ABSTRACT

Title of document:	Employee Departure from Organizations: Three Empirical Essays
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My dissertation includes three essays which focus on employee departure from organizations. In the first two essays, I study how employee departure allows individuals to capture more value from their firms. The third essay examines the effect of employee departure on firm performance. Each essay identifies an important theoretical puzzle related to employee departures and sheds light on its resolution through careful research design. In total, this dissertation aims to challenge the way that researchers typically think about employee departures, both theoretically and empirically.

The theoretical puzzle in the first essay relates to the connection between the failure of one firm and the birth of other firms. Does the failure of a rival firm cause the employees of existing firms to create entrepreneurial startups? On the one hand, theory suggests that the answer is yes – rival failures may release resources that potential entrepreneurs can use to start new firms. On the other hand, theory suggests that the answer is no – rival failures indicate to potential entrepreneurs that the environment is not munificent enough to support new entry. Using US Census data on the legal services industry, I disentangle these two arguments by examining law firm failures that are preceded by the unexpected deaths of highly paid attorneys. I find strong evidence that these quasi-random failures (which are likely to be weakly related to the broader economic environment) cause attorneys in rival firms to create startups, while other types

of rival failures depress entry rates, probably because they proxy for weakness in the local industry. This essay thus provides a theoretical rationale for when the failure of one firm will lead to the creation of another while demonstrating an often-discussed but rarely demonstrated positive side effect of firm failure – a failed firm provides the component parts for new organizations.

The puzzle in my second essay relates to the effect that one employee's departure has on the bargaining power and monetary earnings of his or her colleagues. This issue is likely particularly salient for members of underrepresented groups within an organization, such as women in the legal services industry, which hosts this study. Theory might dictate that the departure of a highly paid woman would hurt her female colleagues' earnings by reducing their bargaining power through the elimination of a mentor and advocate. However, an alternative mechanism suggests that the departure of a highly paid woman might provide her colleagues with increased bargaining power due to a relative scarcity of women in the organization.

Using unexpected death as a stand-in for departure, I find that women experience an 8% average *increase* in earnings after a female colleague passes away suddenly. This increase is significantly larger than what women experience when the deceased colleague is male, and it outpaces gains that male attorneys experience when a colleague of either gender passes away. Additional analyses suggest that the departure of a woman from a law firm may imbue her female colleagues with increased bargaining power related to client acquisition or the firm's interest in maintaining gender diversity. This essay points out a paradox related to the bargaining power of underrepresented groups. While much of the organizations' literature suggests that a group's overall bargaining power will

increase as a function of the group's size, I find that an *individual's* bargaining power increases as the size of her overall group shrinks.

The third essay extends the literature connecting employee turnover and organizational performance by inserting the firm's manager into the causal system linking these two processes. The theoretical puzzle I address in this essay is whether managerial tenure weakens or exacerbates employee-level turnover's negative effect on organizational performance. Managers with longer tenure may have superior knowledge of their firm's routines and remaining stock of human resources, allowing them to respond effectively to key employee departure. However, theories related to organizational inertia suggest an opposite effect: managerial stability may reduce an organization's ability to respond to change. I analyze data from the National Football League to test this argument. Using injuries to quarterbacks as a quasi-random source of employee departure and casting the head coach as the team's top manager, I find support for the second argument. Analyses indicate that NFL teams whose coaches have longer tenure perform worse following quarterback injury. The contribution of this essay is to show that stability at one level of the organization can exacerbate turmoil at another level, potentially due to the ossification of the organization's ability to alter its routines.

EMPLOYEE DEPARTURE FROM ORGANIZATIONS: THREE EMPIRICAL ESSAYS

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2013

Advisory Committee:

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Acknowledgements

Rajshree Agarwal has held my hand, kicked my butt, spiked a volleyball in my face, convinced me to move back to Champaign-Urbana as a twenty-three year old man, and otherwise given me every single advantage a grad student could hope for from an adviser. Many would say I have had it too easy. When I was evaluating PhD programs, I tried to get an idea of what it would be like to work with potential advisers by reading the acknowledgements to their student's dissertations. If anyone in a similar situation is reading this in 2013 or 2050 – heed these words: work with Rajshree. She will match you effort-for-effort, she will care about you the person as much as you the student, and there will be no limitations on what you can achieve.

If Rajshree is my Yoda, then Ben Campbell is my Obi Wan Kenobi. Ben started working with me when I was so raw that I used "data" as a singular noun (Mahoney, 2008), and he taught me the nuts and bolts of research and how our career works. If you read a collection of Ben's emails to me over the years, you will see the source material for this dissertation and a survival guide to grad school. I could not ask for a better mentor, coauthor, and friend.

I owe an incredible amount of thanks to my dissertation committee. No one asks better questions than Cristian Dezsö, and he generously applied that talent to this project, reading each of my drafts, attending all of my talks, and giving me outstanding, unvarnished advice at every turn. Waverly Ding gave me a similarly generous investment and her influence is the primary reason that my work is starting to head down a more sociology-oriented road. David Sicilia is an extremely thoughtful scholar, and the best signal that I have of the quality of this document is that David passed me despite the fact that I did not fulfill his pre-defense request for chocolate donuts. It is not hyperbole

to say that Dave Waguespack's class on research design completely changed my approach to research, and it happened at the crucial time when I was casting around for dissertation ideas. I will not write another paper without first asking myself, "What is the ideal experiment?"

Each of the other faculty at UMD provided insight or support at some point during my time here. In particular, Wilbur Chung, Bennet Zelner, and Maggie Zhou gave me excellent job market advice, and Gilad Chen, Brent Goldfarb, David Kirsch, and Subra Tangirala gave me crucial tips on my job market presentation.

The faculty at Illinois left an imprint on me that will not rub off anytime soon. Joe Mahoney is a scholar in the truest sense, and his memory intimidates me in the same way that Rajshree's energy does – watching him quote papers verbatim from memory while including page numbers is a formative moment for any student who passes through Illinois. Glenn Hoetker had the same impact on my knowledge of empirical methods that Dave Waguespack had on my perspective on research design. Deepak Somaya has been a terrific coauthor and mentor who, like Ben Campbell, gives me a model for writing papers about people while still being a strategy researcher. Brooke Elliott, Andrew Isserman, Steve Michael, and Mark Peecher each encouraged me to try academia, and I would not be here without their nudges.

My lasting memory of U of Maryland will be discussions of identification and other problems with some combination of Dave Anderson, Justina Blanco, Beth Campbell-Bush, Justin "Frosted" Frake, Shweta Gaonkar, Brad "Are We Not Doing This?" Greenwood, Shannon Lantzy, Daniel Malter, Mahka Moeen, Dan Olson, MaryJane Rabier, Bryan Stroube, Robert Vesco, and Annie Zavyalova. Many of these

conversations made my dissertation much, much better than it would have been otherwise, and they certainly kept me happy and sane over the last three years. Greenwood deserves special thanks for organizing it all and teaching me a number of lessons. MaryJane deserves special thanks for showing me why business cards are important. Mahka deserves special thanks for never letting anything slide.

Because I did my coursework there, my lasting memory of grad school at Illinois will be incomprehensible matrices, but Erik Bushey, Greg Fisher, Martin Ganco, Chih Lui, John Mawdsley, and especially Marleen "Not Baumol" McCormick made my second go-round in Champoo-Banana completely worth it. Atsushi Ohyama gave me crucial empirical tips on the first essay and is an econometrics wizard.

While I have climbed up the shoulders of every student who preceded me at UIUC and UMD, Martin Ganco and Daniel Malter deserve special thanks for providing me with a template for success and clearing a path that made my job search much easier.

I owe a special debt - at least one year of school - to Frank Limehouse and Arnie Reznek at the Census Bureau for bending over backwards to give me more time with the data used the first two essays. The Kauffman Foundation and the Strategy Research Foundation also provided critical funding for this project.

Turning to my debts outside of school, Nate VanRaden single-handedly kept me going in my first year, and being able to sleep on the couch owned by Nate, Adam Bialon, and Pat Boland in my second year gave me the motivation I needed to make trips to Chicago to use Census Data. The roadtrips I took with Nate and Nick Poggioli gave me something to work towards and will undoubtedly stand among the best memories of

my life. An open question in the literature is whether I will get tenure before Radha Gajjar finishes watching *The Wire*.

My family deserves the biggest thanks of all. My sister Meredy is my best friend, so much so that I married her this month. To Paul Dahlgren (Greenwood, 2013). The fact that someone so talented and wonderful thinks that I am talented and wonderful gives me the confidence to crush anything in my path. My parents, Patty and KC, created a home where two things are inexhaustible – unconditional love and the motivation to get things done. On top of that, they have struck a perfect balance: Meredy and I know that we are the most important thing in the world to them, but they let us live our lives and have never treated us like conduits for their own existence. I am probably the only PhD student who can say that his favorite job talk took place in his family's living room and lasted for three hours. What a reader can learn from the essays that follow these acknowledgements is debatable, but at the very least you now know how much my family means to me and how proud I am to be their son and brother.

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INTRODUCTION

My dissertation includes three essays which focus on employee departure from organizations. Employee departure is a significant phenomenon - voluntary turnover rates averaged between 9.8% and 12.5% between 2008 and 2012 in the United States, with higher skilled industries like banking and finance and legal services at the upper end of the distribution (CompData, 2012). Consequently, departures attract the attention of a wide range of researchers, including those in management, economics, sociology, and psychology. My dissertation draws on work in each of these areas, combining insights from each to identify and disentangle novel causal processes and mechanisms related to the departure of employees from organizations.

Because employee departure is a broad-based phenomenon, I focus my contribution by examining two of its implications across the three essays of this dissertation. In the first two essays, I study how employee departure allows individuals to appropriate more value. Departure has long been cast as an important value appropriation mechanism, as the threat of mobility allows individuals to capture more value from their current firm (Coff, 1999), and individual earnings often increase following a change in organizations (Tope1, 1992). I depart from this existing work in the first essay by examining departures where employees start new firms, framing entrepreneurship as a value appropriation mechanism which allows individuals to exploit valuable resources without having to share the spoils with an organization. In the second essay, I provide a new perspective on the connection between departure and value appropriation by examining how the earnings of *colleagues* change when a highly paid person exits the firm. I argue that the departure of a highly paid individual releases contestable organizational resources while shifting the bargaining coalitions of the firm, creating unique opportunities for individuals to capture value.

The third essay examines the effect of employee departure on firm performance. While a growing literature examines the connection between these two constructs, it focuses entirely on the employee group at risk of departure while abstracting away from the multi-level nature of most organizations, where employees are often supervised by managers. This essay takes a multi-level approach to the connection between employee departure and firm performance by examining whether stability at the managerial level reduces or exacerbates the negative effect on firm performance that stems from the departure of a key employee.

A major challenge in the empirical study of employee departures is the identification of causal effects. An employee's decision to exit an organization is complex and likely to be interrelated with the processes determining individual earnings, firm performance, or any host of other constructs that a researcher may try to correlate with employee departure. Consequently, scholars must grapple with a host of identification problems related to omitted variable bias, simultaneity, and reverse causality. Measurement error can also be an issue, as many studies of employee turnover rely on single-informant surveys which aggregate firm-level turnover rates over extended periods of time. Complicating matters is the fact that laboratory experiments are difficult to conceive for this phenomenon, because artificially designed "organizations" from which employees may depart are unlikely to have strong external validity.

My dissertation attends to these methodological issues while providing a rich set of theoretical contributions that challenge the way that researchers think about employee departure from organizations. Each essay identifies a theoretical puzzle or tension in the existing literature and sheds light on its resolution through careful research design. As a consequence, this dissertation tries to meet the often competing demands of theoretical richness and empirical

clarity. I next describe the theoretical puzzle and research design present in each of the three essays.

The theoretical puzzle in the first essay relates to the connection between the failure of one firm and the birth of other firms. Prior work has examined this question from the industry level of analysis. An important issue with examining the connection between failure and entry at the industry level is that the analyst cannot determine *who* is starting new firms – "new" firms might simply smaller pieces of failed firms helmed by the failed firm's employees. I solve this issue by focusing on the employees of firms that do not fail and casting the research question as one of employee departure – does the failure of a rival firm cause employees in healthy firms to quit their jobs in order to start new organizations?

The puzzle inherent in this question comes from the two competing effects that a rival firm's failure will likely have on a potential entrepreneur in an existing organization. On the one hand, the rival failure releases resources like customers, land, employees, intellectual property, and so on, into the marketplace, where they can likely be obtained for a lower price than when the rival firm continued to operate (Hiatt, Sine & Tolbert, 2009). The availability of these resources may increase the attractiveness of new firm creation for employees of existing organizations. On the other hand, the failure of a rival firm may also send a negative signal to potential entrepreneurs elsewhere in the industry (Barnett & Amburgey, 1990). A rival firm's failure may indicate that the current industry environment is not munificent enough for a new firm, thus reducing the probability that employees of existing firms will depart their jobs to start new organizations.

I resolve this theoretical tension empirically by identifying a class of firm failures that should be less related to the broader economic environment than others – failures preceded by

the unexpected death of one of the firm's highly paid members. These types of failures arguably provide a weaker signal about environmental munificence to potential entrepreneurs in other firms, since the unexpected death of the highly paid person should be the primary causal factor behind the firm's dissolution, and it is arguably exogenous to the broader economic environment. Analyzing confidential employer-employee linked microdata on the legal services industry from the US Census Bureau, I find that rival firm failures that are preceded by the death of a highly paid attorney increase new firm creation by employees working for existing firms. Here, the clients and attorneys that are released by the rival firm's unexpected dissolution likely create opportunities for employees of other firms to depart their jobs and start new organizations. Conversely, when a rival's failure is not preceded by an attorney's unexpected death, I find that employees of existing firms are less likely to start new organizations. In these cases, the failure of the rival firm may proxy for weakness in the economic environment, sending a signal to potential entrepreneurs working for other organizations that conditions are not appropriate for new firm creation.

In addition to resolving a long standing theoretical and empirical tension in the literature related to the causal relationship between firm exit and firm creation, this essay connects the large literature concerned with the effect of the external environment on employee departure with the literature examining employee entrepreneurship. March and Simon (1958: 53) first discuss of the importance of the "availability of external alternatives" in determining whether workers will quit their jobs. Prior papers in this tradition would likely predict that rival failures would reduce the probability of employee departure, since fewer external alternatives in the form of jobs with existing firms would likely be available following a rival's failure (Haveman & Cohen, 1994). This paper highlights the difference between entrepreneurial departures and other

departures by showing how the destruction of jobs with existing firms can lead to the creation of jobs with new firms via the release of resources from failed firms and their recombination with resources (particularly employees) of existing organizations.

This insight has particularly important implications for firm strategy. Whereas a strict market power (Porter, 1980) view of strategy would likely view the dissolution of a competitor as a positive outcome, here we see that a competitor's failure has an important side effect – it releases resources that may incentivize an existing firm's employees to exit the organization to start a new competitor. Prior work suggests that these new ventures may be particularly harmful to their parent firms (Phillips, 2002), especially in a professional services context like legal services (Campbell et al., 2012), where competition for clients is likely to be relatively easy for startups as compared to new firms in other industries, particularly those industries with large economies of scale.

The puzzle in my second essay relates to the effect that one employee's departure has on the monetary earnings of his or her colleagues. This issue may be particularly salient for members of underrepresented groups within an organization. If a highly paid member of an underrepresented group, such as a female executive, departs the organization, the consequences for the women she leaves behind are unclear. On the one hand, organizational mentors are crucial inputs for career success (Briscoe & Kellogg, 2011), and to the extent that homophily drives the sorting of mentoring relationships (McPherson, Smith-Lovin & Cook, 2001), the departure of an important ally could weaken the remaining individual's position in the firm. On the other hand, the departure of a similar colleague may cause the bargaining power of remaining individuals to increase due to a sudden scarcity of skills that may be unique to that group (Burt, 1997), allowing them to capture more value.

I examine this puzzle through the lens of gender earnings inequality in American legal services firms. I use a novel empirical design to distinguish between these two theoretical predictions. Using the same confidential and comprehensive employer-employee matched legal services microdata as in the first essay, I analyze how attorneys' earnings change after a highly paid colleague dies unexpectedly. While the process that determines traditional turnover is likely highly interrelated with the process that determines colleague earnings, unexpected deaths are arguably randomly assigned (e.g. Azoulay, Graff Zivin, and Wang, 2010). Matching firms that experience a death to observably equivalent firms that do not, our quasi-experimental set up allows us to make relatively credible causal inferences about the impact of highly paid attorney exit on the earnings of colleagues who remain with the organization.

The results are striking. Following a highly paid colleague's death, the average earnings of other members of the firm *increase* about 3% relative to the control group. This increase may occur because surviving members of the firm inherit the client work that was formerly completed by the deceased attorney. The deeper insight comes when we examine how the gender of the deceased and the gender of the surviving attorney moderate this result. Men experience a relatively small increase in earnings when a colleague passes away, and this increase is not dependent on whether the deceased person is male or female. Women, on the other hand, experience a large (8%) increase in earnings after a female colleague passes away; this increase is significantly larger than what women experience when the deceased colleague is male. This suggests that women are somehow uniquely positioned to benefit when one of their female colleagues exits the firm.

Examining moderators of this key relationship in order to pin down the mechanisms at play, we find that the result is stronger when the surviving female attorney is higher in the firm's

earnings distribution at the time of her colleague's death, older then her deceased female colleague, or higher in the firm's earning distribution than her deceased female colleague. These results suggest that highly paid women benefit the most from the death of their highly paid female colleagues. Although I cannot observe client relationships in the data, it is plausible that this outcome may be a result of gender homophily (Beckman & Phillips, 2005; McPherson et al., 2001), where clients of the deceased woman are more likely to migrate to her female coworkers. It may also suggest that an increase in bargaining power following a highly paid woman's death flows most directly to other highly paid women whose visibility makes them important for the firm's maintenance of legitimacy in a marketplace that may have preferences for gender diversity (DiMaggio & Powell, 1983).

The primary contribution of this essay is to point out a paradox related to the bargaining power of underrepresented groups in organizations. While much of the organizations' literature suggests that a group's overall bargaining power will increase as a function of the group's size (Coff, 1999; Blau & Murningham, 1998), I find that an *individual's* bargaining power increases as her overall group shrinks in size. This is consistent with Burt's (1997) notion that employees have increased bargaining power when their skills are scarce, and expands his idea to include demographic characteristics which may have external legitimacy as well as functional considerations.

This paper is also among the first to view an employee's exit from the firm through the lens of his or her remaining colleagues. Other literature in this vein mostly examines how one employee's departure might spur the departure of others (Ballinger et al., 2010; Felps et al., 2009), but this essay highlights the release of resources that accompanies a highly paid person's

exit from the firm and illuminates how demographic characteristics may leave some remaining employees better positioned to capture these resources than others.

The third essay connects employee departures to organizational performance. To this point, most work has focused on studying and quantifying the disruptive effects of turnover. In this paper, I extend this line of inquiry by envisioning the effect of departures on organizational performance as a multi-level process. In particular, I examine how stability at the managerial level of the organization moderates the negative effect of turnover at the employee level. The exit of a key employee puts the onus on the organization's manager to craft a response that minimizes disruption and allows the organization to continue effective operations (March & Simon, 1958). Surprisingly, prior work abstracts away from the interaction between management and turnover.

The theoretical puzzle I address in this essay is whether managerial tenure weakens or exacerbates the negative effect on organizational performance of turnover at the employee level. Some theories of organizational routines in the tradition of Nelson & Winter (1982) suggest that longer tenured managers should be well-positioned to shield their organizations from the negative effects of the departure of important lower-level employees. Managers with longer tenure may have superior knowledge of their firm's routines and remaining stock of human resources, allowing them to respond effectively to key employee departure. However, theories related to organizational inertia (e.g. Hannan & Freeman, 1984; Leonard-Barton, 1992) suggest an opposite effect of routinization: stability can reduce an organization's ability to respond to change. When a manager has longer tenure, the organization's experience with change is rather limited, so its routines for dealing with change may ossify. As a result, when a key employee

departs, the organization may be unable to respond effectively to the shock, and performance may decline even more precipitously than if the manager had shorter tenure.

I implement a robust empirical design to test this argument. The endogenous connection between employee departure and organizational performance makes causal inference a unique challenge for empirical researchers. I overcome this issue by studying the performance of National Football League teams during the 2011 and 2012 regular seasons, using injuries to the team's most important player, the quarterback, as a quasi-random (Stuart, 2012) source of employee exit. I cast the head coach of each NFL team as the organization's top manager. In addition to the quasi-random nature of injuries, the depth of historical performance and career data available in this context allows me to include variables that are typically omitted in organizational performance regressions in other settings.

My results support the second argument – teams with longer tenured coaches perform *worse* following injury to the team's quarterback. This result is not driven by the overlap in tenure between the coach and quarterback, and is robust to controls for observable managerial quality. It appears that well-established routines may buckle after the shock of an employee's departure.

The primary contribution of this essay lies in its consideration of employee departure as a multi-level organizational phenomenon. It is the first to consider how stability at one level of the organization can compensate for or exacerbate turmoil at a different level of the organization. This insight is important because it connects the literature on organizational routines to the literature on employee turnover while focusing on organizational performance, thus drawing together conversations between scholars interested in strategy, organizational theory, and human resource management.

To strategy scholars, this essay provides micro-level insights to upper echelon theory. Stability at the top of the firm can become a performance liability when the organization experiences sudden change further down in the organization's hierarchy. For organization theorists, the essay suggests that well-established routines can crumble quickly when one of their main exponents exits the organization. Finally, for human resource management scholars, I provide the insight that the causal chain connecting employee turnover to organizational performance runs through the managers that remain with the firm after employees depart. Each of these ideas has to potential to stimulate follow-on work.

Through three essays, this dissertation connects employee departures to individual value capture and organizational performance. In each essay, I identify an important theoretical puzzle related to one of these topics. I shed light on each of these puzzles by building multi-disciplinary theory and testing its arguments with careful research design. The results have important implications for individual careers, firm strategy and performance, and economic growth.

NEW FIRM CREATION FOLLOWING FAILURE OF RIVAL ORGANIZATIONS

Although a long line of organizational scholars (e.g. Tushman & Anderson, 1986; Henderson & Clark, 1990) have examined creative destruction (Schumpeter, 1942) of incumbent organizations by entrepreneurial entrants, the reverse of this process – the causal effect of incumbent failure on entrepreneurship remains largely unexamined. Are the employees of existing firms more likely to create startups following the demise of rival organizations? This question has important implications for individual careers, firm strategy, and structure of markets. An affirmative answer implies that firm failure may create positive spillovers by spurring other individuals in the industry to start new firms, reallocating resources and potentially stimulating long-run economic growth. In addition, while failure of rivals is generally considered a positive strategic outcome, existing firms may experience negative side effects if the failure spurs their own employees to create competing startups as a result.

The causal effect of rival failure on new venture creation by employees of existing firms is not clear ex ante. For the focal employee contemplating exit from her current job to create a startup, the failure of a rival firm would likely have two competing effects. Rival firm failure may be a signal that the current economic environment is not conducive to successful firm creation and survival (Aldrich, 1990). Alternatively, rival firm failure releases numerous resources, including intellectual property (Hoetker & Agarwal, 2007), customers (Hiatt, Sine & Tolbert, 2009), physical assets (Fortune & Mitchell, 2011), and employees (Haveman & Cohen, 1994; Rider, 2012a).

Scholars considering the relationship between incumbent failure and entrepreneurial entry have not resolved this puzzle. Organizational ecology and evolutionary economics scholars use the population or industry level of analysis and find negative correlations (Barnett & Amburgey, 1990; Land, Davis & Blau, 1994; Halliday, Powell & Granfors, 1987), positive correlations (Delacroix & Carroll, 1983; Dunne, Roberts & Samuelson, 1989; Hiatt et al., 2009; Pe'er & Vertinsky, 2008), and insignificant correlations (Agarwal & Gort, 1996) between the failure rate of incumbents and the entry rate in a variety of industries. However, researchers examining organizational genealogy (e.g. Phillips, 2002) and employee entrepreneurship (e.g. Agarwal et al., 2004) emphasize that the decision to start a new firm is a multi-level phenomenon, heavily dependent on the attributes of the individual (e.g. Elfenbein, Hamilton & Zenger, 2010) as well as those of the employer (e.g. Sørenson, 2007) in which she is currently embedded.[1]

Integrating population level insights from organizational ecology and evolutionary economics with micro-level mechanisms highlighted by organizational genealogists, I theorize that rival firm failure represents an important resource release that employees of existing firms contemplating new venture creation may capitalize upon. I further suggest that this resource release may be a more important mechanism for entrepreneurial entry for employees with higher earnings and employees who may be under-rewarded at their current organizations.

Empirically, I solve the puzzle highlighted above by leveraging a design that allows me to disentangle the release of resources that accompany rival failure from the negative signal that failure sends about the business environment. Examining confidential employer-employee

[1] As Carroll and Khessina (2005: 2) point out in their review of ecological explanations for entrepreneurship, "the types of theoretical explanations offered for population-level processes may not be directly applicable to individuals." See also Aldrich (1990) for a similar critique. Haveman and Cohen's (1994) study of ecological events and individual mobility in the savings and loan industry also calls for a deeper examination of how population-level processes affect individuals.

linked US Census microdata on the legal services industry obtained from the Longitudinal Employer-Household Dynamics program (LEHD), I use the unexpected deaths of partner attorneys as an instrument for the failure of the firms that employ them. Without instrumentation, I find a negative correlation between the failure of rivals and the probability that an employee of an existing firm will leave her job for entrepreneurship. However this correlation is likely plagued by a number of unobserved variables, such as a decline in the business environment. Using the instrument to set aside these unobservables and examine the causal effect, I find the exact opposite result: rival failure has a strong positive effect on entrepreneurship by existing firm employees, suggesting that the release of resources following firm failure may enable the creation of startups by other individuals in the industry. I predict and find that this relationship is stronger for higher wage attorneys who are better positioned to capture resources and weaker for attorneys working for firms with stronger pecuniary incentives. The rewards provided by these firms may obviate the need capture value from released resources via entrepreneurship (Sørenson & Sharkey, 2010). Further unpacking the mechanism behind this result through supplementary analyses and qualitative interviews, it appears that the availability of startup cofounders is primarily responsible for the increase in startup propensity following rival dissolutions.

