WHEN DOES CORPORATE SOCIAL RESPONSIBILITY REDUCE EMPLOYEE TURNOVER? EVIDENCE FROM ATTORNEYS BEFORE AND AFTER 9/11

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When Does Corporate Social Responsibility Reduce Employee Turnover? Evidence from Attorneys Before and After 9/11

Seth Carnahan
University of Michigan
scarnaha@umich.edu

David Kryscynski
(corresponding author**)**
Brigham Young University
dk@byu.edu

Daniel Olson
University of Washington
dmolson@uw.edu

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Abstract

This study places important boundary conditions on the generally accepted notion that CSR will reduce turnover. Our primary argument is that CSR will be most effective at reducing turnover that is motivated by a preference for more meaningfulness at work. We find empirical support for this idea using microdata on attorneys employed by large law firms. We find that firms with higher levels of CSR have moderately lower rates of turnover to startup law firms and turnover through occupation changes, moves which are more likely to be motivated by a preference for meaningfulness than moves to non-startup law firms. Strikingly, the retention benefits of CSR are much stronger after attorneys experience mortality-related shocks that likely cause them to re-evaluate their jobs (i.e., for New York City-born attorneys following the 9/11 terror attacks). To our surprise, we also find that firms with higher levels of CSR experience *higher* turnover rates to non-startup law firms. In addition to our arguments about the importance of meaningfulness, the study provides two important extensions to work examining CSR and turnover: (1) it may be useful to view the CSR-turnover relationship through a risk-management lens, and (2) investments in CSR may *increase* employee departures from organizations under certain conditions.

INTRODUCTION

When do firms' investments in corporate social responsibility (CSR) initiatives, defined as "discretionary corporate activit[ies] intended to further social welfare" (Barnett, 2007), have the strongest employee retention benefits? An emerging stream of research generally suggests a positive relationship between a firm's CSR investments and employee retention. Scholars in this stream argue that CSR-type activities reduce turnover intentions (Stewart, Volpone, Avery, & McKay, 2011) and actual turnover (Bode, Singh, & Rogan, 2015) in part by providing a sense of meaningfulness[1] (Grant, 2012a) that increases an employee's identification with the organization (Doh, Stumpf, & Tymon, Jr., 2011) and fulfills an employee's desire for meaning and purpose at

[1] We follow Pratt & Ashforth (2003) and Rosso, Dekas, & Wrzesniewski (2010) by defining meaningfulness as "the amount of significance something holds for an individual" and meaningful work as "work experienced as particularly significant and holding more positive meaning for individuals."

work (Cascio, 2003). We extend existing theory on the retention benefits of CSR in two primary ways. First, we propose that a firm's CSR activities may constrain some job changes more than others. Different types of job changes may reflect different underlying employee motivations (March & Simon, 1958). To the extent that a firm's CSR activities create meaningfulness for employees at work, by investing in CSR activities, a firm may be able to reduce the likelihood of job changes by individuals who would otherwise leave in pursuit of meaningfulness. Prior research suggests that founding a new firm (see, e.g., Baron, Franklin & Hmielski, 2013; Benz & Frey, 2008) or making a major occupational transition (Wrzesniewski, 2002) may be driven by an employee's desire to pursue more meaningfulness on the job. Thus, we argue that a firm's investments in CSR will be most effective at limiting job changes that result in employees founding new firms or making major occupational changes, as compared to those that result in employees simply moving to existing firms in the industry. CSR activities may at least partially substitute for the meaningfulness that is a more natural feature of these other employment destinations.

Second, we propose that a firm's CSR activities may be effective at reducing the likelihood of job changes after particular events in the lives of employees. Turnover researchers provide persuasive evidence (Holtom, Mitchell, Lee, & Inderrieden, 2005; Lee, Gerhart, Weller, & Trevor, 2008) that an employee's departure from the firm is often preceded by a "shock to the system [that] causes the person to pause and think about the meaning or implication of the event to his or her job" (Lee & Mitchell, 1994: 60). In particular, shocks that enhance an individual's awareness of her own mortality may increase her desire for more meaningfulness on the job (Grant & Wade-Benzoni, 2009). If the employee cannot find sufficient meaningfulness in the current organization, the shock may lead her to quit and seek it elsewhere. If a firm's CSR

activities create meaningfulness for employees, CSR activities may limit turnover following these types of shocks.

We test our predictions using ten years of longitudinal, employee-employer linked data that allow us to examine the job mobility of attorneys working for the 200 largest law firms in the US (the "AmLaw 200"). The primary CSR activity in this setting is pro bono legal services, which is the provision of free legal assistance to clients who cannot afford it (Burbano, Mamer, & Snyder, 2014; Rhode, 2003). To test whether CSR reduces turnover following mortality related shocks, we examine how the terror attacks of September 11, 2001 influence turnover behavior of attorneys born in the New York City (NYC) metro area.

Our main empirical tests provide support for our hypotheses, but also reveal important nuances that enhance the contribution of the paper. Consistent with our theory, we find evidence that attorneys working for higher CSR firms are less likely to found new law firms and less likely to change occupations. Also consistent with our expectations, we find that NYC metro born attorneys working for higher CSR firms are less likely to make these major occupation changes after the 9/11 attacks. While CSR has a moderate effect on limiting occupation-switching and startup-founding during normal, steady-state operations of the firm, the effect size is very significant following the mortality-related shock of 9/11. This empirical pattern implies an important nuance in the relationship between CSR investments and retention: although CSR may have only a small effect on retention on a day-to-day basis, it may be critical for preventing employees from looking elsewhere for meaningfulness following certain life shocks. This suggests that researchers and managers may want to consider the retention advantages of CSR in risk management or insurance-like terms (e.g., Godfrey, 2005; Koh, Qian, & Wang, 2014; Minor & Morgan, 2011).

Secondary empirical analyses yield a potentially surprising additional result. Taking the existing literature as a baseline, we expected a general negative relationship between CSR activities and turnover and we theorized a stronger negative relationship for major occupation changes, moves to found startups, and moves after mortality related shocks, as compared to moves to established firms in the industry. However, we find that law firms with higher levels of CSR actually experience *higher* rates of employee departures to other law firms, such that there is a null relationship between firms' CSR activities and firms' overall turnover rates (i.e. departure to any destination). This important finding illustrates the need for additional theory explaining the conditions under which CSR may actually *increase* turnover.

Overall, therefore, our theory and empirical results contribute to the literature by emphasizing an important boundary condition for extant theory—rather than provide a general reduction in turnover, CSR may primarily reduce turnover when moves are at least partially motivated by a search for greater meaningfulness. CSR activities may be much less effective at reducing employee departures that are motivated by other factors. In some cases, CSR activities may actually increase employee turnover, perhaps by providing employees with marketable skills or by pushing away employees who may have weaker preferences for CSR participation. We discuss these implications in detail in the conclusion of the paper.

CSR ACTIVITIES AND EMPLOYEE TURNOVER

A growing body of organizational research generally suggest that CSR activities result in better employee-related outcomes for firms (Burbano, 2015; Doh, Stumpf, & Tymon, 2011; Flammer & Luo, 2015; Greening & Turban, 2000; Gubler, Larkin, & Pierce, 2014; Hansen, Dunford, & Boss, 2011; Jones, 2010). Within this stream, researchers suggest that firms with higher levels of CSR activity may have higher retention for at least two important reasons. First,

firm engagement in CSR activities may create a prosocial reputation for the firm (Davis, 2014; De Roeck & Delobbe, 2012). This reputation may facilitate deeper candidate pools (Greening & Turban, 2000; Turban & Greening, 1997). Deeper candidate pools may allow high CSR firms to select candidates who "fit" better with the organization (e.g., Kristof-Brown, Zimmerman, & Johnson, 2005) and thus are less likely to subsequently leave the firm (Jovanovic, 1979; O'Reilly, Chatman, & Caldwell, 1991). In addition, employees may receive positive utility from their affiliation with an organization that outsiders view as a "good" actor (Bartel, 2001; Brammer, Millington, & Rayton, 2007; Dutton & Dukerich, 1991; Korschun, Bhattacharya, & Swain, 2014), making them less likely to depart, ceteris paribus.

Second, CSR activities may reduce turnover by increasing the amount of meaningfulness that employees experience through their jobs. CSR activities likely influence meaningfulness by enhancing meaningfulness both *in* work and *at* work (Michaelson, Pratt, Grant, & Dunn, 2013; Pratt & Ashforth, 2003). Participating in CSR activities, such as volunteer projects (Bartel, 2001; Rodell, 2013) or pro bono consulting engagements (Bode et al., 2015), may provide employees with a sense of meaningfulness "in" their work (Bauman & Skitka, 2012) by allowing employees to produce a direct and visible social impact (Grant, 2012a, 2012b). An employee's sense of meaningfulness *at* work may also be higher in firms that provide more CSR activities, in part because these activities reflect an organization with a more positive, prosocial climate and culture (Collier & Esteban, 2007; Linnenluecke & Griffiths, 2010). This increased sense of meaningfulness at work may enhance employee's identification with (Rodrigo & Arenas, 2007) and commitment to the firm (Bartel, 2001; Brammer et al., 2007), resulting in lower turnover.

These two reasons provide the backbone for the baseline expectation that higher CSR firms should have lower overall turnover. Despite this growing body of work we find only one study,

the recent work of Bode et al. (2015), that empirically tests the CSR-turnover relationship. While rich and well-done, their work is limited to a single firm study that prevents analysis of the CSR-turnover relationship across firms. It thus seems important to build additional empirical support for the main expectation in the literature as well as explore potential boundary conditions of the retention benefits of CSR.

WHY CSR REDUCES SOME TYPES OF JOB CHANGES MORE THAN OTHERS

With regard to boundary conditions, our core argument is that CSR will have the greatest retention benefits for moves motivated by a search for greater meaningfulness. If an employee believes that the current job is lacking on an important dimension when compared to others, the employee is likely to attempt a move to a job that better suits her preferences on that dimension (Mobley, 1982; Price, 1977). If that dimension is meaningfulness, then CSR may reduce turnover by preventing employees from searching elsewhere for that meaningfulness. Our review of the organizational literature suggests two types of job changes that may be motivated, at least in part, by a desire for greater meaningfulness: 1) occupation changes (see, e.g., Wrzesniewski, 2002) and 2) decisions to found a new firm (see, e.g., Benz & Frey, 2008; Dempsey & Sanders, 2010; Fox, 2015). We discuss each in detail below.[2]

Does CSR Reduce Major Occupational Changes?

Theories explaining occupation change tend to focus on employee dissatisfaction with job attributes that employees perceive as stable and enduring aspects of an occupation, as opposed to job attributes that are idiosyncratic to a particular organization (Rhodes & Doering, 1983, 1993).

[2] Theoretically and empirically, we model an employee's turnover decision as containing four mutually exclusive and collectively exhaustive choices: 1) the employee can move to a different occupation, 2) the employee can found a new firm while maintaining current occupation, 3) the employee can move to an already-established competitor while maintaining current occupation, or 4) the employee can remain with the current firm.

As Blau (2007: 138) notes, "If employees perceive that changing jobs but remaining in the same occupation will continue their basic job duties they may view simply changing jobs as going 'from the frying pan to the fire.'" Thus, individuals are likely to make occupational changes when they believe that the factors causing them dissatisfaction are occupation-related rather than firm-related (Blau & Lunz, 1998).

Prior literature suggests that occupations differ in their inherent levels of meaningfulness. Occupations, such as zookeeper (Bunderson & Thompson, 2009), social worker, emergency room doctor, and special education teacher are generally thought to provide increased meaningfulness on the job (Hall & Chandler, 2005). In contrast, many high status professional occupations may have higher average monetary compensation (e.g., U.S. Bureau of Labor Statistics, 2007), but may score relatively poorly on job satisfaction (Schiltz, 1999) and meaningfulness (Daicoff, 2008). We observed this in interviews for this study with attorneys who left their jobs to pursue other occupations in search of greater meaningfulness at work.

Thus, when an employee in a high status but relatively low meaningfulness occupation chooses to change occupations, the change may be partially motivated by a desire for greater meaningfulness at work. By contrast, movements within the occupation may be more likely to reflect other motivations, such as a desire to increase intra-professional status or compensation (Rider & Tan, 2014). If the primary retention benefits of CSR activities stem from their provision of meaningfulness at work, firms that invest more in CSR activities should be more effective at limiting occupation/industry transitions compared to moves to competitors within the same industry. CSR activities may provide an imperfect substitute for meaningfulness that is a more natural feature of these alternative jobs in other occupations and industries.

