>j</i> lawyers from lawyer <i>i</i> 's law school)	0.99	1.05	0.11	0.20	-0.22	--												
(5) ln (# overlapping attendance)	0.21	0.44	0.20	0.12	-0.15	0.62	--											
(6) ln (# non-overlapping attendance)	0.93	1.03	0.07	0.20	-0.21	0.99	0.53	--										
(7) ln (# of firm-office <i>j</i> lawyers not from lawyer <i>i</i> 's law school)	3.66	1.30	0.06	0.33	-0.21	0.55	0.34	0.55	--									
(8) ln (# of lawyers firm-office <i>j</i> hired from lawyer <i>i</i> 's prior firm)	0.40	0.92	0.70	0.04	-0.08	0.17	0.19	0.14	0.15	--								
(9) ln (# from lawyer <i>i</i> 's prior practice)	0.23	0.70	0.74	0.03	-0.07	0.12	0.16	0.10	0.11	0.88	--							
(10) ln (# not from lawyer <i>i</i> 's prior practice)	0.28	0.74	0.55	0.04	-0.08	0.17	0.17	0.15	0.15	0.92	0.67	--						
(11) % change in headcount at firm <i>j</i> , 2008-09	-0.02	0.09	0.09	-0.17	0.13	-0.08	-0.03	-0.08	-0.15	0.14	0.11	0.16	--					
(12) ln (# of dissolved firm partners hired by firm <i>j</i> , t_0 to t_{0-4})	0.12	0.36	0.12	0.15	-0.14	0.07	0.07	0.07	0.07	0.20	0.09	0.23	0.01	--				
(13) ln (# of firm <i>j</i> partners hired by dissolved firm, t_0 to t_{0-4})	0.11	0.34	0.12	0.14	-0.13	0.11	0.10	0.10	0.10	0.19	0.11	0.21	-0.03	0.33	--			
(14) Firm <i>j</i> 's revenues per lawyer (\$1,000s)	752.7	231.0	-0.05	0.53	-0.27	0.19	0.10	0.19	0.33	-0.07	-0.06	-0.07	-0.19	0.03	-0.01	--		
(15) Firm <i>j</i> 's profits per equity partner (\$1,000s)	1,116.7	652.6	-0.07	0.51	-0.25	0.20	0.11	0.20	0.34	-0.11	-0.09	-0.11	-0.20	0.00	-0.01	0.88	--	
(16) Firm <i>j</i> 's leverage ratio	4.2	1.4	0.02	0.08	-0.01	0.09	0.05	0.09	0.14	0.01	0.02	-0	-0.02	0.08	0.06	0.11	0.37	--

Table 6

Conditional logit models of the likelihood that firm-office j hires lawyer i ($Y_{ij} = 1$ if "Yes"; 0 if "No").