THEORY

Conflicting Results on the Relationship between Failure and Entry

Prior scholars examining the connection between incumbent failure and entrepreneurial entry operate primarily at the population level of analysis and find conflicting results. Using either the lens of organizational ecology or evolutionary economics, some scholars predict and find positive correlations between incumbent failure and entry, and others predict and find

negative correlations between incumbent failure and entry. Ecology researchers arguing that the relationship should be positive focus on the release of relatively immobile resources such as physical capital, customers, and employees (e.g. Delacroix & Carroll, 1983; Hiatt et al., 2009; Pe'er & Vertinsky, 2008) that potential entrepreneurs can seize upon to create new firms. Evolutionary economists who find a positive relationship emphasize the recursive relationship between failure and entry: since entry increases competition in the industry, it may hasten the exit of incumbent firms (Dunne et al., 1988), resulting in a positive correlation between exit and entry rates. Scholars finding a negative relationship focus on the negative signal that failures send to potential entrepreneurs about the quality of the business environment (Barnett & Amburgey, 1990; Land et al., 1994; Holliday et al., 1987).

Operating at the population level of analysis makes it difficult for researchers to disentangle the mechanisms underlying the opposing theoretical claims. Two problems arise in testing theory that predicts a positive relationship between the failure rate and the entry rate. First, as evolutionary economists point out, rather than incumbent failure stimulating entry, entry may be causing incumbent failure by increasing competition in the industry. Second, authors cannot distinguish *who* is starting the firms that are formed in the wake of failure. The theory of resource release seems to apply to agents who are not former employees of the failed organizations, but it is quite possible that recently unemployed individuals may be starting many of new firms observed in the wake of failure because they have few other employment options (Hachner & Granrose, 1985). Consequently, incumbent failure may not cause entrepreneurial entry per se, but rather it may result in a proliferation of new firms that are merely modular pieces of recently failed organizations. Finally, arguments for a negative relationship between incumbent failure and entrepreneurial entry rely not as much on failures themselves causing

lower entry, but on failures proxying for general weakness in the economic environment which suppresses entry by potential entrepreneurs (Barnett & Amburgey, 1990).

Entrepreneurship by Employees of Existing Organizations

The resolution of these issues may be possible by shifting focus from the population to the individual level of analysis and examining the causal effect of rival failure on entrepreneurial entry by employees of existing firms. Employees of existing firms may be interested in founding new startups to capture higher pecuniary returns (Campbell, 2012) or to gain the nonpecuniary benefits that come with increased autonomy (Benz & Frey, 2008). However, they likely face significant resource constraints that may prevent them from creating new firms (Wasserman, 2012).

As Delacroix and Carroll (1983) and others note, a firm's failure releases resources such as intellectual property (Hoetker & Agarwal, 2007), customers (Hiatt et al., 2009), and physical assets (Fortune & Mitchell, 2011). In addition, the failed firm's former employees are released as well (Rider, 2012a) and may be more willing to co-found or join (Roach & Sauermann, 2011) startups given their reduced opportunity costs (Haveman & Cohen, 1994) or disenchantment with traditional employment (Hachner & Granrose, 1985). The availability of these resources may enable entrepreneurship by employees of existing organizations. In fact, an important means for potential entrepreneurs to access the resources of a failed firm may be to join with the failed organization's former employees through new venture formation. Because many of a firm's resources, such as knowledge (Song, Almeida & Wu, 2003), buyer-supplier relationships (Somaya, Williamson & Lorinkova, 2008), other interorganizational ties (Rider, 2012b), and routines (Phillips, 2002; Wezel, Cattani & Pennings, 2006), adhere to its employees, partnering

with a former employee of a failed organization may be an effective means for a potential entrepreneur to gain access to a failed rival's resources[2].

Thus, while failures among rivals may proxy for weakness in the external environment that may deter entrepreneurial entry by employees of existing firms[3], the causal effect of failure on entrepreneurial entry is likely related to the release of demand side resources (e.g. customer relationships) and supply side resources (e.g. employees) that stimulate entrepreneurial entry by employees of existing firms. As a result, the dissolution of a competitor firm may increase the ability of employees of existing organizations to quit their jobs to create startups.

Hypothesis 1: The failure of a competitor firm increases the probability that an employee of an existing firm exits her job to found a startup firm.

Moderators of the Relationship between Rival Failure and Entrepreneurship

While the previous section expands the prior literature examining the connection between failure and entry by moving the dependent variable (entrepreneurial entry) to the individual level of analysis and focusing on employees of existing firms, the proposed mechanism of resource release is still a fundamentally population-level concept. In this section, I introduce individual- and firm-level logic from the sociology-oriented literature in organizational genealogy (e.g. Burton, Sørenson & Beckman, 2002; Phillips, 2002) and the economics- and strategy-oriented literature in employee entrepreneurship (e.g. Agarwal et al., 2004; Elfenbein et al., 2010) to further extend the foregoing theory about the connection between firm failure and firm creation.

[2] In the empirical section, I address whether or not existing firm employees are founding or joining the startup firms with which they are affiliated. I excise startups that are predominantly composed of employees of failed firms. In firms that are founded by relatively equal numbers of employees of existing and failed firms, empirical analyses suggest that employees of existing and failed firms cofound startups on an equal basis.

[3] I describe my instrumental variable strategy for dealing with this omitted variable problem in the methods section of the paper.

At least two important individual- and firm-level questions remain unanswered by the previous analysis, and examining them will help unpack the mechanisms underlying the first hypothesis. First, are higher or lower quality employees likely to start new firms in the wake of competitor failure? Second, what role does the firm's incentive structure play in determining whether its employees will leave for startups following the failure of a rival organization? The first question is important to answer because the quality of the individual agents reallocating the released resources will in turn have important implications for subsequent population-level outcomes. For example, if lower quality employees create startups following competitor failure, the strategic and economic growth implications may be relatively minimal; these individuals may be merely joining startups that would have been created anyway, or they may be utilizing the failed firm's lower quality resources to form startups with dim growth prospects. The answer to the second question relates to how firm-level incentives affect individual-level choices to exploit released resources inside of new firms, the outcomes of which are eventually expressed in shifts in the distribution of startup organizations at the population-level. Specifically, to the extent that employees of existing firms venture outside the organization to appropriate returns from exploitation of released resources in startups, provision of adequate incentives within existing firms may cause them to exploit these resources within the existing firm itself, rather than forming a new venture[4].

Individual Compensation

Recent work suggests that employees in the upper tail of the ability distribution, whether measured by performance (Groysberg et al., 2009) or compensation (Campbell et al., 2012; Elfenbein et al., 2010), are more likely to enter entrepreneurship. Higher wage employees may

[4] A caution is worth noting before proceeding. While Hypotheses 1 builds logic supporting a causal relationship, I lack exogenous instruments for the moderators discussed below. Therefore, the following section should be interpreted as an exploration into the mechanisms underlying causal connection between competitor failure and entrepreneurship.

be better positioned to start new firms due not only to the superior skills reflected in their wages, but also due to their superior ability to attract complementary resources such as employees (Alvarez & Barney, 2005) and financial investment (Hallen, 2008).

As a result, higher wage employees should be well-positioned to obtain the resources that are released following the demise of a rival firm. For example, higher wage individuals likely possess better external reputations that facilitate the attraction of client and supplier relationships dislodged by the rival's failure. In addition, the skills reflected in their wages make them more attractive cofounders and collaborators for the former employees of dissolved firms. Consequently, if resource release and attraction are indeed key mechanisms underlying the connection between rival firm dissolution and entrepreneurial entry, employees with higher wages should be more likely to create startups following the demise of a competitor.

> *Hypothesis 2: The positive relationship between the failure of a competitor firm and the probability that an employee leaves her job with an existing firm to found a startup is stronger for employees with higher compensation.*

Pecuniary Opportunities within the Current Employer

The prior section argues that failure of competitor firms releases resources that may be utilized by aspiring entrepreneurs to create startup organizations. However, these resources may just as well be exploited internally by employees of existing firms, rather than these individuals choosing the more uncertain alternative of a new venture. Prior theoretical work in the economics of entrepreneurship suggests that the financial incentives provided by the employer have a strong bearing on whether or not employees choose to exploit internally recognized opportunities within the current employer, "push" them out of the firm into a new venture, or simply let them lie fallow (Anton & Yao, 1995; Hellmann, 2007; Pakes & Nitzan, 1983).

Analogously, the employer's pecuniary opportunities may also help determine whether employees react to the release of rival resources by starting a new firm, "pulling" the released resources into the existing firm, or doing nothing at all. If the employer provides significant opportunities for pecuniary gain, the employee may be more likely to pull the resources into the existing firm since she may be able to personally profit from their internal utilization. She may also choose to do nothing with the resources, since the significant financial gains available within the firm may mean that current internal opportunities are sufficient for the employee to earn an adequate return to her talents. On the other hand, if the employer provides weak pecuniary incentives, the employee may choose to create a startup in order to maximize rent capture following the dissolution of a rival firm.

The highest level of compensation within the firm, also called the firm's wage ceiling, is an important indicator of the pecuniary opportunities offered by the organization (Baron & Bielby, 1980; Stewman & Konda, 1983). The wage ceiling provides a proxy for the highest level of financial attainment to which employees can aspire while working for the firm. As a firm's wage ceiling increases, its employees may have less need to enter entrepreneurship in order to earn larger pecuniary returns because significant returns can likely be earned within the organization (Sørenson & Sharkey, 2010). Consequently, employees of firms with high wage ceilings may have less motivation to create a startup firm in order to exploit the resources that are released following the failure of competitor organizations.

> *Hypothesis 3: The positive relationship between the failure of a competitor firm and the probability that an employee leaves her job in an existing firm to found a startup is weaker for employees working for firms with higher wage ceilings.*

DATA

US Census Legal Services Microdata

I examine these hypotheses in the legal services industry. This setting is ideal for several reasons. First, the professional services sector, which includes legal, financial, management consulting, education, and health care, constitutes an increasingly important portion of the US economy; in 2007, services constituted 68 percent of US GDP, with professional services comprising about half of this activity (Bureau of Economic Analysis, 2008). Furthermore, human assets represent the most important complementary assets for firm creation in professional services, especially in comparison to manufacturing where physical assets are relatively more important (Teece, 2003). Since the data, which I describe next, allow me to observe all individuals working in the industry, I am able to track the mobility of each failed firm's most important assets, which is critically important for untangling the mechanisms underpinning my analyses.

I obtain data from a custom extract of the Longitudinal Employer-Household Dynamics (LEHD) Project available at US Census Research Data Centers. The data are built from state-level unemployment insurance records and several data products from different government agencies. Each quarter, firms that contribute to their state's unemployment insurance fund must list all employees covered by the state unemployment insurance program along with their taxable earnings (including partnership distributions) and some firm data. Combining these mandatory records with other Census and government data products, the LEHD program constructs data files containing rich individual level information as well stable individual and firm identifiers. The latter aspect allows me to track individuals across firms and identify firm starts and exits.

The data identify all individuals employed in U.S. legal services in the following states: California, Florida, Illinois, New Jersey, North Carolina, Oregon, Pennsylvania, Texas, Virginia, and Wisconsin. The earliest states entered the LEHD program in 1990, and others entered in subsequent years up to 1994. The final year for all states is 2004.

Sample construction

I want my sample to consist of attorneys working for firms that do not fail, i.e. existing firms. To ensure I focus on attorneys (the data contain all employees of law firms but do not note an individual's occupation), I use the rich individual wage and age information. First, I eliminate all individuals who make less than $25,000 to scrub weak attachments to the labor market. Research indicates that law firms typically employ one associate per partner and one support staff per associate (Parkin and Baker, 2006), so I next eliminate the remaining employees in the bottom 33 percent of each firm's wage distribution. I finally require that each individual must make $50,000 in at least one year of the data. The 2004 survey by the National Association of Legal Professionals (NALP) indicates that the median associate salary was $66,000 in that year (I inflate wages to the 2004 level); while NALP does not provide a standard deviation for this figure, the $50,000 minimum, combined with the wage percentile restriction, appears to be a reasonable cut off for capturing relatively lower paid associates. At the firm level, to ensure I focus on existing firms, I limit my sample to firms which have been in the data for at least three years[5], have at least five attorneys and do not exit the data within the next two years. It is important to emphasize that while failed firms are included in my dataset and are used to construct several of the variables that I describe below, their employees are excluded from my

[5] The first restriction (age at least three years) is necessary due to the manner in which I define the firm's competitors. More information is below.

estimation sample - I do not examine their probability of entering entrepreneurship. The results are based on employees of existing firms.

METHODOLOGY AND VARIABLES

To ideally test the above predictions, a researcher would randomly select firms to fail and then observe the entrepreneurship decisions of the employees of the failed firms' competitors. Such an experiment, however, is neither feasible nor socially desirable. In the following sections I describe my approach to coming as close as possible to this experimental ideal.

My unit of analysis is the individual-year. My primary specification is a linear probability model with individual-firm fixed effects. The individual-firm fixed effect controls for time-invariant unobserved heterogeneity at the level of the employee-employer match as well as at the level of the employee and the firm for which the employee works. As a result, I identify how a change in the competitor failure rate during an employee's tenure *with a given firm* affects her decision to quit that organization and start a new firm[6]. This specification also allows for instrumental variable estimation, which is my primary approach for dealing with omitted variables that might bias my results away from the experimental ideal described above. In the robustness section, I present results using a multinomial logit model.

Dependent Variables

While the main idea of the paper relates to an employee's decision to start a new firm, this decision could be related to the more general choice to change firms. In the models that follow, I thus examine the probability that an individual starts a new firm as well as the probability that she moves to an established firm to ensure that rival failures do not merely cause

[6] Note that the individual-firm fixed effect is more restrictive than the individual fixed effect, which would only control for time invariant differences in individuals (such as gender, education, and race). Controlling for time invariant differences in individual-firm matches is important to the extent that individuals face different internal incentives and different competitive environments when working for different firms.

mobility generally, but rather specifically drive employees of existing firms to create startups. These two types of exit are mutually exclusive for any given person-year observation. I examine exit at t+1 in order to ensure a causal ordering of my independent and dependent variables; in essence, all of the independent variables in my model are lagged by one year.

Founding a startup (Hypothesis 1): For the dependent variable in Hypothesis 1, a dummy variable takes a value of one if an individual's dominant employer changes from the previous year and her new employer appears in the data for the first time in that year. To ensure that I am capturing significant startup firms, each must employ at least three attorneys (only about half of startups in the data meet this criteria)[7]. The results presented below are similar, but smaller in magnitude, when I include smaller startup firms.

Campbell (2005: 146) notes that administrative recodes, mergers of existing firms, or out-of-state firms opening their first branches in a state may "look" like entrepreneurship events to researchers using administrative identifiers to measure new ventures. I use flows of workers to eliminate these Type I errors (Benedetto, Haltiwanger, Lane & McKinney, 2007). Specifically, I identify administrative recodes when 80 percent or more of a new firm's employees come from the same previous firm (or parent firm) and the parent firm exits the data in the current or following year.[8] I identify merged entities as those where 80 percent or more of a new firm's employees come from the same N entities and each of those N entities exit the data in the current or following year. Finally, I identify out-of-state branches as firms where, in the year of the firm's birth, more than 80 percent of the employees appear in the data for the first time. After

[7] Sole proprietorships are not included in the LEHD data, so my measure of entrepreneurship is conservative in that I do not capture attorneys who are "hanging out a shingle."

[8] This restriction is particularly important for my context, as I want to ensure that I am capturing startup founding instead of mere mobility to a firm that is a modular piece of a pre-existing law firm that broke apart following the death of a partner attorney.

these rules are applied, none of the startups are larger than 50 employees in size, giving comfort that mergers, recodes, and other Type I errors have been eliminated.

Movement to a different established firm: This dummy variable takes the value of one if an individual's dominant employer changes from the previous year and her new employer is a firm that already existed in the data.

Explanatory variable

Deaths of Rival Firms: Before computing this variable, I need to define who is a "rival firm." While the LEHD data do not contain information on law firm specialties, it nevertheless has rich employee mobility data that enables me to define a firm's competitors. To the extent that attorneys primarily move among a firm's competitors, a reasonable assumption given highly specialized investments in human capital (Gnilson & Mookin, 1985), mobility patterns reveal the focal firm's rivals. Accordingly, I define a firm's competitors as those firms (1) which employ at least 5 attorneys, (2) reside in the same Metropolitan Statistical Area and state as the focal firm, and (3) share an employee mobility tie with the focal firm within the last three years. An attorney creates a mobility tie between two firms when she leaves one firm to join the other, so a tie is bidirectional. Using these data, I define an ego-centric network of competitor firms for each firm in my main sample. Each firm has an average of 9.5 competitors (See Table 1). Note that each firm in my sample has been in the data for at least three years to allow me to properly measure its competitor network. Results are unchanged when I move the window to five years.

To identify rival firm dissolutions, I first capture all instances where a firm has exited the data. I then purge "non-failure" exits, using an approach similar to my strategy for identifying true firm births. I identify mergers and recodes as above, and I identify acquisitions when a firm

exits the data, but 80 percent of its employees subsequently move to the same firm in the next two years.

Since my hypotheses relate to the impact of firm failure due to resource release, I focus on failures of competitor firms where the firm experiences positive employment and total wage growth in the two years before exit. A "slow sliding" firm's exit does not necessarily result in the sudden release of resources that is the focus of my theoretical arguments – workers and other stakeholders will depart the firm as they witness its gradual decline (Jensen, 2006; Rider, Negro & Roberts, 2011). No firm experiences more than one of these sudden competitor failures in a given year. Because the failure of a small firm will likely release many fewer resources than the failure of a large firm, I weight these sudden competitor failures by the number of attorneys working for the rival firm at the time of its demise. I name the resulting variable # *competitor firms that fail unexpectedly (weighted by # attys)*. I log this variable because as Table 1 shows, it is skewed (mean: 11.2, standard deviation: 63.1). The Bayesian Information Criterion (BIC) indicates that the logged specification provides a better fit to the data than the unlogged or unweighted specifications, and results of hypothesis tests are similar across all three.

Instrumental variable – unexpected deaths

While the individual-firm fixed effect in the LPM help address the problem of time-invariant unobserved heterogeneity and lagging the independent variables provides some comfort around the issue of reverse causality, it is likely that time-variant, unobserved changes in a number of unmeasured variables may be driving both the failure of competitor firms and the entrepreneurship decisions of attorneys in existing organizations. Important examples might include changes in the business environment such as the arrival or departure of important client organizations in the geographic area or decline in the quality of competitor firms. These omitted

variables would likely bias my results away from confirmation of Hypothesis 1 (more entrepreneurship) because the failure of competitor firms would simply proxy for an economic decline that restricts the number of opportunities for entrepreneurship. I use an instrumental variable approach to account for these unobservable factors.

Specifically, I use the *# of competitor partner attorneys that die unexpectedly* as an instrument for my key endogenous variable: *# competitor firms that fail unexpectedly (weighted by # attys)*. The variable measures the number of partner attorneys who die unexpectedly while working for competitors at time *t*. I next describe why this instrument is valid, i.e. as good as randomly assigned, correlated with the endogenous variable and uncorrelated with the error term in the primary estimation equation (Angrist & Pischke, 2009; Murray, 2006).

Like previous researchers, I focus on unexpected deaths because these deaths arguably provide a random source of variation[9]. Deaths that follow a prolonged illness are incorporated in the individuals' expectations and thus become an endogenous feature of the data. While the LEHD data contain information on the exact day of death of each individual in the data if she dies during the sample period, I do not have access to information on the cause of death. Individual identities in the data are anonymous, which precludes linkages to external data sources such as obituaries, so I use the detailed information the LEHD provides on age and compensation to identify unexpected deaths. Specifically, I count a death as unexpected when (a) the individual is under 60 years of age, and (b) the individual experiences positive seasonalized wage growth in the two quarters prior to the quarter of death[10]. These criteria are

[9] Using individual deaths as a source of exogenous variation is well established in social science research. Aizenman and Kletzer (2011) examine how author death impacts forward citations to economics journal articles, Bennedsen, Perez-Gonzalez and Wolfenzon (2009) examine how CEO death impacts firm performance, Jones and Olken (2005) connect the death of national leaders to declines in economic growth, Johnson et al. (1985) examine stock market reaction to CEO deaths, while Azoulay et al. (2010) and Oetll (2012) examine how scientist death influences coauthor productivity.
[10] This means that wages in quarter Q_i (where i=1,2,3,4) in year Y_i are compared to wages in quarter Q_i in year Y_{i-1}, in order to account for fluctuations in wages owing to the yearly business cycle.

reasonable given the significant jump in the mortality rate in the United States for individuals in their 60s (6.4 percent) relative to individuals in their 50s (3.1 percent) and 40s (1.4 percent) (Center for Disease Prevention and Control, 2009). In addition, a partner attorney's productivity would likely decline during a time of illness or hospitalization, making it difficult for her to earn UI-covered wages during that time period. Only a small percentage of partner attorney deaths in the data (about 25 percent) meet these two criteria[11].

With regard to instrument strength, there are clear reasons to anticipate that the unexpected death of competitor partner attorneys should be correlated with the dissolution of competitor firms. Legal services firms have few assets that are not linked to the human beings that work for the firm (Lazega, 2001). Therefore, partner attorney exit should increase the mortality of the firm because it may sever relationships with clients (Broshack, 2004) as well as reduce the productivity and increase the turnover (Felps et al., 2009) among other partners and associates that worked closely with exiting partner. The instrument represents an exogenous source of partner attorney exit for rival firms.

Anecdotally, even though Massachusetts is not represented in the sample, the power of the instrument is exemplified by the failure of Testa, Hurwitz & Thibeault (Testa, Hurwitz) following the unexpected death of managing partner Dick Testa in late 2002 (American Lawyer, 2005; Dahl, 2005; Mass High Tech, 2002). A Boston law firm that employed 400 lawyers at its peak, Testa, Hurwitz was forced into dissolution after the sudden death of Dick Testa left the firm without a guiding force. As one legal services consultant noted "when [Testa] suddenly died, it was too much of a shock," (Dahl, 2005). Most of the attorneys from the private equity group, Testa's primary practice area and the firm's core expertise, left the firm, and the

[11] Though not directly comparable, my set of criteria seem to be about as restrictive as Azoulay et al. (2010) who impose an age cutoff of 67 and find that about 45% of their deceased scientists pass away suddenly.

remaining attorneys could not meet the significant rent on the firm's Boston headquarters, forcing the firm to dissolve.

With regard to the exclusion restriction, I also expect these instruments to be uncorrelated with the error term in the LPM predicting employee entrepreneurship. While the death of partner attorneys working for other firms may release resources for potential entrepreneurs, I expect that the marginal effect of these shocks on the entrepreneurship decisions of the attorneys in my sample should be relatively small since these partners are working in other organizations. In effect, my assumption is that—conditional on all of my control variables—partner attorney deaths will only stimulate entrepreneurship by individuals working for rivals when the death is significant enough to result in the dissolution of the partner's firm and the release of its constituent resources. I underscore a few key controls in the following section.

Other Independent Variables

Annual earnings: The moderator for Hypothesis 2, earnings include all forms of taxable compensation that an employee received in a given calendar year including salary, bonuses (including partnership distributions that are crucial in legal services), and other reported income.

Maximum annual earnings in the firm (wage ceiling): The moderator for Hypothesis 3, the firm's wage ceiling measures the highest wage paid to an employee in the current year (Sørenson & Sharkey, 2010).

I include control variables at the individual, firm, and competitive environment levels of analysis. At the individual level, in my primary models I utilize employee-employer fixed effects, so variables that do not vary at the level of the employee-employer match, such as gender, race and education, drop out of the model. I also include *age*, age^2 in all models. I control for *tenure* as flexibly as possible: a dummy for each of 1-14 years of same-firm tenure.

The LEHD data do not contain information on whether a particular worker is an associate or partner. However, I rely on my rich wage and demographic data to impute partner status. Recent work analyzing a representative national sample of attorneys from the comprehensive Martindale Hubbell database suggests that attorneys usually enter law school at age 22 and take about nine years to advance to partner (Parkin & Baker, 2006). Furthermore, law firms during the time period covered by my data typically have a leverage ratio of one associate per partner, regardless of firm size (Parkin & Baker, 2006). Consequently, I define a *partner* as an employee who is age 34 or greater and lies in the upper 50 percent of the firm's earnings distribution (after removing the bottom 33 percent of the firm's wage distribution to eliminate non-attorneys). Results are robust from age 33 to 36 and for percentile cutoffs of 66 percent and 75 percent. I indicate whether an individual is a partner using a dummy variable. This definition of partner is also used in the calculation of the instrumental variable.

At the firm level, I include size (*# of attorneys*), quality (*revenue per attorney*), and compensation structure (*coefficient of variation in earnings*), as prior work shows each of these to be correlated with mobility and entrepreneurship decisions, and each could plausibly be associated with the failure of competitor firms. I control for size using dummy variables for firm size categories of 5-10, 11-25, 26-50, 51-100, 101-500 and 500+ sized firms, which conform to the categories used by the National Association of Legal Professionals (NALP). The coefficients on these dummies (as well as those for tenure) are not reported owing to their lack of theoretical interest and Census disclosure concerns around dichotomous variables. I further include the *# of attorneys hired from failed competitors,* which ensures that attorneys hired from failed rivals do not "push" entrepreneurs out of their firms.

I include a number of important controls related to the focal firm's rivals. I include the size (*# of competitor firms*) and quality (*avg revenue per attorney,* weighted by competitor firm size) of the firm's competitor network. While my key explanatory variable focuses on the post-growth failure of competitor firms, I also include the *# competitor firms that fail after decline (weighted by # of attys)*. To account for an aging human asset base that might create correlation between my DV and instruments via openings in the vacancy chain, I include the *avg age of competitor attorneys.* To account for larger trends in mobility and entrepreneurship that might drive competitor failure and individual decisions to create startups, I include *percent competitor turnover rate* and *percent competitor startup rate*. These variables also help account for changes in the vacancy chain, perhaps owing to rival partner death, that might create correlation between my DV and instruments. I also include the number of law firms in the firm's MSA and its squared term (*density*).

RESULTS

Descriptive statistics are provided in Table 1. The average attorney in the data is about 42 years old, makes about $114,000 ($2004), and has spent the last four years with her current firm (though tenure is left censored). Another variable of note is the wage ceiling, which averages $500,000. Mobility to an established firm is about three times as likely as creation of a new venture.