It is important to note that CSR may reduce occupation changes due to a combination of

selection and treatment effects. Professionals who chose to work with firms that have higher levels of CSR activities may have preferences for meaningfulness at work (Turban & Greening, 1997) and may be less likely to find themselves in situations where a lack of meaningfulness motivates a change in occupation. Thus, a firm's CSR activities may provide greater protections against occupation changes in part by attracting a workforce that is less likely to leave in search of more meaningfulness at work. Treatment effects are likely also important: if individuals experience a change in preferences and find that they have a heightened desire for meaningfulness at work, they do not need to leave their occupation to find it. As a consequence, both selection and treatment effects may help CSR buffer firms against employee turnover through major occupational changes.

H1a: The probability that an employee changes occupation is lower in firms with higher levels of CSR activity.

H1b: The buffering effect of higher levels of CSR on turnover will be stronger for employees leaving their occupation than for employees moving elsewhere within their occupation. In other words, the relationship between a firm's level of CSR activity and the probability that an employee leaves the occupation will be more negative than the relationship between a firm's level of CSR activity and the probability that an employee moves to a new firm within the same occupation.

Does CSR Reduce Employee Decisions to Found New Firms?

Just as the decision to change occupations may reflect a desire to pursue more meaningfulness, the decision to found a new organization within the focal industry may also reflect a preference for more meaningfulness. Prior research suggests that the decision to found a new firm is distinct from other employment transitions, including the decision to move to a small but already established organization. Although individuals may found firms in order to increase their residual claimancy and potential financial rewards (Campbell, Ganco, Franco, & Agarwal, 2012; Groysberg, Nanda, & Prats, 2009), research also suggests important non-

financial drivers behind the decision to found a new firm, including a desire for meaningfulness. For example, some founders create firms in order to establish a durable legacy (Fox, 2015), which may imbue a firm founder's work with a greater sense of meaningfulness relative to that of an employee. In addition, a firm founder typically has unique autonomy at work (Benz & Frey, 2008), including the ability to define the scope, strategy, and mission of the organization.[3] In some sense, a founder is uniquely able to "craft" a job that suits her preferences (Wrzesniewski & Dutton, 2001), and prior work suggests that individuals who are able to craft their jobs derive more meaningfulness from work (Wrzesniewski, LoBuglio, Dutton, & Berg, 2013). In addition, a founder may create a startup with an explicit social mission, specifically tailored to the desires of the founder to pursue meaningfulness via work (Dempsey & Sanders, 2010). For example, Dhar Law, LLP, was founded by brothers Vilas and Vikas Dhar in 2010 so that they could devote more time to pro-bono matters (Field, 2013).

Given the probability that employee decisions to found their own firms is at least in part motivated by a desire for greater meaningfulness, a firm's investments in CSR may also reduce the probability that an employee leaves the firm to found a new firm. The logic connecting CSR and turnover is identical to the logic described above for employees changing occupations in search of greater meaningfulness. A firm that provides CSR activities may infuse an employee's job experience with enough meaningfulness that the marginal attractiveness founding a new firm declines. Also, as argued for the previous hypothesis, it is likely that both selection and treatment effects are at play.

H2a: The probability that an employee founds a startup firm is lower in firms with higher

[3] Note that we are focusing on independent businesses – a deep body of work considers the compromises that founders must make with venture capitalists and other resource providers.

levels of CSR activity.

H2b: The effect of CSR on turnover will be stronger for employees founding new firms than for employees moving to established firms. In other words, the relationship between a firm's level of CSR activity and the probability that an employee starts a new firm will be more negative than the relationship between a firm's level of CSR activity and the probability that an employee joins an established firm.

WHY CSR REDUCES TURNOVER AFTER MORTALITY SHOCKS

If employee preferences for meaningfulness in the workplace shift over time, the ability of CSR activities to retain the employee may change as well. The well-established unfolding model of employee turnover finds that shocks in the lives of employees cause them to reconsider the extent to which their jobs match their goals, desires, and preferences (Holtom et al., 2005; Lee & Mitchell, 1999; Lee, Mitchell, Wise, & Fireman, 1996). In particular, shocks that cause employees to consider their own mortality may cause them to search for more meaningfulness in their lives and their work (Grant & Wade-Benzoni, 2009).

Employees who experience tragedies that increase their awareness of death may experience an increase in their desire for meaning and purpose (Greenberg et al., 1990; Jonas, Schimel, Greenberg, & Pyszczynski, 2002). These mortality-related shocks may cause individuals to reevaluate the prosocial and generative aspects of their lives, including their interpersonal relationships and the opportunities available to them to make a positive difference in the world (Joireman & Duell, 2005; Peterson & Stewart, 1996). Grant and Wade-Benzoni (2009) suggest that mortality-related shocks incite this type of reevaluation because they remind an individual that life is finite, enhancing the value of "mak[ing] lasting contributions and [feeling] connected with others." These feelings may be tied to a desire to transcend death and leave a durable legacy (Becker, 1973, 1975; Fox et al., 2010; McAdams & de St. Aubin, 1992) or arise from a desire to find fulfillment during life while it lasts (Tedeschi & Calhoun, 2004).

As a consequence, if CSR activities help provide employees with more meaningfulness on the job, then firms that provide higher levels of CSR activities should be better able to retain employees who suffer mortality related shocks. In contrast to Hypotheses 1 and 2, where it is difficult to disentangle selection and treatment effects, examining the effect of mortality related shocks allows us to explore treatment effects more directly, because these shocks occur after the employee has selected into the firm and may increase employee preferences for meaningfulness beyond what she may have considered when joining the firm. In the presence of this shift in preferences, a firm's level of CSR may satisfy the new desire for meaningfulness and prevent the employee from leaving the focal firm for more meaningful employment elsewhere.

H3: The probability that an employee leaves the legal occupation (or founds a new firm)[4] *after a mortality related shock is lower in firms with higher levels of CSR activity.*

EMPIRICAL ANALYSES

Setting and Data

We test our hypotheses in the American legal services industry by studying the job changes of attorneys working for large corporate law firms. Our data source for attorneys is an electronic version of the nationwide Martindale-Hubbell Law Directory (Martindale). Martindale has been referred to as the "white pages for lawyers." Portions of Martindale have been used for previous organizational studies (e.g., Phillips, 2001). Martindale has been in existence for over 140 years, although our extract is limited to firms and lawyers in the United States from 1999-2011. There is a strong norm in the legal services industry for firms and attorneys to list themselves in the Martindale directory, particularly for the large firms that form the basis for our sample,

[4] We include both destinations in this hypothesis to be theoretically consistent with the prior hypotheses, but the low frequency of foundings in our data make it impossible to test new foundings after the shock of 9/11.

regardless of geographic location or practice area.[5]

The fundamental unit in our analysis is an attorney-firm-year. Martindale provides unique identifiers for each attorney and unique identifiers for each firm that are stable over time. It also includes information such as attorney education, birthplace, areas of practice (e.g. corporate versus family law), and the address of an attorney's office. In the small number of situations where attorneys are listed in multiple firms or multiple offices of a single firm in the same period, we assign the attorney to a unique firm and unique office following the procedure described in Baker & Parkin (2006), who use similar data.

Variables

Dependent Variables

Occupation change (H1). This dichotomous variable takes a value of one when an attorney is no longer affiliated with a law firm in the Martindale data at the beginning of the following year, meaning that he or she made a move at some point during the focal year. We require that the attorney remain absent for two years to ensure that we do not mischaracterize data errors as exits. Attorneys may exit the Martindale data for a number of reasons. They may continue to use their legal skills at in-house legal departments (such as a general counsel position), they may move to a different job type, such as high school teaching, or they may leave the workforce due to family choices, retirement or death.[6] We limit our sample to attorneys who are under the age of 65 to limit the influence of death on our results. While Martindale coverage is quite comprehensive, individuals may be coded as changing occupations if they move to law firms that

[5] We compared Martindale data to Linkedin data for a random sample of 200 attorneys, and found only one minor discrepancy, giving us confidence in the quality of the data (see Appendix B).

[6] Note that among the victims of the 9/11 attacks, the mortality related shock we use to test H3, two were employed by AmLaw 200 firms. We exclude these individuals and they do not affect our results.

do not list themselves in the Martindale data. Nevertheless, each move, whether it is a retirement, a move to an in-house legal department, a move to a completely different occupation, or a move to a non-listed firm, represents a significant occupation change from employment in a large law firm, and this type of change is the focus of our theoretical arguments.

Founds a startup law firm (H2). This dichotomous variable takes a value of one when the attorney founds a new law firm in the following year. We follow Phillips' (2002) approach in identifying founding events. First, the attorney must appear in a newly created law firm within three years of her exit from the current organization; founders may not list themselves in Martindale immediately. Second, the attorney must be a "name partner," meaning that her last name must appear in the name of the law firm in the firm's first appearance in the data. Non-founders who work for the firm in its first year, such as associates or partners who are not name partners, are not included in the measure. To ensure that these founding events are not data recodes or mergers, we place a size cap of 25 attorneys on these new firms, and we eliminate "new" firms whose workforce constitutes >50% of the workforce of a prior employer. About .5% of attorney-years end in startup founding (Table 1) comparable to the 1% rate reported by Campbell et al. (2012), given that they are unable to identify named founders.

Leaves the firm for an established law firm. This dichotomous variable takes a value of one when the attorney appears in a listing for a different law firm in the following year, and the attorney is not a founder of this different firm. 7% of the attorney-year observations in the main sample end in this type of movement. Occupation change, founds new law firm, and leaves firm for an established law firm are collectively exhaustive of alternative employment destinations.

Explanatory variables

Employer's CSR activities. The provision of pro bono legal services is the primary corporate

social responsibility initiative among American law firms (Burbano et al., 2014). Latin for "for the good," pro bono legal work is generally done by an attorney free of charge, typically for underprivileged individuals and/or nonprofit organizations that do not have the resources to afford legal help. Encouraging pro bono legal work has a number of tangible benefits for law firms, including enhancing the firm's image in the eyes of external stakeholders and providing lower-risk training and evaluation opportunities for newer associates (Burbano et al., 2014). Pro bono activities also provides attorneys with meaningfulness at work (Cummings & Rhode, 2010). Rhode (2003) describes the benefits that pro bono work provides to attorneys:

> Examples [of cases] included death row criminal appeals, prison suits, sweatshop labor litigation, and political asylum claims. For many of those attorneys, pro bono matters provided their most rewarding professional experiences. As one ABA winner put it, after lawyers leave law school, the "altruistic sense of what the profession is about . . . disappears pretty quickly. Pro bono is a way to get this passion back. This makes you feel alive and like you are doing something worthwhile." . . . One attorney noted, "If I couldn't do pro bono, I wouldn't practice law. It makes me feel like I am making a difference." Lawyers often contrasted their public service with their largely commercial practices, and reported greater satisfaction from promoting social reform or helping a disadvantaged client than from wrangling over money (Rhode, 2003: 447).

Rhode's (2003) surveys indicate the top two reasons that attorneys perform pro bono work are "personal satisfaction" and a "sense of professional obligation," with other motivations such as gaining new skills and experience following next. Pro bono is an "investment" with important trade-offs. Firms often emphasize the costliness of their pro bono activities. For example, Reed Smith LLP, a large international law firm, prominently advertises that it commits about 4% of billable time to pro bono work, representing an opportunity cost of about $30 million in 2013.[7]

The American Lawyer (AmLaw), a magazine covering the legal services industry, annually ranks the 200 largest law firms in the US (the AmLaw 200) on their commitment to pro bono

[7] http://www.reedsmith.com/aboutus/probono/ (accessed November 13, 2014).

activities, based on firm-reported survey data.[8] We use this ranking as our primary measure of a firm's commitment to CSR activities.

