NETWORKS, HIRING, AND ATTAINMENT: EVIDENCE FROM LAW FIRM DISSOLUTIONS.

Christopher I. Rider

Goizueta Business School
Emory University
1300 Clifton Road
Atlanta, GA 30322
crider@emory.edu

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ABSTRACT

Social networks are widely believed to facilitate hiring by reducing information asymmetries between individual job candidates and employing organizations. Causal effects of network contacts on organizational hiring and individual attainment are unclear, though, because networks probably influence both the likelihood that an individual changes employers and the quality of the position they attain by doing so. In this study, I theorize how prior education and prior employment networks differentially alleviate information asymmetries to influence labor market matching and intraprofessional status attainment. Leveraging the unexpected dissolutions of six U.S. law firms as an exogenous shock to mobility, I analyze 1,426 lawyers' post-dissolution labor market outcomes. The results demonstrate that both law school alumni and co-worker contacts influence hiring; however, status attainment is aided by former co-workers and hindered by alumni. Implications for studies of networks and inequality are discussed.

INTRODUCTION

That social networks play a central role in labor markets is taken-for-granted as fact by most sociologists (Granovetter, 1995; Marsden and Gorman, 2001) and by many economists, too (e.g., Rees, 1966; Montgomery, 1991; Calvo-Armengol and Jackson, 2004). On the supply side of labor markets, networks provide some individuals with access to novel and timely information on job opportunities (Granovetter, 1973, 1995; Wegener, 1991). On the demand side, networks enable some organizations to attract and select suitable individuals for employment (Fernandez and Weinberg, 1997; Fernandez, Castilla, and Moore, 2000; Peterson, Saporta, and Seidel, 2000).

Because network contacts may be utilized by both individuals and organizations to alleviate information asymmetries associated with hiring, social networks are considered particularly effective at facilitating employee-employer matching. For example, if employees refer candidates that are similar to themselves (Doeringer and Piore, 1971) then employers may simply leverage referrals from their best employees to identify good individual-organization matches (Montgomery, 1991). In parallel, individuals may simply seek positions with the employers of their similar associates (Mouw, 2003).

Surprisingly, empirical studies offer mixed evidence on just how network contacts influence the subjective quality of positions obtained by individuals (see Marsden and Gorman, 2001 and Mouw, 2003 for reviews; also Davern, 1999) and the subjective quality of employees hired by organizations (Simon and Warner, 1992; Sicilian, 1995; Castilla, 2005; Yakubovich and Lup, 2006). But, recent work enhances our understanding of networks and hiring by analyzing multiple stages of a single organization's hiring process (e.g., Fernandez and Weinberg, 1997; Fernandez, et al., 2000; Peterson, et al., 2000; Fernandez and Fernandez-Mateo, 2006; Yakubovich and Lup, 2006 and by probing fundamental assumptions of the literature across multiple, large data sets (Mouw, 2003). Yet, credible inferences about causal network effects in labor markets remain challenging (Mouw, 2003, 2006).

To illustrate the challenge, consider a few assumptions informed by prior work. Assume that networks do indeed help individuals learn of open positions by channeling novel and timely information (Granovetter, 1973, 1974[1995]) and also persuade employers to hire job candidates who are connected to employees (Montgomery, 1991; Yakubovich, 2005). Assume further that, all else equal, an organization benefits more from hiring a candidate referred by an employee contact than from hiring an unconnected candidate (Fernandez, et al., 2000; Castilla, 2005).

Combining these assumptions implies that individuals with high quality networks (i.e., networks optimized for job leads and referrals) are most likely to change employers willingly and, further, that the greater one's network quality the more willing organizations will be to hire them into the best positions. If contacts influence both the demand and supply side of labor markets in these two ways, then networks simultaneously affect both the likelihood of an individual changing employers as well as the quality of the position attained by doing so.

This line of reasoning implies that job-switchers are not representative of all labor market participants. On the one hand, if high quality networks provide individuals with access to better job opportunities and, therefore, increase the rate of voluntary turnover (McPherson, Popielarz, and Drobnic, 1992) then individuals with high quality networks may be over-represented in samples of job-switchers. On the other hand, the distribution of network quality among samples of job-switchers may be bi-modal because both on-the-job performance (Podolny and Baron, 1997; Mizruchi and Stearns, 2001; Burt, 1992, 2004) and the likelihood of organizational exit (Krackhardt and Porter, 1986; McPherson, et al., 1992) have been found to vary with one's network properties. If network quality enhances on-the-job performance then individuals with high quality networks are likely to be retained by organizations and those with low quality networks dismissed (e.g., Jovanovic, 1979). But, if high performers' connections to other organizations' employees induce them to change employers, too, then labor markets may be populated by many individuals with high and low quality networks but few of moderate quality. Extant research offers limited insight into the network quality distributions for individuals who do and do not change jobs.

These issues make it difficult to obtain samples of individuals whose networks are representative of an occupation, profession, or industry and to then identify unambiguously how their networks influence either demand-side (e.g., hiring) or supply-side (e.g., mobility) labor market outcomes. For example, how should one account for mobility determinants in evaluating the relationship between network properties and labor market outcomes? Is mobility indicative of a high or a low quality network? In short, the role of networks in labor markets is difficult to assess with conventional research designs (e.g., large samples of job-switchers, single-organization studies of hiring or turnover). It is even more difficult to study how particular types of contacts (e.g., weak ties, strong ties, friends, co-workers) differentially influence matching and outcome quality.

A field experiment could address these inferential issues. For example, network data could be collected from many individuals within an occupation or profession prior to randomly selecting half for dismissal from their current position. Researchers could then follow the dismissed group's transitions to subsequent employers and evaluate the relationship between their network characteristics and their labor market outcomes, relative to those not selected for dismissal. But such experiments are neither practical nor desirable, especially 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 natural experiment. The context would, ideally, enable one to examine the labor market outcomes of many individuals simultaneously displaced from their employers for reasons unrelated to their networks or their job performance. Holding constant the cause of inter-organizational mobility for a representative sample of individuals, one could simply analyze where individuals regained employment, how (subjectively) good their subsequent position was, and how their network contacts influence both destination and attainment. This is the research design employed in this study.

I investigate how network relationships influence the labor market outcomes of over 1,400 lawyers who lost their jobs due to the unexpected 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 (Heinz, Nelson, and Laumann, 2001), as the lawyers range from first-year associates to partners with decades of legal 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 depended (at least partially) on contacts at other firms and the employment transitions of co-workers. All participated in the labor market for reasons largely independent of networks, ability, or job performance. They were simply employed by the wrong firm at the wrong time.

Theoretically, I discuss typical information asymmetries between employers and job candidates and then consider two types of networks that influence the organization-individual matching process: prior education and employment networks. I develop predictions about how these two types of networks alleviate information asymmetries and influence labor market outcomes, in terms of specific destination and relative quality. Empirically, I examine how a lawyer's post-dissolution employer relates to the presence of fellow law school alumni at potential employers as well as the concurrent employment transitions of their co-workers.

To analyze network effects on hiring, I employ a matching analysis that explicitly accounts for both demand and supply sides of labor markets. Using a case-control sample of 875 offices of 188 firms that might have employed the 916 lawyers who regained employment at one of the top 250 largest U.S. law firms, I model the likelihood that a lawyer regains employment at their subsequent employer versus other firm-offices within the same metropolitan area. The key independent variables are the percentage of a firm-office's lawyers who are alumni of the focal lawyer's law school and counts of the focal lawyers' co-workers hired by each firm-office.

To analyze network effects on attainment, I examine how these two independent variables influence the intraprofessional status attained by the displaced lawyers. Specifically, I regress the prestige of each lawyer's subsequent employer on the network variables. In these analyses, I employ coarsened exact matching to reduce imbalance in the full sample and also weight observations in the (coarsely) balanced sub-sample with inverse generalized propensity scores to account for the extent to which lawyers, conditional on observed variables, rely upon alumni and co-workers in their job search. The results indicate that both prior education and employment networks influence hiring. But, while attainment is aided by prior employment contacts (i.e., co-workers) it is hindered by prior education contacts (i.e., alumni). Below, I theorize about why one might expect these results.