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Employer prestige percentile rank	0.208 (0.483)	-1.07 * (0.503)	-0.271 (0.500)	-0.149 (0.488)	-1.36 * (0.583)	-0.193 (0.473)	-0.074 (0.531)	-0.347 (0.543)	0.880 (0.774)
employer-candidate prestige differential		-1.93 ** (0.456)			-1.77 ** (0.431)	-0.121 (0.419)	-0.236 (0.457)	-0.230 (0.471)	-0.046 (0.506)
ln (# of firm-office j lawyers from lawyer i 's law school)			0.562 ** (0.152)						
ln (# overlapping attendance)				1.64 ** (0.399)	1.61 ** (0.392)	1.78 ** (0.188)	1.75 ** (0.195)	1.77 ** (0.193)	1.83 ** (0.195)
ln (# non-overlapping attendance)				-0.108 (0.095)	-0.181 * (0.089)	-0.212 * (0.111)	-0.223 * (0.120)	-0.210 * (0.120)	-0.202 * (0.122)
ln (# of firm-office j lawyers not from lawyer i 's law school)					0.061 (0.142)	-0.331 ** (0.093)	-0.289 ** (0.100)	-0.333 ** (0.103)	-0.348 ** (0.100)
ln (# of lawyers firm-office j hired from lawyer i 's prior firm)						1.75 ** (0.088)			
ln (# from lawyer i 's prior practice)							2.06 ** (0.164)	2.12 ** (0.153)	2.15 ** (0.164)
ln (# not from lawyer i 's prior practice)							0.483 ** (0.149)	0.439 ** (0.133)	0.445 ** (0.135)
% change in headcount at firm j , 2008-09								-1.74 (1.33)	-2.32 (1.55)
ln (# of dissolved firm partners hired by firm j , t_0 to $t_{0.4}$)								0.601 (0.357)	0.520 (0.362)
ln (# of firm j partners hired by dissolved firm, t_0 to $t_{0.4}$)								0.092 (0.334)	0.120 (0.307)
Firm j 's revenues per lawyer (\$1,000s)									0.001 (0.002)
Firm j 's profits per equity partner (\$1,000s)									-0.001 (0.001)
Firm j 's leverage ratio									0.194 * (0.092)
N (lawyer/firm-office dyads)	9,817	9,817	9,817	9,817	9,817	9,817	9,817	9,817	8,589
N (lawyers)	912	912	912	912	912	912	912	912	822
N (at-risk firms)	188	188	188	188	188	188	188	188	171
N (hiring firms)	103	103	103	103	103	103	103	103	90
N (metro areas)	21	21	21	21	21	21	21	21	16
Log pseudolikelihood	-2,158.9	-2,131.4	-2,070.7	-1,959.7	-1,939.0	-711.6	-671.1	-655.6	-560.9
Wald Chi-square (d.f.)	0.19 (1)	20.0 (2)	17.8 (2)	20.7 (3)	40.6 (5)	460.7 (6)	399.4 (7)	427.5 (10)	401.1 (13)

All models include lawyer fixed effects. Robust standard errors in parentheses. Observations clustered on hiring firms.

** $p < 0.01$; * $p < 0.05$; two-tailed tests for control variables and one-tailed hypothesis tests.

Table 7

Conditional logit models of the likelihood that firm-office <i>j</i> hires lawyer <i>i</i> ($Y_{ij} = 1$ if "Yes"; 0 if "No").			
	(10)	(11)	(12)
Employer prestige percentile rank	0.461 (0.801)	0.879 (0.794)	0.386 (0.789)
employer-candidate prestige differential	-1.24 * (0.716)	-1.31 * (0.730)	-1.29 * (0.703)
ln (# of firm-office <i>j</i> lawyers from lawyer <i>i</i> 's law school)	0.216 * (0.122)	0.560 ** (0.233)	0.441 * (0.204)
ln (# of lawyers firm-office <i>j</i> hired from lawyer <i>i</i> 's prior firm)	1.74 ** (0.101)	1.73 ** (0.101)	1.73 ** (0.102)
% change in headcount at firm <i>j</i> , 2008-09	-2.78 (1.63)	-2.79 (1.62)	-2.80 (1.62)
ln (# of dissolved firm partners hired by firm <i>j</i> , t_0 to t_{0-4})	0.195 (0.384)	0.203 (0.382)	0.206 (0.384)
ln (# of firm <i>j</i> partners hired by dissolved firm, t_0 to t_{0-4})	-0.131 (0.363)	-0.121 (0.361)	-0.134 (0.360)
Firm <i>j</i> 's revenues per lawyer (\$1,000s)	0.000 (0.002)	0.000 (0.002)	0.000 (0.002)
Firm <i>j</i> 's profits per equity partner (\$1,000s)	-0.001 (0.001)	0.000 (0.001)	0.000 (0.001)
Firm <i>j</i> 's leverage ratio	0.132 (0.104)	0.114 (0.113)	0.122 (0.108)
prestige differential * lawyer experience (years)	0.049 * (0.023)	0.050 * (0.024)	0.050 * (0.023)
ln (# same law school lawyers) * hiring firm prestige		-0.493 * (0.289)	
[ln (# same law school lawyers) * firm <i>j</i> profitability]/10			-0.002 * (0.001)
N (lawyer/firm-office dyads)	8,126	8,128	8,128
N (lawyers)	778	778	778
N (at-risk firms)	171	171	171
N (hiring firms)	90	90	90
N (metro areas)	16	16	16
Log pseudolikelihood	-651.7	-650.2	-650.2
Wald Chi-square (d.f.)	442.6 (11)	437.1 (12)	446.5 (12)

All models include lawyer fixed effects.