Mortality-related shock. We utilize the terrorist attacks of September 11, 2001 as a mortality related shock in the lives of attorneys who were born in the New York City metro area. The 9/11 attacks unexpectedly and tragically resulted in the death of over 3,000 individuals, creating a mortality related shock for many across the US (Ahern et al., 2002; Schlenger et al., 2002). Individuals born in New York City are likely to have been more psychologically affected than others. First, those born in New York City are more likely to have had a personal relationship with someone killed or directly affected by the attacks (see, Pyszczynski, Solomon, & Greenberg, 2003). Second, much of the physical destruction and media coverage focused on New York City. Place identity theory suggests that an individual's birthplace is an important part of one's personal identity (Gieryn, 2000; Manzo, 2003; Proshansky, 1978), implying that NYC metro natives would likely experience the attacks in a much more visceral way than others.

We also find anecdotal support for our choice of treatment group in interviews with 17 attorneys randomly selected from our secondary data sample. Four of these attorneys were born in NYC and three of those made a job change within three years of the 9/11 attacks. All three of these attorneys indicated that 9/11 had an important impact on their job changes. One of them brought up 9/11 organically when asked about the reasons and timing for the change, saying:

> That timing, I know that I left right around 9/11 – and it was after 9/11 that I recall. I was here in Manhattan. I'm a native Manhattan, and certainly everybody in the country knows the ramifications of it, but here in New York it was traumatic. And I think that just brought

[8] Previous strategy researchers (e.g., Burbano et al., 2014) have utilized these data to study law firm's pro bono programs. Two-thirds of the pro bono ranking comes from a firm's average number of pro bono hours per lawyer, while the remaining third comes from the percentage of lawyers at the firm who have completed 20 or more hours of pro bono work.

> things more to home, that I was practicing in a way that I didn't want to practice, and my lifestyle wasn't the way that I wanted it to be, and I was amenable for a change.

The other two attorneys born in NYC did not bring 9/11 up spontaneously, but when prompted about the 9/11 attacks, both indicated that 9/11's trauma contributed to their job changes. In contrast, we interviewed two attorneys who were not born in NYC but were working in NYC at the time of the attacks. Neither indicated that 9/11 affected their subsequent job changes. Additionally, we interviewed seven attorneys who were neither born in NYC nor working in or near NYC at the time of the attacks, and none referenced a psychological impact of 9/11.[9]

The Martindale data contain self-reported place of birth information for attorneys. We identify 3,293 *NYC Metro Born* attorneys as those whose place of birth matches a place located within CBSA #35620, "New York-Newark-Jersey City", or one of the five boroughs of New York City (e.g. "Bronx"). Results are robust to including only attorneys born within NYC proper and to a definition that also includes Bridgeport-Stamford-Norwalk.

Control variables

Experience and demographic measures. We measure *attorney age* using self-reported date of birth information included in the Martindale data. Dummy variables *partner* and *associate* indicate the attorney's position in the firm's hierarchy. The excluded group includes attorneys who are off the partnership track. Martindale surveys attorneys and asks them to rate the quality of their peers in other firms on a scale of A, B, and C. We include a dummy for these *attorney peer ratings* to capture time-varying attorney quality. We include *percent of officemates from*

[9] We utilize NYC-born as our treatment variable, as opposed to working in NYC at the time of the attacks, because the economic shock following 9/11 might have caused firms to lay off attorneys working in NYC. These types of layoffs would be difficult to distinguish from meaningfulness-motivated career changes using our secondary data. In fact, our primary analyses exclude NYC-located law offices to avoid this issue (see below).

attorney's law school as a proxy for an attorney's social connections within the current firm.

To measure gender, we first match the attorneys' first names to the dominant gender indicated by US Social Security Data, and second, to the greater than 95k names present in the Gender Checker Directory. We include dummies for *male first name* and *female first name*. We create a dummy for *top law school* if the attorney graduated from one of the 18 schools that have made it into the top 15 of the US News rankings since they began in 1987.[10]

Areas of practice. The Martindale data contain self-reported, attorney-level information on the practice areas in which attorneys serve their clients, such as corporate versus family law. We categorize the practice area information provided by Martindale Hubbell into 26 unique practice areas using the procedure outlined in Appendix A. We include 19 dummies, one for each of the practice areas that make up at least 1% of the attorney years in the sample, in the analyses. We also include a *practice area overlap* measure, which varies from zero to one and increases with as an attorney's shares more practice areas with the other attorneys in her firm.[11]

Firm characteristics. We use the Martindale data to measure *firm size*, *office size,* and *partner to associate ratio* since these variables are likely to be correlated with a firm's investment in pro bono activities (Burbano et al., 2014) and may also determine turnover rates. We account for organizational performance using *revenue per lawyer* and *profits per partner*,

[10] *Top law schools* include those at Yale University, Harvard University, Stanford University, Columbia University, New York University, University of California—Berkeley, University of Chicago, University of Pennsylvania, Northwestern University, University of Michigan—Ann Arbor, University of Virginia, Cornell University, Duke University, Georgetown University, Vanderbilt University, University of Texas—Austin, University of California—Los Angeles, and University of Southern California.

[11] We first define the position of each attorney and her law firm in "legal specialty" space by constructing a 19-dimension vector – one dimension for each of 19 legal specialties listed in Appendix A. The overlap measure takes the cosine of the angle between the attorney's vector and the employer's vector. If Ai and Fj represent the vectors for attorney i and law firm j, *practice area overlap* would be computed as: Ai ● Fj/|Ai||Fj|, meaning that we use the dot product to compute the angle between the vectors.

two standard measures of performance in the legal services industry (Campbell et al., 2012; Rider and Tan, 2014). Revenue and profit data come from AmLaw surveys. An important alternative explanation for our results is that firms with different levels of CSR activities have different cultures and different human capital strategies. We attempt to account for these characteristics using 1) *% new partners from internal promotions* and 2) *% turnover firm level (t-1)*. These measures should capture the extent to which the firm values its employee stakeholders by measuring the importance of its internal labor market and the time-varying level of attachment that employees feel to the organization. We also include a dummy, *merger*, that takes a value of one if the firm merges at the end of the year.

New York City exposure. Due to the economic disruptions in New York City following the 9/11 attacks, we account for an attorney's economic exposure to the NYC legal market. The Martindale data include information on state bar admissions, so we include a dummy for *New York licensure* to capture whether an attorney may serve clients in New York City. *NYC law school* indicates whether the attorney attended a law school located in New York City. We account for a firm's exposure to the NYC legal market using *percent of firm's attorneys in NYC.*

Sample

While lawyers from firms of all sizes list themselves in Martindale, pro bono investment information is only available for the largest 200 US firms (the AmLaw 200). We thus limit our "at-risk" sample to attorneys employed by the AmLaw 200. In testing the first two (non-shock) hypotheses, we include all attorneys who are members of these firms from 1999-2010 and for whom we have non-missing information for law school affiliation, birth year, and legal specialty; this sample constitutes 532,248 attorney years.

To test H3, we first identify all NYC metro-born attorneys working for AmLaw 200 firms in

1999; they constitute our treatment group. We then identify the NYC metro-born attorneys' 1999 officemates who were not born in other parts of New York or New Jersey; they constitute our comparison group of attorneys and should be relatively more insulated from the attacks. Note that since our outcome entails exit from the firm and from the data, we identify our treatment and control groups in 1999 (rather than, for example, 2001) in order to allow us to evaluate the difference in exit rates prior to the attacks. In our main analyses, we drop attorneys working in offices located in New York and New Jersey to limit alternative explanations related to the economic downturn after the attack. Results are robust to including these offices and to excluding offices that are located only in the NYC area. Our final sample includes 3,293 unique NYC metro-born attorneys and 28,084 unique officemates, constituting 196,263 attorney-years.

Estimation Model

We use logit and multinomial logit models in our primary analyses. As noted by Allison (1999), the logit model is well-suited for data that contains dichotomous outcomes with time-varying covariates and large time units in which many actors make decisions in the same time periods (creating "ties"). The multinomial logit allows us to estimate a "competing risks" model where an employee can choose to either 1) join a different existing law firm, 2) found a new startup law firm, or 3) make a major occupation change indicated by exiting the data. In these models, the baseline outcome is that the attorney remains with her firm. These four outcomes are collectively exhaustive of the potential destinations for an attorney in the following year. In robustness tests, we also estimate linear probability models with firm fixed effects, in order to help account for unobserved firm characteristics that might lead the firm to adopt a particular level of pro bono activities. Because our primary explanatory variable is measured at the firm-year level, we cluster standard errors by firm-years. This enlarges our standard errors and

ensures that the statistical significance of our results is not driven by the fact that we have many thousands of attorney-year observations.

Results

Table 1 displays descriptive statistics for the full sample as well as subsets of NYC-born attorneys and their officemates. Comparing NYC metro-born attorneys and their officemates, we see predictable differences. NYC metro-born attorneys are more likely to have attended a top and/or NYC-based law school, and they are slightly more likely to be licensed to practice in NY.

[INSERT TABLE 1 AND TABLE 2 ABOUT HERE]

Table 2 displays the cross-sectional correlation table for the full sample. Note that firms whose pro bono rankings are larger numbers have "better" pro bono programs. With the exception of whether an attorney graduated from a top law school, no individual-level characteristics correlate strongly with a firm's pro bono ranking. At the firm level, firms with better pro bono programs also have better financial performance, as measured by revenue per lawyer and profits per equity partner.

Table 3 presents tests of H1a-b (occupation change) and H2a-b (founding a startup). Model 1 examines the correlation between a firm's pro bono ranking and the probability that an attorney leaves the firm for any destination. The correlation is not statistically significant, and direction of the point estimate suggests that firms with better pro bono programs may experience more turnover, on average. The other controls are significant and largely in the direction that we would expect based on existing theory – attorneys are less likely to leave when they have more social ties in the office, when they are male, when they have higher peer ratings, higher age, and more authority, suggesting that the model is performing well.

Model 2 presents a multinomial logit with the dependent variable from Model 1 separated

into three mutually exclusive categories: joins an existing law firm, founds a new law firm, or makes occupation change. The null correlation in Model 1 obscures intriguing underlying heterogeneity: firms with stronger pro bono rankings are less likely to lose attorneys to occupation changes and startup founding (supporting H1a and H2a), but they are *more* likely to lose attorneys to existing law firms. Wald tests indicate that the correlation between pro bono ranking is statistically different when comparing mobility to existing law firms and startups and mobility to existing law firms and occupation changes ($p<.01$). These results support H1b and H2b.[12] In terms of the effect size for H1a, the marginal effects presented in Table 3 suggest that an increase in the firm's pro bono ranking by one standard deviation (i.e., 50 ranking slots) corresponds to a 3.6% reduction in occupation changes as compared to the sample mean. A similar calculation for founding new firms (H1a) reveals that a one standard deviation increase in the firm's pro-bono ranking corresponds to a 5% reduction in founding rates as compared to the sample mean. Similarly, increasing pro bono ranking by one standard deviation corresponds to a 5.3% *increase* that an attorney leaves for an existing law firm, as compared to the sample mean.

[INSERT TABLE 3 ABOUT HERE]

The theory we develop suggests meaningful work as a primary mechanism underlying the retention benefits of pro bono. The mortality-related shock caused by the 9/11 tragedy is a uniquely powerful empirical device for helping to examine this mechanism, which is difficult to tease out of the steady-state results presented above. For the analysis in Table 4, we use the subsample composed of NYC metro-born attorneys (working outside of NY-NJ) and their

[12] Results for H1b are also supported if we compute a different multinomial logit that compares occupation changes to within-industry mobility (i.e. by combining the "founds a new law firm" and the "joins established law firm" outcome).

officemates. We use multinomial logit models with two outcomes: 1) joining a different law firm or 2) occupation change, which, along with the reference group of staying with the current firm, are collectively exhaustive of potential attorney destinations. Because of the infrequency of startup mobility events in this sample (see Table 1), we combine mobility to established law firms and startup law firms into the "joining a different law firm" outcome, and results are the same if we instead include foundings in the "occupation change" outcome.

Model 1 in Table 4 shows that NYC metro-born attorneys and their officemates have comparable rates of occupation changes in the years prior to 9/11, which supports the validity of the treatment and comparison groups. In 2002, however, NYC metro-born attorneys are more likely than officemates to make occupation changes (p=.06). The odds ratio suggests that NYC-born attorneys are 29% more likely than officemates to make occupation changes in 2002. We do not see an effect for moving to a different law firm, which helps support the idea (put forward in H1) that occupation changes may reflect a desire to find more meaningful work.