THEORY

Prior work emphasizes two primary ways in which social networks influence hiring (Davern, 1999). First, networks condition individual awareness of opportunities and organizational awareness of candidates (Rees, 1966; Granovetter, 1973, 1995; Wegener, 1991; Montgomery, 1991, 1992; Fernandez, et al., 2000). Second, networks reduce information asymmetries between individual candidates and employing organizations, thereby influencing the likelihood that organizations offer jobs of a certain quality to individuals who accept those offers (Lin, et al., 1981; Lin, 1999; Seidel, Polzer, and Stewart, 2000; Belliveau, 2005; Fernandez and Fernandez-Mateo, 2006; Yakubovich and Lup, 2006).

Given multiple network mechanisms (i.e., information and influence) and hiring process stages (e.g., application, screening, interview, offer, negotiation, acceptance) as well as the inferential challenges discussed above, it is unsurprising that prior research offers mixed findings on the relationship between networks and attainment. For example, a study of working males in upstate New York found that using network contacts to obtain a job aids status attainment (Lin, Ensel, and Vaughn, 1981). But, a study of working males in Detroit found that connections to employers did not aid status attainment (Marsden and Hurlbert, 1988). A later analysis of the Detroit data found that attainment was aided only by using a contact of high occupational prestige in the job search; but, the theorized influence mechanisms could not be differentiated from an alternative mechanism – individuals with prestigious contacts are aware of open positions that are disproportionately high status (Davern, 1999). Later work further speculates that the tendency of individuals to associate with contacts of similar status could largely explain positive effects of using network contacts on attainment (Mouw, 2003). There is even less consensus on the types of network contacts that are most advantageous for individual attainment. Although weak ties are commonly believed to generate more novel job leads than strong ties (Granovetter, 1973; Marsden and Campbell, 1990; Yakubovich, 2005) empirical evidence on this matter is also mixed (Bridges and Villemez, 1986; Marsden and Hurlbert, 1988; Wegener, 1991).

The lack of clarity is discouraging. In an extensive review of the literature on networks and attainment, Lin (1999: 481) concludes that the use of networks (or “informal channels”) “offers no advantage over other channels.” Mouw (2003: 870) similarly concludes that “there is little consistent evidence that using contacts affects wages or occupational prestige." But, it seems premature to dismiss the argument that some networks influence attainment or the argument that some network contacts are more valuable than others. Anecdotal evidence maintains the intuitive appeal. And, importantly, reconciliation of the mixed empirical evidence is necessary to understand how networks contribute to inequality (Montgomery, 1994).

I propose that understanding how networks influence hiring and attainment, one must isolate the determinants of mobility from the outcomes attained via mobility. Otherwise, one wonders if a favorable outcome was attained using a network or if the network is an artifact of people likely to attain favorable outcomes forming social ties with each other (Mouw, 2003). Despite empirical advances in the networks and labor markets literature like expanding samples beyond successful job-switchers (e.g., Fernandez and Weinberg, 1997; Peterson, et al., 2000), accounting for other determinants of attainment (e.g., Lin, et al., 1981; Bridges and Villemez, 1986), and including lagged outcome variables (e.g., Marsden and Hurlbert, 1988) or individual fixed effects (e.g., Yakubovich, 2005) as regression controls for unobserved heterogeneity, the fundamental issue limiting such studies remains unaddressed. One cannot easily dismiss the argument that individuals' networks simultaneously affect both the likelihood that they will change jobs and the status they attain by doing so.

I propose that credible inferences may be obtained by examining the effects of network contacts on the outcomes of individuals who transition from one employer to another for reasons unrelated to their networks (or ability or on-the-job performance). Assuming that the natural experiment studied herein provides such an opportunity, I formulate hypotheses about how prior education and prior employment networks aid the matching of individuals to employers and how the use of these two types of network contacts influence intraprofessional status attainment.

Networks and Information Asymmetries

Networks reduce uncertainty in labor markets (Rees, 1966). Typically, organizations possess greater certainty about the abilities and productivity of their current employees than potential employees. And individuals typically know more about their current employer than prospective employers. Overcoming these asymmetries is fundamental to labor market matching.

Although networks vary in their propensities to make organizations aware of candidates and individuals aware of openings (Granovetter, 1974; Burt, 1992), many types of networks (e.g., alumni, family) can reduce labor market uncertainty simply by restricting the applicants organizations consider and the positions to which individuals apply. Relying upon contacts for job leads or employees for candidate referrals are effective ways for individuals and organizations to identify good matches. In this way, networks reduce information asymmetries by bounding the consideration set of employers and candidates for open positions (Zuckerman, 1999).

Networks also reduce labor market uncertainty by influencing the consideration of applicants (Wial, 1991; Bian, 1994; Fernandez and Weinberg, 1997). Employer uncertainty about applicants can be categorized into two types: (1) uncertainty about an applicant's ability and (2) uncertainty about the applicant's expected productivity with the employer's other factor inputs (e.g., labor, technology). Employees connected to particular applicants may influence hiring decisions by attesting to an applicant's ability and expected productivity. But, some networks provide stronger evidence of ability and expected productivity than others.

One must make some *a priori* assumptions about which types of network contacts to study. Most Americans' core discussion networks are composed largely of kin (Marsden, 1987). But, such strong ties are thought to be poor sources of job leads because our closest associates (e.g., kin) know many of the same people that we know (Granovetter, 1973). Prior research suggests that prior education and employment networks influence labor market matching. For example, Granovetter's (1974[1995]) seminal study indicates that work-related contacts lead to better jobs than do family or friends. Feld's (1982) study of foci and relationships indicates that work is the third most common, after family and neighborhood, focus for relationship formation. Recent work also identifies important influences of prior education contacts in markets and careers (e.g., Cohen, Frazzini, and Malloy, 2008, 2010; Rider, 2011; Kacperczyk, 2011; Shue, 2011).

I, therefore, consider how one's prior education and prior employment networks influence hiring and attainment. I argue that either type of network contact may enable information or influence mechanisms that facilitate matching but also that the ways in which they do so motivate different predictions about their respective effects on attainment.

Prior Education Networks

Institutions of higher education facilitate the formation of inter-personal relations that probably enhance one's career prospects. For example, two individuals are more likely to form a tie and to confide in each other if their educational backgrounds are similar (Marsden, 1987). A study of 800 MBA program graduates found that 80 percent named a classmate as a close friend and that this percentage declined at a rate of only 2 percent per year (Burt, 2001). Another study found that individuals relied upon friends from their school days for support up to ten years after completing their education (Suitor and Keeton, 1997). So, direct ties form while individuals pursue higher education and these ties persist over time. It is, therefore, likely that prior education contacts will provide job leads to individuals and/or candidate referrals to organizations and also influence hiring by attesting to contact-candidate ability.

Even individuals who attended the same institution but at different times (i.e., indirect ties) may aid each other's job searches because one's affiliations with higher education institutions are often laden with sentimentality. Identification with one's college or university encourages behaviors like making charitable donations (O'Reilly and Chatman, 1986; Mael and Ashforth, 1992), sharing valuable information (Cohen, et al., 2008), or, plausibly, helping someone find a job. For example, U.S. law firms exhibit firm- and office-level clustering of partners by law school attended beyond what one could reasonably attribute to geography or school prestige (Parkin, 2006; Oyer and Schaefer, 2010. Such clustering may be attributed to lawyers favoring graduates of their law schools in hiring and promotion decisions or leveraging law school networks for information on potential hires. In either case, shared education backgrounds facilitate hiring.

Prior education networks may also provide advantageous access to information that alleviates information asymmetries. For example, equity analysts provide better stock recommendations for companies whose senior managers attended the same college or university as the analyst, whether they were in the same cohort or not (Cohen, et al., 2010). Venture capital firms tend to co-invest with other firms whose members share more prior education affiliations with their members (Rider, 2011). Mutual fund managers are more likely to transition to entrepreneurship as more prior education contacts make the transition (Kacperczyk, 2011). And executive compensation is more similar within randomly-assigned MBA cohorts than across MBA cohorts (Shue, 2011). These findings imply strong information-sharing within prior education networks.

In summary, prior research implies that prior education contacts will influence labor market matching by enabling one's contacts to attest to their ability and expected productivity. Individuals, then, should be more likely to be hired by a focal organization the more that organization tends to employ graduates of their degree-granting education institution.