Table 2 provides the results of the linear probability models with endogenous regressors. Heteroskedastic and autocorrelation consistent (HAC) Newey-West standard errors clustered by employee-employer dyads are provided. Coefficients can be interpreted as the correlation between the independent and dependent variables. Models 1 and 3 report the effect of rival failure on entrepreneurship and mobility decisions of employees of existing firms. Model 1

shows that increased rival failure is negatively correlated with entrepreneurship, while Model 3 provides results consistent with Haveman and Cohen (1994): increased rival failure is also negatively correlated with mobility to established firms in the industry. In Models 2 & 4, the failures are split into "expected" (preceded by decline in headcount and wages) and "unexpected" (preceded by growth) and reveal similar negative correlations. Interestingly, most of the variation is due to "unexpected" failures.

These results are consistent with the idea that non-random competitor failures proxy for weakness in the business environment rather than the release of resources. If the primary effect of non-random rival failures were indeed released resources, one would expect to see a negative correlation with mobility - unemployed workers would "crowd out" the movement of workers in existing firms (Haveman & Cohen, 1994) - and a positive effect on entrepreneurship as employees in existing firms would join with these workers in startups or use other resources to create new firms (e.g. Delacroix & Carroll, 1983).

Table 3 contains the results of the 2SLS estimation, which attempts to account for unobserved variation in the business environment. The first stage equation in Model 1, which regresses the endogenous variable on the instrument and covariates, provides empirical support for instrument strength - the instrument is positively and statistically significantly correlated with the endogenous variable. In addition, the instrument also has considerable economic significance: the death of a rival partner attorney increases the number of attorneys released by unexpected rival failure by about 2 percent of the sample mean (.0221/1.505 = 2.1 percent). In unreported results using the unlogged version of the endogenous variable, the death of a rival partner attorney is associated with the release of 100.3 attorneys via unexpected competitor failure, providing further support for the practical significance of the instrument. The statistic for

Stock and Yogo's (2005) test of instrument weakness using the Kleibergen and Paap (2006) Wald F-statistic (193.867), strongly rejects the null hypothesis that the instrument is weak at the critical value of 16.38 for 2SLS estimates with one instrument and one endogenous variable. The Angrist-Pischke (2009) multivariate F-statistic of 128.63 also rejects the null hypothesis of instrument weakness. Thus, the instrument appears to be both practically and statistically strong.

Models 2 and 3 of Table 3 provide the focal results of the paper by using 2SLS estimation of linear probability models with employee-employer fixed effects and HAC standard errors clustered by employee-employer dyad. They show the exogenous variation supplied by the instrument variable produces significantly different results than in Table 15. Model 2 displays the results for new venture creation. Not only does the coefficient attain statistical significance at the .001 level, it also becomes practically larger and changes signs from negative to positive as compared to the endogenous estimates in Table 3 (from -0.002 to 0.038). The practical significance of the effect is striking: the failure of a rival firm with 11 attorneys (the average size of a failed firm in the data) causes a 3.9 percent marginal increase in the probability that an attorney will found a startup (log(11)*.0382 = .0397). Since the average probability that an attorney in the sample founds a startup in a given year is 0.9 percent, this 3.9 percent marginal increase represents a 351 percent increase as compared to the sample mean ([3.97-.9]/.9). This Local Average Treatment Effect (LATE; [Imbens & Angrist, 1994]), shows that when rival partner death causes the dissolution of a rival firm, employees of existing firms receive a significant push to start a firm.

The results for mobility in Model 3, in line with the endogenous estimates of Table 3 and Haveman and Cohen (1994), show that unexpected competitor failure reduces mobility. However, while the magnitude of the coefficient is considerably larger than the endogenous

estimates of Table 3 (-0.02 versus -0.001) it does not attain statistical significance (p-value=.55). It appears that when accounting for unobservables, competitor failure does not have a significant causal effect on mobility to established firms in this context.

In order to test the interactions implied by H2 and H3 while retaining the instrumental variable estimation of rival failure, I follow prior research by using the predicted values from the first stage regression in place of the endogenous variable (e.g. Samila & Sorenson, 2011) and interacting this value with the moderators of interest. Results should be interpreted carefully. While OLS estimates the coefficients from this approach correctly, it does not estimate correct standard errors, because 2SLS constructs the residual variance estimator using the original endogenous regressor (which is correct), while OLS uses the first stage fitted values (which is not - Angrist & Pischke, 2009: 188). To overcome this issue, I construct the standard errors using a bootstrap with 10,000 repetitions, following Samila and Sorenson (2011). While still imperfect, this approach relaxes the parametric assumptions associated with estimating traditional standard errors.

Results in Table 4 provide support for H2 and H3. Model 1 replicates the results in Model 2 of Table 3 using the manual two-stage OLS approach described above – reassuringly, the coefficient estimates are identical and the bootstrapped standard errors are quite similar to those provided by 2SLS in Model 2 of Table 3. In Model 2 of Table 4, the interaction between *Annual earnings* and *# competitor firms that fail unexpectedly (weighted by log # attys)* is positive and significant at the 10 percent level for a two-tailed test, while the interaction including the square of annual earnings is not significant. In Model 3 of Table 4, the interaction between *Maximum earnings within the firm* (wage ceiling) and *# competitor firms that fail unexpectedly (weighted by log # attys)* is negative and significant at the .001 percent level for a

two-tailed test. Both interactions retain or increase their significance when simultaneously included in Model 4, providing support for Hypotheses 2 and 3.

Figures 1 and 2 demonstrate the practical significance of the individual earnings and wage ceiling interactions, respectively. When individual earnings increase one standard deviation above the mean (from about $100,000 to about $350,000) the marginal effect of # *competitor firms that fail unexpectedly (weighted by # attys)* increases from about .036 to about .038, an increase of 5.5 percent. When the maximum wage offered by the current employer doubles from the mean (from about $500,000 to about $1,000,000) the marginal effect of # *competitor firms that fail unexpectedly (weighted by # attys)* decreases from about .018 to about .004, a decrease of about 77 percent. Confidence intervals in Figure 2 indicate that rival failure no longer affects entrepreneurship when the wage ceiling exceeds about $650,000. Thus, the incentives offered by the firm have a very strong effect on startup creation following competitor failure.

Robustness tests

While the two-stage least squares approach detailed in the previous section provides numerous advantages, such as the inclusion of fixed effects, it makes two key assumptions that need to be examined. First, because I estimate separate models for mobility to established firms and startups, I am assuming that the decision to start a new firm is independent of the decision to exit the organization. Second, when I use the death of rival partner attorneys as an instrument for the failure of rival firms, I am assuming that these partner attorneys are employed by one of the rival firms that dissolve in the year of the attorney's death. It is possible that one rival may experience the death of a partner while a different rival fails.

I address both of these assumptions by splitting my key endogenous variable into two groups –competitor firms that fail unexpectedly following the death of a partner attorney and competitor failures that are not preceded by the death of a partner attorney. Because these events are relatively rare in my data (15 firms fail unexpectedly following the unexpected death of a partner attorney; each firm has between 5 and 100 lawyers), Census disclosure requirements become a concern. To overcome this issue, I expand my definition of competitor firms by an additional degree of separation in the mobility network. Previously, I defined competitors quite restrictively, using only firms who share a direct employee mobility tie with the focal firm. In the results presented in Tables 5-7, I also include those firms with a direct mobility tie to the firms with which the focal firm has a direct mobility tie (in essence, "competitors of the firm's competitors" by the original definition). This expansion allows the 15 firms that fail unexpectedly to "treat" a larger number of individuals in the sample, thus obviating Census disclosure concerns, while still providing similar results to those with the more restrictive definition of competitors. The variables are named *Competitor firm (large set) fails unexpectedly after death of partner atty(s)* and # *competitor firms (large set) that fail unexpectedly, no death of partner atty* to reflect the increased size of the competitor network.

The identifying assumption underlying this approach is that the first variable (*Competitor firm (large set) fails unexpectedly after death of partner atty(s*)) is essentially random. If we see a firm with two years of positive wage and employment growth exit from the data after at least one of its partner attorneys dies unexpectedly, I assume that this surprising dissolution was triggered by the unexpected, random death of the partner, similar to how the unexpected death of

Dick Testa spurred the dissolution of Testa, Hurwtiz[12]. If this variable is indeed random, I can include it in standard reduced form regression models and its coefficient will have a causal interpretation. This is the approach that I take in Tables 5-7.

Table 5 replicates the main effect results in a multinomial logit model, which allows empirically acknowledgement of the interdependence between the general decision to exit the firm and the specific choice of founding a startup, but does not allow for fixed effect estimation. The employee can choose to remain with the current firm, join an established firm, found a startup, or exit the data. Staying with the current firm is the reference category; results for exiting the data are not shown for brevity. Exits from the data can include retirement, movement to a state not covered by the data, or transition to an non-legal services industry, such as an in-house counsel position.

Table 5 shows that rival failures preceded by the death of a partner have a statistically and economically strong effect on the decision to form a startup but do not have a statistically significant effect on mobility to established firms. Indeed, one of these failures increases the odds that an attorney will leave for a startup versus stay with the current firm by a factor of 8 to 1[13]. Note that the Hausman test accepts the Independence of Irrelevant Alternatives assumption for each outcome, obviating the need for a nested logit or multinomial probit.

Results for interactions with the multinomial logit are presented in Table 6. Interpreting these coefficients is difficult, owing to the link functions and implicit interactions between all variables in non-linear models (Hoetker, 2007). In particular, the p-values of interaction terms are essentially meaningless due to the implicit interactions between each of the variables, and so

[12] While weaker firms maybe more likely than stronger firms to dissolve following a partner's death, this bias will provide a conservative test of my hypotheses because the resources released by these failures should be of lower quality and thus result in fewer opportunities for entrepreneurship.

[13] A Wald test confirms that rival failures preceded by partner death have a stronger impact on entrepreneurship probability than failures not preceded by death (Chi-sq value of 201.2, significant at .001% level). Further, these "exogenous" failures have a stronger effect on startup creation than established firm mobility (218.47; <.001).

I rely on likelihood ratio (LR) tests. Indeed, while Model 2 in Table 6 shows that the interaction term between *annual earnings* and *competitor firm (large set) fails unexpectedly after death of partner atty(s)* is positive but not statistically significant, the likelihood ratio test comparing Model 2 to Model 1 is significant at the 5 percent level, and supports H2 in this specification. The interaction between *wage ceiling* and c*ompetitor firm (large set) fails unexpectedly after death of partner atty(s)* is negative and statistically significant, and the model fit improves significantly from Model 3 to Model 1. Therefore, H3 is supported.

Finally, Table 7 enters the new variables capturing "exogenous" failures in linear probability models predicting mobility and entrepreneurship. While these models do not acknowledge the interdependence of entrepreneurship and mobility, they have the benefit of fixed effects. In Model 2, failures preceded by the deaths of partner attorneys increase the probability of entrepreneurship by .016 (significant at the .001 percent level), nearly doubling the mean probability of .0088, replicating the economically significant impact we saw in the 2SLS estimation[14]. In Model 4, unexpected rival firm failure that is preceded by partner death does not have a statistically or practically significant effect on mobility to established firms. Importantly, the point estimates of the coefficients on *Competitor firm (large set) fails unexpectedly after death of partner atty(s)* do not change much when we add control variables to the models. This provides some empirical support for the assumption that these failures are essentially random (Simcoe & Waguespack, 2011): the magnitudes of the coefficients appear to be relatively independent of the characteristics of the individual, her firm, or her competitive environment. Models with interaction terms are not provided for sake of brevity – interaction terms indicate

[14] A Wald test confirms that rival failures preceded by partner death have a stronger impact on entrepreneurship probability than failures not preceded by death (Chi-sq value of 31.74, significant at .001% level)

that H2 (earnings) is not supported in this specification while H3 (wage ceiling) again receives strong support.

While the above results and robustness tests lend strong support to the idea that rival firm failure releases resources that enable existing firm employees to start new firms, two important questions still need to be addressed. First, what type of resources drive entrepreneurial entry in this industry? Second, are employees of existing firms actually *founding* startups, or are they merely *joining* "new" firms that are merely modular pieces of the recently failed organizations? Qualitative interviews and the fine-grained nature of the data allow me to examine both.

What types of resources drive entrepreneurial entry in the sample?

Popular press articles (e.g. Hurley 2009) and qualitative interviews with attorneys indicate that there are two key resource constraints that potential entrepreneurs must overcome in the legal services industry: a significant client "book" and availability of co-founders or joiners who are interested in working in startup firms. The failure of a law firm precipitated by the death of one of its partners may release both types of resources into the marketplace. For example, interviewees indicated that when Testa, Hurwitz dissolved following Dick Testa's death, clients tied primarily to Testa himself shifted some work to other law firms with whom clients had existing relationships. This sudden abundance of business might provide an attorney at one of these rivals with a large enough client book to form her own firm. At the same time, the Testa, Hurwitz's attorneys had to find other employment options when the firm dissolved, and some moved to startups with colleagues who previously worked for existing rivals.

While I do not have data on the clients served by the anonymized firms in the data, the rich employee mobility data provide a window for assessing the relative importance of cofounders or joiners. Examining the employees of existing organizations who create startups in

the period following the "exogenous" failure of a rival (i.e. *# Competitor firms (large set) that fail unexpectedly following death of partner attorney* = 1), I find that a most of these individuals create startups that also include former employees of the failed organization, rather than startups with only former employees of existing firms. A t-test for the difference between these two percentages has a p-value of less than .05. This suggests that a release of human assets in the form of cofounders and joiners are the key mechanism, though it is important to keep in mind that one of the key inputs provided by cofounders may be client relationships.

Who leads post-failure startups – employees of existing or failed organizations?

The foregoing theory mainly centers on the entrepreneurial action taken by employees of existing firms following the failure of rival organizations. However, the prior section shows that most of these startups contain employees of healthy firms *and* employees of failed firms. What if employees of healthy firms are merely lower level employees who are joining startups created by employees of failed firms? The support for H2 across most of the specifications above helps alleviate this concern, since it suggests that higher wage employees are more likely to create startups following rival failure. In addition, it is important to keep in mind that startups where 80 percent of the founding employees matriculate from the same firm are excised from the data in order to exclude from the startup population "new" firms that are merely modular pieces of recently failed organizations. If rival failure creates "false" startups, the results in Table 2 that indicate a negative association with endogenous rival failures and startup activity by employees of existing firms provides some confidence that this cleaning process has worked effectively.

I also performed additional regression analysis to compare the demographic characteristics of founders from existing firms to the demographic characteristics of founders from failed firms. First, I limited the sample to those startups formed in the wake of

"exogenous" failures which contained founders from both existing and failed organizations. Then I estimated conditional logit models where the DV=1 when the founder hailed from a failed organization and DV=0 when the founder hailed from an existing organization. Independent variables included a variety of individual characteristics, including wage at the previous firm, wage in the first year of the startup's existence, wage in the second year of the startup's existence, partner status in the previous firm, partner status in the startup firm, age at founding, gender, and race. Results (not disclosed due to the small sample size) show that founders hailing from failed organizations were more likely to have been a partner in their previous organization and also earned slightly higher wages in their previous organization as compared to employees hailing from existing firms. However, wage and partner attainment outcomes experienced at the startup were economically and statistically the same across both groups, and the other demographic variables such as age and gender were also not significantly different. This suggests that employees from existing and failed firms created startups on a relatively equal basis. Combining this analysis with the previous section suggests that the primary resource constraint relaxed by rival failure is the availability of co-founders who can help each other pool uncertainty and resources inside of startup firms (Wasserman, 2012).

DISCUSSION AND CONCLUSION

Using confidential, employee-employer linked microdata on the legal services industry from the US Census Bureau, this paper suggests that the failure of rival organizations increases the probability that employees of existing firms quit their jobs to start new ventures (Hypothesis 1). I use the unexpected deaths of partner attorneys working for rival firms as an instrument for the failures of the firms that employ them, which helps me overcome the omitted variable problems typically present research that uses firm failure as an independent variable. The

relationship is particularly strong for employees with higher wages (Hypothesis 2) and employees working for firms with weaker pecuniary incentives (Hypothesis 3). Supplementary analyses suggest that the main causal driver of entrepreneurial entry by employees of existing firms is the greater availability of co-founders in the form of employees of failed organizations.

Limitations and Future Research

This study has a number of limitations that caution against over-generalizing its findings but also open up interesting avenues for future research. The most important limitation is the context. Legal services has a number of characteristics that make it an appealing empirical laboratory for this study: e.g. relatively small firms, low barriers to mobility and entrepreneurship, primacy of human assets. The findings here are likely to be broadly applicable to other service industries which make up about 68 percent of the US economy (Bureau of Economic Analysis, 2008), including important knowledge-based services such as accounting, consulting, software development, and the like. However, in industries with higher barriers to entry, managerial death may be less likely to spur the firm's dissolution and rival firm dissolution may be less likely to spur entrepreneurship by employees in existing firms.

Consequently, using upper level manager death as an instrument for firm death may not be advisable in industries where firms are larger and physical assets are more important. The ability of such firms to endure following the death of an upper level manager may render the instruments too weak to be valid. In addition, industries where technological change is an important impetus for firm failure may show different results. Despite these limitations, I do not expect the direction of the hypotheses in this study to change in a different empirical context, though I do expect the economic significance of the hypothesized effects to be smaller in industries where barriers to entrepreneurship are higher. Future work examining industry-level

heterogeneity in the effect of rival failure on entrepreneurship will provide an important extension to the initial progress made in this manuscript. For example, anecdotal evidence suggests that the release of human capital from the closure of many NASA facilities is spurring entrepreneurship in high tech industries as former NASA employees become key "joiners" of Silicon Valley startups (Kharif, 2012).

Another avenue for future work relates to the industry or system-level efficiency outcomes related to entrepreneurship following firm failure. Pe'er and Vertinsky (2008) provide some suggestive evidence that new firms that are formed in the wake of the dissolution of existing firms are more efficient that the ones that they replace. The current study implies that these new firms may consist of three groups: those that only contain employees of the failed firms, those that only contain employees of existing firms, and those that contain a mixture of the two. Examining firm and individual-level productivity differences between these three types may provide important hints about *who* drives the macro-level growth following firm failure.

Another important question related to creative destruction that cannot be answered with the current data is whether the new firms that are formed in the wake of rival dissolution provide products or services that are significantly more novel than other startups. For example, though I find some evidence in unreported results that rival dissolution has a stronger influence on the founding of larger startups than smaller startups, I cannot measure whether the new law firms implement business models or client services that are more innovative than other members of the industry. Data with fine-grained measures of firm routines or product characteristics would show whether rival dissolution allow for ventures that are more novel than other startup firms.

Contributions

Despite these limitations, the paper provides a unique contribution to the literature, primarily through counterintuitive findings related to spillovers from organizational failure at the individual and firm levels of analysis. At the individual level, prior scholars have viewed rival failure as a negative event for employees of existing organizations, as rival failure eliminates a labor market destination and creates labor market competition via the release of the failed firm's employees (Haveman & Cohen, 1994). In this paper, I provide the insight that rival failure can provide positive spillovers for employees of existing firms because it releases resources that may help these individuals start new ventures. Most prominently, employees of failed firms may be labor market collaborators rather than labor market competitors for employees of existing firms when we expand the notion of the labor market to include startups.

At the firm level, prior work in strategy and economics generally views rival failure as a positive strategic outcome that allows existing firms to increase market power—especially in mature industries like legal services, the context of this study. While work in organizational sociology recognizes that rival failure can harm existing firms by decreasing the legitimacy of the industry, the current paper points out a previously unarticulated negative spillover from rival failure: the sudden availability of resources may spur the firm's own employees to exit the organization to found competing new ventures. Since progeny firms often implement similar routines and occupy similar market niches as their parent firms (Phillips, 2002), these new ventures may even be more challenging rivals than the failed firms that they replace.

By examining individual behavior and leveraging exogenous variation in the failure of firms, the study also untangles a key debate in the literature connecting firm failure to entrepreneurship. While prior studies at the population level have shown negative (Barnett & Amburgey, 1990; Land et al., 1994; Halliday et al., 1987) and positive correlations (Delacroix &

Carroll, 1983; Dunne et al., 1984; Hiatt et al., 2009; Pe'er & Vertinsky, 2008) between failure and entry, this study uses an instrumental variable design to suggest that negative correlations are probably due to an omitted variable bias reflecting environmental weakness that causes the failure of firms *and* retards entrepreneurship. Similarly, for scholars interested in the positive spillovers resulting from firm failure (Hoetker & Agarwal, 2007; Knott & Posen, 2005), the paper provides some of the first causal evidence that the death of one firm leads to the birth of others.

While most studies in the classic entrepreneurship and mobility tradition view employees as conduits of knowledge whose movement decisions are shaped mainly by firm level factors (e.g. Sørenson, 2007), the current study adds to a growing literature that considers the influence of the external environment on individuals' entrepreneurship decisions (Marx, Strumsky & Fleming, 2009; Saxenian, 1994). In particular, I extend Sorenson & Stuart (2003), who document how financial capital liquidity events such as acquisitions and IPOs drive entrepreneurship at the MSA level of analysis by providing the complementary insight that rival failure creates a human resource liquidity event that may drive individuals working for existing firms to enter entrepreneurship.

The conclusions reached in this study at the environmental level somewhat parallel those reached by Agarwal et al. (2004) at the firm level. Those authors find that knowledge-rich firms tend to create spinouts when the firm's knowledge is under-utilized. Similarly, I find in this study that an increase in resource munificence in the external environment leads to an increase in spinout creation, but that firms may mollify this effect when they provide their employees with stronger pecuniary incentives. The study thus underscores how human resource practices—

along with knowledge utilization—may be an important organizational lever for managers seeking to encourage employees to exploit resources and opportunities within firm boundaries.

In a similar way, by focusing on the failure of rival firms as an impetus for entrepreneurship, the current paper provides a slightly different spin on Saxeninan's (1994: 111) discussion of tolerance for failure and the success of the entrepreneurial cluster in Silicon Valley:

> Although many individual firms did not survive [the] competitive struggles, the region as a whole thrived . . . The continuous recombination of differently specialized resources in turn strengthened the region's industrial fabric . . .There is a unique atmosphere here that continually revitalizes itself by virtue of the fact that today's collective understandings are informed by yesterday's frustrations and modified by tomorrow's recombinations . . . Learning occurs through these recombinations. No other geographic area creates recombination so effectively with so little disruption.

Thus, while Saxenian (1994) mainly focuses on how Silicon Valley's unique atmosphere of experimentation encourages entrepreneurship by permitting individuals who worked for failed enterprises to avoid stigma and continue to participate in the economic community, the results in the current paper suggest that the failure of rival firms provides opportunities for entry by employees of *existing* organizations, primarily through increased availability of cofounders. Finally, the paper extends our knowledge about connection between the macro environment and individual careers (e.g. Bidwell & Briscoe, 2010). While potential positive macro-level implications resulting from the resource reallocation following a firm's failure, such as increases in efficiency and long run job creation, are difficult to measure accurately, the paper illuminates the micro-level benefits for would-be entrepreneurs working for existing organizations. While an organization's failure may represent a negative outcome for its own employees, its demise

may increase welfare for other workers by changing the structure of the market and providing these individuals with opportunities to obtain better job matches (Jovanovic, 1979) by working for a startup. While it may be unlikely that the short-run aggregate effect on welfare is positive (i.e. that the initial positive effect for entrepreneurs exceeds the initial negative impact for unemployed workers), a long-run increase may be possible if these superior matches result in more productive and faster growing firms.

DOES THE EXIT OF A HIGHLY PAID WOMAN FROM THE FIRM INCREASE OR DECREASE HER FEMALE COLLEAGUES' EARNINGS? EVIDENCE FROM UNEXPECTED DEATHS IN LEGAL SERVICES

Scholars interested in individual career attainment have long studied how changing jobs affects an employee's career outcomes. It is clear that professional fortunes often wax and wane as a result of inter-firm mobility (Spilerman, 1977), as changing employers appears to have indelible effects on important outcomes like earnings (Bidwell, 2011) and status attainment (Rider, 2012; Wegener, 1991). However, while we have begun to grasp the importance of mobility for the person who exits a firm, we have largely neglected the outcomes for the former colleagues that the departing individual leaves behind. In an economy increasingly defined by interorganizational career ladders (Bidwell & Briscoe, 2010) and correspondingly high rates of inter-firm mobility, it is important to understand how the one individual's departure affects the career outcomes of his or her colleagues.

This issue may be particularly salient for members of underrepresented groups within an organization. If a highly paid member of an underrepresented group, such as a female executive, departs the organization, the consequences for the women she leaves behind are unclear. On the one hand, organizational mentors are crucial inputs for career success (Briscoe & Kellogg, 2011), and to the extent that homophily drives the sorting of mentoring relationships (McPherson, Smith-Lovin & Cook, 2001), the departure of an important ally could weaken the remaining individual's position in the firm. On the other hand, legitimacy concerns may dictate that an organization retain a minimum number of members from underrepresented groups (DiMaggio & Powell, 1983), and the departure of a colleague may cause the bargaining power of remaining individuals to increase, allowing them to capture more value.

In this paper, we examine this puzzle through the lens of gender earnings inequality in American legal services firms. How do the average earnings of female attorneys change when a highly paid woman exits a law firm? The answer is not clear ex ante. If she were an important mentor and advocate for their careers within the firm (Kay and Wallace, 2009), the exit of a highly paid woman may decrease the productivity and bargaining power (and hence earnings) of her female colleagues. However, in order to maintain legitimacy with clients (Beckman & Phillips, 2005) and potential employees (Ely & Thomas, 2001), law firms may have an incentive to maintain a threshold number of women in the upper part of the firm's pay distribution. Consequently, the departure of a highly paid female may create opportunities for remaining women to capture higher earnings, either via deeper connections with clients or by ascension through the organizational hierarchy.

We use unique data and a novel empirical design to distinguish between these two theoretical predictions. Using confidential and comprehensive employer-employee matched

microdata on the legal services industry from the US Census Bureau, we analyze how attorneys' earnings change after a highly paid colleague dies unexpectedly. While the process that determines traditional turnover is likely highly interrelated with the process that determines colleague earnings, unexpected deaths are arguably randomly assigned. Matching firms that experience a death to observably equivalent firms that do not, our quasi-experimental set up allows us to make relatively credible causal inferences about the impact of highly paid attorney exit on the earnings of colleagues who remain with the organization.