Models 2 and 3 in Table 4 test H3 by splitting the sample into firms with pro bono rankings that are above the median ranking of 100 ("better" pro bono firms) and below the median ranking of 100 ("worse" pro bono firms). There are no significant differences between these samples with respect to job changes where the attorney joins a different law firm. However, the effect of 9/11 on occupation changes is concentrated in worse pro bono firms. A Wald test of the difference between "NYC metro-born*2002" in each sample reveals a statistically significant difference (p=.03). The odds ratios suggest that NYC metro born attorneys are 85% more likely than officemates to make occupation changes in 2002 when working for a below-median pro bono firm. There is no difference in occupation changes between NYC-born attorneys and officemates in 2002 for above-median pro bono firms. Figure 1 shows the effect graphically.

[INSERT TABLE 4 AND FIGURE 1 ABOUT HERE]

Robustness tests

Unobserved firm heterogeneity: We estimate linear probability models with firm fixed effects, in order to absorb time-invariant differences among firms (such as organizational culture and superior management) that might drive CSR investments and employee exit behavior. The results for the full sample are presented in Table 5, Models 1-3, while the results for the 9/11 sample are presented in Table 6, Models 2-3. With one exception (the probability of occupation change loses significance in Table 5, Model 3), the results remain consistent.

Involuntary exits: Our secondary data do not allow us to discern the reason for an attorney's departure. Perhaps firms with stronger CSR programs are somehow more "employee friendly" and less likely to dismiss attorneys. Our regressions include numerous observables that might correlate with "employee friendliness," and the pattern of the 9/11 results over time in Table 4 (a sharp increase in occupation changes, followed by a gradual decrease) suggests a sudden shift in preferences rather than a decline in performance leading to termination.

We also implement additional robustness tests. First, partners under age 60 should be less exposed to layoffs and forced retirements than other attorneys due to their age and ability to control client relationships. Limiting the sample to only these higher-value individuals, we see similar results for H2a (Table 5 model 4) and H3 (Table 6 model 1). Results for H1a (occupation changes) become marginally insignificant (p=.12). Second, we evaluate exit behavior following the financial crisis of 2008, which was well-known for resulting in the layoffs of many attorneys (e.g. Gearon, 2012). If firms with higher pro bono investments are more employee friendly, we would expect to see lower rates of exit from these firms in 2008 and 2009, relative to other years. Figure 2 charts the marginal effect of continuous pro bono ranking on occupation changes on a

year by year basis for the full sample, but we see no evidence to support this alternative explanation. Third, we exclude observations in the 9/11 sample where the attorney is licensed to practice in New York State, leaving only attorneys who should be relatively insulated from NYC-specific economic effects. Results (Table 7 Model 2) are similar to those reported above. Finally, we collect yearly data from *Vault* on the "Top 20 Law Firms to Work For" in order to obtain an external measure of "employee friendliness." Including a dummy that indicates whether the firm is included in this list does not affect the results (available from the authors).

[INSERT TABLES 5 AND 6 AND 7 AND FIGURE 2 ABOUT HERE]

Destinations following exit from the data: A related limitation of our analysis is our inability to track attorneys when they exit the Martindale data, which inhibits our ability to tell whether occupation changes are driven by a preference for meaningfulness on the job. To address this limitation, we took a random sample of 200 attorneys who exit the data from the 9/11 sample, used to test H3. 50 attorneys were born in NYC metro and exited the data in 2000 (the year prior to the 9/11 attacks), 50 were born in NYC metro and exited the data in 2002 (the year following the 9/11 attacks), 50 were born outside NYC metro and exited in 2000, and 50 were born outside NYC metro and exited in 2002. This sampling frame allows us to examine whether NYC metro born attorneys were more likely to take more meaningful jobs after 9/11. We used internet searches to locate these attorneys and identify their ultimate destinations. We see that 14% of NYC-born individuals who exit the data in 2002 pursued more meaningful jobs (Appendix B). This represents an increase from 6% of NYC-born individuals who exit the data in 2000, and is the highest of the four groups analyzed. This additional analysis, along with our attorney interviews reported above, provide additional evidence that 9/11 drove NYC-born individuals to pursue more meaningful work.

Job choices of NYC-born law school grads after 9/11: To further respond to our inability to track attorneys when they leave the data and our inability to distinguish involuntary turnover, we also examine how pro bono influences initial job choice decisions, an analysis that does not suffer from either of these limitations. Specifically, we examine the job choices of NYC metro-born law school graduates, compared to other law school graduates, before and after 9/11. If 9/11 changed preferences for meaningfulness, and pro bono activities play a role in fulfilling these preferences, NYC metro-born law school graduates should place a higher premium on a potential employer's pro bono ranking immediately following 9/11.

We display results in Table 8. The sample is limited to recent law school graduates who take jobs with AmLaw firms whose pro bono ranking is available. Attorneys appear in the sample only in the year in which they take their first job. The sample is limited to 1999-2005 because the Martindale data contain limited information on birthplace for attorneys who enter the data after 2005. The dependent variable is the pro bono ranking of the firm in the year prior to the graduate joining the organization. We see that NYC metro born law students who graduate in 2002 accept jobs with firms whose pro bono ranking is on average 9.8 slots better than NYC metro-born law students who graduate in 2001. In all other years, NYC metro-born law students join firms with worse pro bono programs. Table 8 shows that these results are highly robust to different specifications, including those with law school fixed effects (Models 2 and 4). These results are also shown graphically in Figure 3 and provide strong corroboration for our argument that 9/11 shifted the preferences of NYC metro-born attorneys towards more meaningful work, and that pro bono activities were important in meeting these preferences.

Pro bono as a proxy for slack resources rather than meaningful work: We take two additional steps to examine whether pro bono programs are "luxury goods," available only to

high quality firms with slack resources. First, we use Coarsened Exact Matching (Iacus, King, & Porro, 2011) to match our split sample of above-median and below-median pro bono firms on their levels of revenue per attorney and profits per equity partner. Results in Table 7, Model 1 (which also provide more detail on the matching procedure) are similar to those reported above. Second, we perform a placebo test where we split firms at the yearly median of revenue per lawyer. Results in Table 7, Model 3, indicate no difference in exit rates for NYC metro-born attorneys working in firms with revenues per attorney above and below the median.

Startup founding versus small firm mobility: Our theoretical arguments for H2 suggest that founding a startup firm provides an individual with unique access to meaningfulness at work by the creation of a "personalized" organization. We are able to examine this mechanism more directly by examining the influence of pro bono activities on mobility to small firms, a move which allows an attorney to escape the pressures of large firm employment but does not provide the same level of autonomy as the founding of a startup. Table 7 demonstrates that pro bono activities do not have a statistically or economically significant effect on mobility to a small firm (defined as mobility to a firm of under 25 attorneys without being that firm's founder). Moreover, the difference between pro bono's effect on the founding of a startup firm is statistically larger than that on mobility to a small firm (p=.055). These results suggest that pro bono activities have a unique relationship with startup firm founding.

Empirical choices. In testing H1-H2, we rely on a continuous measure of the firm's yearly pro bono ranking while in H3 we split the sample based on median pro bono ranking. To ensure that our results are consistent across specifications, we retest H1 and H2 using four dummies that correspond to the quartile of the firm's pro bono ranking in the current year. Results (available from the authors) show expected results: the effect is mainly driven by firms in the upper

quantiles of the pro bono distribution. Similarly, we perform 9/11 analyses using a three-way interaction among NYC Metro Born*Year*Pro bono rank, rather than the split sample approach documented in Figure 1, which we prefer for its ease of graphical presentation. Results (available from the authors) robust to these alternatives as well as using *% of firm's attorneys w/>=20 hours of pro bono work*, another yearly measure from *American Lawyer*. Our primary 9/11 analyses exclude the states of NY and NJ from our analysis. Results (available from the authors) are robust to including all offices from across the US in the model.

Though we cluster standard errors at the firm level, we ensure that our results are not an artifact of our large sample size by collapsing the data to the firm level. Results in Table 9 are similar in statistical and practical significance to those seen in the individual-level analyses.

Summary. Pro bono activities are not randomly assigned across firms (e.g. Flammer, 2015), and our data limit our ability to directly infer the motivations of the individuals in our sample. Our empirical approach attempts to deal with these issues in multiple ways. Nevertheless, we must caution against drawing causal conclusions from our study, given the imperfect nature of our pro bono measure and the limitations of our empirical design.

[INSERT TABLES 8, 9 AND FIGURE 3 ABOUT HERE]

DISCUSSION

Prior research suggests that firm-level investments in CSR activities may limit employee turnover. Our study contributes to this literature in important ways. We develop theory that explains why meaningfulness at work is likely to be a crucial mechanism driving the retention benefits of CSR. We find empirical support for this theory through a comparison, across firms with heterogeneous investment in CSR activities, of types of employee job changes that are likely to be at least partially driven by a desire for more meaningfulness at work. Specifically,

we find that firm level CSR activities are associated with decreases in (1) occupation changes, (2) moves to found firms within the industry, and (3) moves following a mortality-related shock (the tragic events of 9/11). Thus, we find support for our theory that CSR will have a greater retention benefit when employee moves are motivated by a search for meaningfulness.

Accordingly our work places an important boundary condition on the theory that CSR reduces turnover. We provide evidence that CSR is a particularly useful retention mechanism when turnover might be driven by a search for meaningfulness, but may not be as useful when turnover is driven by other factors such as desires for material gain. Future research may continue to explore the specific conditions under which CSR may or may not enhance retention.

The effect sizes of our empirical results add important nuance to extant work. We find that CSR has a modest relationship with occupation-switching and firm founding during steady state operations of the firm (H1 & H2), but following a mortality-related shock (H3), the effect size is substantial. This empirical pattern seems consistent with the CSR-as-insurance argument (Godfrey, 2005). Godfrey (2005) argues that firm investments in CSR create a sense of goodwill among a firm's stakeholder that has insurance-like benefits when bad things happen for which the firm is reasonably culpable. His argument is that CSR may or may not have steady state benefits for the financial performance of the firm, but when an unanticipated negative event occurs, the insurance benefit of CSR investments may be quite important. It is possible that our results support a similar finding, but in a new context and with a different application. In addition to building a reservoir of goodwill among a set of outside stakeholders who give the firm the benefit of the doubt, pro bono may also build a reservoir of meaningfulness among employees that buffers the firm against turnover when crises occur in employees' lives. Given that our empirical results for the steady state hypotheses have small effect sizes, CSR-as-

insurance logic may provide a compelling theoretical avenue for future work examining CSR and turnover, and our work may have important implications for research that examines CSR from a risk management perspective (e.g., Godfrey, 2005; Koh, Qian, & Wang, 2014).

Our work also contributes to the growing scholarly conversation about why organizations should think about employees who leave to found new firms as engaging in a distinct type of turnover, as compared to employees who leave to join established competitors (e.g. Campbell et al., 2012; Ganco, 2012). Prior work emphasizes the importance of sating the financial motivations of potential founders, while our work suggests that providing an outlet for more work-related meaningfulness may also help prevent employees from leaving to found new firms.

Our study is also the first to provide a multi-firm test of the relationship between CSR activities and individual turnover outcomes. Because multi-firm turnover data are difficult to assemble but allow for more generalizable conclusions, this is an important empirical contribution to the growing literature that examines how CSR activities influence employee outcomes. Prior work examines turnover intentions (Stewart et al., 2011), employee perceptions of their employers (Doh et al., 2011), or actual turnover using data from a single large organization (Bode et al., 2015). We note, for example, the path-breaking work of Bode et. al. (2015) who study the effects of CSR involvement on retention in a global consulting firm. Their detailed single firm data allow them to very precisely estimate the treatment effect of an individual's personal involvement of CSR on that individual's probability of leaving the firm. They also find, however, several factors that weaken the relationship between CSR involvement and turnover, such as the length of time a consultant works in a particular CSR project or whether the project was in a developing country. Our work with theirs together suggests a need to continue examining the nuance around when CSR may or may not increase retention.