Hypothesis 1: The greater the presence of an individual's alumni network at an organization, the more likely an individual seeking employment is hired by the organization.

Prior Employment Networks

Prior employment experiences also facilitate the formation of inter-personal relations that probably influence hiring decisions. For example, the General Social Survey documents that nearly 50 percent of individuals report a co-worker as being among their closest friends (Marks, 1994) and co-workers constitute large portions of managers' core discussion networks (Carroll and Teo, 1996). A study of Iowa state legislators found that friendships tended to form between legislators who served on the same committees (Caldeira and Patterson, 1987). It is, therefore, likely that prior employment networks will provide job leads to individuals and/or candidate referrals to organizations.

Working together may enable co-workers to evaluate each other's abilities, form a common knowledge base (Beckman, 2006), generate trust (Tsai and Goshal, 1998), and develop relationship-specific skills and complementarities (Hayes, Oyer, and Schaefer, 2006) that create incentives to continue working together, even at a different employer. For example, a study found that mutual fund directors and advisory firms continue working with each other even after changing employers (Kuhnen, 2009). Another found evidence of social influence among colleagues' career transition decisions (Stuart and Ding, 2006). One might reasonably expect, then, that prior employment contacts raise awareness of appropriate opportunities and also influence hiring decisions. An individual's awareness of employment opportunities should be increasing with the number of former co-workers employed by an organization. And those co-workers should be in positions to influence hiring decisions by attesting to a candidate's expected productivity, given their insights into the candidate's ability and potential for complementarity. Consequently, individuals should be increasingly likely to find jobs at employers that hire more of their co-workers.

<u>Hypothesis 2</u>: The more of an individual's former co-workers hired by an organization, the more likely an individual seeking employment is hired by the organization.

Thus far, it is argued that both prior education and employment networks facilitate the labor market matching of individuals to organizations. But, there are subtle differences in the theorized mechanisms that motivate different predictions about how prior education and employment contacts influence status attainment. Both prior education and employment networks may help individuals find jobs by channeling information on opportunities. An individual may also be referred for a position by alumni or former co-workers and those contacts may influence decision makers to hire the individual. But, shared prior education and prior employment experiences provide different insights into a candidate's expected productivity. Consequently, the two types of networks motivate competing hypotheses regarding the status attained by using prior education and employment networks in one's job search.

Two aspects of shared prior education experiences may hinder the efforts of one's prior education contacts to aid attainment. First, prior education affiliations are often laden with sentimentality (O'Reilly and Chatman, 1986; Mael and Ashforth, 1992). Personal biases that favor similarly-educated others may lead hiring organizations to discount recommendations made by a candidate's prior education contacts. Second, insights into a candidate's ability and expected productivity based on a shared educational background are not context-specific. Performance in an education setting is a signal of general ability that may inform employer expectations of an individual's on-the-job productivity (Spence, 1973). But, such insights are likely less relevant than context-specific insights into an individual's ability or productivity with the organization's factors of production. Consequently, prior education networks provide limited relief to the information asymmetries that constrain labor market matching.

If prior education networks only weakly address information asymmetries between employers and candidates, then relying on prior education networks instead of contacts in possession of more context-specific, fine-grained insights should hinder one's efforts to attain intraprofessional status. This logic implies that individuals who find positions through alumni networks probably do so because better options are not available to them.

Hypothesis 3: The greater the presence of an individual's alumni network at the hiring organization, the lesser the intraprofessional status attained by the individual.

For three reasons, shared prior employment experiences should enhance the efforts of one's prior employment contacts to aid attainment. First, prior employment affiliations are probably less laden with sentimentality than prior education affiliations. So, former co-workers' assessments of an individual's ability are less likely to be discounted (or discounted less) than assessments offered by a candidate's prior education contacts. Second, working together enables individuals to collect context-specific insights into co-workers' abilities that are probably more relevant to assessments of candidate ability that are shared education experiences. So, prior employment contacts are probably more likely to address hiring organizations' uncertainty about candidates' abilities.

Third, working together enables individuals to develop complementarities in joint work production (Hayes, et al., 2006). Organizational selection and socialization processes tend to retain employees who exhibit similar levels of person-organization fit (Chatman, 1991). Over time, co-workers further develop shared understandings about appropriate work behaviors that enable them to work effectively together (e.g., Baty, Evan, and Rothermel, 1971; Beckman, 2006; Eisenhardt and Schoonhoven, 1990). If so, then the products of their labor are more valuable when working together than when working individually (Groysberg and Lee, 2009). For example, star security analysts who transition to other employers without their co-workers exhibited lower performance than those who transitioned as a team (Groysberg, Lee, and Nanda, 2008).

To the extent that complementarities form among co-workers, then a candidate's former co-workers can speak convincingly about a candidate's expected productivity and influence hiring. If hired, then the candidate will not be working with strangers. So, reasonable expectations about their expected joint productivity can be formed. If prior employment networks strongly address information asymmetries between employers and candidates, then former co-workers are likely to help individuals attain a position of higher status than their prior position.

Hypothesis 4: The more of an individual's former co-workers hired by an organization, the greater the intraprofessional status attained by the individual.

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 organized as partnerships, in these firms 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 revenues-per-lawyer and the greater the firm's profits-per-partner, the greater is the compensation and intraprofessional status of firm members.

Figure 1 depicts these metrics for the 2009 *Vault Top 100 Law Firms* (the Vault 100), an industry ranking of law firm prestige based on annual surveys of thousands of legal professionals, by plotting the mean values for each decile of the Vault 100 rankings. *Figure 1* also indicates assortative matching (Becker, 1974) of prestigious law school graduates into the most prestigious law firms, as evidenced by the lower average numeric law school ranks of lawyers employed by firms in more prestigious deciles. This relationship is in large part attributable to the fact that many law firms restrict associate hiring to specific law schools (Parkin, 2006; Oyer and Schaefer, 2010).

Insert Figure 1 About Here

Most lawyers join a firm as an associate and become eligible for partnership after seven to ten years. Like academics and scientists (e.g., Merton, 1968; Allison, Long, and Krauze, 1982; Zuckerman, 1988), those that visibly demonstrate ability early in their careers with Law School Admission Test scores, law school course grades, and job interview performance improve their chances of being hired by the most prestigious and profitable firms, where they can expect assignments with prestigious clients and mentorship from highly successful partners (Lazega, 2001; Kay, Hagan, and Parker, 2008; Kay and Wallace, 2009). So, they are recognized by high status lawyers who might consider them for positions at other firms, they develop client relationships that increase their chances of attaining partnership at their employer, and they enjoy relationships with law school alumni and co-workers that provide timely access information on mobility opportunities. In this way, upward mobility in the legal profession is governed by a cumulative career advantage process (Merton, 1968; Rider, Negro, and Roberts, 2011).

Six Dissolution Stories

Dissolutions of large law firms are fairly rare (Heinz, 2009) so the dissolution of several around the same time is indicative of increasingly poor economic conditions for U.S. law firms during this time period. The six firms dissolved unexpectedly and fairly quickly. Few employees would have expected their firm to dissolve; in most cases, a merger with another firm is more likely. Importantly, the six firms vary greatly in terms of size, prestige, practice areas, geographic locations, and other key dimensions. So, the six firms are fairly representative of the U.S. legal industry. Below, I briefly detail each of their 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, 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).

Insert Table 1 About Here

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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 National Law Journal, 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 National Law Journal, 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 National Law Journal, 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 Wolf Block also employed government lobbyists 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. Partners departed WolfBlock throughout 2008 and 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.

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 had been operating a ponzi scheme that defrauded hedge fund investors of more than $100 million by selling bogus securities. 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 Drier 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.

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 National Law Journal 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).[1]

Sample

From the biographies collected from the six dissolved law firms' websites, I constructed a sample of 1,459 lawyers. For 1,426 lawyers (97.7 percent) of these lawyers, I was able to collect 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. I excluded 33 lawyers from the analysis because I could identify neither the year they were admitted to the bar nor the law school they attended.

[1] 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.

I then utilized internet searches of 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 full sample graduated from 120 law schools that vary in terms of prestige and 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 legal experience.