The results are striking. Following a highly paid colleague's death, the average earnings of other members of the firm *increase* about 3% relative to the control group. This increase may occur because surviving members of the firm inherit the client work that was formerly completed by the deceased attorney. The deeper insight comes when we examine how the gender of the deceased and the gender of the surviving attorney moderate this result. Men experience a relatively small increase in earnings when a colleague passes away, and this increase is not dependent on whether the deceased person is male or female. Women, on the other hand, experience a large (8%) increase in earnings after a female colleague passes away; this increase is significantly larger than what women experience when the deceased colleague is male.

Examining moderators of this key relationship in order to pin down the mechanisms at play, we find that the result is stronger when the surviving female attorney is higher in the firm's earnings distribution at the time of her colleague's death, older then her deceased female colleague, or higher in the firm's earning distribution than her deceased female colleague. These results suggest that highly paid women benefit from the death of their highly paid female colleagues. This may be a result of gender homophily (Beckman & Phillips, 2005; McPherson et al., 2001), where clients of the deceased woman are more likely to migrate to her female

coworkers. It may also suggest that an increase in bargaining power following a highly paid woman's death may flow most directly to other highly paid women, whose visibility makes them important for the firm's maintenance of legitimacy in a marketplace that may have preferences for diversity (DiMaggio & Powell, 1983).

In the next section, we build theory for each of the opposing predictions regarding the direction that female wages will move when a highly paid woman exits a law firm. We then describe the data and empirical design and present the main results. We conclude with additional analyses that attempt to pin down mechanisms behind the focal results. We finally discuss the limitations of the study and its contributions and implications for research at the intersection of organization theory, gender inequality, and individual careers.

Exit of a Highly Paid Female Colleague and Changes in Women's Earnings – Competing Predictions

Extant theory in the organizations literature provides equally plausible opposing predictions about whether the exit of a highly paid woman from a law firm will depress or enhance the earnings of the women who remain with the organization. The lack of theoretical unanimity on this point reflects the broader complexity of gender issues in organizations. In the following two sections, we articulate the theoretical rationale and mechanisms underpinning each of the competing predictions. To summarize the two viewpoints, if we expect women's earnings to decline upon the departure of a highly paid female colleague, the primary reason may be the loss of a mentor or advocate. On the other hand, if we expect women's earnings to increase, the root cause may be an increase in bargaining power – if external audiences demand that law firms maintain gender diversity in order to remain legitimate, the remaining women in the firm may be able to capture more value. Given the broad range of researchers who have contributed to the

literature examining the role of gender in organizations, we draw on work with roots in sociological, psychological, and economic traditions.

Why Exit of a Highly Paid Female May Decrease the Earnings of Women Who Remain with the Firm

A growing literature documents a phenomenon of "women helping women" in organizations. This work suggests that the exit of a highly paid female attorney from a law firm may decrease the earnings of the women who remain with the firm by depressing their ability to both create and capture value. Value creation explanations center on why females in the upper levels of the firm may enhance the productivity of the other women in the organization. If a highly paid female exits the firm, the other women in the firm may experience a decline in their earnings accompanying a decline in productivity. Value capture explanations examine how sets of coalitions bargain for the total value created by the firm. In this point of view, women in the firm may experience a decline in their earnings because the bargaining power of their coalition has been weakened by their colleague's exit.

With regard to enhancements in value creation, a highly paid woman may enhance the productivity of her female colleagues primarily by providing mentoring. Drawing on homophily arguments (McPherson et al., 2001), prior research shows that gender similarity is a key dimension determining the formation and value of mentor-mentee relationships (Kay & Wallace, 2009), particularly that women are the mostly likely and most effective mentors for other women (O'Neill & Blake-Beard, 2002). Mentees receive a number of positive benefits, including skill development, elucidation of organizational norms, and introductions to valuable interpersonal relationships both within and outside the organization (e.g. Whitley, Dougherty, Dreher, 1991). Underrepresented groups, such as women in law firms, tend to especially derive benefits from

mentoring (Scandura & Ragins, 1993), precisely because they may lack the natural homphily-driven connections possessed by larger groups (Kanter, 1977).

Law firms host many studies of mentoring (e.g. Briscoe & Kellogg, 2011; Kay & Wallace, 2009), underscoring the importance of the practice in this setting. For example, if a highly paid woman exits a law firm, her female mentees may have difficulty activating and extending their interpersonal networks, which are vital for marshalling the resources necessary to serve clients. In addition, they may miss opportunities to work on important projects, losing the chance to develop valuable skills and contacts that will help them later in their careers. Each of these losses may drive a decline in a woman's productivity and an associated dip in her earnings if a highly paid woman exits the firm.

In addition to creating value via mentoring, a highly paid woman may also help the other women in the law firm capture more of the value that they create. Here, the essence of the idea is that gender may be an important organizational fault line (Lau & Murningham, 1998) that determines in-group advocacy for salary, bonuses, and promotions. Prior work finds evidence suggestive of this phenomenon. For example, Matsa and Miller (2012) document a positive correlation between female board representation and subsequent hiring of female executives, Bell (2005) shows that newly hired female executives tend to have higher starting salaries in firms where the CEO is a woman, and Tate and Yang (2012) find that newly hired workers have a lower gender gap in starting salaries. These multi-industry studies suggest that women help other women obtain larger portions of the firm's economic pie.

Law firms are likely to be fertile ground for bargaining. Like many professions, a substantial portion of an attorney's take-home pay is allocated in the form of salary. However, firms also award discretionary bonuses to partners and associates based on perceived

performance (Gilson & Mnookin, 1985). To the extent that obtaining these discretionary rewards requires an individual to have an advocate in the upper levels of a firm, highly paid women may help their female colleagues at all levels of the organization capture more value. As a result, when a highly paid woman departs the firm, other women in the firm may lose an advocate, and their decrease in bargaining power may result in a decrease in earnings.

To summarize the logic behind the first prediction, the exit of a highly paid woman from a law firm may hamper her female colleagues' ability to create and capture value, which will result in a decline in their earnings upon her departure.

Hypothesis 1: A woman's earnings will decrease following the exit of a highly paid female from the organization.

Why Exit of a Highly Paid Female May Increase the Earnings of Women Who Remain with the Firm

The previous section articulates why we would expect the earnings of women to decline following the departure of a highly paid female from the organization. This section assembles theory predicting exactly the opposite effect – the exit of a woman from the organization may *increase* the earnings of her female colleagues who remain with the firm. While the argument in the prior section relied on value creation and value capture arguments, this section mainly focuses on how the departure of one woman can shift value to the female attorneys who remain with the firm. The theory in this section can be broken down into efficiency and institution-oriented mechanisms.

The efficiency argument relies on the stylized empirical fact documented in the previous section – women in the upper levels of organizations are more likely to have female mentees than male mentees (O'Neill & Blake-Beard, 2002). While the previous section argues that the departure of such a mentor may harm the productivity of mentees, it may also create an

opportunity for them to fill the void left by her departure. Mentors share skills and social relationships with mentees. For example, a female law partner may be more likely to invest in associates and junior partners who are female, involving them in her client relationships and providing them with tacit knowledge about the workings of the firm (Kay & Wallace, 2009). Since they have imbibed their mentor's advice, skills, and social relationships, they may be well-placed to benefit from her departure by taking a more prominent role with her clients[15] and assuming her other duties with the organization. As a result of this skill transfer, the departure of a highly paid woman may result in an increase in the earnings of the female attorneys who remain with the firm.

The institutional argument arises from the idea that the firm may need to maintain gender diversity among its upper level members to preserve legitimacy with constituencies outside the organization (DiMaggio & Powell, 1983; Pfeffer & Salancik, 1978), such as clients and potential employees. With regard to clients, Beckman and Phillips (2005) document how large law firms increase the number of females in their partnership ranks after their corporate clients increase the number of women in the ranks of their upper level management. This suggests that client preferences for female legal service providers may drive law firms to increase the representation of women in the upper levels of the firm. As a result, when a highly paid woman exits the firm, the bargaining power of the remaining women may increase because they become more valuable in client service and acquisition. This increase in bargaining power may manifest in higher earnings.

[15] Attorneys are likely to port client relationships across organizations (Baker, Faulkner & Fischer, 1998), making it difficult for a firm to capture value from a client relationship when the relationship's broker has moved to a different firm. In the current paper, we examine individual exit via unexpected death, which will heighten the opportunity for other individuals in the firm to capture value from the departed person's client relationships. Even in the case of traditional mobility, however, individuals may still inherit client relationships to the extent that firms build redundant ties with multiple individuals and significant inter-firm routines make it difficult for a departing attorney to take 100% of a client's business to another law firm.

The firm may also decide to promote a woman following a female departure in order to maintain legitimacy with other resource providers, such as prospective employees. During the period covered by the data in this study (1990-2004), females constituted about 47% of law school enrollees (American Bar Association, 2011). Female law students and potential lateral hires may be reluctant to join a firm that does not have a satisfactory number of highly paid women. This may be due to homophily-based preferences (McPherson et al., 2001) for interacting with similar others (Byrne, 1961), or a lack of successful women in the firm may provide a signal to prospective female attorneys that they may have difficulty succeeding in the organization. Prospective male employees may also prefer to work for gender diverse organizations. The existence of influential organizations such as Building a Better Legal Profession, which provides firm diversity statistics, demonstrate the importance of these issues for attorneys in choosing their employers. As a result, when a highly paid woman exits the firm, the bargaining power of the remaining women may increase because their presence may help the firm attract new talent. This increase in bargaining power may manifest in higher earnings.

Hypothesis 2: A woman's earnings will increase following the exit of a highly paid female from the organization.

DATA

US Census Legal Services Microdata

Our data are drawn from a custom extract of the legal services industry from the US Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) project. These longitudinal data are remarkably rich. They contain stable individual and firm identifiers as well as individual earnings and demographic information (including gender) for all employees in the

legal services industry in the 10 states we analyze[16]. The years covered by the data range from 1990-2004, depending on time the state entered the LEHD program (the earliest state entered in 1990, the latest in 1994). The longitudinal nature of the data enables us to track employees over time and across firms and to observe changes in compensation. As a result, these data are uniquely suited to answer the research question posed in this paper.

The data are constructed from state unemployment insurance records. Each quarter, firms must submit Form ES-202, which lists the social security numbers (SSN) and total taxable earnings for each of the firm's employees, which includes salary, bonuses, stock options, and other reported income, such partnership distributions that are crucial forms of payment in the legal services industry. Combining these core data with Census and other governmental data, each observation in the dataset we analyze is an individual-year which contains the individual ID (anonymized from the SSN), the firm ID (anonymized from the Employer Identification Number used by the Internal Revenue Service), the year of observation, and the taxable earnings for that employee from that firm in that year, along with other individual-level demographic data[17]. Individual demographic data include age, gender, race, and date of death, the latter of which becomes important for the identification strategy described below. While rich firm-level variables can be constructed based on aggregations of this individual-level information, the data contain few firm-level variables other than geographic location (i.e. Metropolitan Statistical Area codes as well as latitude and longitude). For example, the data do not contain information on legal speciality.

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[16] These states include California, Florida, Illinois, New Jersey, North Carolina, Oregon, Pennsylvania, Texas, Virginia, and Wisconsin. These states were not chosen by the author; rather states must opt into the LEHD program, and these 10 were the only ones participating when these data were extracted.

[17] The unit of analysis in the raw LEHD data is the "job-quarter" where a job is an employee-firm dyad. To simplify this data structure to a number of observations that is analytically feasible (analyses must be conducted at Census Research Data Centers using available computing resources), we assign individuals to "dominant" firms based on the employer at which they have the highest earnings in a given year, and we collapse quarters into years by summing the earnings the employee draws from the dominant firm during the year.

While LEHD data are uniquely suited to answer the question posed in this paper, working with Census data have important limitations on that require acknowledgement before proceeding. The first is that all analyses are conducted at Census Research Data Centers using Census equipment and software. The second is that the Center for Economic Studies at the Census Bureau submits all statistical output to a rigorous disclosure review process to ensure that no confidential information are revealed from the anonymized data. Descriptive statistics and regression output (particularly those using dichotomous variables) must focus on a sufficiently large number of individuals and firms in order to meet disclosure requirements. Due to the relatively small final sample used in this study and the large number of dichotomous variables used to measure various treatment states, we are unable to display detailed output for all analyses that we have conducted, but in all cases possible we have provide as much detail as possible given these constraints.

Identification Strategy – Unexpected Deaths

To ideally test the competing predictions above, a researcher randomly assign attorneys to organizations, choose some of those attorneys to exit their firms at random, and then observe the change in earnings among the individuals who remain with the organizations, focusing on gender as a key moderator. We attempt to approach this experimental ideal by identifying unexpected deaths of highly paid attorneys in the LEHD data, matching the firms that experience these deaths to observably similar firms that do not, and then moving to the individual level to observe the change in earnings among the "treated" attorneys (those whose colleague passed away suddenly) as compared to their matched counterfactuals.

Using unexpected death rather than simple exit or turnover has numerous important advantages. Most crucially, an employee's exit from a firm is a complex process that is likely to

be closely interrelated with the same process that determines subsequent earnings of his or her colleagues. Therefore, it is difficult to make a causal claims about correlations between a highly paid attorney's non-random exit from the firm and the subsequent earnings of his or her former colleagues. It is not even clear ex ante which direction the bias would lie. For example, highly paid attorneys may see that the firm is struggling and flee the sinking ship, creating a negative correlation between exit and colleague earnings that has little causal link to the exit itself. Conversely, exit can be indicative of *good* future prospects for former colleagues; competitors might be poaching highly paid attorneys because they want to recreate the current firm's successful environment. In this case, the bias would be positive.

Unexpected deaths, on the other hand, have the advantage of quasi-randomness. While it is possible that stress at one's job may result in a heart attack, stroke or other form of sudden health problem, the process underlying an employee's sudden death is much less likely than turnover to be correlated with the process determining his or her colleagues' earnings. Using unexpected death as a source of turnover enhances our confidence that some unobserved process is not affecting both exit and colleagues' subsequent earnings and allows us to make a stronger claim about the causal link relating exit and colleague earnings.

The relatively exogenous nature of sudden deaths and the researcher's subsequently enhanced ability to make causal claims is the primary reason why prior scholars have used unexpected deaths in the empirical design of prior studies. For example, Johnson et al. (1985) and Bennedsen, Perez-Gonzalez and Wolfenzon (2009) examine how the stock market and the firm's performance (respectively) respond to the death of CEOs in order to determine the value of managers, Jones and Olken (2005) connect the death of national leaders to changes in GDP to determine the importance of national leadership for economic growth, Azoulay et al. (2010) and

Oetll (2012) examine how scientist death influences coauthor productivity to measure the importance of collaboration in scientific endeavor, and Aizenman and Kletzer (2011) examine how author death impacts forward citations to economics journal articles to shed light on why scholars cite each other's work.[18]

It is important that the deaths we examine are unexpected, and not the result of a prolonged illness, because slow deaths blur the timing of the quasi-experimental treatment. The prolonged hospitalization of a highly paid attorney may cause colleagues to change their behavior or exit the firm prior to time of death. This effect will change the characteristics of the treated and control samples prior to the time of treatment, biasing our difference-in-difference estimates.

Defining Unexpected Deaths

While the LEHD data contain information on the exact day of death of each individual in the data if she dies during the sample period, I do not have access to information on the cause of death. Individual identities in the data are anonymous and the Census Bureau does not permit linking individual-level data with outside sources, which precludes linkages obituaries, so I use the detailed information the LEHD provides on age and compensation to identify unexpected deaths. Specifically, I count a death as unexpected when (a) the individual is under 60 years of age, and (b) the individual experiences positive seasonalized wage growth in the two quarters prior to the quarter of death[19]. These criteria are reasonable given the significant jump in the mortality rate in the United States for individuals in their 60s (6.4 percent) relative to individuals

[18] As with any empirical design, there is an important tradeoff in the choice to use sudden deaths as a source of exit. In particular, it is important to think about whether the conclusions we draw from using sudden deaths as a source of exit will apply to more traditional inter-firm mobility. We address this tension between internal and external validity in the discussion section.

[19] This means that wages in quarter Q_i (where i=1,2,3,4) in year Y_i are compared to wages in quarter Q_i in year Y_{i-1}, in order to account for fluctuations in wages owing to the yearly business cycle.

in their 50s (3.1 percent) and 40s (1.4 percent) (Center for Disease Prevention and Control, 2009). In addition, an attorney's productivity would likely decline during a time of illness or hospitalization, making it difficult for him or her to earn UI-covered wages during that time period. Only a small percentage of highly paid attorney deaths in the data (about 25 percent) meet these two criteria[20].

Defining the Unmatched Sample

Because the LEHD data are so universal – indeed, every individual who has ever worked for an employer firm in the legal services industry in the included states in the covered time period is in the data[21]. This means that the data contain all employees, including secretaries, paralegals, and other support personnel, in addition to partners, associates, and of counsel positions. As a consequence, we must make decisions about which individuals and which firms to include in the sample that we will subsequently use to determine our treatment and control groups.

We first drop all individual-years with earnings under $25,000 to eliminate individuals with weak attachment to employment in the legal services industry. Because our theory focuses on attorneys, we want to ensure that the sample is composed primarily of attorneys. The data do not contain information on individual's formal position in the firm or their occupation, so we rely on detailed studies of the legal services industry and our rich earnings information identify attorneys. Parkin and Baker (2006), analyzing a nationally representative sample of legal services firms, indicate that law firms, regardless of size, typically employ one non-attorney for every two attorneys (which can be either partners or associates). This means that the lower one-third of each firm's pay distribution is likely composed of support personnel. We thus drop all

[20] Though not directly comparable, my set of criteria seem to be about as restrictive as Azoulay et al. (2010) who impose an age cutoff of 67 and find that about 45% of their deceased scientists pass away suddenly.

[21] "Employer firm" refers to organizations which pay into the state's unemployment insurance program. This includes all firms except sole proprietorships. We would likely exclude sole proprietors from our analyses anyway, because these individuals by definition do not have colleagues whose earnings we can examine.

individual-years where the employee is in the bottom third of the firm's earning's distribution. Throughout the paper, we refer to individuals in the upper two-thirds of the firm's earnings distribution as "attorneys." We finally drop all firm-years that do not have more than five attorneys. Because we are interested in the earnings of colleagues following the death of a highly paid individual, we must eliminate firm-years where the number of colleagues is small or non-existent. The results we present below are similar if we use a minimum of 10, 15, and 25 attorneys.

Determining the Treatment Sample

From this trimmed sample, we first identify firm-year observations that experience the unexpected death of highly paid attorney[22]. We define highly paid attorneys as those in the upper 50% of the firm's distribution of attorney earnings (i.e. the employees that are left after trimming the bottom third of the earnings distribution in step two above). We again rely on Parkin and Baker's (2006) in-depth study of the legal services industry in making this choice – they indicate that law firms typically employ one associate per partner regardless of firm size, suggesting that the top 50% of the firm's earners are likely partners in the firm. We conservatively refrain from calling these individuals "partners", instead using the term "highly paid attorneys", because we cannot be certain about their membership in the partnership of the firm, but we are certain of their position in the firm's earnings distribution. The results to follow are robust to more restrictive definitions of "highly paid", such as the top 33% and 25% of the firm's attorney earnings distribution, but Census disclosure requirements dictate that we present

[22] We exclude the small number of firm-years where more than one death occurs because it is not clear how to appropriately measure these events. We use dummy variables to examine the effect of deaths over time, so using a continuous variable that "counts" the number of deaths in the firm is not feasible. Treating multi-death events the same as single-death events is one possibility, but becomes a problem if partners of each gender pass away. Measuring multi-event deaths separately is also possible, but fortunately these situations are uncommon enough that there are not a sufficient number of observations to create a separate suite of dummy variables.

results based on the top 50% because the sample size becomes too small with the more restrictive definitions.

After identifying the approximately 250 firm years that experience the unexpected death of at least one highly paid attorney, we confront a common problem in event-type studies: the treatment of "multiple" events – in this case, firms with multiple years where they experience the death of a highly paid attorney, or firm-years where more than one highly paid attorney dies unexpectedly. Authors usually take one of two approaches – either they focus only on the first event or, more conservatively, they drop observations with multiple events (Campbell, 2013). We take the second approach in the analyses presented here, dropping the small number of firms that experience multiple instances of deaths of highly paid attorneys. Results are the same under either approach due to the relatively rarity of multiple events.

The Matching Procedure

To construct our control sample, we match each of the 250 eligible firm-years that experience the death of a highly paid attorney to a "twin" firm-year that does not experience a death. In order to preserve the exclusivity of the treatment and control samples in the panel data analysis, we require that control firms never experience the unexpected death of a partner attorney during the period covered by the data.

We use Coarsened Exact Matching ([CEM] Iacus, King & Porro, 2012) to locate suitable "twin" firm-years that do not experience a death to serve as control observations. CEM ensures that treatment and control observations are similar on a set of observable characteristics selected by the analyst prior to performing regression analysis or mean comparisons. The analyst selects the variables on which she would like the treatment and control observations to be similar, then "coarsens" the variables by breaking them into discrete chunks (e.g. instead of matching the

exact number of lawyers in a firm, we break this variable into the size categories recognized by the National Association of Legal Professionals). Each observation is then assigned to a stratum based on the coarsened values of all covariates. Strata that contain at least one treated and control observation are retained, while strata without a suitable "twin" are discarded. The method is thus similar to propensity score matching, but with the added benefit that we do not have to endure the painful process of ensuring balance in the covariates – balance in all selected covariates is guaranteed by the fact that the treated and control observations lie in the same stratum. While this method was developed by political scientists, it has been adopted by a broad variety of social science researchers. For examples closest to the current analysis, note that Azoulay et al. (2010) and Oettl (2012) each use CEM to construct control samples in their examinations of how scientist death affects the publishing rates of surviving colleagues.

Our goal in matching firms is to find a counterfactual organization that is observationally equivalent to the firm that experiences the death of a highly paid attorney. We select four important observable characteristics at the firm-year level to serve as our matching criteria – *location, size, performance, and female representation in the upper part of the firm's earnings distribution* (See Table 8). Specifically, we first require that firms be located in the same Metropolitan Statistical Area and state, since some MSAs cross state lines but competition between law firms is often conducted on a state-by-state basis given the high costs of passing the bar in multiple states. We next match the number of attorneys in the firm-year (size), the firm-year's compensation per attorney (performance) [23], and the percentage of highly paid (top 50% of attorney earnings distribution) attorneys in the firm who are female (representation of women). The latter criteria is especially important – gender diversity is not randomly distributed

[23] Law firms return most of their revenue to partners as income and law firms have few costs beyond human capital, so compensation closely tracks revenues and revenues closely track profits; revenue is also the key performance metric used in industry publications evaluating law firms (e.g. AmLaw Top 100).

among organizations, and we want to guard against the possibility that firms which experience the death of a highly paid female may differ systematically from firms that experience the death of a highly paid male. We return to this issue many times below.

We match on our criteria in both the year prior to treatment and the two years prior to treatment in order to ensure that the trend between the treatment and control samples is similar, since differences in pre-treatment trends would render our fixed effect estimates inconsistent. To further ensure a match on the trend, we create a dummy that takes a value of one if the firm increased its number of attorneys between t-2 and t-1 and also match firms based on this criterion. Table 8 displays the exact "cut points" for each of the coarsened variables.

Table 9 displays the outcome of the matching procedure. We first note that of the 250 treated firm-years, a suitable match was obtained for 62 of them for a match rate of about 25%. While each treated firm-year was assigned to a unique stratum, after the initial match, 15 strata contained multiple control firm-years. In these cases, the best control firm-year was identified as the one whose size was the closest to the treated firm-year. T-tests demonstrate good balance on each of the selected covariates at times t-1 and t-2. At time t-1, the average treated firm has 49 attorneys and $100,000 in compensation per attorney, and 21% highly paid individuals in the firm are women. These values compare favorably to the mean values in the overall trimmed sample from which these matched firm-years are drawn, increasing our confidence that despite the relatively low match rate (25%), our matched sample is representative of the larger sample.

Comparing Treatment Samples by Gender of Deceased Attorney

Of the 62 treated firms, ten experience the death of female partners. While this is a relatively small number of observations, many studies utilizing a difference-in-difference framework rely on a single shock to obtain treatment and control samples. The smaller number

of deaths of highly paid females as compared to highly paid males reflects not only the under-representation of women in the upper echelons of law firms (about 20% of partners are women according to National Association of Legal Professionals surveys during the sample period; the same number holds in our sample [see Table 8] when we use position in the firm's pay distribution as a proxy for partnership), but also the fact that women tend to have lower mortality rates than men (Center for Disease Prevention and Control, 2009).

As mentioned above, an important concern is that that firms that experience the death of a female partner might differ systematically from firms that experience the death of a male partner. While the ideal approach would be to match triads of firms – i.e. one with a female partner death, one with a male partner death, and one with no death – the small size of the sample and the relative infrequence of unexpected deaths makes this approach infeasible. Thus, to assuage these concerns, we follow Oettl (2012) and also compare the balance in covariates across the sample of firm-years that experience the death of a female partner and the sample of firm-years that experience the death of a male partner. The small size of the samples precludes us from displaying average values of covariates, but t-tests indicate that these firms are statistically indistinguishable on each selected covariate, save for one – the percentage of highly paid individuals who are female. This difference is not surprising since firms with more highly paid females are more likely to be the locus of highly paid female deaths.

While the similarity on the other dimensions suggests that these firms are comparable, a higher percentage of highly paid female means that firms that experience highly paid female deaths may be systematically different as compared to firms that experience highly paid male deaths, particularly with regard to gender equality. We address this issue in two ways. First, our forthcoming regressions contain a number of control variables to help parametrically account for

potential differences in gender climate. In addition, in robustness tests, we split the sample based on the gender of the deceased individual, so that we can be certain that our 10 firms experiencing the death of woman are compared only to their 10 matched counterfactuals.

Characteristics of the Final Sample

Following the match, we identify all attorneys working for the treated and control firm-years, and we obtain their entire career histories preceding and following the year of treatment, including years in which they worked for other organizations in the legal services industry. Our primary analyses examine how the earnings of the treatment group differ from those of the control group, allowing the gender of the focal individual and the gender of the deceased partner to moderate these differences.