Robust standard errors in parentheses. Observations clustered on hiring firms.

** $p < 0.01$; * $p < 0.05$; two-tailed tests for control variables and one-tailed hypothesis tests.

Appendix 1: Six firm dissolutions.

1. Heller Ehrman LLP (“Heller”) was headquartered in San Francisco and also operated large offices in Los Angeles, London, New York, San Diego, Seattle, Silicon Valley, and Washington. Heller was widely viewed as one of the most prominent law firms in the San Francisco Bay Area and regularly received high ratings from legal industry publications for diversity, pro bono work, and employee satisfaction. The firm was ranked 62nd in the 2008 Vault 100 ranking of prestigious law firms and 56th in the 2008 American Lawyer 200 rankings of U.S. law firms by gross revenues. According to the National Law Journal (NLJ), Heller was the 65th largest firm in the U.S. in 2007, employing approximately 600 lawyers.

Heller attorneys represented major corporate clients like Apple, GE, Levi Strauss, McDonald’s, Microsoft, Northrup Grunman, and Yahoo!. In 2008, their client list included Lehman Brothers and Washington Mutual, two large corporations that failed in 2008 and left Heller with large uncollectable receivables. Like many law firm dissolutions (Phillips, 2002; Heinz, 2009), Heller’s collapse was accelerated by the departure of fifteen intellectual property attorneys for competitor Covington & Burling LLP. This departure triggered a default clause in the firm’s loan agreements and Heller was unable to satisfy its creditors’ capital requirements. Shortly thereafter, reported merger talks with Mayer Brown ceased. Heller announced its dissolution on September 26, 2008, officially dissolved in late November of 2008, and filed for bankruptcy in December of 2008. In mid-October of 2008, I extracted 352 website biographies for lawyers employed in Heller’s U.S. offices at the time of dissolution (see *Table 1* for details).

2. Thelen LLP (“Thelen”) was a bicoastal law firm formed by two mergers, one in 1998 and one in 2006, between a California-based law firm and two New York-based firms. Thelen had offices in Hartford, Los Angeles, New York, San Francisco, Silicon Valley, and Washington, DC. The firm was ranked 75th in the 2008 Vault 100 ranking of prestigious law firms and 76th in the 2008 American Lawyer 200 rankings of U.S. law firms by gross revenues. According to the NLJ, Thelen was the 78th largest firm in the U.S. in 2008, employing approximately 550 lawyers.

Thelen’s construction practice was widely-regarded as one of the best in the country and the firm’s clients included Cisco, Ford, Merrill Lynch, News Corporation, and several major

public utilities. Thelen had difficulty integrating attorneys acquired in the merger with Brown Raysman in 2006 and experienced numerous partner departures in 2007 and 2008. After merger talks with Nixon Peabody failed, Thelen announced its dissolution in October of 2008, closed its doors in December of 2008, and entered bankruptcy in September of 2009. In October of 2008, I extracted 392 website biographies for those lawyers employed in Thelen's offices (see *Table 1* for details).

3. Thacher Proffitt Wood LLP ("Thacher") was headquartered in New York City and also operated offices in Washington, DC and New Jersey. The firm was ranked 90th in the 2008 Vault 100 ranking of prestigious law firms and 131st in the 2008 American Lawyer 200 rankings of U.S. law firms by gross revenues. According to the NLJ, Thacher was the 156th largest firm in the U.S. in 2008, employing almost 300 lawyers.