Insert Tables 2a and 2b About Here

Analyses and Dependent Variables

First, I use probit models to estimate the likelihood that a lawyer obtains employment and is located by my sampling methods ("employment analyses"). In these employment analyses, the dependent variable is coded as 1 for the 1,248 lawyers for whom I could find subsequent employment data (88 percent) and 0 for the 178 lawyers for whom I could not (12 percent). To account for sample selection bias in subsequent analyses, I also model the likelihood that a lawyer is employed by a NLJ 250 firm (a more restrictive coding of this dependent variable). This outcome is coded as 1 for the 933 lawyers who I found to be re-employed in a NLJ 250 firm and 0 for all others. Of the 1,426 lawyers in the sample, 933 (or 65 percent) regain employment within the NLJ 250.

Second, for all lawyers that regain employment in a NLJ 250 firm, I model the likelihood that a given firm hires a focal lawyer after their firm dissolves ("matching analyses"). In these matching analyses, 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 utilize conditional logit models that compare each lawyer's subsequent employer to other firm-offices within the same metropolitan area that might have hired the lawyer. The set of "at-risk" hiring firm-offices is sampled from the NLJ 250. I formed a sample by including all realized outcomes (i.e., the firm-office that hires each lawyer) and matching each of those observations to up to 10 firm-offices within the same Core Based Statistical Area (CBSA), as defined by U.S. Office of Management and Budget. Due to an inadequate number of matches in CBSAs with few NLJ 250 offices, I included only 916 of the 933 NLJ 250 lawyers in this analysis. This produced a sample of 9,983 lawyer-firm-office dyads for 916 lawyers in 19 metropolitan areas that could have been hired by one of the 875 offices operated by the 188 law firms in the sample. By analyzing the specific lawyer-firm-office matches realized and the presence of alumni and co-workers at not only the realized employer but also potential employers of each lawyer, I test Hypotheses 1 and 2 -- the predicted effects of prior education and employment network contacts on hiring.

Third, I use both ordinary least squares regressions and probit models to model status attainment for each lawyer that regains employment in a NLJ 250 firm ("attainment analyses"). Missing data further reduced the attainment sample to 902 lawyers. Two dependent variables are used. The first is the hiring firm's average prestige score in the 2009 Vault 100 rankings of U.S. law firms. 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' scores, I obtained prestige scores for the top 167 firms included in the

Vault survey. For the 16 firms included in the analyses but not in the Vault data, I assigned the lowest prestige score (2.247) of firms included in the 2009 Vault rankings. This effectively places a lower bound on firm prestige.

The second dependent variable in these attainment analyses is a binary outcome coded as 1 for upward status mobility if the lawyer regained employment at a firm with a higher prestige score than their prior (dissolved) firm received in 2008. For example, former lawyers employed by the 62nd ranked firm in the 2008 Vault 100, Heller Ehrman, who regained employment at the 11th ranked firm in the 2009 Vault 100, Covington & Burling, experienced upward status mobility. Note that some lawyers from WolfBlock, Dreier, or Morgan & Finnegan might regain employment at higher status firms that are not ranked in the NLJ 250 but that I cannot reasonably infer the relative status of these lawyers' prior and subsequent employers.[2] I can only code movement from outside the Vault 100 into the Vault 100 as upward status mobility so this is the outcome I model in these mobility analyses. For example, a former lawyer of WolfBlock, which was not ranked in the 2008 Vault 100, who regained employment at Akin, Gump, Strauss, Hauer & Feld, LLP, the 36th most prestigious firm in the 2009 Vault 100 experienced upward status mobility.

Independent Variables

To analyze the effects of prior education network contacts on finding a job and attaining status, I constructed a measure to proxy for lawyers' access to job opportunities through law school alumni networks. I obtained education data from the Martindale-Hubbell Law Directory for over 107,000 lawyers in all offices of the largest 250 U.S. law firms (by number of lawyers). This set of firms was identified by starting with the NLJ 250 rankings in 2008 and augmenting that list with the

[2] Note that firm-level fixed effects account for firm-level differences and that excluding these three firms in sub-sample analyses produced results similar to those reported here.

law firms listed on LawPeriscope.com (the data source for the sample used in Oyer and Schaefer, 2010). I also added any firms that hired a lawyer in the sample as of July 2009, when most of the data was obtained. The process produced data on 1,179 offices of 267 U.S. law firms. After random sampling from this data to construct a case-control "at-risk" sample of NLJ 250 firms and offices (described below), the data consist of 875 offices of 188 U.S. law firms.

The Martindale-Hubbell data enabled me to produce a geographic distribution of law school alumni networks. For firms or offices not listed in the Martindale-Hubbell directory or for those with missing data, I obtained comparable data from West Law. For each firm-office, I constructed a measure that is the number of attorneys that graduated from a specific law school divided by the total number of attorneys employed in that office (i.e., firm-office-school share). For each lawyer listed in the Martindale-Hubbell data, I also coded their level (e.g., associate, partner), area(s) of practice, office location, law school attended, and, if available, year in which they passed the bar. Hypothesis 1 predicts a significant positive coefficient on this variable in the matching analyses and Hypothesis 3 predicts a significant negative coefficient on this variable in the attainment analyses.

To analyze the effects of prior employment contacts on labor market matching and status attainment, I created a variable that is the count of lawyers formerly employed by Dreier, Heller, Morgan & Finnegan, Thacher, Thelen, or WolfBlock, respectively, that were hired into each of the firm-offices in the "at-risk" sample (minus the focal lawyer, if hired by that firm-office). 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 each of the two lawyers in the analyses (i.e., Arnold & Porter hired one other Heller lawyer). Note that 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 add 1 to all observations and then transform the sum by its natural log in order to desensitize coefficient estimates to this variable's skewed distribution (i.e., a long right tail).[3] Hypothesis 2 predicts a significant positive coefficient on this variable in the matching analyses and Hypothesis 4 predicts a significant positive coefficient on this variable in the attainment analyses.

Importantly, the co-worker variable counts simultaneous transitions of a lawyer's former co-workers to a post-dissolution employer. This is, of course, different from the count of former co-workers that left the failed firm prior to dissolution and are now "inside" contacts at potential employers. My rationale for constructing the variable this way is that moving with co-workers directly addresses the information asymmetries regarding ability and complementarity that one's former co-workers might address on "the inside" to influence hiring decisions. My identifying assumption, then, is that these co-worker co-movements proxy for the influences of prior employment networks.

Control Variables

I rely primarily on fixed effects to account for heterogeneity by dissolved firm, geographic location, and practice area, but also include several control variables. All models include fixed effects for the six dissolved firms (i.e., Heller, Thelen, Thacher, WolfBlock, Dreier, and Morgan & Finnegan). For each lawyer, I also include indicator variables for office locations and practice areas in the models as unreported fixed effects.

The office location fixed effects include Los Angeles, Northern New Jersey, New York, Philadelphia (including suburban areas in Southern New Jersey), San 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

[3] This 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.

offices in the greater New York City area, the San Francisco Bay Area, Philadelphia, or Washington, DC.

The 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." See *Tables 2a* and *2b* for more information on both office locations and practice areas.

For each lawyer, I recorded gender by coding first names commonly associated with the male or female gender (e.g., Andrew, James, Jessica, Valerie) as either male or female. For ambiguous names, I relied upon lawyer photos or biographical information like membership in a women's bar association to code gender. This coding scheme was checked by four research assistants who worked independently; I corrected 9 observations where the majority of research assistants coded gender differently than I did. Using the same photos and biographical information, these research assistants also coded 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 (4 research assistants and the author) coded an individual as "White" or "Black," respectively, and 0 otherwise. The omitted category includes lawyers classified primarily as Arab, Asian, Indian, Hispanic, Latino, or Middle Eastern descent.

To account for local access to alumni networks, I included a variable for each lawyer that is the percentage of all NLJ 25*0* lawyers within the lawyer's CBSA that graduated from the focal lawyer's law school.[4] I identified each lawyer's law school by using firm website biographies or the online versions of either the Martindale-Hubbell or the West Law directory. I also included law school rank to account for heterogeneity in law school prestige using law school ranks that were obtained from the 2008 *U.S. News & World Report "Best Law School"* (*USN&WR*) rankings.[5] All unranked law schools were assigned a rank of 120.