Consequently, while we matched at the firm-year level, we use the individual-year level of analysis in our regressions. We could conduct our analysis at the firm level and perhaps use the average disparity in wages of men and women in the firm as the dependent variable, and examine how this disparity changes following the death of attorneys of different genders. We choose the individual level approach, examining how individual earnings changes following attorney death, for two key reasons. First, it allows us to account for selection bias by tracking individuals across firms. A firm's gender disparity may change over time following a death due to mobility in and out of the organization, and we want to rule out this possibility. Second, conducting our analysis at the individual level allows us to account for unobserved, time-invariant heterogeneity in individual attorney quality (driven by factors such as law school affiliation) via individual-level fixed effects. We do not have detailed data on these time-invariant individual level characteristics that might be important ingredients in determining

attorney earnings, so utilizing individual fixed effects in our regressions is important, and would be impossible at the firm level of analysis.

Table 10 provides means and standard deviations for key variables for the full matched sample at the individual level of analysis. We see that the average attorney has earnings of about $102,000 per year, is 43 years old, and has 3.6 years of left-censored tenure. 50% of the individuals in the sample are female, which is slightly higher than the industry average of about 45% reported by the National Association of Legal Professionals (NALP 2011) during the study period. We also see that the individual-year observations are split evenly between the treatment and control groups, which provides some comfort that individuals who experience a death are not necessarily more or less likely to remain within the legal services industry (recall that when individuals leave the industry, they also exit the data).

We also see that individuals experiencing a death are split about evenly between male (13% of all observations) and female (12% of all observations) observations, but that most of the treatment sample is exposed to the death of a highly paid male (23% of all observations) rather than a highly paid female (2% of all observations). This disparity reflects the greater number of men in the upper part of law firm earnings distributions, not only in our sample but in the industry more generally (both in the sample [see Table 8] and in national surveys compiled by NALP, only about 20% of partners are female), as well as the higher risk of death for men as compared to women.

Table 10 also includes information for firm-level "gender climate" variables that we use to account for differences in firms that experience the death of highly paid females as opposed to highly paid males. These variables include the percentage of the firm's attorneys that are female, the percentage of the firm's partners (those in the upper half of the attorney pay distribution) that

are female, and the ratio of average female earnings in the firm to average male earnings. Each of these variables is included in the regressions that follow.

Regression Estimation

The first empirical model we estimate is

(1) $\text{Log}(Y_{it}) = \beta_1 Death_{it} + \beta_2 Controls_{it} + \beta_2 Controls_{jt} + \varphi_{it} + \mu_{it} + \gamma_i + \delta_t + \varepsilon_{it}$

Where Y_{it} is the taxable earnings of individual *i* in year *t*, $Death_{it}$ is an indicator variable that switches to one in the period that individual *i*'s highly paid colleague dies and remains at one in all periods thereafter, $Controls_{it}$ are the vector of individual-level control variables listed in Table 10, $Controls_{jt}$ is the vector of firm-level control variables listed in Table 10, φ_{it} is a vector of more than 20 individual age dummies (one for every two years, e.g. 25-26, 27-28, and so on), μ_{it} is a vector of thirteen tenure dummies (one for every possible years of left-censored tenure in the fourteen years of data in the sample), γ_i are a vector of individual fixed effects, δ_t is a set of year dummies, and ε_{it} is a vector of Huber-White robust standard errors. These standard errors are adjusted for 124 clusters corresponding to each of the 62 control and 62 treatment firm-years that were the basis of the sample. This clustering helps account for dependence over time in the observations of individuals who experience the death of the same colleague, even if these individuals depart for other organizations in the legal services industry. Results are qualitatively similar for other cluster choices, including 6368 clusters (one for each individual *i*), and 62 clusters (one for each death in the sample; in this case treatment and control observations are assigned to the same cluster).

β_1 provides the estimate of the treatment effect, the effect on individual *i*'s earnings following the death of a highly paid colleague relative to the earnings of the counterfactual individual-years that have not experienced a death. Table 11 provides the estimate of this effect.

Since the dependent variable in Equation 1 is a logarithm and $Death_{it}$ is a dummy, the coefficient indicates that the average individual experiences an increase in wages of about 3% following the unexpected death of a highly paid colleague.

Figure 3 shows how the treatment effect varies over time. The graph displays the value of coefficients for a regression including each of the variables and fixed effects of Equation 1, but $Death_{it}$ has been replaced by interactions between 1) a set of dummies corresponding to the years before and after a highly paid attorney's death and 2) a dummy variable indicating whether individual *i* is a member of the treatment or control group. Figure 3 allows us to further evaluate the quality of the matched sample and the causal effect of colleague death. Since the earnings of the treatment group are indistinguishable from those of the control group until a death occurs (at which time they experience a relatively monotonic increase) this gives us some confidence that the timing of these deaths are relatively unexpected and that the earnings of treatment and control samples were not already diverging prior to treatment.

Though these first results do not yet shed light on the gender-related questions of the paper, they are themselves interesting. While Azoulay et al. (2010) and Oettl (2012) find that the death of star scientists reduces the productivity of their coauthors in terms of quality-weighted publications, we find that in legal services, individual earnings *increase* following the death of a highly paid colleague. This is almost certainly due to important differences in the two settings. As we discussed in the theory section, it is possible to see why coworkers' earnings might increase because of the unexpected death of a colleague in legal services. Not only might partners inherit more business from the clients of the deceased individual, but deaths create openings in vacancy chains. The death of a highly paid person might make way for the ascent of surviving individuals through the hierarchy of the firm. Neither of these possibilities exist to the

same degree in academic science, which is also a setting noted for the importance of collaboration and team production. In that setting, the loss of a colleague is much more likely to be harmful to one's career prospects. The fact that attorneys seem to benefit, on average, from the unexpected deaths of colleagues underscores the "dog-eat-dog" world that scholars have noted in prior studies of legal services (e.g. Hagan & Kay, 2007).

The shape of the increase in earnings displayed by Figure 3 also deserves mention. The fact that the individual increase in earnings persists and increases over time suggests that those who experience the death of a highly paid colleague are provided an advantage that continues over the course of their career, whether it is a particularly valuable client relationship that leads to other similar ties, or a promotion that sets an individual's career on a faster and more profitable track than would have otherwise materialized had their highly paid colleague not passed away suddenly.

The more interesting question, and the question at the heart of this paper, is whether this average increase in earnings masks underlying heterogeneity based on the gender of the highly paid attorney who dies and his or her colleagues who continue working in the industry. Do these deaths shift compensation away from men and towards women, or vice versa? Or is there no substantial difference?

Estimating Equation 2 allows us to shed light on this question.

(2) $$Log(Y_{it}) = \beta_1 FemaleLivingFemaleDeath_{it} + \beta_2 MaleLivingFemaleDeath_{it} + \beta_3 FemaleLivingMaleDeath_{it} + \beta_4 MaleLivingMaleDeath_{it} + \beta_5 Controls_{it} + \beta_6 Controls_{jt} + \varphi_{it} + \mu_{it} + \gamma_i + \delta_t + \varepsilon_{it}$$

Equation 2 is exactly the same as Equation 1 in most respects – it contains the same control variables and fixed effects. The difference is that the $Death_{it}$ treatment effect from Equation 1 is divided into four separate groups represented by four different dichotomous variables, based on

the gender of the highly paid colleague that passed away and the gender of individual *i*. The omitted group in this set up is the set of individual-years that have not experienced a death. We will compare outcomes based on gender by comparing the statistical differences among β_1 through β_4.

Table 11 shows the results for Equation 2, and Figure 4 displays the coefficients and 90% confidence intervals for β_1 through β_4. These are the focal results of the paper. We see that women who experience the death of a female colleague experience the greatest increase in earnings among the four possible gender combinations – an 8% increase. The 90% confidence interval for β_1 does not overlap with those of the other coefficients; this indicates that it is significantly different from the others at a 5% level. Note that each of the other coefficients have overlapping confidence intervals, suggesting that they are statistically equivalent. These results provide confirmation of Hypothesis 2 as opposed to Hypothesis 1 – it appears that the death of a highly paid woman increases the earnings of her female colleagues who remain in the legal services industry. Moreover, this increase is greater than the increase women experience from the death of a highly paid male colleague, and is higher than increases seen by men when a highly paid colleague of either gender passes away.

Exploring Mechanisms behind the Focal Result

In the theory leading up to Hypothesis 2, we provided two potential reasons why the exit of a highly paid woman might increase the earnings of her female colleagues. The first was that mentoring relationships might naturally create a transfer of skills between females in the firm, allowing women to inherit clients and new responsibilities in the organization upon the departure of a female colleague. The other explanation is a bargaining power-centric, institutional argument related to the firm's need to retain and promote females in order to maintain legitimacy

in the market for clients and potential employees. While it is difficult to differentiate between these explanations without detailed, multi-firm data on mentoring relationships, we explore moderation effects with our available variables in an effort to pin down the mechanism.

We use two variables to capture a woman's potential bargaining power – the surviving person's raw position in the firm's earnings distribution, and the percentage of the firm's highly paid attorneys that are female at the time the death occurs. If bargaining power is the primary mechanism, we should see that women in the apex of the firm's earnings distribution experience greater earnings increases following female death, and we should also see weaker increase when the firm has more women in highly paid positions.

We also identify two variables that should capture the effect of mentoring - the distance between the deceased and surviving individual in terms of position in the firm's earnings distribution and distance between the deceased and surviving individual in terms of age. Women that are younger or beneath the deceased person in the firm's earnings should be more likely to be mentees of the deceased person, and stronger effects for these individuals provide support for the mentoring argument.

First we examine the bargaining power variables. Table 12 interacts the dummies in Equation 2 with each individual's within-firm attorney earnings percentile (following trimming for lower paid support staff) at the time of the death. If lower paid attorneys are more likely to benefit following female partner death, this helps rule out the institutional argument since lower paid women would not be externally visible members of the firm and would also not be in as strong a position to benefit from gender homophily in client relationships. Note that this variable does not change over time within each individual, so its direct effect is absorbed by the individual fixed effect. Inspecting the interaction terms, we see that the increase in earnings for

women following female partner death is increasing with the surviving attorney's earnings percentile at the time of the death. This suggests that among surviving women in the firm, other highly paid females are more likely to benefit from a colleague's unexpected death. Figure 5 provides the graph of this effect. Inspecting confidence intervals, we see that the effect is only significant for women who are in the top 50% of the firm's pay distribution when their female colleague passes away. In most law firms (Parkin & Baker, 2006), these individuals will likely be partners in the organization.

Table 14 replaces the earnings percentile variable with a variable measuring the percentage of the firm's partners that are female at the time of the death. This variable arguably also helps capture bargaining power – if women's earnings increase more substantially when there are fewer highly paid females in the firm, then remaining females might be exploiting the scarcity of their group and the importance of their presence to external audiences to extract more value from the firm. We see that the increase in earnings for women following female partner death is statistically unaffected by the percentage of female partners in the firm at the time of the death. We also experimented with other measures, such as the raw number of women in the upper echelons of the firm and using different earnings percentile cutoffs to measure partner attorneys, and results remained the same. Note again that the direct effect of this variable does not vary within individual, so it is absorbed by the individual fixed effect.

Next we move to the interactions that help test the mentoring mechanism. We first use the difference in the age of the surviving and deceased individuals at the time of the death as the moderating variable. The variable is measured as (age of surviving individual at time of colleague's death - age of deceased person at time of death). If we find that this variable positively moderates the effects in Equation 2, then this weakens support for the mentoring

argument because women who are older than the deceased woman are less likely to be her mentees. Analyses for this interaction have been conducted but not yet submitted for Census Bureau review and disclosure. In unreported analyses, we indeed see this positive moderation, which suggests that women who are older than the deceased woman are more likely to see earnings increases upon her death. Note once again that the direct effect of this variable does not vary within individual, so it is absorbed by the individual fixed effect[24].

To further test the mentoring argument, we use the difference in the within-firm earnings percentile of the surviving and deceased individuals at the time of the death as the next moderating variable. Analyses for this interaction have been conducted but not yet submitted for Census Bureau review and disclosure. The variable is measured as (earnings percentile of surviving individual at time of colleague's death - earnings percentile of deceased person at time of death). If we find that this variable positively moderates the effects in Equation 2, then this weakens support for the mentoring argument because women who are higher in the firm's earnings distribtuon than the deceased woman are less likely to be her mentees. In undisclosed analyses, we indeed see this positive moderation (significant at the 5% level), which suggests that women who are higher in the firm's pay distributon than the deceased woman are more likely to see pay increases upon her death. Note once again that the direct effect of this variable does not vary within individual, so it is absorbed by the individual fixed effect.

Putting these results together, we see that three of the four interaction terms seem to support the institutional argument instead of the mentoring argument. The fourth interaction is inconclusive. Women who are older or better compensated than their deceased colleague or sit higher in the firm's raw earnings distribution are more likely to see larger pay increases upon her

[24] We have conducted analyses using the raw age of the woman when her colleague passes away. We also find positive moderation here, suggesting that older women experience a stronger earnings increase when a highly paid female passes away. Table 8 displays the results of these analyses.

death. We likely obtain these results either because these women are well positioned to inherit a female colleague's clients due to gender homophily in service relationships (Beckman & Phillips, 2005), or because a sudden relative scarcity of women and an institutional preference for diversity provide them with increased bargaining power.

Robustness tests

When estimating Equation 1, the counterfactual observations include all individuals who do not experience a death during their time in the data (the full control group), as well as individual-years for attorneys who will experience a death in later periods. These counterfactual observations are entirely appropriate when examining Equation 1.

Equation 2 is a slightly different. Here, we are comparing each of the four gender-based dummy variables to the same counterfactual observations as Equation 1. However, we noted in the description of the matching results that firms experiencing a female death (and their control observations) have significantly more women than firms that experience a male death (and their control observations). Thus, lumping individuals that experience female deaths (and their matched controls) into the same sample as individuals that experience male deaths (and their matched controls) may not be appropriate in terms of pinning down the correct counterfactual observations.

To address, we split the sample into 1) the treated and matched individuals who experience female death (the "female death sample") and 2) the treated and matched individuals who experience male death (the "male death sample"). We find results that are consistent with those presented above – women benefit most colleague death in the female death sample, while there is not a significant difference based on gender in the male death sample.

DISCUSSION AND CONCLUSIONS

In this paper, we examine whether female attorney earnings increase or decrease following the exit of a highly paid woman from the organization. We provide deductive theoretical arguments as to why one might plausibly expect either an increase or decrease in women's earnings following the departure of a highly paid female colleague. Using unexpected deaths as a quasi-experimental stand-in for departure from the firm, we find that female attorneys' earnings *increase* about 8% following the death of a highly paid female colleague. This increase is statistically higher than the increase that women experience when a male colleague dies unexpectedly, and is also statistically higher than the earnings change that men experience when a colleague of either gender dies unexpectedly.

Utilizing a variety of interactions in an effort to explain the mechanism behind this result, we find the effect is stronger when the surviving female attorney is older and more highly paid than her deceased colleague, while the density of highly paid women in the organization does not appear to have an effect. This pattern suggests that the firm's more powerful women benefit most when one of their highly paid female colleagues passes away. The increase likely stems from gender homophily in client relationships (where women are more likely to inherit the deceased woman's clients) or "window dressing" in which the firm shares more rents with surviving women since their presence in the firm is suddenly more important to the firm's ability to retain legitimacy with clients and other stakeholders.

This study has a number of limitations that caution against over-generalizing its findings but also open up exciting opportunities for future work. The first is whether the conclusions we draw from deaths can apply to more general kinds of turnover, such as dismissal or voluntary mobility to a different firm. The most important difference between death and mobility is that in

the case of death, the client relationships held by the focal individual are suddenly "up for grabs" whereas a living person who changes firms is likely to port these relationships to their next job (Baker et al., 1998). Consequently, the magnitude of the pay increases seen in this study might be smaller if we were to conduct an ideal experiment with "random" mobility instead of the second-best solution of unexpected death.

The second boundary condition of this study is related to the context. Legal services is an idiosyncratic industry where institutional processes and client relationships play a pivotal role (Lagella, 2001), perhaps more so than in other industries. The results here likely apply rather directly to other professional services contexts such as architecture, accounting, and consulting, which are a large and growing portion of the United States economy. However, in other industries where work is more collaborative and client relationships are less appropriable by single individuals, we might expect earnings to *decline* following the death of a highly paid colleague, similar to the results seen by Azoulay et al. (2010) and Oetll (2012) in their study of productivity of academic scientists following the death of a prominent coauthor. With regard to gender effects, in industries where the pressure for external legitimacy is lower, we might expect to see women's earnings decline following the death of a highly paid female colleague because internal advocacy and bargaining might take precedence over external legitimacy concerns in those industries. In addition, legal services is an industry which is statutorily required to organize in partnerships, meaning that the firm's managers are also its residual claimants. Industries that are organized in corporate forms where pay may be set by an independent board of directors are likely to have much different pay bargaining processes and thus might display different results. However, it is important to note that the underrepresentation of women in

corporate board rooms parallels that in law firm partnerships, suggesting that some of the basic mechanisms at play in this study may apply to corporate settings (Matsa & Miller, 2012).

The third limitation relates to the empirical design. The ideal design would allow us to match triads of firms – those that experience the death of a female, those that experience the death of a male, and those that experience no death – and compare outcomes across the three types. Due to lack of data, we were unable to locate sufficient matches to conduct this type of analysis. While we have done our best to parametrically account for differences across firms that might explain gender sorting and gender differences in pay, such as the percentage of women in the firm, the percentage of women in the apex of the firm's pay distribution, and the ratio of men's and women's earnings, the possibility exists that this matching process might be affecting our results. For example, it might be possible that firms that are more likely to experience a female death employ more women and thus have a more "female friendly" environment that puts women on a strong earnings trajectory.

It would also be interesting to see whether the results we see in this paper for gender also apply to other individual characteristics such as race, national origin, or sexual orientation. The data used in this provide information on race, but the small number of highly paid, non-white members of legal services firms made for insurmountable Census disclosure constraints.

Despite these limitations, this paper makes a number of contributions at the intersection of literatures examining gender inequality, careers, and inter-firm mobility. We provide evidence which suggests that firms may use gender diversity as "window dressing" to maintain legitimacy with external audiences such as clients and potential employees. This external pressure gives a paradoxic bargaining power to highly placed members of groups that are traditionally thought to be disadvantaged, such as women in law firms in the current study.

While most studies of bargaining power and individual rent appropriation focus on the uniqueness of an individual's skills and abilities (Coff, 1999) or position in a social network (Burt, 1992), the results in this paper suggest that demographic characteristics can also be a source of bargaining power as well – especially when outside audiences may judge firms based on the appearance of their upper level members. The results also have interesting implications for studies of vacancy chains (Chase, 1991). Prior studies of movement through vacancy chains have focused on the functional skills of employees who move into jobs that are left open by the departure of others. We add the insight that, similar to the Westphal and Zajac's (1996) examination of the differences between CEOs and their successors, demographic characteristics also matter.

TURNOVER, MANAGERIAL TENURE, AND ORGANIZATIONAL PERFORMANCE: EVIDENCE FROM INJURIES IN THE NATIONAL FOOTBALL LEAGUE

How does turnover, the exit of employees from the organization, affect firm performance? A long line of scholars have given careful thought to this question. While some scholars suggest that low levels of turnover can refresh the organization (Glebbeek and Bax, 2004; Siebert & Zubanov, 2009; Staw, 1980), most empirical work finds a negative relationship between the exit of employees and unit-level performance (Shaw, Gupta & Delery, 2005; Shaw et al., 2005; Kacmar et al., 2006; Stuart, 2012). One of the important theoretical rationales behind these results is that departure of employees disrupts routines and makes it harder for those who remain with the organization to do their jobs effectively (Ton & Huckman, 2008).

To this point, most work has focused on studying and quantifying the disruptive effects of turnover. In this paper, I extend this line of inquiry by examining organizational factors that may weaken or exacerbate the disruptive effects of employee exit. I focus in particular on the manager who remains with the firm after an important subordinate departs. The exit of a key

employee puts the onus on the organization's manager to craft a response that minimizes disruption and allows the organization to continue effective operations (March & Simon, 1958). Surprisingly, prior work abstracts away from the interaction between management and turnover.

I draw on theories of organizational routines in the tradition of Nelson & Winter (1982) to determine whether managers with longer organizational tenure attenuate or exacerbate the negative effect of lower level employee exit on organizational performance. On the one hand, managers with longer tenure may have superior knowledge of their firm's routines and remaining stock of human resources, allowing them to respond effectively to key employee departure. On the other hand, organizations where the manager has longer tenure may be unaccustomed to dealing with change, and well-established routines may fail after the shock of employee departure. At a broader level, this theory presents a multi-level view of the relationship between organizational stability and change, with tenure and turnover as key proxies for each of these processes. Effectively, I examine whether constancy at the managerial level of the organization may help an organization overcome departures and turbulence at the employee levels.

I implement a robust empirical design to differentiate between these two arguments. The endogenous connection between employee departure and organizational performance makes causal inference a unique challenge for empirical researchers. I overcome this issue by studying the performance of National Football League teams during the 2011 and 2012 regular seasons, using injuries to the team's most important player, the quarterback, as a quasi-random (Stuart, 2012) source of employee exit. I cast the head coach of each NFL team as the organization's top manager. In addition to the quasi-random nature of injuries, the depth of historical performance

and career data available in this context allows me to include variables that are typically omitted in organizational performance regressions in other settings.

My results support the second of the two competing hypotheses – teams with longer tenured coaches perform *worse* following quarterback injury. This result does not appear to be driven by the coach's observable quality, nor the overlap in tenure between the coach and quarterback. After considering multiple inferences related to this result, my favored interpretation underscores the complex relationship between micro-level organizational stability and change at multiple levels of the organization. When a manager has longer tenure, the organization's routines are likely to be relatively unchanged over time. Consequently, when an employee departs, the routines may be inflexible and fracture under the strain of the shift. I discuss the implications of this idea for the study of turnover, organizational routines, and human resource management in the discussion section.

Key Employee Turnover and Organizational Performance

Scholars have long demonstrated an interest in the causal effect of employee departure, or turnover, on organizational performance. Turnover captures scholars' attention because of the costs that it imposes on the organization (Hausknecht & Trevor, 2011), particularly in the short term. Employees disrupt the firm's stock of human and social capital upon their departure (Dess & Shaw, 2001), necessitating hiring and training of new employees (Kacmar et al., 2006), recalibrating the organization's routines to conform to the new employee group (Ton & Huckman, 2008), and managing the emotions of the remaining employees to ensure that their colleague's choice does not permeate to others in the organization (Felps et al., 2009).

Some scholars note that turnover can have positive consequences in the form of refreshing the organization (Abelson & Baysinger, 1984; Staw, 1980) and spreading its social

connections to other firms (Somaya, Williamson & Lorinkova, 2008). However, most studies examining the correlation between turnover and unit-level performance find a negative relationship (Batt, 2002; Glebbeek & Bax, 2006; Shaw, Gupta & Delery, 2005; Shaw et al., 2005; Kacmar et al., 2006), particularly when the exiting employees are important workers (Aime et al., 2010), such as those that are central in an organization's cooperative network (Stuart, 2012).

As March and Simon (1958) note, employee exit is a given part of organizational life. However, while the negative effect of key employee turnover is starting to gain empirical credence, few studies have addressed the organizational factors that can attenuate or exacerbate its disruption. Ton & Huckman (2008) suggest that robust organizational routines can allow employees to move through the organization without it experiencing a notable drop in productivity; however, these strong routines can limit employee creativity in the long run. Arthur (1994) provides some evidence that organizations with stronger human resource practices experience a lower correlation between turnover and organizational performance.

Hauskenecht and Holwerda (2012), in their theoretical reframing of the unit-level turnover-performance relationship, suggest that researchers would profit from focusing on the individuals that remain with the firm after leavers depart. Indeed, turnover's negative causal effects occur *after* the departed employee has gone, meaning that how the remaining members of the organization respond to the departure will determine its ultimate effect on organizational performance. While Hauskenecht & Holwerda's (2012) theoretical discussion mainly takes place within one level of the organization (e.g. the competencies of employees at the same hierarchical rung as the departing employee), looking across levels to the employee's superiors may help us understand how organizations can better manage the departure of important

employees. Inspired by Barnard's observation that one of the key executive functions is to "replace the losses that continually take place by reason of death, resignation, 'backsliding,' emigration, discharge, excommunication, ostracism." (Barnard, 1968: 227), in this paper we focus on the role of the firm's manager in buffering the firm from the negative effects of employee exit.

My theoretical conceptualization of an organization's manager focuses on the individual who has direct responsibility for the performance of the collective that an exiting employee departs (Hauskenecht & Trevor, 2011). For example, in the case of law firms, when the employee is a senior associate, the manager would be the partner in charge of the associate's office. In the case of manufacturing plants, when the employee is a production engineer, the manager would be the plant-level executive in charge of the plant's operations. In the case of sports teams, if the employee is an athlete or player, the top manager would be the head coach.

Managerial Tenure, Key Employee Turnover, and Organizational Performance

Nelson and Winter (1982) provide the important insight that routines are the genes of nearly all enduring organizations. Organizations are valued because of their ability to consistently reproduce products and services (Hannan & Freeman, 1984), and routines provide the grist for this consistency by implicitly or explicitly defining how employees conduct day-to-day tasks.

The exit of an important employee from the organization is likely to disrupt the firm's routines, particularly in the short run (Ton & Huckman, 2008). Depending on the employee's role, a replacement may need to hastily be put into place, or other employees must divide the departed person's work among themselves. In either case, the individual's departure creates a ripple effect through the routines of the organization – adjustments to the routines in which the

departed individual participated will spill over into other routines of the organization as employees reallocate time and resources to keep the organization functioning. Since many organizational routines are the result of an uncodified understanding among employees (Weick & Roberts, 1993), disruptions may occur even when resources are not stretched by the departure. Remaining workers may lack the tacit knowledge required to carry out the firm's functions as before.

The organization's manager likely plays an important role in this process. Researchers have long implicated managers in the creation and maintenance of organizational routines. For example, Barnard (1968:215) states that "the functions of executives relate to all the work essential to the vitality and endurance of an organization, so far, at least, as it must be accomplished through formal coordination." It follows then that managers who are well-versed in the organization's routines and processes may be able to shepard the organization through the disruption of routines caused by an important employee's departure.