Deeper Analysis of the Surprising Relationship between CSR and Turnover to Competitors

While we expected that CSR would reduce occupation changes more than CSR would reduce within-occupation mobility, we were surprised to find that firms with higher levels of CSR activities were *more* likely to lose attorneys to competing law firms (i.e. to within-occupation mobility). An examination of the organizational literature suggests two main reasons why we might find these results. First, Turban and Greening (2000; 1997) suggest that CSR activities may allow firms to attract higher quality employees, and Burbano et al. (2014) (who also use data on AmLaw 200 law firms) suggest that law firms that conduct pro bono legal work often provide better training to associate attorneys. Thus, it is possible that a firm's CSR activities help it to attract and build higher quality talent, and that that higher quality talent is an attractive poaching target for rivals who also are able to observe the firm's CSR investments.[13]

Second, CSR activities (or the type of firm culture that may attach to CSR activities) may not be attractive to all attorneys. Though firms make efforts to publicize their pro bono activities, it may be difficult for attorneys to assess their preferences for the firm's pro bono work and/or the firm's culture until they enter the organization. In addition, prior work suggests that CSR activities may be a perquisite that is consumed by managers who want their organizations to appear prosocial to external constituents (Chin, Hambrick & Trevino, 2013; Koh, Qian, & Wang, 2014). Managers who do not have these preferences may move to other law firms.

Table 10, which shows results from linear probability models with firm fixed effects (logit models show similar results), indicates that both of these explanations is likely operating in our setting. If the "poaching" explanation is important, we should see that CSR activities have a

[13] This may be especially true in legal services, where non-compete agreements are not enforceable.

stronger relationship with turnover to existing law firms for associate attorneys and for younger attorneys (each of whom are much more likely to develop skills by working on pro bono cases; see Burbano et al. [2014]). Results in Model 1 and Model 2 show precisely these outcomes – for example, the relationship between CSR and competitor mobility is about 75% stronger for associate attorneys as compared to partner attorneys or "of counsel" attorneys.

[INSERT TABLE 10 ABOUT HERE]

If the "taste for CSR" explanation is important, then we should see that CSR activities have a weaker relationship with turnover to existing law firms for attorneys whose observable characteristics suggest that they will be more likely to value CSR activities. We capture attorneys' taste for CSR using their practice area, their gender, and their political ideology. Approximately 5% of attorneys in the data practice environmental law; these individuals likely place more value on prosocial aspects of work than other attorneys. Consistent with this idea, Model 3 shows that the relationship between CSR and movement to competitors is about 30% weaker for these attorneys. Prior research suggests that women may value CSR activities more than men (Turban & Greening, 2000); consistent with this idea, Model 4 shows the relationship between CSR and movement to competitors is about 28% stronger for male attorneys.

Finally, prior research suggests that individuals who have a more left-leaning or liberal political ideology are more likely to value CSR activities (Chin et al., 2013). We examine this possibility by linking, at the individual level, attorneys in the Martindale data to individuals in Bonica's (2014) Database on Money and Ideology in Elections (DIME), which contains all donations of at least $200 to politicians in state and federal elections from 1982-2012. 45% of the attorneys make at least one donation, which corresponds to the match rate found by Bonica et al. (2015), who also link DIME data to Martindale data. Following a large body prior work (e.g.

Chin et al., 2013), we capture whether an attorney is more liberal using her *% of Donations to Democrats*. Consistent with the idea that liberal individuals will be more likely to value CSR, Model 5 shows the relationship between CSR and movement to competitors is weaker for attorneys who donate more money to Democratic politicians.

A very important opportunity for future work lies in a deeper examination of this finding. For example, is this effect limited to the legal services industry, where CSR activities (and the training they provide) are easily observable to competitors and employees may possess weaker prosocial motivations (e.g. Galanter & Palay, 1991)? Are employees "turned off" by CSR activities themselves, or do they leave because they do not prefer the prosocial firm culture that might co-occur with CSR activities (e.g. Chatman, 1989)? Do attorneys who invest more in CSR actually have a deeper sense of personal ownership over their work rather than crediting the firm for the sacrifice (Bode et. al. 2015), which may reduce the attachment created by CSR? This result also sharpens the managerial implications of our work by underscoring two potential downsides of CSR activities. First, CSR activities might allow employees to increase their marketability to competitors. Second, CSR activities might "push" out individuals who do not prefer them. We hope this result sparks further research that explores more deeply the conditions under which CSR may lead to higher employee turnover.

In the context of the current paper, this surprising result provides additional empirical support to the central thesis of our paper: CSR is most likely to reduce employee turnover that is motivated by a search for meaningfulness. Occupation changes, law firm founding, and moves following mortality related shocks are much more likely than moves to existing law firms to be motivated by a preference for meaningfulness at work, and we see that CSR attenuates turnover in those situations much more than turnover to existing law firms.

Limitations

Despite important contributions, our study has several limitations. First, there are limits to generalizability of our work. Our study focuses on employees of large corporate law firms, a job typically characterized as high in status but low in meaningfulness. This feature may have two effects. On the one hand, compared to other settings, our results may understate the value of CSR activities in reducing turnover, because large corporate law firms probably attract individuals who are less motivated by meaningfulness and thus less likely to value the firm's CSR activities. This effect might cause us to overstate the positive relationship that we see between CSR activities and within-occupation mobility, as compared to contexts which attract workers who have stronger preferences for meaningfulness (e.g. medicine). On the other hand, low baseline levels of meaningfulness in this setting may mean that CSR activities can exert a larger marginal effect on meaningfulness and thus a larger marginal effect on retention than other settings where meaningfulness is a more natural feature of the job. This effect might cause us to overstate the negative relationship that we see between CSR activities and out-of-occupation mobility, since a job in a corporate law firm is likely to have much lower meaningfulness than a job different occupation or in a startup law firm. We think our work generalizes rather easily to other important professional services settings, such as investment banking, accounting, or consulting, but future work in other settings might uncover different and interesting results. Similarly, the 9/11 terror attacks, which we use as a mortality-related shock, were unique. It is possible that different mortality shocks could have different effects. Additionally, we study CSR *activities* in which employees are directly engaged in helping others. A recent study by Cuypers, Koh, & Wang, (2015) suggests that the benefits of CSR accrue to firms who are perceived by their stakeholders as acting in substantive rather than symbolic ways. It is possible that the type

of CSR we study is particularly substantive and the effects may not generalize to more symbolic forms of CSR.

Furthermore, our data have important limitations. For example, they do not allow us to observe individual participation in CSR activities like the data used by Bode et. al. (2015). We also cannot precisely measure the reason why employees depart their firms. For example, the founding of a startup or an occupation change may be driven by motivations other than a desire for more meaningfulness at work. In addition, we are unable to track the destinations of all attorneys who exit the Martindale data. While an analysis of job choices of a subsample of 200 lawyers who exit the Martindale data shows significant career changes, additional research using comprehensive data on occupation changes may provide important insights into the relationship between CSR and these changes.

We also acknowledge the difficulty in perfectly identifying the meaningfulness mechanism that is the focus of our arguments. While we examine turnover and job selection decisions following an exogenous shock, we use models with firm fixed effects, and we include multiple controls for observable firm characteristics like slack, performance, and "employee-centeredness" that might provide alternative explanations for our results, there are limits to our ability to clearly isolate the meaningfulness as the key drivers of our observed outcomes.

CONCLUSION

Overall, our theory and findings suggest an important boundary condition on the ability of CSR activities to reduce employee turnover: it is likely important that the turnover be motivated by a desire for more work-related meaningfulness. Our study joins a recent stream of literature that explores the strategic use of CSR activities by firms in achieving desired employee outcomes, including developing and enhancing human capital and organizational commitment

and limiting employee turnover (e.g., Bode et al., 2015; Burbano et al., 2014; Flammer & Luo, 2015). Our study also suggests that investments in CSR may provide previously unexplored insurance-like benefits to firms (e.g., Godfrey, 2005; Koh, Qian, & Wang, 2014; Minor & Morgan, 2011), aiding in employee retention following mortality-related shocks in the lives of their employees. Finally, additional results suggest that CSR activities correlate with higher turnover to competing law firms, a surprising finding that we hope sparks additional research on the boundary conditions of CSR as a retention device.

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Table 1: Summary Statistics

	(1) Sample for H1 and H2					(2) 9-11 Sample: NonNYC-Metro Born				(3) 9-11 Sample: NYC-Metro Born			
	n	Mean	StDev	Min	Max	n	Mean	Min	Max	n	Mean	Min	Max
Leaves the firm for any dest.	524407	0.11	0.31	0.0	1.0	174703	0.08	0.0	1.0	21560	0.08	0.0	1.0
Joins existing law firm	524407	0.07	0.25	0.0	1.0	174703	0.05	0.0	1.0	21560	0.05	0.0	1.0
Founds startup law firm	524407	0.005	0.08	0.0	1.0	174703	0.005	0.0	1.0	21560	0.003	0.0	1.0
Makes occupation change	524407	0.04	0.20	0.0	1.0	174703	0.03	0.0	1.0	21560	0.03	0.0	1.0
% office partners from atty's law school	524407	0.09	0.11	0.0	1.0	174703	0.10	0.0	0.9	21560	0.08	0.0	0.8
Female first name	524407	0.26	0.44	0.0	1.0	174703	0.21	0.0	1.0	21560	0.20	0.0	1.0
Male first name	524407	0.68	0.47	0.0	1.0	174703	0.74	0.0	1.0	21560	0.76	0.0	1.0
Peer rating: A	524407	0.31	0.46	0.0	1.0	174703	0.51	0.0	1.0	21560	0.50	0.0	1.0
Peer rating: B	524407	0.04	0.20	0.0	1.0	174703	0.05	0.0	1.0	21560	0.04	0.0	1.0
Peer rating: C	524407	0.00	0.06	0.0	1.0	174703	0.00	0.0	1.0	21560	0.00	0.0	1.0
Associate	524407	0.37	0.48	0.0	1.0	174703	0.17	0.0	1.0	21560	0.15	0.0	1.0
Partner	524407	0.53	0.50	0.0	1.0	174703	0.74	0.0	1.0	21560	0.76	0.0	1.0
Legal specialty overlap with officemates	524407	0.09	0.06	0.0	1.0	174703	0.08	0.0	1.0	21560	0.08	0.0	1.0
Top 18 law school	524407	0.36	0.48	0.0	1.0	174703	0.36	0.0	1.0	21560	0.46	0.0	1.0
NYC-based law school	524407	0.10	0.30	0.0	1.0	174703	0.02	0.0	1.0	21560	0.10	0.0	1.0
Attorney age	524407	42.31	10.02	25.0	65.0	174703	47.16	8.0	65.0	21560	47.55	22.0	65.0
Tenure with firm	524407	4.07	3.33	0.0	12.0	174703	6.23	0.0	13.0	21560	6.17	0.0	13.0
Licensed to practice in NY	524407	0.12	0.32	0.0	1.0	174703	0.16	0.0	1.0	21560	0.18	0.0	1.0
Pro bono rank continuous (200=best)	524407	112.56	50.57	11.0	200.0	174703	105.54	1.0	200.0	21560	111.53	1.0	200.0
Pro bono rank best qrtile	524407	0.27	0.44	0.0	1.0	174703	0.23	0.0	1.0	21560	0.27	0.0	1.0
Pro bono rank 2nd qrtile	524407	0.29	0.46	0.0	1.0	174703	0.28	0.0	1.0	21560	0.31	0.0	1.0
Pro bono rank 3rd qrtile	524407	0.30	0.46	0.0	1.0	174703	0.32	0.0	1.0	21560	0.27	0.0	1.0
Pro bono rank worst qrtile	524407	0.14	0.35	0.0	1.0	174703	0.12	0.0	1.0	21560	0.08	0.0	1.0
% firm attys w/>=20 pro bono hours (1000s)	521428	37.16	19.66	0.0	100.0	166528	35.18	0.0	100.0	20403	37.24	0.0	100.0
Revenue per lawyer ($100k)	524407	5.90	1.80	2.5	16.3	174703	5.35	2.5	15.2	21560	5.57	2.5	15.2
Profit per eq. partner ($100k)	524407	7.75	4.57	2.0	44.4	174703	6.46	2.0	44.4	21560	6.88	2.0	44.4
Firm participates in merger	524407	0.00	0.05	0.0	1.0	174703	0.00	0.0	1.0	21560	0.00	0.0	1.0
Firm size (# attorneys, 100s)	524407	4.66	2.65	0.0	17.0	174703	4.32	0.1	17.0	21560	4.37	0.1	17.0
% turnover firm level (t-1)	524407	10.99	5.95	0.0	100.0	174703	10.62	0.0	100.0	21560	10.88	0.0	100.0
% new partners from internal promotions	524407	0.42	0.28	0.0	1.0	174703	0.45	0.0	1.0	21560	0.42	0.0	1.0
Office size (# attorneys, 100s)	524407	1.40	1.05	0.0	7.5	174703	1.55	0.0	7.5	21560	1.48	0.0	7.5
Partner-associate ratio, office	524407	1.20	1.91	0.0	113.0	174703	1.04	0.0	113.0	21560	1.10	0.0	113.0
% of firm attorneys in NYC	524407	0.15	0.23	0.0	1.0	174703	0.07	0.0	1.0	21560	0.09	0.0	1.0
# AmLaw rival attorneys in city (1000s)	524407	4.47	4.22	0.0	14.6	174703	2.99	0.0	14.8	21560	4.13	0.0	14.8
Observations	524407					174703				21560			