I coded a partner indicator variable as 1 if a lawyer was a partner at their prior (dissolved) firm and 0 if the lawyer was an associate, counsel, or another title. I computed the number of years of legal experience for each lawyer by subtracting the year in which the lawyer was first admitted to a state bar from 2008; I added one and transformed the sum by the natural logarithm to adjust for the skewness of experience (long right tail).

For the matching analyses, I included additional control variables to account for otherwise unobserved heterogeneity among potential hiring firms. I 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 potential hiring firm scale, I constructed a variable that is the number of lawyers employed in each firm-office listed in the Martindale Hubbell data, or West Law if Martindale-Hubbell figures were unavailable. I also computed a firm-level variable that is the percentage change in firm headcount between 2008 and 2009, according to the *NLJ 250*. For firms

[4] 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 five lawyers in such CBSAs.

[5] This data is discussed extensively in Espeland and Sauder (2007), Sauder (2008), and Sauder and Espeland (2009).

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. These variables account for the potential hiring firm's recent financial performance.

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 of 1,248 lawyers may be biased by the sampling methods. 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). Holding level constant (i.e., partner or otherwise), 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. Perhaps an early indicator of the importance of local alumni networks, 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 to include only those lawyers who were employed by firms in the 2009 NLJ 250 (n = 933 lawyers). Note that the likelihood of being included in the NLJ 250 sample does not vary with law school prestige. Results from Model 6 were used to generate predicted values for the first-stage of the Heckman sample selection correction technique used in the status attainment analyses. The inverse Mills ratio was calculated as the reciprocal of the predicted probability that a lawyer was employed and located by the sampling methods, using the coefficients of Model 6.

We now turn to the hypothesis tests. Summary statistics and correlations for the variables in the matching analyses are presented in *Table 5*; results are presented in *Table 6*. Note that the lawyers included in this sample must have regained employment at a NLJ 250 firm. I model the likelihood that a focal lawyer regains employment within a given NLJ 250 firm's office.

Conditional logit models parsimoniously account for all lawyer-specific covariates by grouping observations on the focal lawyer (equivalent to a lawyer fixed effect).

Insert Tables 5 and 6 About Here

Model 1 indicates that, consistent with Hypothesis 1, lawyers are more likely to be hired into NLJ 250 firm offices populated with proportionately more graduates of the lawyer's law school. Model 2 indicates that, consistent with Hypothesis 2, lawyers are more likely to be hired into NLJ 250 offices that hire more of the focal lawyer's former co-workers. Model 3 includes both covariates in the same model; both effects are significant and positive in Model 3. These results support the argument that prior education and employment networks facilitate labor market matching of individuals and organizations.

Model 4 includes two more control variables to account for variance in the availability of positions at the firm-offices in the sample: the number of attorneys employed in the focal firm's office and the change in firm-level headcount between 2008 and 2009. All else equal, larger firms should be more likely to make a hire in a given year. It is not clear whether shrinking or growing firms will be more likely to hire in a given year but the headcount change variable should account for either tendency. The key results are largely unaffected. Model 5 includes additional controls to account for omitted variable bias. 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. But, these coefficients are not statistically significant (and neither are alternative time lags) and the key results are unaffected by their inclusion.

Model 6 includes the two firm-level performance measures and the leverage ratio (partners per associate) for the subset of lawyers (n = 824) 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). The positive effects of the alumni and co-worker variables are robust to the inclusion of these controls. These matching analyses strongly support Hypotheses 1 and 2. A lawyer's mobility is facilitated by prior education and prior employment network contacts.

The next analysis examines the relative value of prior education and prior employment network contacts to intraprofessional status attainment. Summary statistics and correlations for the variables in these attainment analyses are presented in *Table 7*; results of the hypothesis tests for Hypotheses 3 and 4 are presented in *Table 8*. I analyze the sub-set of 902 lawyers whose subsequent employers were identified as NLJ 250 firms by my internet search methods and whose law school alumni data could be obtained for the hiring firm. The explanatory variables include the same variables included in the employment analyses as well as the proxies for inter- and intra-organizational relationships.

The first dependent variable is the average firm prestige score compiled by Vault using ratings made annually by thousands of U.S. attorneys. Models 1 through 4 use ordinary least squares regressions of firm prestige on individual covariates. The results reported here are manually corrected for the sample selection rule (i.e., regained employment in the *NLJ 250*) using the inverse Mills ratio generated by Model 6 of Table 4 as a covariate in all models. Model 1 presents a controls-only model; a lawyer's post-dissolution attainment prospects are largely determined by the prestige of their dissolved firm, their geographic location, and their area of legal practice (all included as unreported fixed effects). Model 2 includes the law school alumni variable and indicates that the prestige of one's subsequent employer is decreasing with the percentage of firm-office lawyers who are alumni of the focal lawyer's law school. This result is consistent with Hypothesis 3. Model 3 includes the co-workers variable and, inconsistent with Hypothesis 4, the effect of moving with co-workers is statistically insignificant. Model 4 produces similar results when both alumni and co-workers variables are included in the specification.

Insert Tables 7 and 8 About Here

Model 5 depicts results of a probit model in which the dependent variable takes a value of 1 if the focal lawyer regained employment at a higher status firm than their (dissolved) prior employer and 0 otherwise; approximately 51 percent of the 902 lawyers do so. This model supports both Hypothesis 3 and Hypothesis 4: a lawyer's chances of experiencing upward status mobility are decreasing with the percentage of the hiring firm-office's lawyers who graduated from the focal lawyer's law school and increasing with the number of co-workers who move with the focal lawyer to the hiring firm-office.

These results are supportive of the Hypotheses 3 and 4 but skepticism is warranted. Given that Table 6 demonstrates a tendency of lawyers to leverage prior education and/or employment networks to regain employment, the "treatment" variables of interest in Table 8 (i.e., alumni and co-workers) are not randomly generated and neither are the "treatment dosages" (i.e., alumni percentages and co-worker counts). The true effects of these variables on attainment may be biased by differences in control variables across observations that vary in "treatment" status as well as a lack of balance across observations covering the full range of "treatment dosages." Two statistical adjustments are warranted. Coarsened exact matching reduces covariate imbalance within the full sample by enabling analysts to drop observations with poor counterfactual matches (Blackwell, Iacus, King, and Porro, 2009). Further weighting the remaining observations by a generalized propensity score, or predicted treatment dosage conditional on observed covariates, enables one to account for biases associated with differences among the remaining observations' covariates (Hirano and Imbens, 2004).

To put the concern and the remedy in context-specific terms, one could be reasonably concerned that lawyers of varying experience levels (e.g., partners versus associates) or practicing a particular type of law in a given geographic area (e.g., intellectual property in Silicon Valley), for example, will be more or less likely to move with former co-workers. This would create imbalance in the experience, practice area, and location variables for observations in the high and low ranges of the co-worker variable. Coarsening the control variables (i.e., reducing continuous variables to ordinal variables) and then matching observations exactly on the coarsened values enabled me to identify and drop observations that lack a good counterfactual match. Further weighting the observations by the predicted value of the key explanatory variable, conditional on the controls, enabled me to further reduce bias associated with some covariates influencing the propensity to move with co-workers (or the number of co-workers).

The large number of firm, geographic, and practice area fixed effects (up to 23, depending on specification) included in the models prevents exact matching on all covariates. But, those chosen for coarsened matching are reasonably important in this context. I implemented coarsened exact matching and generalized propensity score weighting by using the Stata 12 "cem" command (Blackwell, et al., 2009) to match the 902 observations in the attainment analysis sample exactly on the female, partner, white, and dissolved firm indicator variables. I further matched observations exactly on three geographic indicator variables: Northern California (i.e., San Francisco or Silicon Valley), Southern California (i.e., Los Angeles or San Diego), and the Eastern Seaboard (i.e., Boston, Hartford, New Jersey, New York, Philadelphia, Stamford, Washington, or Wilmington). I also matched exactly on Litigation and Corporate practice area indicator variables.[6] I then coarsened the law school rank variable into three levels (i.e., ranked top 10, ranked 11 to 25, and ranked higher than 25) and both the legal experience and CBSA area law school concentration variables into two levels: (1) below the median and (2) median and above.