A manager's tenure is likely an important indicator of his knowledge of organizational routines and processes. Firm-specific knowledge takes time to acquire (Dierickx & Cool, 1989), and the longer a manager spends with a firm, the more information, both tacit and explicit, that he absorbs. In addition, over time, the manager will likely shift some organizational routines and implement new ones, further deepening the connection between himself and the organization.

As a result, a longer tenured manager may be well-positioned to reduce the negative performance of the organization following employee exit. He or she may have deeper knowledge of the skills and abilities of the firm's remaining employees (as compared to a manager with lower firm-specific experience), and this may assist him or her in reallocating workers across tasks. Longer tenured managers may also have more tacit knowledge about the

organization's operations, which may limit the total loss of uncodified knowledge when an important employee exits. Finally, longer experience with remaining workers may provide the manager with knowledge about how to effectively motivate them, which may be important given that they may have to expend more effort to compensate for the absence of their former colleague.

These arguments lead to the following hypothesis:

Hypothesis 1: The negative effect of key employee departure on the performance of the organization will be lower when the organization's manager has longer tenure.

The above argument ignores a potentially harmful flip side of stability at the managerial level in the face of the change precipitated by the departure of a key employee. The logic underlying the potentially harmful side effects of stability draws on a long line of literature in organization theory. Hannan and Freeman (1984) note that organizations are valued for their accountability and reproducibility, giving organizations incentives to create structures and routines (Nelson & Winter, 1982) that maximize each of these qualities. However, they note that these same structures and routines that usually ensure the survival of an organization also make it difficult for an organization to consciously change capabilities and strategies in response to external challenges without increasing its risk of mortality. Leonard-Barton (1992) makes a similar point using the frame of "core rigidities." She argues that an organization's core competencies, normally the source of its competitive advantage, can become a liability in the firm focuses too much on the routines and processes surrounding those competencies and does not attend to new developments in the external environment.

With relative ease, we can apply this logic to managerial tenure. While longer managerial tenure may imbue a firm with valuable stability, that stability may become a liability

in the face of the sudden shock of a key employee's departure. For example, studies in the upper echelons literature find that longer tenured CEOs are slower to respond to environmental change (e.g. Miller, 1991; Wiersema & Bantel, 1992), or that firms helmed by longer tenured CEOs perform worse in dynamic industries (Henderson, Miller, and Hambrick, 2006).

Taking these two literatures together suggests that increased managerial tenure can become a liability in the face of external shifts. This danger can also extend to *internal* disturbances, such as the departure of an important lower level employee. A longer tenured manager may mean that the organizations routines remain relatively unchanged over time, increasing the amount of tacit knowledge that is required among employees to make routines function correctly. The departure of one key employee may reduce this stock of tacit knowledge so substantially that interpersonal coordination becomes difficult, and organizational performance suffers. Additionally, a longer tenured manager means that the organization does not have recent experience dealing with change at the managerial level. This ossification of routines for change (Adler, Goldoftas and Levine, 1999) in managerial personnel may make it difficult for the organization to respond to departures at the employee level.

These arguments lead to the following hypothesis, which stands in direct contradiction to the first hypothesis.

Hypothesis 2: The negative effect of key employee departure on the performance of the organization will be higher when the organization's manager has longer tenure.

DATA

2011 and 2012 National Football League Regular Season

I test these competing arguments in the context of all regular season games in the 2011 and 2012 seasons in the National Football League, the most popular league for the sport of American

football. I follow a long tradition of scholars in management, sociology, and economics that have used sports as a context to answer questions for which data acquisition is difficult using traditional organizations (see Wolfe et al., 2008 for an in-depth review of sports data in social science research).

American football is played between two teams of approximately 50 players each, of which only 11 are on the field at any given time. Of these 50 players, about 25 are starters who participate in most of the competition, and the other 25 are substitutes who enter the game when the starters are tired, injured, or ineffective. The object of the game is to score more points than the other team by moving the ball across goal lines which are spaced 100 yards apart. Teams advance the ball by running and passing. In a given season, teams play once a week for a total of 16 games.

Of the 50 players on the team, the most important is the starting quarterback. The team's gameplay flows through this player. He touches the ball nearly every time he is on the field, throwing all of the team's passes. The competitive marketplace underscores the importance of the quarterback: of the 10 highest paid players in the NFL in 2012, six were quarterbacks (Forbes, 2012). The other four play the position of defensive end, whose primary task is to harass the opposing team's quarterback into poor performance (Forbes, 2012).

Teams are managed by coaching staffs of approximately 10 individuals, the most senior of whom is referred to as the head coach. The head coach hires the other coaches, sets the team's strategy and culture, and usually serves as the team's public face. The lower level coaches train the players and determine the team's tactical approach on a week by week basis. To complete the analogy between a football team and traditional organization, one can think of the 50 players as employees and the 10 coaches as management. The team's quarterback is a key

employee carrying out tasks that directly determine organizational performance, and the head coach is analogous to a CEO, providing a strategy and culture in which employees may succeed or fail (Aime et al., 2010). In the analyses that follow, we examine whether the quality and experience of the head coach softens the blow the team experiences when its starting quarterback is unable to play due to injury.

Hosting this study in the NFL provides us with notable empirical advantages. First, it is extremely difficult for researchers to identify a casual effect of employee departure on organizational performance. Since workers do not quit firms randomly, it is difficult to tell whether turnover causes performance, performance causes turnover, or an unobserved third factor influences both processes. Injuries to athletes, on the other hand, are quasi-random events that should be less related to organizational performance than turnover while still providing a shock similar to that of a traditional departure. Indeed, athletes, coaches, and media members often file injuries under the heading of "bad luck," underscoring the relatively random nature of injuries. Stuart (2012) pioneers this convenient aspect of injuries in her study of the National Hockey League.

The second advantage of this setting is the ability to observe organizational performance objectively and over time while controlling for a host of alternative explanations. Many studies connecting turnover to firm performance use surveys to measure both constructs simultaneously or in two waves, and it is understandably difficult for researchers to obtain detailed information on other firm and individual-level variables from respondents. Here, we have an extremely objective measure of turnover in the form of injury and several objective measures of organizational performance. The longitudinal nature of the data and the number of variables

available allows me to ensure a causal ordering of variables and control for a variety of competing explanations for my findings.

The unit of analysis in this paper is the team-game. My empirical strategy is to determine whether an NFL team performs better or worse following an injury to its starting quarterback and then examine whether the tenure of its head coach impacts this main effect. I analyze data from 2011 and 2012 because these are the only two seasons for which nfldata.com, my primary data source, has information on player injuries. There are 32 teams in the NFL, each team plays 16 regular season games per year, and I have two seasons of data. This results in 1024 team-game observations.

Variables

Dependent Variable

Organizational performance – The object of an NFL game is to win by scoring more points than the opponent. Accordingly, I use *margin* of victory or defeat as my primary indicator of organizational performance. This variable is positive when a team scores more points than its opponent and negative when it scores fewer points than its opponent. Because any team's positive margin of victory is exactly offset by its opponent's negative margin of defeat, the sample mean of this variable equals zero (see Table 16). For example, the winner of a game where the score is 20-14 would have a value of 6 for this variable, while the loser would have a value of -6. In robustness checks, I use alternative measures including *win* (a dichotomous outcome) and *points scored* (since quarterbacks are mainly tasked with scoring points, this might be a more direct measure of the effect that their absence has on organizational performance). The values for these variables come from nfldata.com, a for-profit provider of data on NFL teams, and were cross-checked with values from NFL.com.

Explanatory Variables

Employee departure – This dummy variable takes a value of one in the first game that a team's starting quarterback misses due to a specific injury and returns to zero when he is no longer listed as "Out" on a team's injury report. These data come from nfldata.com and were cross checked with injury reports for the 2012 season on NFL.com. Nfldata.com obtains this information from weekly injury reports that each team is required to publish in the days leading up to a game. Players with injuries are listed as "Probable", "Questionable", "Doubtful", or "Out" on these reports. Players with the first three categorizations may play in that week's game, but players listed as "Out" will not. Table 17 lists each of the starting quarterbacks who were listed as "Out" during the 2011 and 2012 seasons. It is possible that these injuries occurred in the previous week's game, but since this is uncertain, I count a player's first game missed when his name appears on one of these reports for consistency. This measure varies at the team-game level of analysis.

Manager tenure – As discussed above, I cast a team's head coach as its top manager. A large literature in management, sociology, and economics has made this analogy since the head coach sets a team's strategy and culture and is its most visible public face (Wolfe et al., 2005). I identify a coach's first year with a team from yearly *NFL Record and Fact Books* (2011, 2012), and I measure tenure as the number of years he has spent with his current team. Only a handful of coaches in the sample were promoted from within; results do not change if I measure their tenure upon their first entry into the organization or the assumption of the head coaching role.

Controls

Part of the reason for the NFL's popularity in the United States is its attractiveness to gamblers. Based on the relative quality of two opponents, casinos and other sports gambling bookkeepers

assign a "point spread" to each game. The point spread indicates by how many points the bookkeeper expects a team to win or lose that week's game. For example, the game between the New Orleans Saints and Green Bay Packers in Week 1 of the 2011 season put the Packers at 4 point favorites, meaning that the bookkeepers expected the Packers to win by 4 points based on their evaluation of the teams' relative strengths. Given that NFL gambling is a multi-billion dollar a year industry, these bookkeepers have a strong incentive to choose these numbers wisely. This *point spread* measure makes for a very convenient control variable in our analyses, as it parsimoniously tracks the idiosyncratic talent differentials between all opponents in the data. Bookkeepers also install an *over-under* for each game, which is the total number of points that the two teams are expected to score. This serves as another convenient control in the analyses, particularly for regressions where the dependent variable is the number of points scored by the focal team. I also include a dummy variable that indicates whether the focal team is the *home team*, as home teams enjoy a number of advantages including crowd support and lack of travel. Home team status may also be correlated with holding players out of games due to injury – visiting teams may be more likely to keep injured players out of games since it is less likely that the visitors will win the game, and visitors run less risk of offending their hometown fans who prefer to see the team's best players when attending games. These values come from nfldata.com, which catalogs point spread and over-under information from nationally syndicated betting lines.

With regard to focal team control variables, while the point spread should account for talent differentials between teams, I also include *team total salary* to capture the amount of money the focal team spent on player salaries in the current season. This helps capture the team's total stock of human capital, which could be correlated with its ability to win games and

keep its quarterback from getting injured. The NFL places a ceiling on the total amount of money that teams can spend on player salaries in order to increase league competitiveness. NFL teams were allowed to spend about $120M each on player salaries in 2011 and 2012. Salary data come from sportrac.com. I also include a control for *# of other injured players*. This counts the number of players who do not play quarterback who are listed as "Out" on the current week's injury report. This control helps account for the possibility that a particularly difficult game in the previous week may have resulted in an injury to the team's quarterback while also reducing its chances of performing well in the current week. I also account for observable head coach quality using his *Career NFL win %*. I want to ensure that my measure of head coach tenure picks up the length of time the head coach has been with the team, rather than the amount of time that the quarterback has been with the team. These two may be highly related since head coaches often carefully select the team's quarterback. Accordingly, I control for *Starting QB tenure* and *Head coach / QB tenure overlap*.

Day of the week dummies help account for the time off a team may have between games (games are played primarly on Sundays, though some games take place on Monday, Thursday, or Saturday) which may affect injury recovery time. *Week of season* dummies help control for the fact that injuries are more common as the season progresses and players accumulate wear and tear that makes injuries more likely.

I also include a number of opponent-specific controls that help account for the possibility that teams may decide whether or not to play a quarterback who is marginally injured based on the strength of the current week's competition. *Opposing head coach career NFL wins, Opposing team total salary ($M),* and *Opposing team's total wins last year* help rule out this possibility.

METHODOLOGY AND RESULTS

Table 16 presents sample means and Table 17 provides correlations. Recall that the unit of analysis is the team-game. As expected, we see that the sample mean of for *margin* is zero, since every winning team's margin of victory is offset exactly by each losing team's margin of defeat. Similarly, 49.9% of the game-year observations result in a win, since there was only one tie in the NFL in 2011 or 2012. Turning to the explanatory variables, we see that 6% of teams play a game without their starting quarterback during the sample period; 2% of a team's games take place during a quarterback's first absence due to injury. We see that the sample mean of head coach win percentage is 46% and tenure is 3.46 years. Quarterbacks tend to have slightly shorter tenures than head coaches, with a mean of 3.06 years.

As expected, the sample value of point spread is zero, since one team's expected margin of victory is offset exactly by its opponent's expected margin of defeat. It is interesting to note that the average NFL team paid $111M to player salaries in this period. This number is tightly distributed, reflecting the competitive balance in the league which is controlled through the use of an upper limit that each team may spend on player salaries. We also see that the average team misses about 1.5 players per game due to injury.

Before turning to regressions, it is useful to do a simple cross tabulation of the effect of starting quarterback injury on team performance. Table 19 presents mean comparisons for probability of victory and the margin of victory/defeat based on starting quarterback injury status. We see that team missing their starting quarterback win only 18% of their games (with an average margin of defeat of 9 points); this number drops to 6% (with a 14 point margin) when it is the quarterback's first game out. These averages are different from their respective "untreated" comparison groups at $p<.0001$.

The equation we estimate is the following, where the unit of analysis is the team-game:

(3) $Y_{it} = \beta_1 QBOut_{it} + \beta_2 CoachTenure_{it} + \beta_3 CoachTenure*QBOut_{it} + \beta_4 Controls_{it} + \beta_5 Controls_{jt} + \varphi_{day} + \delta_{year} + \mu_{week} + \gamma_i + \varepsilon_{it}$

Where Y_{it} is the performance of team *i* in week *t*, $QBOut_{it}$ is an indicator variable that switches to one in the first week that team *i*'s starting quarterback first misses a game with injury and returns to zero when he is no longer listed on the injury report, $CoachTenure_{it}$ is a continuous variable, $Controls_{it}$ are the vector of team *i* control variables, $ControlsOpp_{it}$ is the vector of controls related to team *i*'s opponent, φ_{day} is a vector of dummies indicating the day of the week that the game is played (Monday, Thursday, Saturday, Sunday), δ_{year} is a dummy taking the value of one in the 2012 season, μ_{week} is a vector of dummies for each of 17 weeks of the NFL regular season, γ_i are a vector of team fixed effects, and ε_{it} is a vector of Huber-White robust standard errors adjusted for 32 clusters corresponding to each NFL team.

Table 20 presents the results of the estimation of equation one. Model 1shows that an injured starting quarterback does not statistically significantly change a team's margin of victory or defeat once we account for control variables. Model 2 estimates a linear probability model where the DV takes a value of one if the focal team wins its game[25]. We see that an injured starting quarterback reduces a team's probability of winning its game by 18%, quite an effect size, with a p-value of .03. Model 3 indicates surprisingly that quarterback injury does not have a statistically significant impact on the team's ability to score offensive points. This result may obtain because replacement quarterbacks may be more likely to make mistakes which make it easier for the team's opponents to score more points, while not necessarily inhibiting the scoring ability of his own team.

[25] A conditional logit model shows similar results; I present linear probability models to ease interpretation of coefficients. Robust standards errors help deal with heteroskedasticity, and there are few predicted probabilities that fall outside the [0,1] range.

Turning to coach tenure, we see that, conditional on control variables, teams with higher tenured coaches are more likely to lose, lose by more points, and score fewer offensive points. Unreported regressions show that this relationship is not curvilinear, as the square of coach tenure is not statistically significant and the fit of the model does not improve with the addition of the squared term. It appears that NFL teams are invigorated by a change in coach. The San Francisco 49ers, who went from six wins 10 losses in the year before Jim Harbaugh became head coach to 13-3 in 2011, his first season, provide an illustrative example, as do the Indianapolis Colts, who improved by nine wins after a coaching change in 2012.

Turning to interaction effects, we see that the interaction terms between *Head coach tenure* and starting quarterback injury are negative and significant at $p<.05$ for the outcomes of point margin and probability of victory. These results support the second of the paper's competing hypotheses – managerial tenure appears to exacerbate the negative effect of a key employee exit. Figures 6 and 7 graph these effects to aid interpretation. In Figure 1, we see that a team whose head coach has four years of tenure (approximately the sample mean) can expect to lose by about eight additional points when its quarterback is injured. This effect increases to about 12 points (a 50% increase) when we increase coach tenure by about one standard deviation. In Figure 7, we see that the probability that a team wins its game when its quarterback is injured declines about 25% when the coach has four years of tenure, and declines about 40% when the coach has eight years of tenure. This represents a 60% decline ([40-25]/25).

Robustness tests

The above models use team-level fixed effects. Since the primary variable of interest, *QB Injured*, varies at the team-game level of analysis, it is possible to use team-season fixed effects instead. These very restrictive models absorb the main effect of variables, such as *Head*

coach tenure, which do not vary within a given season for a given team. Models 1-3 in Table 21 display results from these regressions. We see that the interaction between head coach tenure and quarterback injury becomes insignificant for margin of victory or defeat but retains significance at the 10% level when the dependent variable used is probability of victory.

While the above regressions include main effect controls for observable coach quality (in the form of *Coach career NFL win %*) and co-evolution of the quarterback's team specific knowledge and routines and the coach's team-specific knowledge and routines (in the form of *QB tenure* and *Head coach / QB tenure overlap,* respectively), it is possible that the interaction term used to discern between the two competing hypotheses (*Head coach tenure * QB Injured*) picks up some of these effects. Accordingly, I estimate additional models that include an interaction of the *QB Injured* variable with each of these three additional control variables. As can be seen in Models 4-6 in Table 21, results do not change substantially from those shown in Table 20, either in effect size or statistical significance; the p-value on the interaction in Model 4 is .059.

Inspecting the credibility of the injury variable

To evaluate the credibility of the randomness of the quarterback injury variable, it is useful to inspect Table 18. A potential concern is that a team may withhold quarterbacks from ex ante unwinnable and/or meaningless games after the outcome of its season has already been decided (i.e. it is clear that the team will miss the postseason). Table 18 suggests that this latter point is probably not a concern – although most injuries occur in the latter part of the season (likely due to cumulative wear and tear on player's bodies), we do not see injuries clustered in Week 17, when teams often know whether or not they will be advancing to the postseason and

may strategically withhold players because they have already decided that the game is meaningless or unwinnable.

It is also useful to see how the effect of a quarterback's injury varies over time. For example, although a quarterback may first appear on a team's injury report in week *t,* the injury likely occurred in week *t*-1, and may have affected the team's performance at that time as well. If this is the case, we may need to alter the timing of our measurement of quarterback injury in Equation 1. Figure 8 presents the estimates of an OLS regression using a vector of dummies to capture weeks since a team's quarterback was first listed on the injury report. The counterfactual group in this regression is all team-games where the team's quarterback does not miss a game during the season due to injury. Figure 8 shows that there is a significant decline in team performance in week *t-1*. Accordingly, we re-estimate Equation 1, moving the time of treatment backward by one week for all teams that experience a quarterback injury. Results remain unchanged.

Figure 8 is itself interesting. While it makes sense that the negative effect of quarterback injury is mostly confined to the week of injury (Table 18 suggests that most injuries cause quarterbacks to miss only one game), it does not appear that teams incur a "reintegration" penalty as the quarterback rejoins the team after a week's absence. It is also important to note that teams experiencing quarterback injury are not categorically worse than other teams in the NFL during this time period. If this were the case, the line in Figure 8 would be significantly different from zero at many points on the graph, not merely those at the time of injury. This provides some additional comfort around the exogeneity of the variable.

DISCUSSION AND CONCLUSION

The purpose of this paper was to examine a research question that flows from the combination of the literatures regarding organizational routines and employee turnover – do longer tenured managers attenuate or exacerbate the negative short-term effects of key employee turnover? The answer to this question is not theoretically clear ex ante, so I developed competing hypotheses related to attenuation and exacerbation. To adjudicate between them, I collected and analyzed data from the 2011 and 2012 seasons in the National Football League, casting the team's quarterback as a key employee and the team's head coach as its top manager. Using quarterback injuries to identify quasi-random turnover events, I find support for the exacerbation argument: teams whose coaches have longer tenure exhibit *worse* performance following quarterback injury than their counterparts that have shorter tenure. These results are robust to the inclusion of numerous control variables for coach quality and quarterback tenure.

I see two primary inferences related to this result. The first relates to the microfoundations of organizational rigidity (Hannan & Freeman, 1984; Leonard-Barton, 1992). An enduring question in management research is why organizations fail to adapt to changing conditions in their competitive environments. Leonard-Barton (1992) posits that a firm's core competencies become its core rigidities as the firm becomes too routinized in today's successful processes to anticipate tomorrow's changes to its industry. The results here present a related but different explanation for a slightly different phenonmenon – organizations with longer tenured managers may become so accustomed to current routines that the skills needed to respond to change become ossified. As a result, when a problem, such as the departure of a key employee, presents itself, organizations with longer tenured managers are less able to react appropriately. While Leonard-Barton (1992) and others suggests that high performing organizations are unable

to *see* upcoming threats (Christiansen and Bower, 1996; Henderson & Clark, 1990), the results here suggest that organizations with longer tenured managers are less able to cope with problems that are within view and fully realized.

Control variables help me rule out an important alternative explanation relating to the coevolution of managerial and employee tenure. It is possible that a coach could achieve longer tenure (i.e. not get fired) by virtue of being in the "right place at the right time" with a skilled quarterback, and when that quarterback gets injured, the coach's lack of quality is revealed. Including a measure of coach and quarterback tenure overlap as well as a measure of the quarterback's tenure with the team helps me rule out this important competing explanation.

A primary contribution of this article is to add to the growing literature that underscores the hazards of organizational stability. Chen, Williams, and Agarwal (2011) note that diversifying entrants into new industries are better able than other firms to withstand technological change, perhaps due to the experience in organizational change that they gained by entering the industry in the first place. The results here suggest a similar logic at the micro level of analysis – firms that have not undergone change at the managerial level may benefit from stability, but when change occurs at the *employee* level due to turnover, the organization may suffer for its lack of experience with sudden adjustments.

The findings here also have important implications for the theory and management of employee turnover. The fact that longer tenured managers do not, on average, ameliorate the negative effect of turnover is perhaps surprising and underscores the practical challenge that turnover presents to organizations. If stability at one level of the organization does not unambiguously provide benefits in the face of disruption at other levels, organizations must carefully plan for transition events both within and across organization levels. It seems quite

likely that cross-level (i.e. manager and subordinate) analyses will continue to bear interesting fruit for researchers interested in the connection between turnover and unit-level performance. Future work examining the connection between managers and turnover could make strides well beyond those in this paper by specifying specific leader actions that may help reduce the negative effect of turnover. Do leaders who are more or less empowering of employees help buffer the negative effects of turnover? What about transformational leaders? These are interesting topics to pursue in future work.

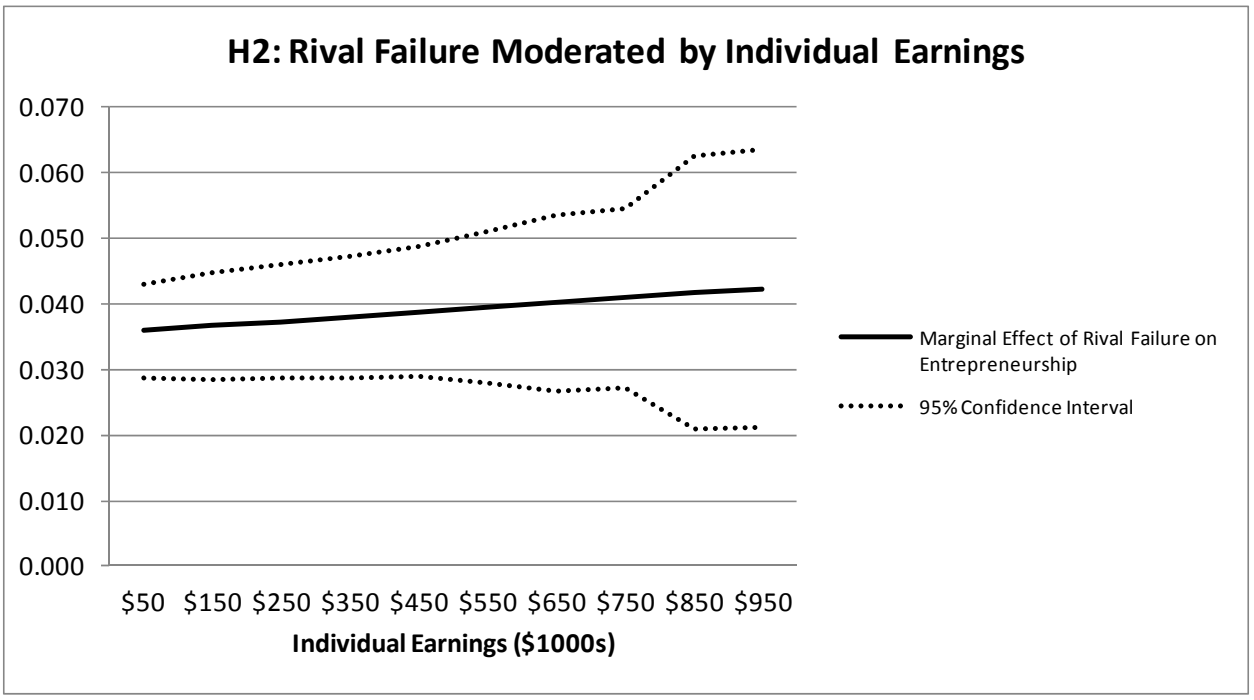
It is important to address the limitations of this study which open up other opportunities for other future scholarly work. The first is the generalizability of the setting. While professional sports is a convenient empirical laboratory for studying management questions and has been utilized by a number of prior scholars (see Wolfe et al., 2005 for a summary), it is an idiosyncratic context. The fact that individual performance is easier to observe may cause employees to behave differently than in traditional organizations. While on-field performance has the characteristic of more traditional organizational performance metrics like efficiency and productivity, the analogy to financial performance (the ultimate goal of most organizations studied by management scholars) is less clear. Additionally, injuries, while they represent a convenient empirical opportunity due to their quasi-random nature, do not have a direct analog in traditional organizations (although the CEO hospitalization events studied by Bennedson, Perez-Gonzalez & Wolfenson, 2011 are similar in character – a key employee departs and then returns to the organization in a semi-random fashion).