Level of analysis is the attorney-year

Table 2: Correlation table, full sample

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27
1 Leaves the firm for any dest.	1.00																										
2 Joins existing law firm	0.76	1.00																									
3 Founds startup law firm	0.13	0.18	1.00																								
4 Occupation change	0.61	-0.06	-0.01	1.00																							
5 % office partners from atty's law school	-0.02	-0.02	0.01	-0.01	1.00																						
6 Female first name	0.05	0.01	0.00	0.06	0.02	1.00																					
7 Male first name	-0.06	-0.02	0.00	-0.07	-0.01	-0.87	1.00																				
8 Peer rating: A	-0.12	-0.08	-0.01	-0.09	0.07	-0.18	0.19	1.00																			
9 Associate	0.17	0.12	0.02	0.12	-0.02	0.20	-0.22	-0.01	1.00																		
10 Partner	-0.18	-0.12	-0.02	-0.13	0.03	-0.23	0.25	0.00	-0.82	1.00																	
11 Legal specialty overlap with officemates	0.03	0.04	0.01	0.01	0.20	-0.02	0.02	0.02	0.01	-0.01	1.00																
12 Top 18 law school	-0.01	-0.02	-0.01	0.00	-0.13	-0.05	0.05	-0.02	-0.07	0.08	-0.05	1.00															
13 NYC-based law school	0.02	0.02	0.00	0.01	-0.07	0.00	-0.01	-0.01	0.03	-0.03	-0.01	0.16	1.00														
14 Attorney age	-0.13	-0.10	-0.01	-0.08	0.02	-0.22	0.23	-0.01	-0.70	0.61	-0.02	0.05	-0.01	1.00													
15 Tenure with firm	-0.10	-0.08	-0.01	-0.06	0.02	-0.09	0.11	0.01	-0.45	0.45	-0.10	0.05	-0.03	0.46	1.00												
16 Licensed to practice in NY	-0.04	-0.03	0.00	-0.02	0.00	-0.06	0.07	0.00	-0.21	0.19	0.00	0.03	0.00	0.20	0.13	1.00											
17 Pro bono rank continuous (200=best)	0.00	0.00	-0.01	0.00	-0.10	0.01	-0.01	-0.01	0.01	-0.03	-0.08	0.17	0.07	-0.02	0.03	0.00	1.00										
18 Revenue per lawyer ($100k)	-0.01	-0.01	-0.01	0.00	-0.16	0.01	-0.02	-0.01	0.04	-0.07	-0.05	0.17	0.17	-0.02	0.22	-0.02	0.38	1.00									
19 Profit per eq. partner ($100k)	0.00	0.00	-0.01	0.00	-0.15	0.01	-0.02	-0.01	0.06	-0.08	-0.04	0.17	0.19	-0.06	0.15	-0.02	0.31	0.90	1.00								
20 Firm participates in merger	0.10	0.12	0.00	0.01	-0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.01	0.01	0.00	-0.02	0.00	-0.01	1.00							
21 Firm size (# attorneys, 100s)	-0.01	-0.03	0.00	0.01	-0.06	0.02	-0.02	0.00	0.03	-0.05	-0.13	0.03	-0.02	-0.04	0.05	-0.03	0.15	0.26	0.26	-0.04	1.00						
22 % turnover firm level (t-1)	0.07	0.08	0.00	0.01	-0.06	0.00	-0.01	-0.02	0.07	-0.06	0.08	0.04	0.05	-0.06	-0.09	-0.01	0.01	0.06	0.09	0.02	-0.09	1.00					
23 % new partners from internal promotions	0.01	0.01	0.00	0.01	0.02	0.00	0.00	0.00	0.04	0.00	0.00	0.03	0.00	-0.05	0.02	-0.01	0.00	0.02	0.08	0.01	-0.13	0.03	1.00				
24 Office size (# attorneys, 100s)	-0.03	-0.04	-0.01	0.00	-0.16	0.02	-0.02	-0.02	0.07	-0.05	-0.56	0.12	0.02	-0.08	0.11	-0.03	0.18	0.23	0.23	-0.02	0.25	-0.06	0.07	1.00			
25 Partner-associate ratio, office	0.02	0.01	0.00	0.01	-0.05	0.01	-0.01	-0.01	0.08	-0.09	0.04	0.05	0.07	-0.07	-0.04	-0.01	0.07	0.15	0.18	-0.01	-0.01	0.05	0.03	0.02	1.00		
26 % of firm attorneys in NYC	0.03	0.03	-0.01	0.00	-0.12	0.00	0.00	-0.02	0.06	-0.07	0.04	0.13	0.31	-0.04	0.01	0.00	0.17	0.45	0.51	0.01	-0.11	0.17	0.04	0.03	0.21	1.00	
27 # AmLaw rival attorneys in city (1000s)	0.01	0.01	-0.02	0.01	-0.25	0.01	-0.01	-0.03	0.02	-0.05	-0.13	0.18	0.31	0.00	0.03	-0.01	0.22	0.49	0.47	0.00	0.09	0.05	-0.03	0.18	0.08	0.43	1.00

Level of analysis is attorney-year, N=524,407

Table 3: Attorney Departures as a Function of Firm's Pro Bono Ranking. Marginal effects

	(1) Logit	(2) MLogit[a]		
	Leaves firm for any dest.	Joins existing law firm	Occupation change (H1)	Founds startup law firm (H2)
Pro bono rank continuous (199=best) (1000s)	0.043	0.074**	-0.029*	-0.005+
	(0.032)	(0.027)	(0.013)	(0.003)
% office partners from atty's law school	-0.065***	-0.053***	-0.015***	0.000
	(0.005)	(0.004)	(0.003)	(0.001)
Female first name	-0.001	-0.004*	0.002	-0.000
	(0.002)	(0.002)	(0.001)	(0.000)
Male first name	-0.011***	0.003+	-0.013***	0.001+
	(0.002)	(0.001)	(0.001)	(0.000)
Peer rating: A	-0.024***	-0.006***	-0.019***	0.001*
	(0.002)	(0.002)	(0.001)	(0.000)
Peer rating: B	0.000	0.000	-0.002	0.001***
	(0.003)	(0.002)	(0.002)	(0.000)
Peer rating: C	-0.013+	-0.007	-0.005	-0.001
	(0.007)	(0.006)	(0.005)	(0.001)
Associate	0.039***	0.027***	0.014***	-0.000
	(0.002)	(0.002)	(0.001)	(0.000)
Partner	-0.075***	-0.028***	-0.045***	-0.002***
	(0.002)	(0.002)	(0.001)	(0.000)
Legal specialty overlap with officemates	0.065***	0.041***	0.017*	-0.002
	(0.013)	(0.010)	(0.007)	(0.002)
Top 18 law school	0.000	-0.004***	0.005***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)
Attorney age	0.000**	-0.001***	0.001***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure with firm	-0.000	-0.001*	0.000+	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Revenue per lawyer ($100k)	-0.005*	-0.005**	-0.000	-0.000
	(0.002)	(0.002)	(0.001)	(0.000)
Profit per eq. partner ($100k)	0.000	0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.000)	(0.000)
Firm participates in merger	0.305***	0.201***	0.077**	0.002
	(0.070)	(0.043)	(0.026)	(0.002)
Firm size (# attorneys, 100s)	-0.000	-0.001*	0.001**	0.000**
	(0.001)	(0.000)	(0.000)	(0.000)
% new partners from internal promotions	0.004	-0.001	0.004	0.000
	(0.005)	(0.004)	(0.003)	(0.000)
% turnover firm level (t-1)	0.002***	0.001***	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Office size (# attorneys, 100s)	-0.009***	-0.008***	-0.000	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)
Partner-associate ratio, office	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
# AmLaw rival attorneys in city (1000s)	0.001***	0.001***	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Estimation	Logit		MLogit	
Legal specialty dummies / Year dummies	Yes / Yes		Yes / Yes	
N atty-year obs	524407		524407	
Pseudo R-sq	.071		.070	
Mean of DV	.11	.07	.04	.005

Robust standard errors clustered on firm-years, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests

[a]The excluded group is "Remains with current firm." Combined, the four outcomes are collectively exhaustive of the possible destinations for an attorney in the following year. Marginal effects calculated using -margins- in Stata 13, covariates held at sample means

Table 4: Attorney departures following 9/11, sample split based on firm's pro bono ranking. Relative risk ratios.

	(1): MLogit: Full Sample		(2): MLogit: Pro Bono Better Than Median		(3): MLogit: Pro Bono Worse than Median	
	Joins any law firm	Occupation change	Joins any law firm	Occupation change	Joins any law firm	Occupation change
NYC Metro Birth*Year 1999	1.181	1.230	1.075	1.091	1.229	1.378
	(0.167)	(0.176)	(0.222)	(0.221)	(0.240)	(0.279)
NYC Metro Birth*Year 2000	1.008	1.198	1.174	1.192	0.851	1.207
	(0.146)	(0.170)	(0.238)	(0.235)	(0.172)	(0.247)
NYC Metro Birth	0.996	0.932	0.952	1.022	1.058	0.867
	(0.115)	(0.100)	(0.156)	(0.156)	(0.172)	(0.132)
NYC Metro Birth*Year 2002	1.156	1.293^{+}	1.292	0.932	0.951	1.851**
	(0.189)	(0.193)	(0.280)	(0.207)	(0.240)	(0.365)
NYC Metro Birth*Year 2003	1.284	1.058	1.283	0.972	1.323	1.125
	(0.212)	(0.198)	(0.270)	(0.226)	(0.357)	(0.372)
NYC Metro Birth*Year 2004	1.102	1.110	1.077	0.853	1.148	1.480
	(0.198)	(0.189)	(0.253)	(0.207)	(0.316)	(0.363)
NYC Metro Birth*Year 2005	1.094	0.974	1.159	1.004	1.034	0.698
	(0.231)	(0.215)	(0.308)	(0.266)	(0.364)	(0.336)
NYC Metro Birth*Year 2006	1.143	0.662^{+}	1.361	0.527^{+}	0.870	0.856
	(0.265)	(0.158)	(0.432)	(0.178)	(0.275)	(0.286)
NYC Metro Birth*Year 2007	0.871	0.903	1.057	0.784	0.779	1.071
	(0.243)	(0.224)	(0.325)	(0.244)	(0.306)	(0.434)
NYC Metro Birth*Year 2008	1.159	1.061	1.051	0.854	1.374	1.449
	(0.284)	(0.242)	(0.338)	(0.265)	(0.482)	(0.494)
NYC Metro Birth*Year 2009	1.314	0.764	1.375	0.659	1.147	0.809
	(0.289)	(0.282)	(0.398)	(0.317)	(0.374)	(0.378)
Year 1999	0.694***	0.730***	0.765*	0.664***	0.616***	0.782^{+}
	(0.066)	(0.064)	(0.101)	(0.081)	(0.081)	(0.100)
Year 2000	0.999	0.706***	0.993	0.623***	0.960	0.771*
	(0.084)	(0.059)	(0.125)	(0.078)	(0.102)	(0.084)
Year 2002	1.049	0.852^{+}	1.165	1.021	0.973	0.731*
	(0.100)	(0.071)	(0.158)	(0.114)	(0.123)	(0.092)
Year 2003	1.151	0.762**	1.204	0.974	1.136	0.621***
	(0.125)	(0.071)	(0.177)	(0.129)	(0.179)	(0.079)
Year 2004	1.439**	0.998	1.495*	1.416*	1.540*	0.769^{+}
	(0.176)	(0.104)	(0.251)	(0.208)	(0.274)	(0.113)
Year 2005	1.430*	0.910	1.457*	1.476*	1.554*	0.590**
	(0.203)	(0.123)	(0.266)	(0.281)	(0.325)	(0.111)
Year 2006	1.496*	0.759^{+}	1.552*	1.214	1.604^{+}	0.558**
	(0.257)	(0.114)	(0.321)	(0.251)	(0.425)	(0.123)
Year 2007	2.397*	0.655*	1.831*	1.007	3.648*	0.500**
	(0.872)	(0.112)	(0.434)	(0.237)	(2.116)	(0.127)
Year 2008	2.131***	0.760	2.424**	1.198	2.035*	0.586^{+}
	(0.474)	(0.143)	(0.653)	(0.319)	(0.677)	(0.160)
Year 2009	2.510***	0.624*	2.617***	1.264	2.904**	0.354**
	(0.580)	(0.142)	(0.750)	(0.413)	(0.967)	(0.113)
Pro bono rank continuous	0.864	0.618	0.204	0.170^{+}	0.115	0.729
	(0.456)	(0.260)	(0.231)	(0.181)	(0.175)	(0.821)
Estimation	MLogit		MLogit		MLogit	
Controls / Legal specialty FE?	Yes		Yes		Yes	
N atty-year obs	196263		104383		91880	
Pseudo R-sq	0.093		0.091		0.104	

Robust standard errors clustered on firm-years. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests

Table 5: Robustness tests: H1-H2. Firm fixed effects models; limit to partner attorneys only.