When estimating the alumni treatment effect on attainment, I coarsened the co-worker hires variable using Stata 12's automated coarsening algorithm. When estimating the co-worker treatment effect on attainment, I coarsened the alumni hires variable. Stata's automated coarsening algorithm for these variables produces reasonable balance in the data without allowing user choices to bias the process. Note that I matched on alumni when estimating effects of co-workers (and vice versa), so the regressions enable evaluation of only one independent variable at a time.

Coarsening for balance drastically reduced the sample size. For the alumni effect, I was able to analyze 612 lawyer observations involving 95 hiring firms (reduced from 902 lawyers and 109 firms, respectively, in the full *NLJ* 250 sample). For the co-worker effect, I analyzed 409 lawyer observations involving 71 hiring firms. I checked the balance of observations across three levels of each of the explanatory variables of interest (i.e., zero, below median, median and above) and found no statistically significant ($p > 0.05$) differences in covariate means across these levels. This indicates that the data are reasonably well-balanced. Note that the greater reduction for the co-worker effect sub-sample indicates greater imbalance on the alumni variable within the full sample.

A coarsely-balanced sub-sample may still produce biased estimates of mean treatment effects if covariates influence the level of treatment (i.e., the dosage). Therefore, I also used generalize propensity score weighting for the remaining observations to further account for potential bias. Using the "gpscore" command in Stata 12, I regressed the alumni variable on all

[6] Although matched coarsely on these aggregated variables, the full set of geographic labor market indicator variables and practice area indicator variables are included in the full regressions.

other covariates included in the models, generated generalized propensity score (i.e., the predicted level of treatment), and then weighted observations by the inverse of the generalized propensity score. The results of these analyses are reported in Model 1 of Table 9. Similarly, I regressed the co-worker variable on all other covariates, generated generalized propensity scores, and weighted all observations by the inverse of each lawyer's score to produce the results reported in Models 2 and 3 of Table 9.

Insert Table 9 About Here

Table 9 presents results of the attainment analyses adjusted for balance and predicted treatment dosage. Model 1 of Table 9 indicates that after reducing imbalance and weighting observations by their predicted level of treatment, Hypothesis 3 is supported. The more alumni employed by the hiring firm-office, the lesser is the prestige of the individual's subsequent employer. To put this effect in context, a one standard deviation increase in the share of alumni employed by the firm-office that hires a focal lawyer reduces expected firm prestige by approximately 4 percent. The equivalent change in the prestige expectation is the difference between the firm ranked 83rd in the Vault 100 rankings and the firm ranked 94th.

Model 2 of Table 9 supports Hypothesis 4. The more co-workers an individual moves with following their employer's dissolution, the greater the prestige of their subsequent employer. To put this effect in context, a one standard deviation increase in the number of co-workers that move with a focal lawyer to the hiring firm-office increases expected firm prestige by approximately 4 percent. This is equivalent to the difference between the firm ranked 81st in the Vault 100 rankings and the firm ranked 75th. Note that the mean expected level of prestige differs for Model 1 and Model 2 (83rd versus 81st) because coarsened exact matching produced different sub-samples for the analyses. But, if a lawyer faced a choice between leveraging their alumni network and moving with co-workers then the results indicate that moving with co-workers leads to a position at a substantially more prestigious firm.

Model 3 of Table 9 replicates the co-worker results when all former Thacher lawyers are excluded from the matching, propensity score, and regression steps of the analysis. So, the results are insensitive to including or excluding the 94 lawyers who moved together to Sonnenschein, Nath, & Rosenthal's New York City office following Thacher's dissolution. In sum, these results provide strong support for the argument that both prior education and employment networks facilitate labor market matching but that alumni hinder status attainment while co-workers aid attainment.

DISCUSSION

Recent work on networks, labor markets, and careers has called for research designs that more clearly identify network mechanisms than influence individual labor market outcomes and, more broadly, contribute to inequality (Mouw, 2003, 2006; DiPrete and Eirich, 2006). This study answers these calls by utilizing an exogenous cause of inter-organizational mobility to identify the role of network contacts in structuring opportunities to attain greater intraprofessional status by changing employers. The results of this study indicate that prior education and employment network contacts do indeed facilitate hiring but that only prior employment contacts, and not prior education contacts, are likely to aid intraprofessional status attainment.

As work in personnel economics indicates, individuals who work together develop employer specific-skills (Groysberg, Lee, and Nanda, 2008) or other complementarities (Oyer & Schaefer, 2010) that help lawyers create greater value together at one employer than they could create separately at multiple employers. Such complementarities seem a plausible explanation for why lawyers that move to new employers with more co-workers are more likely to experience upward status mobility than lawyers who move with fewer or no co-workers. To the extent that potential employers face uncertainty about a job candidate's context-specific ability and their potential to complement others in team production, prior employment networks are probably more capable of addressing these concerns than are prior education networks. Shared prior education experiences are, after all, not context-specific and judgments of ability are likely to be biased by institution-specific sentimentality.

More generally, in many professions occupants of high status positions receive disproportionate recognition and support for their work. A common explanation for such disparities is that initially-small advantages of one individual (or group) over another exert positive causal effects on future recognition and resources, amplifying the initial advantage and attracting further recognition and resources (Merton, 1968, 1988). Numerous studies of cumulative career advantage (Cole and Cole, 1973; Zuckerman, 1977; Allison, Long, and Krauze, 1982; Smith and Abbott, 1983; Bielby and Bielby, 1996; Fernandez-Mateo, 2009) document how initially-small advantages of individuals (and groups) produce increasing disparities over time by structuring training and employment opportunities, access to resources, and attention paid to one's work (Merton, 1988).

This study indicates that the development of co-worker complementarities contributes to the accumulation of career advantages. But, professional careers are becoming increasingly inter-organizational (Capelli, 1999; Bidwell and Briscoe, 2010). More specifically, legal labor market activity indicates that developing such complementarities is increasingly difficult because the rate of lateral transitions by partners of the largest law firms has increased over the last decade. An analysis of Incisive Legal Intelligence's Lateral Partner Moves Database (Rider and Tan, 2011) indicates that while the modal lateral partner transition in 2000 involved a lawyer moving from a firm outside of the American Lawyer 200 into the American Lawyer 200 (57 percent of all transitions in 2000) in 2009 the modal transition was from one American Lawyer 200 firm to another (44 percent of moves). As partners move laterally at greater rates, it becomes more difficult for junior lawyers to develop valuable co-worker complementarities, thereby rendering initial placement and within-firm promotions more relevant for status attainment.

As for initial placement, most firms limit the law schools from which they recruit associates. Assortative matching of prestigious law firms and graduates of prestigious law schools (Phillips & Zuckerman, 2001) probably is becoming more influential in shaping mobility and attainment opportunities for U.S. lawyers. As *Figure 1* illustrates, graduates of prestigious law schools are more likely to be employed by prestigious law firms than graduates of less prestigious law schools.

Such sorting may be become increasingly important to the accumulation of legal career advantages. This insight is consistent with the sociological view that institutions of higher education sort and stratify populations (Jencks and Riesman, 1968; Blau and Duncan, 1967; Karabel, 1984, 2005) by "regulating the mobility processes underlying the allocation of privileged positions in the society" (Stevens, Armstrong, and Arum, 2008: 128). Future research might investigate how associates are allocated to practice areas or specific partners and how such allocations influence career outcomes like likelihood of mobility and status attainment. The role of law school alumni networks in law firm hiring and staffing practices also seems worthy of investigation.