The second limitation is the data generating process surrounding the variable of head coach tenure. In the NFL, where head coach tenure is often short, the tenure of a head coach is determined by many of the same processes that affect his team's current performance. I have

utilized panel data and tried to include many of the omitted variables that might bias results (such as the coach's observable quality in the form of win percentage, the quality of the team in terms of its total investment in human capital, the tenure of the team's quarterback, and the co-tenure of the coach and quarterback). At a basic level, however, head coaches are not randomly assigned lengths of tenure, so inferences related to this variable must be made with caution.

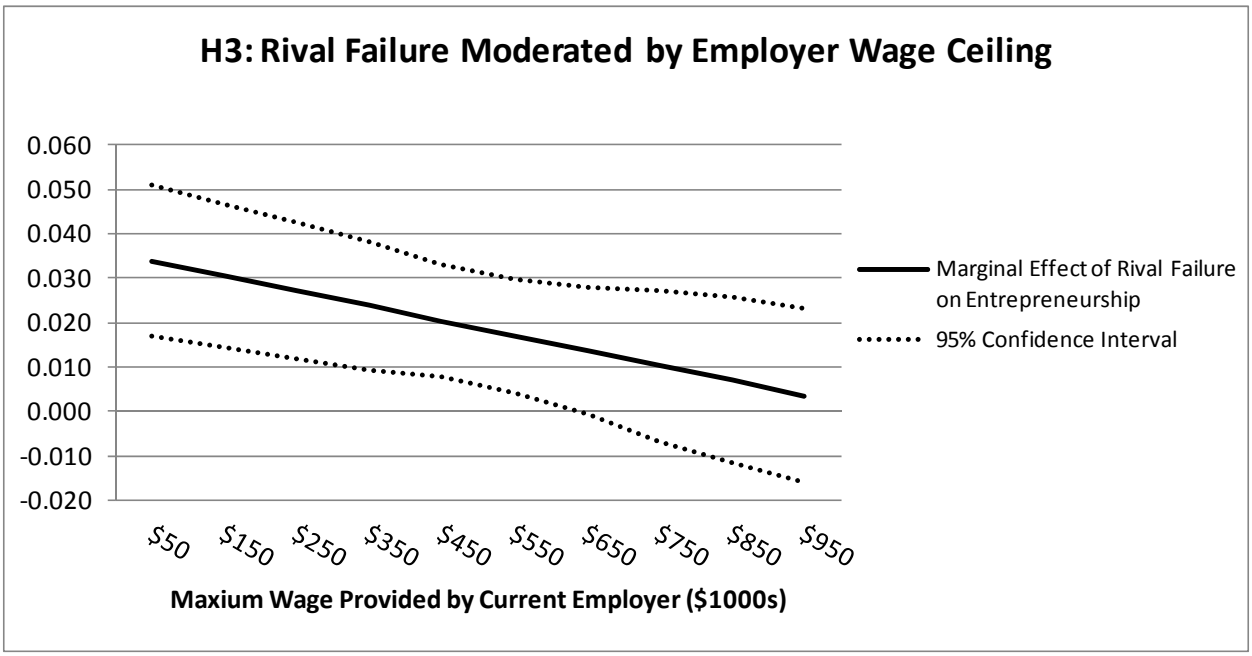
Despite these limitations, this paper makes important contributions to the study of employee turnover, the microfoundations of organizational change, and role of managers in organizational performance. The key insight that I provide is that organizational stability has a negative side effect – it ossifies the routines that organizations use and the skills that managers need to respond to sudden, disruptive change. My hope is that this study spurs additional work at the intersection of employee departure and organizational performance.

Figure 1



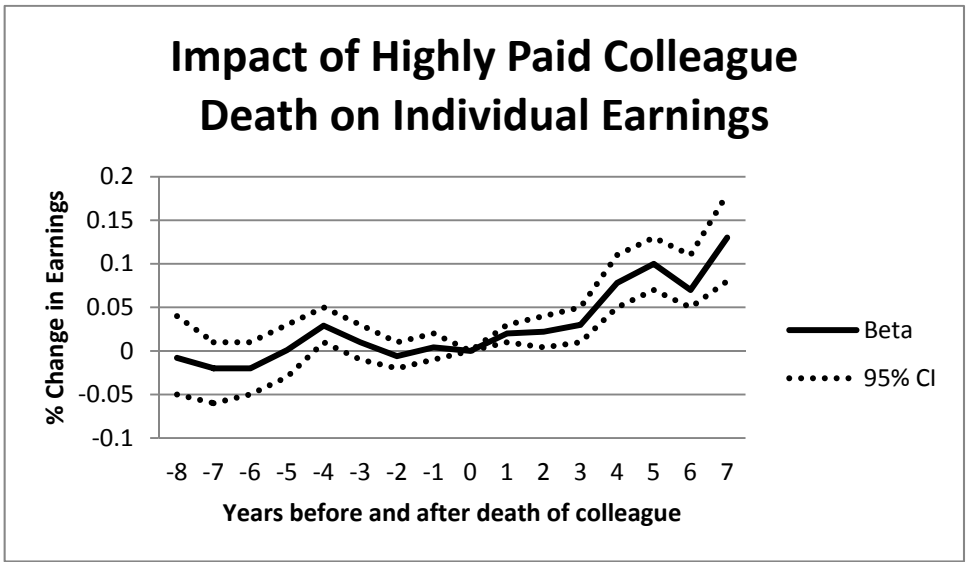
Note: Graph uses values from Table 4

Figure 2



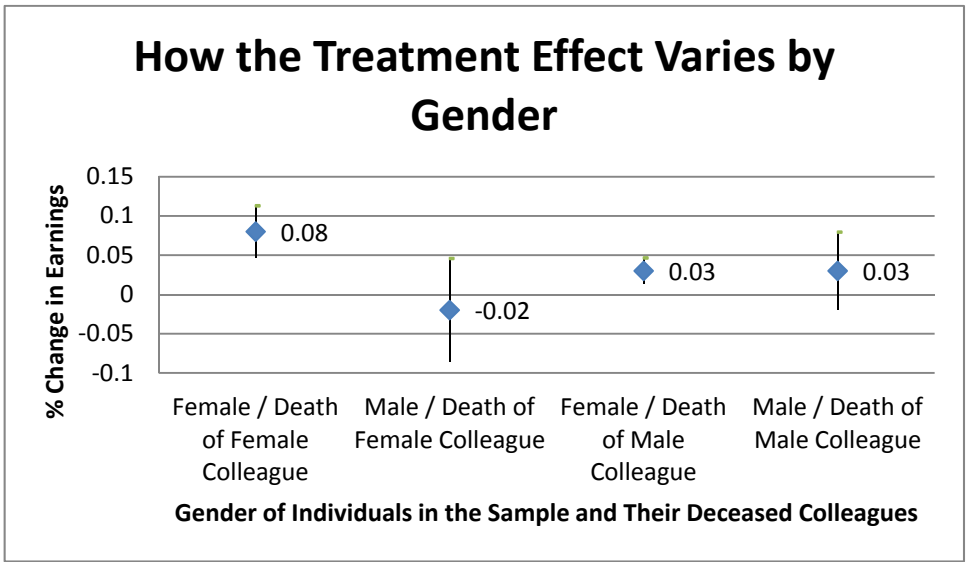
Note: Graph uses values from Table 4

Figure 3



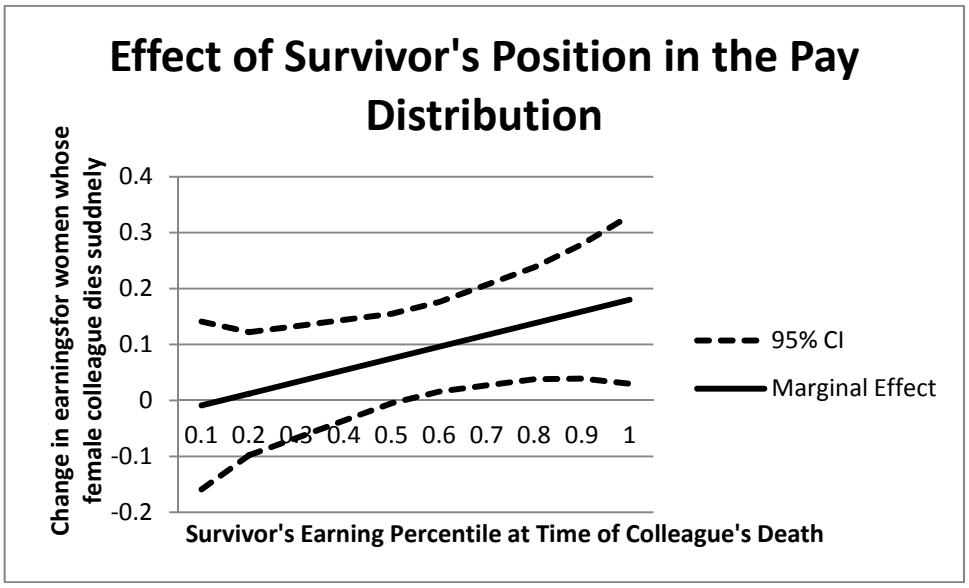
The regression underlying this figure is the same as Equation 1 (presented in Table 11), except $Death_{it}$ has been replaced by a set of interactions between 1) a dummy indicating membership in the treatment group that experiences the unexpected death of a colleague and 2) a set of dummies indicating the amount of time between the current period and the death of individual *i*'s colleague. The dependent variable is the logged value of individual *i*'s total earnings in year *t*. The regression includes individual fixed effects and robust standard errors clustered by each of the 124 firms in the treatment and control groups.

Figure 4



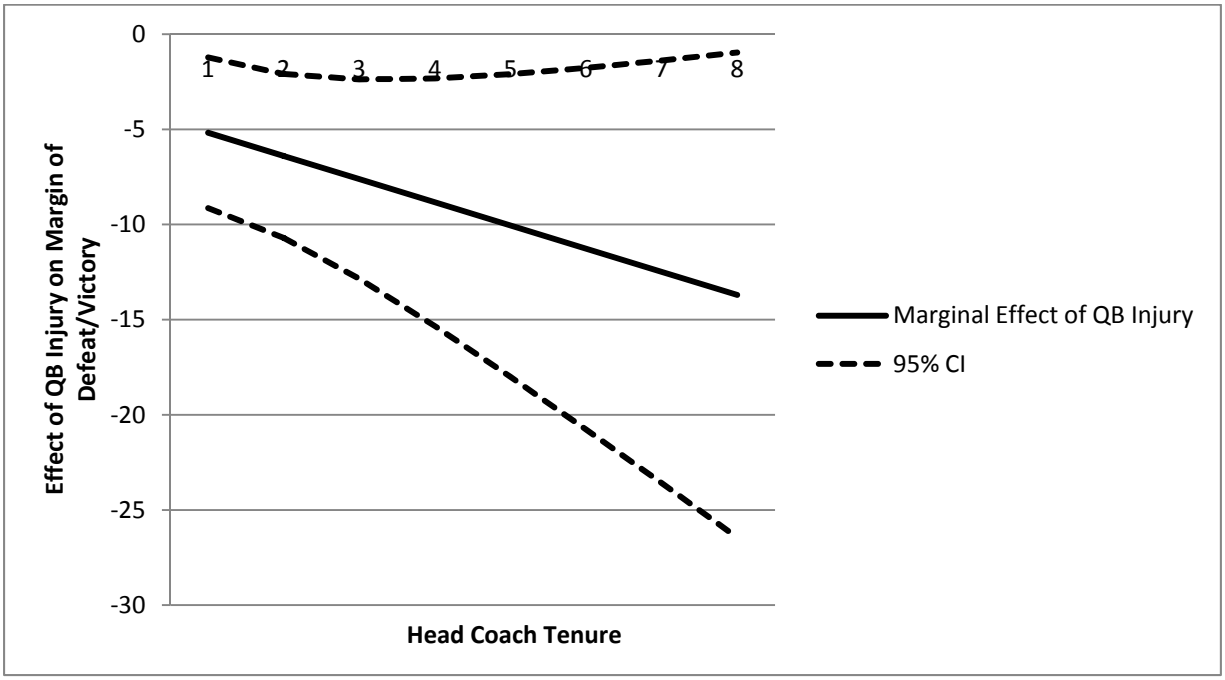
The regression underlying this figure is Equation 2, whose results are displayed in Table 12. The numbers provided correspond to the coefficient estimates in Table 12. The thin lines displayed here correspond to 90% confidence intervals for each coefficient. Lack of overlap between these lines indicates statistically significant difference in coefficients at p=.05. Wald tests indicate statistically significant differences between the female-female coefficient (β=.08) and all other coefficients at the p=.10 level.

Figure 5



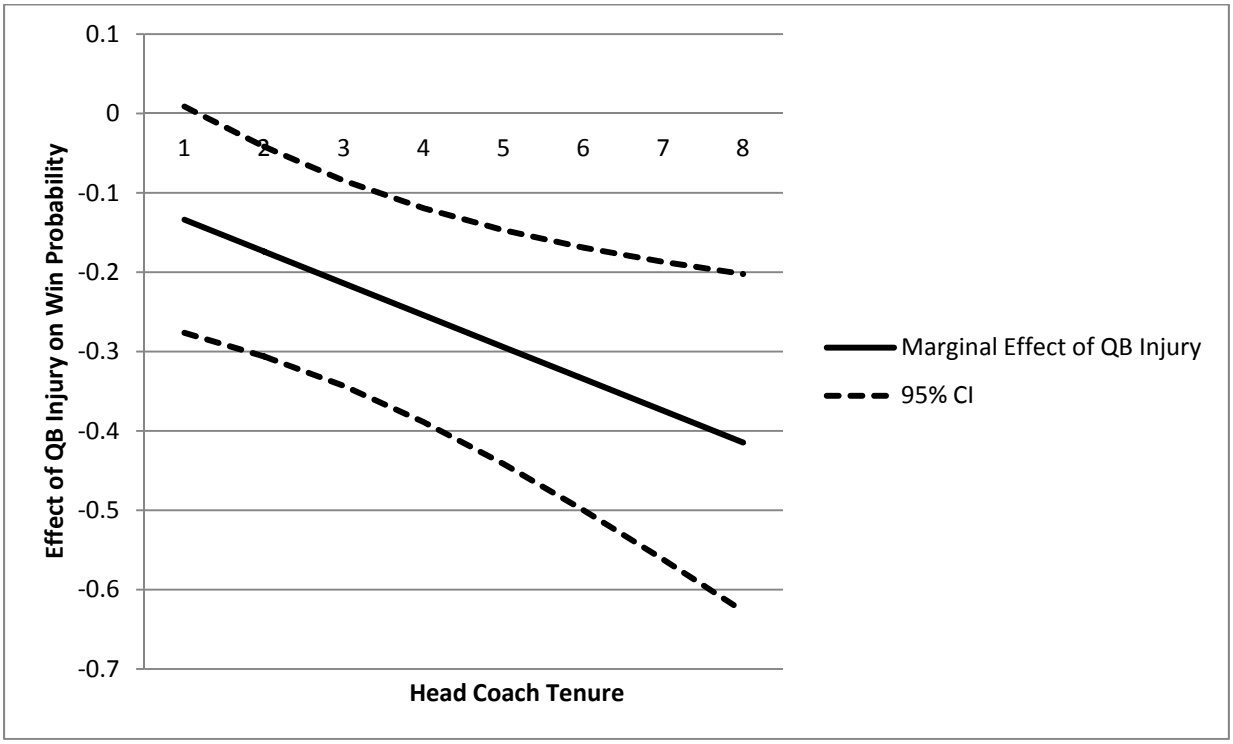
The regression underlying this Figure can be seen in Table 13.

Figure 6: Interaction of Coach Tenure and QB Injury on Margin of Defeat / Victory



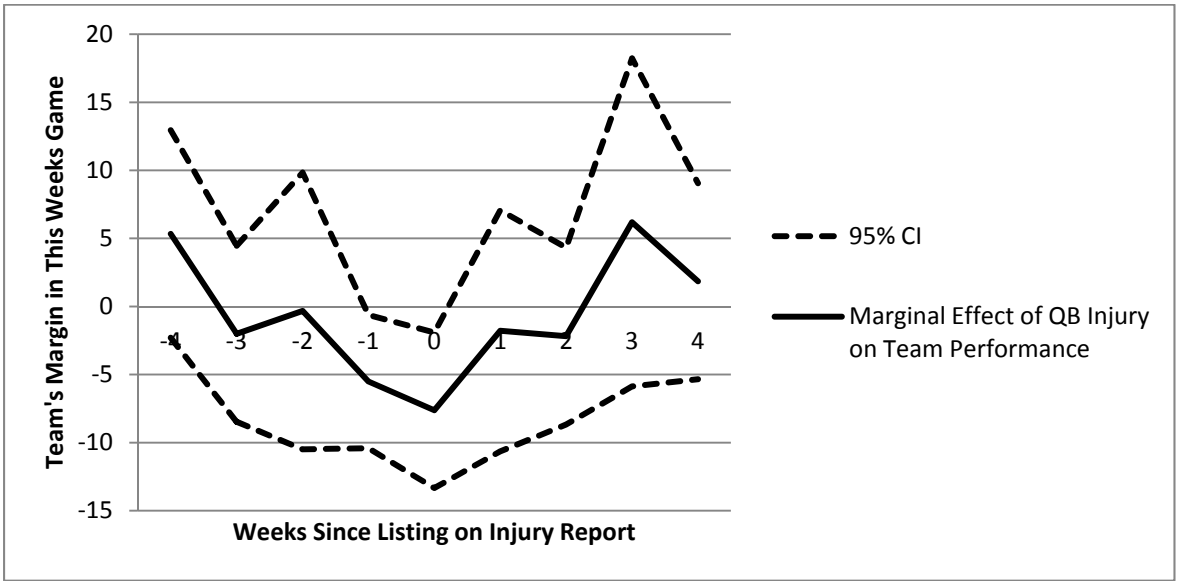
Note: Graph derived from Model 4 of Table 20

Figure 7: Interaction of Coach Tenure and QB Injury on Probability of Victory



Note: Graph derived from Model 5 of Table 20

Figure 8: How the Effect of QB Injury Varies over Time



Note: Values in chart come from an OLS regression with same control variables as Equation 3. Experimental time is measured using dummy variables that count the weeks since the team's starting quarterback was first listed on the injury report.

Table 1: Sample Means and Standard Deviations

VARIABLE NAME	Mean	Std Dev	# Obs
Dependent Variables - Individual Level			
Joins startup in following year? (Dummy)	0.009	0.094	750937
Joins different established firm in following year? (Dummy)	0.033	0.178	750937
Independent Variables			
Key Endogenous Variable - Firm Level			
# Competitor firms that fail unexpectedly[26]	<.05		750937
# Competitor firms that fail unexpectedly (weighted by # attys)	11.232	63.153	750937
Instrumental Variable - Firm Level			
# Competitor partner attorneys that die unexpectedly	0.100	0.472	750937
Other Independent Variables - Individual Level			
Annual earnings	114,798	246,050	750937
Employee age (years)	41.903	10.215	750937
Tenure (years)	4.440	2.814	750937
Partner (Dummy)	0.379	0.485	750937
Other Independent Variables - Firm Level			
Total # competitor firms that fail	0.190	0.524	750937
Total # competitor firms that fail (weighted by # attys)	23.077	638.637	750937
# Competitor firms that fail following decline	0.172	0.486	750937
# Competitor firms that fail following decline (weighted by # attys)	11.845	97.773	750937
Max annual earnings in the firm (wage ceiling)	496,644	1,282,689	750937
# Employees hired from failed firms	0.119	0.843	750937
Revenue per attorney	81,128	58,386	750937
# Attorneys	254.472	375.879	750937
Coefficient of variation (wages)	1.611	25.352	750937
Avg age, competitor employees	39.667	3.449	750937
Turnover rate, competitor employees	0.066	0.089	750937
Startup rate, competitor employees	0.012	0.052	750937
# Competitor firms	9.583	14.437	750937
Avg revenue per attorney, competitor firms	74,398	45,388	750937
# law firms in the MSA	19,859	16,356	750937
# Competitor firms (large set) that fail unexpectedly following death of partner attorney [18]	<.05		750937
# Competitor firms (large set) that fail unexpectedly w/out death of partner attorney	0.297	1.288	750937
# Competitor firms (large set) that fail following decline	3.271	8.102	750937

Each observation is an individual-year.
Note: Variable minimums and maximums excluded in compliance with disclosure standards of US Census Bureau.

[26] Precise values for this variable are suppressed because relatively small number of observations creates Census disclosure concerns

Table 2: OLS Estimates: Linear Probability Models Predicting Entrepreneurship and Mobility (Endogenous Regressors)

	(1)			(2)			(3)			(4)		
	DV: Founds startup, t+1						DV: Mobility to established firm, t+1					
Sample mean of dependent variable	0.0088						0.0328					
# Competitor firms that fail (weighted by log # attys)	-0.0014	(0.0001)	***				-0.0006	(0.0002)	**			
# Competitor firms that fail unexpectedly (weighted by log # attys)				-0.0015	(0.0001)	***				-0.0008	(0.0002)	***
# Competitor firms that fail following decline (weighted by log # attys)				-0.0004	(0.0002)					-0.0001	(0.0005)	
Annual earnings (100,000s)	-0.0001	(0.0001)		-0.0001	(0.0001)		-0.0017	(0.0002)	***	-0.0017	(0.0000)	***
Annual earnings^2	0.0000	(0.0000)		0.0000	(0.0000)		0.0000	(0.0000)	***	0.0000	(0.0000)	***
Age	-0.0014	(0.0003)	***	-0.0013	(0.0003)	***	0.0168	(0.0008)	***	0.0168	(0.0008)	***
Age^2	0.0000	(0.0000)	***	0.0000	(0.0000)	***	-0.0002	(0.0000)	***	-0.0002	(0.0000)	***
Partner (Dummy)	-0.0001	(0.0004)		-0.0001	(0.0004)		-0.0163	(0.0009)	***	-0.0163	(0.0009)	***
Max annual earnings in the firm (100,000s)	0.0032	(0.0004)	***	0.0032	(0.0004)	***	0.0014	(0.0003)	***	0.0014	(0.0000)	***
# attorneys hired from failed firms	0.0005	(0.0001)	***	0.0005	(0.0001)	***	-0.0014	(0.0003)	***	-0.0014	(0.0003)	***
Revenue per employee (100,000s)	-0.0005	(0.0009)		0.0000	(0.0000)		-0.0021	(0.0009)	*	-0.0021	(0.0000)	***
Coefficient of variation (wages)	0.0000	(0.0000)		0.0000	(0.0000)		0.0000	(0.0000)		0.0000	(0.0000)	
Avg age, competitor attorneys	0.0000	(0.0001)		0.0000	(0.0001)		-0.0001	(0.0001)		-0.0001	(0.0001)	
Turnover rate, competitor attorneys	-0.0081	(0.0015)	***	-0.0083	(0.0015)	***	-0.0232	(0.0030)	***	-0.0231	(0.0030)	***
Startup rate, competitor attorneys	0.1230	(0.0053)	***	0.1221	(0.0052)	***	-0.0232	(0.0044)	***	-0.0236	(0.0044)	***
# Competitor firms	-0.0102	(0.0005)	***	-0.0101	(0.0005)	***	0.0297	(0.0008)	***	0.0298	(0.0008)	***
Avg revenue per employee, competitor firms (100,000s)	-0.0028	(0.0005)	***	-0.0028	(0.0005)	***	0.0011	(0.0009)		0.0011	(0.0000)	***
# law firms in the MSA (100s)	0.1605	(0.0100)	***	0.1598	(0.0100)	***	-0.0666	(0.0142)	***	-0.0672	(0.0001)	***
# law firms in the MSA^2	-0.0014	(0.0001)	***	-0.0014	(0.0001)	***	0.0009	(0.0002)	***	0.0009	(0.0000)	***
Tenure and firm size dummies?	YES			YES			YES			YES		
Year dummies?	YES			YES			YES			YES		
Employee-employer fixed effect?	YES			YES			YES			YES		
Number of observations	750,937			750,937			750,937			750,937		
Number of groups	183,257			183,257			183,257			183,257		
R-squared within	0.053			0.0531			0.0514			0.0560		

Each observation is an individual-year.

Robust standard errors clustered by employee-employer dyad in parentheses. * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.

Table 3: Two Stage Least Squares Estimates: Linear Probability Models Predicting Entrepreneurship and Mobility

	(1) - First Stage			(2) - Second Stage			(3) - Second Stage		
	DV: # Competitor firms that fail unexpectedly (wtd by log # attys)			DV: Founds startup, t+1			DV: Mobility to established firm, t+1		
Sample mean of dependent variable	1.0505			0.0088			0.0328		
# Competitor partner attorneys that die unexpectedly	0.0221	(0.0024)	***						
# Competitor firms that fail unexpectedly (weighted by log # attys)				0.0382	(0.0103)	***	-0.0202	(0.0266)	
# Competitor firms that fail following decline (weighted by log # attys)				-0.0037	(0.0007)	***	-0.0014	(0.0016)	
Annual earnings (100,000s)				-0.0001	(0.0001)		-0.0017	(0.0000)	***
Annual earnings^2				0.0000	(0.0000)		0.0000	(0.0000)	***
Age				-0.0073	(0.0016)	***	0.0199	(0.0040)	***
Age^2				0.0000	(0.0000)	**	-0.0002	(0.0000)	***
Partner (Dummy)				0.0006	(0.0004)		-0.0166	(0.0010)	***
Max annual earnings in the firm (100,000s)				0.0034	(0.0005)	***	0.0013	(0.0000)	***
# attorneys hired from failed firms				-0.0001	(0.0002)		-0.0011	(0.0005)	*
Revenue per employee (100,000s)				-0.0006	(0.0009)		-0.0020	(0.0000)	***
Coefficient of variation (wages)				0.0000	(0.0000)		0.0000	(0.0000)	
Avg age, competitor attorneys				0.0000	(0.0001)		-0.0001	(0.0001)	
Turnover rate, competitor attorneys				-0.0195	(0.0032)	***	-0.0178	(0.0077)	*
Startup rate, competitor attorneys				0.0810	(0.0115)	***	-0.0034	(0.0278)	
# Competitor firms				-0.0112	(0.0005)	***	0.0303	(0.0010)	***
Avg revenue per employee, competitor firms (100,000s)				-0.0037	(0.0005)	***	0.0016	(0.0000)	***
# law firms in the MSA (100s)				0.2009	(0.0148)	***	-0.0881	(0.0003)	***
# law firms in the MSA^2				-0.0025	(0.0003)	***	0.0015	(0.0000)	***
Tenure and firm size dummies?	YES			YES			YES		
Year dummies?	YES			YES			YES		
Employee-employer fixed effect?	YES			YES			YES		
Number of observations / Number of groups	750,937 / 183,257			750,937 / 183,257			750,937 / 183,257		
R-squared within	0.05								
Kleibergen & Paap (2006) Wald F Statistic	193.867								

Each observation is an individual-year.