	(1) Joins existing law firm	(2) Occupation change (H1)	(3) Founds startup law firm (H2)	(4): Multinomial Logit: Partners Only[a] Joins existing law firm	Occupation change (H1)	Founds startup law firm (H2)
Pro bono rank continuous (199=best) (1000s)	0.070***	-0.002	-0.010*	0.065+	-0.015+	-0.005
	(0.014)	(0.012)	(0.005)	(0.034)	(0.009)	(0.0037)
Estimation	OLS	OLS	OLS		MLogit	
Firm fixed effects	Yes	Yes	Yes		No	
Legal specialty fixed effects	Yes	Yes	Yes		Yes	
Year fixed effects	Yes	Yes	Yes		Yes	
N atty-year obs	524407	524407	524407		278035	
Pseudo R-sq	.055	.003	.040		.042	
Mean of DV	.07	.04	.005	.04	.02	.005

Robust standard errors clustered on firm-years. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests.
[a]Marginal effects calculated using -margins- in Stata 13, covariates held at sample means

Table 6: Robustness tests: H3, Attorney departures following 9/11. Partners only; firm fixed effects.

	(1) MLogit: Partners Only		(2) &(3) Firm Fixed Effects	
DV:	Occupation change,	Occupation change,	Occupation change,	Occupation change,
Split sample:	Pro Bono Strong	Pro Bono Weak	Pro Bono Strong	Pro Bono Weak
NYC Metro Birth*Year 1999	0.893	1.271	0.006	0.014
	(0.326)	(0.416)	(0.009)	(0.009)
NYC Metro Birth*Year 2000	0.721	1.135	0.008	0.009
	(0.264)	(0.397)	(0.009)	(0.009)
NYC Metro Birth	1.171	0.840	-0.002	-0.007
	(0.239)	(0.222)	(0.006)	(0.007)
NYC Metro Birth*Year 2002	0.725	1.840+	-0.001	0.025*
	(0.227)	(0.628)	(0.008)	(0.010)
NYC Metro Birth*Year 2003	0.652	0.591	0.000	0.005
	(0.217)	(0.330)	(0.008)	(0.009)
NYC Metro Birth*Year 2004	0.531+	1.504	-0.002	0.015
	(0.182)	(0.579)	(0.009)	(0.010)
NYC Metro Birth*Year 2005	0.976	0.282+	0.002	-0.003
	(0.336)	(0.210)	(0.009)	(0.008)
NYC Metro Birth*Year 2006	0.410+	0.894	-0.009	0.000
	(0.190)	(0.414)	(0.008)	(0.009)
NYC Metro Birth*Year 2007	0.696	0.842	-0.002	0.004
	(0.294)	(0.568)	(0.008)	(0.009)
NYC Metro Birth*Year 2008	0.543	0.680	-0.000	0.010
	(0.274)	(0.392)	(0.008)	(0.011)
NYC Metro Birth*Year 2009	0.667	0.890	-0.003	0.000
	(0.367)	(0.542)	(0.008)	(0.009)
NYC Metro Birth*Year 2010	0.489	1.055	-0.005	-0.002
	(0.216)	(0.535)	(0.008)	(0.010)
Pro bono rank continuous (199=best) (1000s)	0.095	3.329	-0.042	-0.033
	(0.145)	(5.048)	(0.034)	(0.038)
Estimation	MLogit	MLogit	OLS	OLS
Firm fixed effects	No	No	Yes	Yes
Legal specialty fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N atty-year obs	78849	67438	104383	91880
Pseudo R-sq / R-sq	0.045	0.080	.029	.029

Multinomial logit estimates for Pr(Joins any law firm) suppressed to save space. MLogit models display relative risk ratios. Robust standard errors clustered on firm-years + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests.

Table 7: Robustness tests: H3, Attorney departures following 9/11. Matched sample, exclude NY-licensed attys, rev. per atty placebo test.

	(1) MLogit: CEM Matched Sample[a]		(2) MLogit: Exclude NY-Licensed Attys		(3) MLogit: Split on Rev. per Atty	
DV:	Occupation change,	Occupation change,	Occupation change,	Occupation change,	Occupation change,	Occupation change,
Split sample:	Pro Bono Strong	Pro Bono Weak	Pro Bono Strong	Pro Bono Weak	Rev. per Atty Strong	Rev. per Atty Weak
NYC Metro Birth*Year 1999	1.063	1.466^{+}	1.000	1.184	0.376^{+}	0.075
	(0.215)	(0.301)	(0.227)	(0.255)	(0.227)	(0.190)
NYC Metro Birth*Year 2000	1.130	1.232	1.318	1.124	0.604**	-0.159
	(0.223)	(0.256)	(0.275)	(0.252)	(0.217)	(0.195)
NYC Metro Birth	1.043	0.844	0.991	0.912	-0.253	0.062
	(0.157)	(0.132)	(0.160)	(0.146)	(0.175)	(0.141)
NYC Metro Birth*Year 2002	0.957	1.886**	0.932	1.671*	0.347	0.189
	(0.211)	(0.378)	(0.220)	(0.359)	(0.236)	(0.193)
NYC Metro Birth*Year 2003	0.944	1.259	0.934	1.080	0.151	-0.037
	(0.222)	(0.409)	(0.227)	(0.362)	(0.270)	(0.267)
NYC Metro Birth*Year 2004	0.855	1.507^{+}	0.964	1.494	0.038	0.168
	(0.208)	(0.373)	(0.240)	(0.380)	(0.252)	(0.228)
NYC Metro Birth*Year 2005	1.017	0.732	1.138	0.817	0.126	-0.118
	(0.270)	(0.352)	(0.328)	(0.391)	(0.318)	(0.311)
NYC Metro Birth*Year 2006	0.498^{+}	0.921	0.546	0.678	-0.278	-0.487
	(0.179)	(0.308)	(0.208)	(0.259)	(0.334)	(0.347)
NYC Metro Birth*Year 2007	0.784	1.154	0.835	1.217	-0.113	-0.054
	(0.243)	(0.471)	(0.270)	(0.531)	(0.402)	(0.313)
NYC Metro Birth*Year 2008	0.954	1.504	0.935	1.372	0.406	-0.256
	(0.291)	(0.512)	(0.299)	(0.495)	(0.311)	(0.332)
NYC Metro Birth*Year 2009	0.647	0.876	0.790	0.712	-0.006	-0.422
	(0.336)	(0.412)	(0.408)	(0.371)	(0.508)	(0.533)
NYC Metro Birth*Year 2010	0.616	0.868	0.790	0.660	-0.198	-0.272
	(0.231)	(0.374)	(0.255)	(0.347)	(0.390)	(0.356)
Pro bono rank continuous (199=best) (1000s)	0.183	0.909	0.220	0.547	-0.400	-1.251*
	(0.196)	(1.042)	(0.248)	(0.632)	(0.748)	(0.579)
Estimation	MLogit	MLogit	MLogit	MLogit	MLogit	MLogit
Legal specialty fixed effects / Year effects	Yes	Yes	Yes	Yes	Yes	Yes
N atty-year obs	100229	90260	86535	77218	86376	109887
Pseudo R-sq	0.091	0.104	0.092	0.104	0.100	0.096

MLogit estimates for Pr(Joins any law firm) suppressed to save space. Robust standard errors clustered on firm-years + p<0.10, * p<0.05, ** p<0.01, *** p<0.001

[a]We use Coarsened Exact Matching (Iacus et al., 2011) to match firms with better than median pro bono rankings to firms with worse than median pro bono ranking. We match on revenue per attorney and profits per equity partner. The match rate is 89%. Prior to matching, the groups differ by $110k for rev. per atty and $260k for profits per equity partner (p<.05). Following matching and weighting, these differences decline to $87k and $189k, respectively (p>.05).

Table 8: Pro bono ranking of initial jobs of law school grads. Compare NYC metro born to other students.

	(1) Pro bono rank (200=best)	(2) Pro bono rank (200=best)	(3) Pro bono rank (200=best)	(4) Pro bono rank (200=best)
NYC Metro Birth*Grad. Year 1999	-6.077	-6.464	-0.054	-0.060
	(4.398)	(4.358)	(0.040)	(0.039)
NYC Metro Birth*Grad. Year 2000	4.516	4.677	0.034	0.036
	(4.133)	(4.032)	(0.035)	(0.034)
NYC Metro Birth	0.059	0.368	-0.001	0.001
	(2.260)	(2.319)	(0.020)	(0.020)
NYC Metro Birth*Grad. Year 2002	8.670*	8.572*	0.070*	0.069*
	(4.098)	(4.174)	(0.034)	(0.034)
NYC Metro Birth*Grad. Year 2003	-8.360*	-8.103*	-0.074*	-0.073*
	(3.756)	(3.666)	(0.034)	(0.033)
NYC Metro Birth*Grad. Year 2004	-12.084*	-12.019*	-0.106*	-0.105*
	(5.399)	(5.445)	(0.048)	(0.048)
NYC Metro Birth*Grad. Year 2005	-18.151***	-17.589***	-0.158***	-0.152***
	(4.197)	(4.174)	(0.038)	(0.038)
Graduation Year 1999	0.571	0.471	0.006	0.005
	(1.482)	(1.371)	(0.014)	(0.013)
Graduation Year 2000	-0.311	-0.216	-0.003	-0.002
	(1.185)	(1.146)	(0.011)	(0.010)
Graduation Year 2002	-3.932*	-2.973	-0.036*	-0.028+
	(1.856)	(1.854)	(0.016)	(0.016)
Graduation Year 2003	-4.622*	-4.170*	-0.042*	-0.038*
	(2.097)	(2.020)	(0.018)	(0.018)
Graduation Year 2004	-5.934*	-4.456+	-0.052**	-0.039*
	(2.378)	(2.359)	(0.020)	(0.019)
Graduation Year 2005	-10.238**	-7.530*	-0.093**	-0.069*
	(3.421)	(3.369)	(0.029)	(0.028)
Estimation	OLS	OLS	Poisson	Poisson
Law school fixed effects	No	Yes	No	Yes
Additional controls from Table 1?	Yes	Yes	Yes	Yes
Legal specialty fixed effects	Yes	Yes	Yes	Yes
N atty-year obs	19284	19284	19284	19258
Mean of DV	109.474	109.474	109.474	109.517

Robust standard errors clustered on law schools, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests
Level of analysis is law school graduate (i.e. attorneys enter the sample only once). Sample includes all associate attorneys who work an AmLaw 200 firm in the year of their law school graduation or the year immediately after. Firm variables are measured in the year prior to graduation so as to approximate the information available to the graduate when matching with a law firm.