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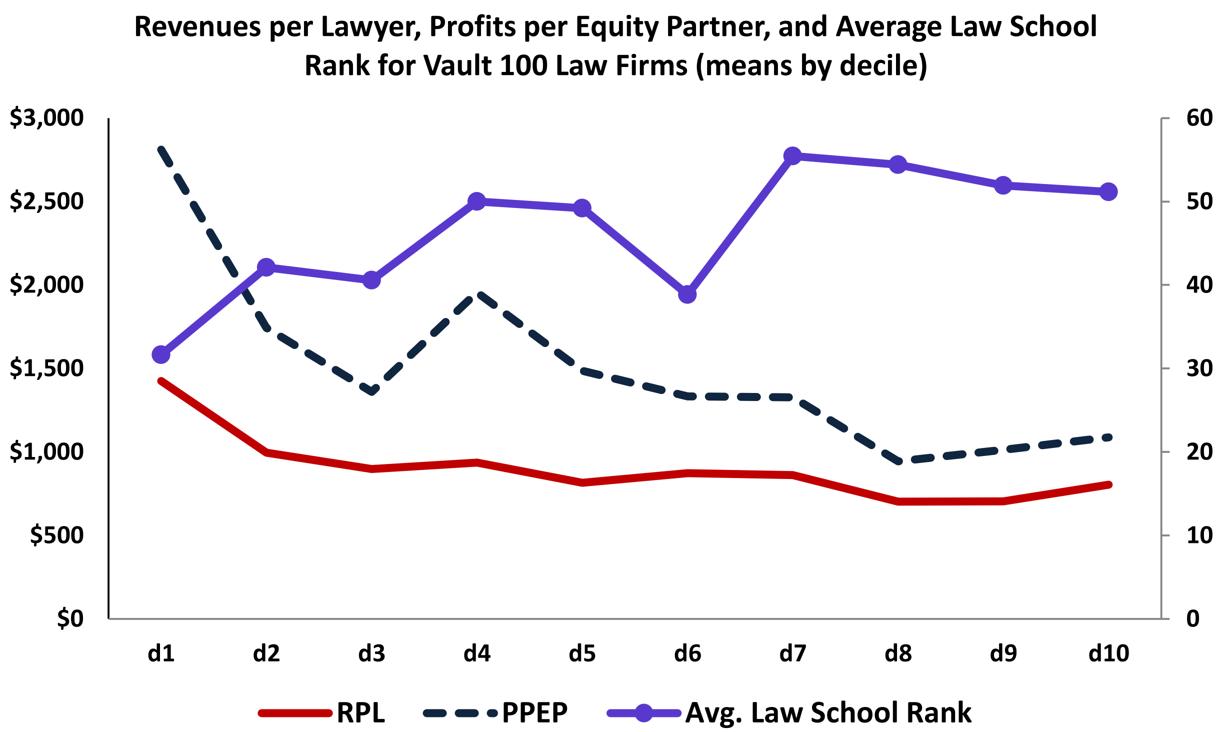
Figure 1



Revenues per Lawyer, Profits per Equity Partner, and Average Law School Rank for Vault 100 Law Firms (means by decile)
$3,000
$2,500
$2,000
$1,500
$1,000
$500
$0
60
50
40
30
20
10
0
d1
d2
d3
d4
d5
d6
d7
d8
d9
d10
RPL
PPEP
Avg. Law School Rank

Table 1: Lawyers in sample, by dissolved firm.

Firm	Partners	Associates	Other	Total	Employed	% Employed
Dreier	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	188	152	52	392	367	94%
WolfBlock	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.

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

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

Labor market	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 (n=1,426 lawyers).

		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	0.52	1.00							
(3)	**Female (0/1)**	0.31	0.46	-0.06	-0.05	1.00						
(4)	**Partner (0/1)**	0.42	0.49	0.16	0.18	-0.20	1.00					
(5)	**ln (years of legal experience)**	2.48	0.97	0.03	0.04	-0.25	0.61	1.00				
(6)	**Rank of law school attended**	40.6	37.9	0.00	-0.04	0.02	-0.07	-0.09	1.00			
(7)	**% of local attorneys from lawyer's law school**	0.08	0.06	0.18	0.12	-0.03	0.03	0.05	-0.13	1.00		
(8)	**Black (0/1)**	0.04	0.18	-0.11	-0.07	0.07	-0.01	-0.06	-0.02	0.02	1.00	
(9)	**White (0/1)**	0.86	0.34	0.19	0.12	-0.17	0.19	0.21	0.00	0.07	-0.38	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)	
[dependent variable]	*[employed]*		*[employed]*		*[employed]*		*[employed]*		*[employed]*		*[NLJ 250]*	
Female (0/1)	-0.058		-0.051		-0.038		-0.067		-0.058		-0.030	
	(0.100)		(0.103)		(0.106)		(0.103)		(0.106)		(0.084)	
Partner (0/1)	0.810	**	0.871	**	0.931	**	0.867	**	0.921	**	0.645	**
	(0.122)		(0.123)		(0.127)		(0.125)		(0.129)		(0.100)	
ln (years of legal experience)	-0.246	**	-0.265	**	-0.290	**	-0.260	**	-0.289	**	-0.175	**
	(0.066)		(0.066)		(0.068)		(0.065)		(0.067)		(0.052)	
Rank of law school attended	0.002	*	0.004	**	0.003	*	0.004	**	0.003	*	0.001	
	(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)	
% of local attorneys from *i*'s law school	5.31	**	5.84	**	6.88	**	5.81	**	6.84	**	2.36	**
	(1.15)		(1.17)		(1.22)		(1.17)		(1.21)		(0.705)	
Black (0/1)	-0.514	*	-0.586	*	-0.543	*	-0.602	*	-0.570	*	-0.345	
	(0.218)		(0.227)		(0.242)		(0.237)		(0.252)		(0.227)	
White (0/1)	0.535	**	0.520	*	0.501	**	0.513	**	0.485	**	0.371	**
	(0.125)		(0.126)		(0.129)		(0.128)		(0.131)		(0.118)	
Constant	0.653	**	0.658	*	1.01	**	0.313		0.641	†	0.801	**
	(0.195)		(0.264)		(0.339)		(0.317)		(0.388)		(0.306)	
N (observations)	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	-467.76		-444.27		-422.18		-437.35		-415.27		-754.36	
Wald Chi-square (d.f.)	107.71	(7)	156.8	(12)	190.6	(20)	184.1	(23)	226.5	(31)	283.3	(31)

Robust standard errors in parentheses.

** $\rho < 0.01$; * $\rho < 0.05$; † $\rho < 0.10$; two-tailed hypothesis tests.

Table 5: Summary statistics and correlations of variables in lawyer-firm-office matching analyses (n= 9,983 dyads).

		Mean	St. Dev.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
(1)	Lawyer *i* hired by firm-office *j* (0/1)	0.09	0.29	1.00									
(2)	% of firm-office *j* lawyers from lawyer *i*'s law school	0.06	0.10	0.07	1.00								
(3)	ln (# of lawyers firm-office *j* hired from lawyer *i*'s prior firm)	0.39	0.92	0.69	0.06	1.00							
(4)	# of lawyers in firm-office *j*	75.1	108.3	0.04	-0.04	0.08	1.00						
(5)	% change in headcount at firm *j*, 2008-09	-0.02	0.09	0.08	0.02	0.14	-0.11	1.00					
(6)	Dissolved firm partners hired by focal firm (t_0 to t_{0-4})	0.26	1.10	0.15	0.02	0.19	0.03	0.01	1.00				
(7)	Focal firm partners hired by dissolved firm (t_0 to t_{0-4})	0.24	0.85	0.08	0.04	0.15	0.05	-0.03	0.25	1.00			
(8)	Firm *j*'s revenues per lawyer (RPL), in $1,000s	742.5	224.5	-0.04	-0.01	-0.06	0.36	-0.20	0.04	-0.01	1.00		
(9)	Firm *j*'s profits per equity partner (PPEP), in $1,000s	1,089.2	640.1	-0.06	-0.02	-0.09	0.37	-0.19	0.04	-0.01	0.88	1.00	
(10)	Firm *j*'s leverage ratio (associates per partner), 2008	4.17	1.42	0.03	-0.03	0.02	0.12	-0.02	0.09	0.07	0.14	0.39	1.00