Thacher was so strongly associated with sub-prime mortgages that mortgage traders commonly referred to purchase agreements for mortgage-backed securities as "Thacher docs." Thacher clients included Citibank and UBS and the firm's biggest client was Bear Stearns. In late December of 2008, merger talks with King & Spalding ceased and approximately 100 lawyers announced that they would leave Thacher for a competitor, Sonnenschein, Nath & Rosenthal, LLP. Thacher partners voted to dissolve the firm shortly after the announcement. In December of 2008, I extracted 175 website biographies for those lawyers employed in Thacher's offices (see *Table 1* for details).

4. WolfBlock LLP ("WolfBlock") was based in Philadelphia and also operated offices in New York, New Jersey, Harrisburg, and Wilmington, Delaware. Although WolfBlock was not ranked in the published list of Vault 100 law firms in 2008, data obtained directly from Vault indicates that WolfBlock was the 138th-ranked most prestigious U.S. law firm in 2008. WolfBlock was ranked 135th in the 2008 American Lawyer 200 rankings of U.S. law firms by gross revenues and, according to the NLJ, WolfBlock was the 149th largest firm in the U.S., employing approximately 300 lawyers in 2008.

The firm's core practice was its real estate group so WolfBlock's business was hurt badly by the 2008 economic downturn. Corporate clients included Comcast and Rite Aid and a government lobbying practice operated in Harrisburg, PA and Washington, DC. WolfBlock attempted to merge with Philadelphia's Cozen O'Connor in 2007 and with Florida's Akerman Senterfitt in 2008, but both attempts failed. As partners departed WolfBlock throughout 2008 the firm's largest creditor, Wachovia, restricted the firm's access to credit and the partners voted to dissolve in March of 2009. In March of 2009, I extracted 318 website biographies for lawyers employed in WolfBlock's offices (see *Table 1* for details).

5. Dreier LLP ("Dreier LLP") was based in New York. The firm also maintained a small office in Stamford, Connecticut and several lawyers worked in Los Angeles. The firm's corporate clients included General Dynamics, PepsiCo, and the New York Life Insurance Company. The firm was not ranked in the 2008 Vault 100, American Lawyer 200, or the National Law Journal 250 (NLJ 250).

Marc Dreier, the firm's namesake founder and sole equity partner, was arrested in early December of 2008 and charged with securities fraud following his impersonation of a Canadian pension fund official. The ensuing investigation revealed that Dreier operated a ponzi scheme that defrauded clients and investors of more than \$400 million. Dreier's arrest shocked lawyers employed by his firm and resulted in quick public disavowals by firm partners (all non-equity). Wachovia, a firm creditor, also sued Dreier for defaulting on more than \$9 million in loans and Dreier entered the firm into Chapter 11 bankruptcy on December 16, 2008. In mid-December of 2008, I extracted 120 website biographies for all of Dreier's lawyers listed on the firm website (see *Table 1* for details). Marc Dreier pled guilty to charges of money laundering, conspiracy, securities fraud, and wire fraud in May of 2009. He was sentenced to 20 years in prison in July of 2009.

6. Morgan & Finnegan LLP ("Morgan & Finnegan") was an intellectual property boutique firm based in New York but with several lawyers located in Washington and California. Morgan & Finnegan's clients included Canon, DuPont, Nokia, and Research in Motion. The firm was not ranked in the 2008 Vault 100, American Lawyer 200, or the NLJ 250. The firm's revenues

fell sharply in 2008 and many partners departed. A former partner also sued Morgan & Finnegan for altering the firm's partnership agreement to create financial disincentives for leaving the firm. A large group of partners left the firm for Locke Lord Bissell & Liddell in February of 2009 and Morgan & Finnegan filed for Chapter 7 bankruptcy in March of 2009. In 2009, I extracted 72 website biographies from the Internet Archive for all of Morgan & Finnegan lawyers listed on the firm website in January of 2008, the last date available (see *Table 1* for details).¹⁰

¹⁰ Given the difficulty of identifying departure dates, I estimated the models reported in this draft with and without the Morgan & Finnegan lawyers; results are largely insensitive to the inclusion of Morgan & Finnegan lawyers in the sample.