Table 9: Robustness tests: Firm level results

	(1) % changing occupation (H1)	(2) % founding startups (H2)	(3) % joining existing law firms
Pro bono rank continuous (199=best) (1000s)	-0.037*	-0.010**	0.102*
	(0.017)	(0.003)	(0.040)
Estimation	OLS	OLS	OLS
Legal specialty controls / Year fixed effects	Yes	Yes	Yes
Other controls (see below)	Yes	Yes	Yes
N firm-year obs	1758	1758	1758
R-sq	0.116	0.133	0.276
Mean of DV	0.051	0.003	0.071

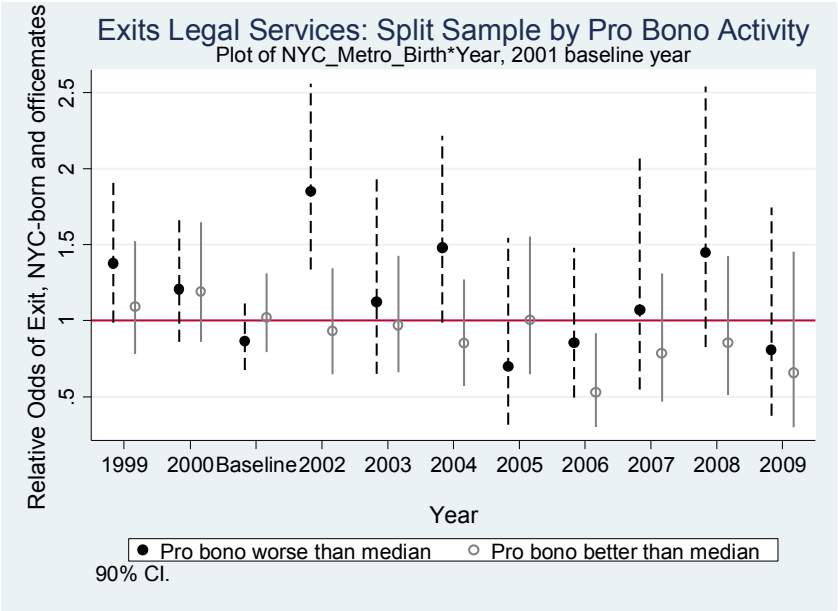
Robust standard errors, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests
Other controls include firm size, profit per equity partner, revenue per lawyer, partner-associate ratio, merger participation, and whether the firm is ranked in top 20 "Best Firms to Work For" by Vault.

Table 10. Examining Unexpected Positive Relationship between CSR and Attorney's Mobility to Other Law Firms

	(1) Joins existing law firm	(2) Joins existing law firm	(3) Joins existing law firm	(4) Joins existing law firm	(5) Joins existing law firm
Associate*Pro bono rank	0.092**				
	(0.030)				
Attorney age*Pro bono rank		-0.002*			
		(0.001)			
Environmental law*Pro bono rank			-0.053+		
			(0.030)		
Male*Pro bono rank				0.034*	
				(0.016)	
% attorney's political donations to Democrats*Pro bono rank					-0.061*
					(0.031)
Pro bono rank continuous (199=best) (1000s)	0.130*	0.270***	0.167***	0.141**	0.392***
	(0.053)	(0.057)	(0.049)	(0.048)	(0.090)
Associate	0.030***	0.040***	0.040***	0.040***	0.040***
	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)
Attorney age	-0.000***	-0.000	-0.000***	-0.000***	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Environmental law	0.004*	0.004*	0.010**	0.004*	0.004*
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)
Male first name	0.003+	0.003+	0.003+	-0.001	0.003+
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
% attorney's political donations to Democrats					0.008*
					(0.004)
Estimation	OLS	OLS	OLS	OLS	OLS
Firm, year, and legal specialty fixed effects	Yes	Yes	Yes	Yes	Yes
N atty-year obs	524407	524407	524407	524407	258127
R-sq	0.050	0.050	0.050	0.050	0.094
Mean of DV	.07	.07	.07	.07	.07

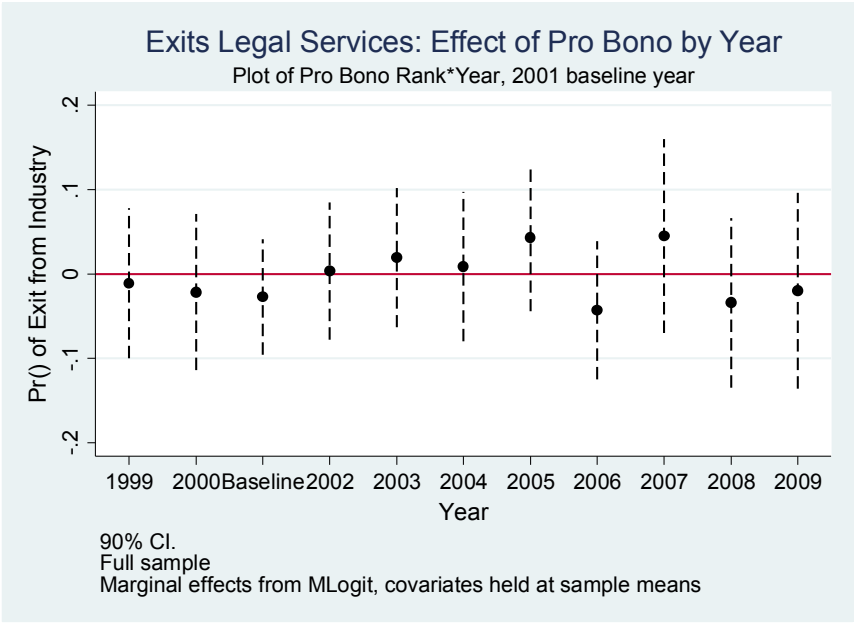
Robust standard errors clustered on firm-years, + p<0.10, * p<0.05, ** p<0.01, *** p<0.001, two-tailed tests

Figure 1



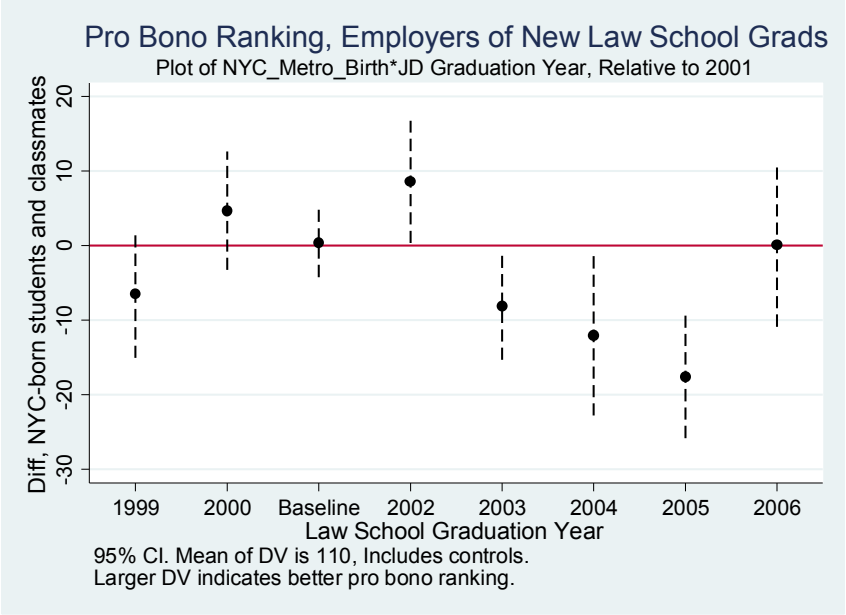
Results come from Table 4, Models 4 and 6.

Figure 2



Results (not tabulated to save space) are from a regression exactly like that in Table 3, Model 2, except that we interact the firm's pro bono ranking with the set of year dummies.

Figure 3



Results come from Table 9, Model 1.

Appendix A: *Attorney Practice Areas*

The Martindale data contain self-reported information on each attorney's legal specialty. Attorneys can list multiple specialties in a given year. These 3,000 unique free-text strings were pared down to list of 26 cleaned legal specialties using a three step process. First, we matched the strings with an exact match to 215 different areas of practice provided by Martindale. For the last several years, Martindale has forced attorneys to choose specialties from this list. This exact match covered about 75% of the records in the data. Second, for those strings that did not match, one of the authors, who holds a JD from a top five law school and who worked for six years as an attorney before entering academia, matched the strings by hand to the list provided by Martindale, using his/her expert judgment to choose the most appropriate match. This matching process covered another 15% of the records in the data, for a total coverage of 90%. Remaining unmatched strings were coded as "Other". In the third and final step, s/he matched the 215 areas of practice to a list of 25 areas of practice provided by MLAGlobal, a prominent legal consulting firm (http://www.mlaglobal.com/community/thought-leadership/practice-area-summary), again using expert judgment. The table below displays the frequency of each specialty area that comprises at least 1% of the attorney years in the sample.

	(1) Sample for H1 and H2				(2) 9-11 Sample: Non NYC-Metro Born		(3) 9-11 Sample: NYC-Metro Born	
	n	Mean	Min	Max	n	Mean	n	Mean
Administrative law	524407	0.084	0.0	1.0	21560	0.116	174703	0.102
Bankruptcy law	524407	0.052	0.0	1.0	21560	0.048	174703	0.049
Civil rights law	524407	0.013	0.0	1.0	21560	0.015	174703	0.017
Corporate law	524407	0.470	0.0	1.0	21560	0.475	174703	0.477
Criminal law	524407	0.028	0.0	1.0	21560	0.037	174703	0.028
Education law	524407	0.005	0.0	1.0	21560	0.006	174703	0.008
Energy law	524407	0.027	0.0	1.0	21560	0.034	174703	0.033
Entertainment law	524407	0.025	0.0	1.0	21560	0.036	174703	0.027
Environmental law	524407	0.056	0.0	1.0	21560	0.070	174703	0.075
Government law	524407	0.063	0.0	1.0	21560	0.089	174703	0.077
Healthcare law	524407	0.040	0.0	1.0	21560	0.058	174703	0.050
Insurance law	524407	0.050	0.0	1.0	21560	0.068	174703	0.063
Intellectual prop. law	524407	0.161	0.0	1.0	21560	0.159	174703	0.144
International law	524407	0.022	0.0	1.0	21560	0.027	174703	0.023
Labor law	524407	0.120	0.0	1.0	21560	0.129	174703	0.139
Litigation law	524407	0.376	0.0	1.0	21560	0.402	174703	0.389
Real estate law	524407	0.112	0.0	1.0	21560	0.109	174703	0.120
Tax law	524407	0.063	0.0	1.0	21560	0.065	174703	0.080
Trusts and estates law	524407	0.031	0.0	1.0	21560	0.029	174703	0.039
Observations	524407				21560		174703	

Appendix B: *Destination for Random Sample of Attorneys Who Exit Martindale Hubbell Data*

		Destination									
		In house counsel		*Law firm*[a]		*Government*		*Meaningful work (teaching, advocacy)*		*Other (not found, retired, unclear)*	
	Year→	2000	2002	2000	2002	2000	2002	2000	2002	2000	2002
NYC Birth	*Yes*	32%	16%	20%	26%	8%	8%	6%	14%	34%	36%
	No	40%	28%	16%	26%	4%	6%	4%	2%	36%	38%

From the sample used to test H3, we took a random sample of 200 attorneys who exit the Martindale Hubbell data. 50 attorneys were born in NYC metro and exited in 2000, 50 were born in NYC metro and exited in 2002, 50 were born outside NYC metro and exited in 2000, and 50 were born outside NYC metro and exited in 2002. The destination rates for each of these four groups are reported above. Meaningful jobs include transitions to professorships (4 attorneys), primary/secondary school teaching (5), advocacy groups (3), and artistic occupations (2). We see from the table that attorneys born in NYC metro are the most likely to pursue meaningful jobs in 2002, relative to the other three groups. While this evidence is intended to be qualitative, the p value of the difference in difference for (NYC Metro Born*2002) is .15, which is marginally statistically significant.

[a]About 90% of these moves involve the founding of a new law firm.

Author Bios

Seth Carnahan (scarnaha@umich.edu) is an assistant professor in the strategy group at the Ross School of Business of the University of Michigan. He received his Ph.D. from the University of Maryland. His research examines labor markets, firm performance, and entrepreneurship.

David Kryscynski (dk@byu.edu) is an assistant professor of strategy at the Marriott School of Management of Brigham Young University. He received his Ph.D. from Emory University. His current research focuses on understanding the conditions under which human capital can lead to sustained competitive advantage.

Daniel M. Olson (dmolson@uw.edu) is an assistant professor of strategy and entrepreneurship at the University of Washington Foster School of Business. He received his Ph.D. from the University of Maryland. His research explores the impact of the interaction between competition and other social forces on individual- and firm-level decisions and outcomes.