Table 6

Conditional logit models of the likelihood that lawyer *i* is hired by firm-office *j* (Y_{ij} = 1 if "Yes"; 0 if "No").	(1)	(2)	(3)	(4)	(5)	(6)
% of firm-office *j* lawyers from lawyer *i*'s law school	2.47 **		3.37 **	3.45 **	3.46 **	3.44 **
	(0.387)		(0.493)	(0.474)	(0.489)	(0.544)
ln (# of lawyers firm-office *j* hired from lawyer *i*'s prior firm)		1.77 **	1.77 **	1.83 **	1.82 **	1.83 **
		(0.088)	(0.088)	(0.087)	(0.089)	(0.092)
# of lawyers in firm-office *j*				-0.002	-0.002	-0.001
				(0.001)	(0.001)	(0.001)
% change in headcount at firm *j*, 2008-09				-2.43 †	-2.43	-3.04 †
				(1.44)	(1.50)	(1.61)
Dissolved firm partners hired by focal firm *j* (t_0 to t_{0-4})					0.099	0.096
					(0.106)	(0.113)
Focal firm partners hired by dissolved firm *j* (t_0 to t_{0-4})					-0.070	-0.033
					(0.191)	(0.171)
Firm *j*'s revenues per lawyer ($1,000s)						0.001
						(0.002)
Firm *j*'s profits per equity partner ($1,000s)						0.000
						(0.001)
Firm *j*'s leverage ratio						0.109 †
						(0.105)
N (lawyer/firm-office dyads)	9,983	9,983	9,983	9,983	9,983	8,749
N (lawyers)	916	916	916	916	916	824
N (firms)	188	188	188	188	188	171
N (hiring firms)	103	103	103	103	103	90
N (metro areas)	19	19	19	19	19	19
Lawyer fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Log pseudolikelihood	-2,157.99	-797.63	-781.32	-773.50	-768.65	-674.52
Wald Chi-square (d.f.)	40.58 (1)	406.91 (1)	423.15 (2)	500.32 (4)	543.47 (6)	496.26 (9)

Robust standard errors in parentheses. Observations clustered by 103 hiring firms (90 firms in Model 6).

** ρ < 0.01; * ρ < 0.05; † ρ < 0.10; two-tailed tests for control variables and one-tailed hypothesis tests.

Table 7: Summary statistics and correlations of variables in attainment analyses (n=902 lawyers).

		Mean	St. Dev.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
(1)	Prestige score of firm that hires focal lawyer	4.42	1.27	1.00											
(2)	Lawyer experiences upward status mobility (0/1)	0.51	0.50	0.81	1.00										
(3)	Female (0/1)	0.29	0.45	0.08	0.07	1.00									
(4)	Partner (0/1)	0.49	0.50	-0.09	-0.05	-0.19	1.00								
(5)	ln (years of legal experience)	2.52	0.90	-0.14	-0.11	-0.26	0.72	1.00							
(6)	Rank of law school attended	41.5	38.4	-0.25	-0.19	-0.01	-0.11	-0.08	1.00						
(7)	% of local attorneys from lawyer *i*'s law school	0.08	0.06	-0.11	-0.15	-0.01	0.00	-0.01	-0.05	1.00					
(8)	Black (0/1)	0.03	0.16	-0.07	-0.07	0.05	0.02	-0.01	-0.04	0.06	1.00				
(9)	White (0/1)	0.89	0.32	-0.10	-0.09	-0.17	0.21	0.23	0.04	0.02	-0.38	1.00			
(10)	Inverse Mills ratio (employed in NLJ 250)	0.47	0.28	-0.08	-0.11	0.10	-0.40	-0.21	0.00	-0.23	0.12	-0.28	1.00		
(11)	% of firm-office *j* lawyers from lawyer *i*'s law school	0.08	0.10	-0.13	-0.15	-0.02	-0.03	-0.07	0.01	0.63	0.00	0.04	-0.13	1.00	
(12)	ln (# of lawyers firm-office *j* hired from lawyer *i*'s prior firm)	2.2	1.2	0.03	0.10	-0.01	0.04	0.02	0.00	0.09	-0.06	0.02	-0.12	-0.01	1.00

Table 8

Analyses of individual-level, post-dissolution labor market attainment.

	(1)	(2)	(3)	(4)	(5)
model	*OLS*	*OLS*	*OLS*	*OLS*	*probit*
[dependent variable]	*[firm prestige]*	*[firm prestige]*	*[firm prestige]*	*[firm prestige]*	*[greater prestige]*
Female (0/1)	0.054	0.049	0.054	0.050	0.214
	(0.074)	(0.072)	(0.072)	(0.071)	(0.144)
Partner (0/1)	0.035	0.043	0.083	0.088	0.424
	(0.202)	(0.197)	(0.203)	(0.198)	(0.403)
ln (years of legal experience)	-0.044	-0.057	-0.059	-0.069	-0.216
	(0.074)	(0.073)	(0.076)	(0.075)	(0.138)
Rank of law school attended	-0.002	-0.002 †	-0.001 †	-0.001 †	-0.003 *
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% of local attorneys from lawyer's law school	-0.680	0.355	-0.579	0.364	1.56
	(0.673)	(0.801)	(0.682)	(0.815)	(1.91)
Black (0/1)	-0.354	-0.367	-0.345	-0.358	-0.578
	(0.304)	(0.304)	(0.306)	(0.306)	(0.560)
White (0/1)	-0.068	-0.057	-0.038	-0.029	0.343
	(0.145)	(0.142)	(0.143)	(0.141)	(0.293)
Inverse Mills ratio (employed in NLJ 250)	-0.092 †	-0.086	0.066	0.062	0.743
	(0.568)	(0.551)	(0.576)	(0.562)	(1.18)
% of firm-office *j* lawyers from lawyer *i*'s law school		-1.02 *		-0.935 *	-1.95 *
		(0.431)		(0.404)	(1.01)
ln (# of co-workers hired by hiring firm-office *j*)			0.094	0.089	0.261 **
			(0.065)	(0.064)	(0.105)
Constant	2.93 **	2.96 **	2.60 **	2.64 **	-4.16 **
	(0.483)	(0.473)	(0.512)	(0.50)	(1.12)
Lawyers included	All	All	All	All	All
N (observations)	902	902	902	902	902
N (hiring firms)	109	109	109	109	109
Firm fixed effects	Yes	Yes	Yes	Yes	Yes
Office city fixed effects	Yes	Yes	Yes	Yes	Yes
Practice area fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared (d.f.)	0.516 (31)	0.520 (32)	0.522 (32)	0.525 (33)	--
Log pseudolikelihood (d.f.)	--	--	--	--	-306.03 (33)

Robust standard errors in parentheses. Observations clustered by hiring firm.

** ρ < 0.01; * ρ < 0.05; † ρ < 0.10; two-tailed tests for control variables and one-tailed hypothesis tests.

Table 9

OLS regressions of hiring firm prestige, with approximately-balanced covariates and observations weighted by inverse generalized propensity scores.

	(1)		(2)		(3)	
Female (0/1)	-0.115		-0.049		-0.012	
	(0.152)		(0.252)		(0.247)	
Partner (0/1)	0.143		-0.397		-0.437	
	(0.229)		(0.332)		(0.372)	
ln (years of legal experience)	0.062		0.145		0.151	
	(0.111)		(0.116)		(0.124)	
Rank of law school attended	-0.001		-0.001		-0.001	
	(0.001)		(0.001)		(0.001)	
% of local attorneys from lawyer's law school	3.00	†	-0.309		-0.650	
	(1.53)		(1.71)		(1.73)	
Black (0/1)	-0.203					
	(0.227)					
White (0/1)	-0.134		-0.096		-0.287	
	(0.221)		(0.339)		(0.666)	
Inverse Mills ratio (employed in NLJ 250)	0.518		-0.803		-0.969	
	(0.545)		(1.06)		(1.24)	
% of firm-office *j* lawyers from lawyer *i*'s law school	-1.59	*	-1.94	†	-2.05	†
	(0.735)		(1.08)		(1.19)	
ln (# of co-workers hired by hiring firm-office *j*)	0.094		0.165	*	0.169	*
	(0.081)		(0.088)		(0.089)	
Constant	1.93	**	2.56	**	2.77	**
	(0.658)		(1.01)		(1.17)	
Unmatched covariate	*alumni*		*co-workers*		*co-workers*	
Lawyers included	All		All		All but Thacher	
N (observations)	612		409		350	
N (hiring firms)	95		71		71	
Firm fixed effects	Yes		Yes		Yes	
Office city fixed effects	Yes		Yes		Yes	
Practice area fixed effects	Yes		Yes		Yes	
R-squared (d.f.)	0.571	(31)	0.481	(30)	0.513	(29)

Robust standard errors in parentheses. Observations clustered by hiring firm.

Two-tailed tests for control variables and one-tailed hypothesis tests.

** ρ < 0.01; * ρ < 0.05; † ρ < 0.10