Robust standard errors clustered by employee-employer dyad in parentheses. * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.

Table 4: Two Stage Least Squares Estimates: Linear Probability Models Predicting Entrepreneurship and Mobility (Interactions)

		(1)	(2)	(3)	(4)
		DV: Founds startup, t+1			
Sample mean of dependent variable		0.0088	0.0088	0.0088	0.0088
Annual earnings*# Competitor firms that fail unexpectedly (weighted by log # attys)	H2		0.0004 (0.0002) †		0.0007 (0.0002) **
Annual earnings^2*# Competitor firms that fail unexpectedly (weighted by log # attys)	H2		0.0000 (0.0000)		0.0000 (0.0000)
Max annual earnings in the firm*# Competitor firms that fail unexpectedly (weighted by log # attys)	H3			-0.0029 (0.0009) **	-0.0034 (0.0010) ***
# Competitor firms that fail unexpectedly (weighted by log # attys)		0.0382 (0.0103) ***	0.0379 (0.0094) ***	0.0364 (0.0094) ***	0.0356 (0.0094) ***
Annual earnings (100,000s)		-0.0001 (0.0001)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Annual earnings^2		0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Max annual earnings in the firm (100,000s)		0.0034 (0.0005)	0.0000 (0.0000)	0.0000 (0.0000)	0.0000 (0.0000)
Control variables?		YES	YES	YES	YES
Tenure and firm size dummies?		YES	YES	YES	YES
Year dummies?		YES	YES	YES	YES
Employee-employer fixed effect?		YES	YES	YES	YES
Number of observations		750,937	750,937	750,937	750,937
Number of groups		183,257	183,257	183,257	183,257
Log likelihood (p-value of LR Test nested w/ Model 1)		930,997	931,015**	931,048**	931,061**
R-squared within		0.0526	0.0528	0.0529	0.0529

Each observation is an individual-year.
Standard errors (calculated with 10,000 bootstraps) in parentheses.
† p-value<.10, * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.
Note: Each model contains all control variables included in Table 3. # *Competitor firms that fail unexpectedly (weighted by log # attys)* uses the predicted value calculated in the first stage of Table 3.

Table 5: Multinomal Logit Model Predicting Entrepreneurship and Mobility (Robustness Test Approach)

	(1)				(2)			
	DV: Founds startup, t+1				DV: Moves to established firm, t+1			
	β	SE		Odds	β	SE		Odds
Competitor firm (large set) fails unexpectedly after death of partner atty(s)	1.9759	(0.1168)	***	7.2131	-0.1319	(0.0868)		0.8764
# Competitor firms (large set) that fail unexpectedly, no death of partner atty	0.3251	(0.0108)	***	1.3841	-0.0064	(0.0072)		0.9936
# Competitor firms (large set) that fail following decline	-0.0452	(0.0031)	***	0.9558	-0.0085	(0.0013)	***	0.9916
Annual earnings ($100,000s)	-0.0010	(0.0000)	***	0.9990	-0.0043	(0.0000)	***	0.9957
Annual earnings^2	0.0000	(0.0000)	***	1.0000	0.0000	(0.0000)	***	1.0000
Max annual earnings in the firm ($1s)	0.0001	(0.0000)	***	1.0001	0.0001	(0.0000)	***	1.0001
Includes control variables from Table 3?	YES							
Race, gender, in/out MSA, state, year dummies?	YES							
Number of observations	750,937							
McFadden's Pseudo R-squared	0.077							
Log-pseudolikelihood	-281977.21							
p-value of Hausman test, IIA Assumption	0.7668				0.45556			

Each observation is an individual-year.
Remaining with same firm is reference category. Choice to exit industry also estimated but excluded for brevity.
Robust standard errors clustered by individual in parentheses. * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.

Table 6: Multinomal Logit Model Predicting Entrepreneurship and Mobility (Robustness Test Approach - Interactions)

		(1)			(2)			(3)			(4)		
		DV: Founds startup, t+1											
Annual earnings*Competitor firm (large set) fails unexpectedly after death of partner atty(s)	H2				0.4850	(0.8070)					-0.7490	(0.6860)	
Annual earnings^2*Competitor firm (large set) fails unexpectedly after death of partner atty(s)	H2				0.0000	(0.0000)					0.0000	(0.0000)	
Max annual earnings in the firm*Competitor firm (large set) fails unexpectedly after death of partner atty(s)	H3							-0.0070	(0.0013)	***	-0.0063	(0.0013)	***
Competitor firm (large set) fails unexpectedly after death of partner atty(s)		1.9759	(0.1168)	***	2.4688	(0.4245)	***	4.1333	(0.3336)	***	4.5336	(0.4577)	***
Annual earnings ($100,000s)		-0.0010	(0.0000)	***	-0.0960	(0.0225)	***	-0.0997	(0.0229)	***	-0.0992	(0.0228)	***
Annual earnings^2		0.0000	(0.0000)	***	0.0000	(0.0000)	***	0.0000	(0.0000)	***	0.0000	(0.0000)	***
Max annual earnings in the firm ($1s)		0.0001	(0.0000)	***	0.0001	(0.0000)	***	0.0001	(0.0000)	***	0.0001	(0.0000)	***
Control variables from Table 3?		YES			YES			YES			YES		
Race, gender, in/out MSA, state, year dummies?		YES			YES			YES			YES		
Number of observations		750,937			750,937			750,937			750,937		
Log pseudolikelihood (p-value of LR Test nested w/ Model 1)		-281,977			-281,967*			-281,894*			-281,887*		
McFadden's Pseudo R-squared		0.0768			0.0768			0.0770			0.0771		

Each observation is an individual-year.
Remaining with same firm is reference category. Choice to exit industry also estimated but excluded for brevity.
Robust standard errors clustered by individual in parentheses. * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.

Table 7: OLS Estimates: Linear Probability Models Predicting Mobility and Entrepreneurship (Robustness Test Approach)

	(1)	(2)	(3)	(4)
	DV: Founds startup, t+1		DV: Mobility to established firm, t+1	
Sample mean of dependent variable	0.0088		0.0328	
Competitor firm (large set) fails unexpectedly after death of partner atty(s)	0.0172 (0.0023) ***	0.0159 (0.0024) ***	-0.0014 (0.0032)	-0.0016 (0.0032)
# Competitor firms (large set) that fail unexpectedly, no death of partner atty		0.0024 (0.0001) ***		-0.0013 (0.0002) ***
# Competitor firms (large set) that fail following decline		-0.0005 (0.0000) ***		0.0000 (0.0001)
Tenure and firm size dummies?	NO	YES	NO	YES
Control variables from Table 3?	NO	YES	NO	YES
Year dummies?	YES	YES	YES	YES
Employee-employer fixed effect?	YES	YES	YES	YES
Number of observations	750,937	750,937	750,937	750,937
Number of groups	183,257	183,257	183,257	183,257
R-squared within	0.0339	0.0560	0.0211	0.0505

Each observation is an individual-year.

Robust standard errors clustered by employee-employer dyad in parentheses. * p-value<.05, ** p-value <.01, *** p-value<.001, two-tailed tests.

Table 8: Criteria Used in the Firm-Level Matching Procedure

Theoretical Concept	**Empirical Measure**	**Coarsening Criteria**
Location	State	Exact match
Location	Metropolitan Statistical Area	Exact match
Size	# Attorneys, t-1	NALP size categories: 5-25 attys, 26-100, 101-250, 251-500, 501-1000, >1000
Size	# Attorneys, t-2	
Performance	Revenue per attorney, t-1	From $50k to $1M in $50k increments
Performance	Revenue per attorney, t-2	
Female representation	% of partners who are female, t-1	0-5%, 5-10%, 10-20%, 20-30%, 30-40%, 40-50%, 50-80%, 80-100%
Female representation	% of partners who are female, t-2	
Growth trend	Did the # of attorneys increase from t-1 to t-2?	Dummy variable

Table 9: Outcome of the Firm-Level Matching Procedure

	TREATMENT SAMPLE			CONTROL SAMPLE			
	Obs	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**	**T-Stat**
Variables Used in Coarsened Matching							
# Lawyers, t	62	48.90	92.98	62	42.92	80.63	-0.38
# Lawyers, t-1	62	44.55	84.92	62	40.98	76.01	-0.25
Avg earnings per lawyer, t	62	$100,562	$27,639	62	$98,040	$28,023	-0.50
Avg earnings per lawyer, t-1	62	$98,792	$31,307	62	$98,418	$32,110	-0.07
% Partners who are female	62	0.21	0.21	62	0.22	0.21	0.06
MSA and state	62	Exact match					
Other Key Variables							
Avg Age	62	42.33	3.92	62	41.67	4.67	-0.85
Avg Tenure (left censored)	62	2.96	1.83	62	3.10	2.00	0.40
% Turnover	62	0.06	0.10	62	0.10	0.20	0.56
% of employees who are female	62	0.58	0.35	62	0.55	0.21	-0.59
Female death?	62	0.16	0.37	62	0.16	0.37	0.00

Each observation in this table is a firm-year.
The treatment condition is a firm-year in which an individual in the top third of the firm's pay distribution dies unexpectedly.
10 of the 62 treatment observations result from unexpected deaths of female attorneys.

Table 10: Descriptive Statistics for the Full Individual-Level Regression Sample

Description	Obs	Mean	Std. Dev.
Variables Measured at the Individual Level			
Annual Earnings	47905	$102,198	$160,220
Individual age	47905	42.87	10.30
Is the worker female?	47905	0.50	0.50
Tenure (left-censored)	47905	3.61	3.16
Partner? (imputed – wage percentile >.66 and age>34)	47905	0.49	0.50
Does the worker experience a death?	47905	0.51	0.50
Did the worker experience a death in a prior period?	47905	0.25	0.43
Is the worker a female who experienced a death in a prior period?	47905	0.12	0.32
Is the worker a male who experiences a death in a prior period?	47905	0.13	0.34
Does the worker experience the death of a female in a prior period?	47905	0.02	0.14
Does the worker experience the death of a male in a prior period?	47905	0.23	0.42
Female experienced death of female colleague in a prior period?	47905	0.01	0.10
Male experienced death of female colleague in a prior period?	47905	0.01	0.10
Female experienced death of male colleague in a prior period?	47905	0.11	0.31
Male experienced death of male colleague in a prior period?	47905	0.12	0.33
Variables Measured at the Firm Level			
% Turnover	47905	0.07	0.15
Coefficient of variation	47905	1.45	5.21
Revenue per attorney	47905	$72,643	$30,588
# of Attorneys	47905	234.77	438.23
# Law firms in the MSA	47905	9982.38	7914.42
Avg Age	47905	40.35	3.13
% of firm's attorneys that are female	47905	0.50	0.17
% of firm's partners that are female	47905	0.36	0.20
Male-female wage ratio	47905	0.36	0.23

The unit of analysis in this table is the individual-year. The table includes members of the treatment and control samples.

Table 11: Ordinary Least Squares Regressions with Individual Fixed Effects

DV: Log(Taxable annual earnings)	**Coef.**	**Std. Err.**	**t**
Death occurred in this or previous period (Dummy)	0.030	0.015	1.97
Partner? (imputed)	0.279	0.011	25.89
% Turnover	0.072	0.026	2.78
Coefficient of variation	-0.001	0.000	-1.72
Revenue per attorney	2.99E-006	8.94E-007	3.34
Log(# attorneys)	0.038	0.005	7.07
# of Law firms in the MSA	-7.09E-006	4.28E-006	-1.65
# Law firms in the MSA ^2	2.59E-010	1.59E-010	1.63
Avg age of firm's lawyers	-0.013	0.003	-4.6
% of firm's lawyers that are female	0.298	0.056	5.32
% of firm's partners that are female	-0.113	0.047	-2.41
Ratio of male wages to female wages in the firm	0.011	0.024	0.44
Exits firm in next period? (Dummy)	YES		
14 tenure dummies?	YES		
18 person age dummies?	YES		
Year dummies?	YES		
Within R-Squared	0.383		
Number of individual-years	47905		
Number of unique individuals	6368		

There are the results of the estimation of Equation 1. Models include individual fixed effects. Robust standard errors adjusted for clusters in 124 treated and control firms are provided.

Table 12: Ordinary Least Squares Regressions with Individual Fixed Effects

DV: Log(Taxable annual earnings)	**Coef.**	**Std. Err.**	**t**
Female experienced death of female colleague	0.08	0.02	4.01
Male experienced death of female colleague	-0.02	0.04	-0.59
Female experienced death of male colleague	0.03	0.01	2.24
Male experienced death of male colleague	0.03	0.03	1.04
Partner? (imputed)	0.28	0.01	26.14
% Turnover	0.07	0.03	2.79
Coefficient of variation	0.00	0.00	-1.72
Revenue per attorney	2.99E-006	8.96E-007	3.33
Log(# attorneys)	0.04	0.01	7.04
# of Law firms in the MSA	-7.11E-006	4.28E-006	-1.66
# Law firms in the MSA ^2	2.59E-010	1.60E-010	1.62
Avg age of firm's lawyers	-0.01	0.00	-4.47
% of firm's lawyers that are female	0.30	0.06	5.37
% of firm's partners that are female	-0.11	0.05	-2.42
Ratio of male wages to female wages in the firm	0.01	0.02	0.44
Exits firm in next period? (Dummy)	YES		
14 tenure dummies?	YES		
18 person age dummies?	YES		
Year dummies?	YES		
Within R-Squared	0.383		
Number of individual-years	47905		
Number of unique individuals	6368		

There are the results of the estimation of Equation 2. Models include individual fixed effects. Robust standard errors adjusted for clusters in 124 treated and control firms are provided.

Table 13: Ordinary Least Squares Regressions with Individual Fixed Effects

DV: Log(Taxable annual earnings)	**Coef.**	**Std. Err.**	**t**
Female experienced death of female colleague*Within-firm earnings percentile at time of death	0.21	0.12	1.67
Male experienced death of female colleague*Within-firm earnings percentile at time of death	0.14	0.07	1.9
Female experienced death of male colleague*Within-firm earnings percentile at time of death	0.25	0.06	4.42
Male experienced death of male colleague *Within-firm earnings percentile at time of death	0.18	0.11	1.6
Female experienced death of female colleague	-0.03	0.07	-0.47
Male experienced death of female colleague	-0.11	0.07	-1.5
Female experienced death of male colleague	-0.07	0.03	-2.22
Male experienced death of male colleague	-0.09	0.06	-1.54
Partner? (imputed)	0.28	0.01	26.26
% Turnover	0.07	0.03	2.78
Coefficient of variation	0.00	0.00	-1.74
Revenue per attorney	2.98E-006	8.96E-007	3.32
Log(# attorneys)	0.04	0.01	7.04
# of Law firms in the MSA	-6.57E-006	4.19E-006	-1.57
# Law firms in the MSA ^2	2.44E-010	1.59E-010	1.53
Avg age of firm's lawyers	-0.01	0.00	-4.39
% of firm's lawyers that are female	0.29	0.06	5.2
% of firm's partners that are female	-0.11	0.05	-2.38
Ratio of male wages to female wages in the firm	0.01	0.02	0.3
Exits firm in next period? (Dummy)	YES		
14 tenure dummies?	YES		
18 person age dummies?	YES		
Year dummies?	YES		
Within R-Squared	0.383		
Number of individual-years	47905		
Number of unique individuals	6368		

These are the results of the estimation of Equation 2 with an additional interaction with the individual's within-firm earnings percentile at the time of death.
Models include individual fixed effects.
Note that the direct effect of the interacted variable is not estimated as it is absorbed by the individual fixed effect.
Robust standard errors adjusted for clusters in 124 treated and control firms are provided.

Table 14: Ordinary Least Squares Regressions with Individual Fixed Effects

DV: Log(Taxable annual earnings)	**Coef.**	**Std. Err.**	**t**
Female experienced death of female colleague*% of firm's partners that are female at time of death	0.06	0.08	0.72
Male experienced death of female colleague *% of firm's partners that are female at time of death	0.17	0.14	1.17
Female experienced death of male colleague* % of firm's partners that are female at time of death	0.10	0.05	1.95
Male experienced death of male colleague *% of firm's partners that are female at time of death	0.30	0.09	3.45
Female experienced death of female colleague	0.05	0.05	0.87
Male experienced death of female colleague	-0.09	0.07	-1.26
Female experienced death of male colleague	-0.01	0.02	-0.42
Male experienced death of male colleague	-0.06	0.04	-1.73
Partner? (imputed)	0.28	0.01	25.85
% Turnover	0.07	0.03	2.9
Coefficient of variation	0.00	0.00	-1.68
Revenue per attorney	0.00	0.00	3.32
Log(# attorneys)	0.04	0.01	7.24
# of Law firms in the MSA	0.00	0.00	-1.86
# Law firms in the MSA ^2	0.00	0.00	1.85
Avg age of firm's lawyers	-0.01	0.00	-4.42
% of firm's lawyers that are female	0.28	0.06	5.01
% of firm's partners that are female	-0.11	0.04	-2.39
Ratio of male wages to female wages in the firm	0.01	0.02	0.37
Exits firm in next period? (Dummy)	YES		
14 tenure dummies?	YES		
18 person age dummies?	YES		
Year dummies?	YES		
Within R-Squared	0.383		
Number of individual-years	47905		
Number of unique individuals	6368		

These are the results of the estimation of Equation 2 with an additional interaction with the percentage of the firm's partners (imputed) that are female at the time of the death.
Models include individual fixed effects.
Note that the direct effect of the interacted variable is not estimated as it is absorbed by the individual fixed effect.
Robust standard errors adjusted for clusters in 124 treated and control firms are provided.

Table 15: Ordinary Least Squares Regressions with Individual Fixed Effects

DV: Log(Taxable annual earnings)	**Coef.**	**Std. Err.**	**t**
Female experienced death of female colleague *Age at time of colleague death	0.0046	0.0019	2.45
Male experienced death of female colleague * Age at time of colleague death	-0.0062	0.0027	-2.28
Female experienced death of male colleague * Age at time of colleague death	0.0004	0.0008	0.52
Male experienced death of male colleague * Age at time of colleague death	-0.0041	0.0015	-2.79
Female experienced death of female colleague	-0.12	0.08	-1.38
Male experienced death of female colleague	0.25	0.13	1.96
Female experienced death of male colleague	0.01	0.03	0.39
Male experienced death of male colleague	0.21	0.06	3.60
Partner? (imputed)	0.28	0.01	26.14
% Turnover	0.07	0.03	2.76
Coefficient of variation	0.00	0.00	-1.71
Revenue per attorney	0.00	0.00	3.34
Log(# attorneys)	0.04	0.01	7.16
# of Law firms in the MSA	0.00	0.00	-1.74
# Law firms in the MSA ^2	0.00	0.00	1.67
Avg age of firm's lawyers	-0.01	0.00	-4.42
% of firm's lawyers that are female	0.30	0.06	5.35
% of firm's partners that are female	-0.11	0.05	-2.34
Ratio of male wages to female wages in the firm	0.01	0.02	0.55
Exits firm in next period? (Dummy)	YES		
14 tenure dummies?	YES		
18 person age dummies?	YES		
Year dummies?	YES		
Within R-Squared	0.383		
Number of individual-years	47905		
Number of unique individuals	6368		

These are the results of the estimation of Equation 2 with an additional interaction with the individual's age at the time of death.
Models include individual fixed effects.
Note that the direct effect of the interacted variable is not estimated as it is absorbed by the individual fixed effect.
Robust standard errors adjusted for clusters in 124 treated and control firms are provided.

Table 16: Sample Means

Variable	Obs	Mean	Std. Dev.	Min	Max
Dependent Variables					
Margin	1024	0.00	15.68	-58.00	58.00
Win?	1024	0.50	0.50	0.00	1.00
Points Scored by Team	1024	22.47	10.38	0.00	62.00
Explanatory Variables					
Starting QB Injured?	1024	0.06	0.23	0.00	1.00
Starting QB Injured? (first game out)	1024	0.02	0.12	0.00	1.00
Head coach tenure	1024	3.42	3.32	0.00	13.00
Head coach career NFL wins	1024	45.50	48.18	0.00	175.00
Head coach career NFL win %	1024	0.46	0.23	0.00	0.81
Head coach career NFL experience	1024	10.48	7.59	0.00	27.00
Controls					
Over-Under	1024	44.12	4.48	33.00	56.00
Point spread	1024	0.00	6.41	-20.50	20.50
Home team?	1024	0.50	0.50	0.00	1.00
# of other injured players (first game out)	1024	0.56	0.84	0.00	6.00
# of other injured players	1024	1.59	1.44	0.00	7.00
Team total salary ($M)	1024	$111.16	8.55	91.90	123.55
Opposing head coach career NFL wins	1024	45.44	48.25	-4.00	175.00
Opposing team total salary ($M)	1024	$111.16	8.55	91.90	123.55
Opposing team's total wins last year	1024	8.00	3.09	2.00	15.00

Note: the unit of observation is the team-game

Table 17: Sample Cross-Sectional Correlations

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1	Margin	1.00																	
2	Win?	0.77	1.00																
3	Points Scored by Team	0.76	0.58	1.00															
4	Head coach career NFL wins	0.14	0.10	0.15	1.00														
5	Head coach career NFL win %	0.10	0.09	0.07	0.46	1.00													
6	Head coach tenure	0.14	0.10	0.16	0.63	0.41	1.00												
7	Head coach career NFL experience	0.12	0.07	0.17	0.77	0.31	0.37	1.00											
8	Starting QB Injured? (first game out)	-0.11	-0.11	-0.12	-0.02	-0.01	0.03	-0.06	1.00										
9	Starting QB Injured?	-0.15	-0.15	-0.14	-0.10	0.08	-0.04	-0.16	0.51	1.00									
10	Over-Under	0.00	0.00	0.24	0.15	-0.01	0.18	0.16	-0.09	-0.11	1.00								
11	Point spread	-0.47	-0.42	-0.35	-0.19	-0.19	-0.25	-0.12	0.10	0.20	0.00	1.00							
12	Home team?	0.18	0.14	0.14	0.00	0.00	0.00	0.00	-0.08	-0.05	0.00	-0.38	1.00						
13	# of other injured players (first game	-0.02	-0.02	-0.02	0.02	0.00	0.07	0.01	0.01	0.01	0.06	0.01	-0.05	1.00					
14	# of other injured players	0.00	0.01	0.02	0.00	0.01	0.03	0.03	0.02	0.03	0.10	-0.03	-0.02	0.63	1.00				
15	Team total salary ($M)	0.16	0.11	0.19	0.06	0.04	0.01	-0.02	-0.05	-0.04	0.21	-0.25	0.00	-0.01	-0.03	1.00			
16	Opposing head coach career NFL wins	-0.14	-0.10	-0.06	0.01	0.03	-0.02	-0.01	0.01	-0.02	0.15	0.19	0.00	-0.02	-0.02	0.04	1.00		
17	Opposing team total salary ($M)	-0.16	-0.12	-0.06	0.04	0.04	0.03	0.03	0.01	0.01	0.21	0.25	0.00	0.01	-0.01	-0.08	0.06	1.00	
18	Opposing team's total wins last year	-0.15	-0.13	-0.06	0.00	0.00	-0.01	0.01	0.00	0.04	0.21	0.35	0.00	-0.07	-0.05	0.01	0.22	0.21	1.00

Table 18: List of Starting Quarterback Injuries during 2011-2012 Season

Season	First Week Out	Player	Position	Team	Opponent	Body Part Injured	Duration (Weeks)
2011	7	Jason Campbell	QB	OAK	KC	Collarbone	9
2011	8	Sam Bradford	QB	STL	NO	Ankle	1
2011	11	Matt Cassel	QB	KC	NE	Right Hand	1
2011	12	Jay Cutler	QB	CHI	OAK	Right Thumb	1
2011	13	Michael Vick	QB	PHI	SEA	Ribs	1
2011	15	Colt McCoy	QB	CLE	ARI	Head	3
2012	5	Jake Locker	QB	TEN	MIN	Left Shoulder	4
2012	6	Matt Cassel	QB	KC	TB	Concussion	1
2012	9	Brady Quinn	QB	KC	SD	Head	2
2012	7	Kevin Kolb	QB	ARI	MIN	Ribs	4
2012	11	Michael Vick	QB	PHI	WAS	Concussion	1
2012	11	Ben Roethlisberger	QB	PIT	BAL	Right Shoulder	3
2012	11	Jay Cutler	QB	CHI	SF	Concussion	1
2012	12	Byron Leftwich	QB	PIT	CLE	Ribs	2
2012	17	Brandon Weeden	QB	CLE	PIT	Right shoulder	1
2012	17	Carson Palmer	QB	OAK	SD	Ribs	1

Note: There are 16 total injury events. Byron Leftwich was starting in place of Ben Roethlisberger when Leftwich was injured in Week 12 of 2012, and Brady Quinn was starting in place of Matt Cassell when Quinn was injured in Week 9 of 2012.

Table 19: Mean Comparisons for Victory and Margin Based on Starting Quarterback Injury

	Group	Obs	Mean	Std. Err.	T-Stat	P-value
DV: Win	No QB Injury	965	0.518	0.016	5.000	0.000
	QB Injured	59	0.186	0.051		
	Combined	1024	0.499	0.016		
DV: Margin	No QB Injury	965	0.585	0.505	4.880	0.000
	QB Injured	59	-9.576	1.573		
	Combined	1024	0.000	0.490		
DV: Win	All other team-games	1008	0.506	0.016	3.538	0.000
	QB Injured (first week out)	16	0.063	0.063		
	Combined	1024	0.499	0.016		
DV: Margin	All other team-games	1008	0.224	0.493	3.653	0.000
	QB Injured (first week out)	16	-14.125	3.001		
	Combined	1024	0.000	0.490		

Table 20: Fixed Effect OLS Regressions of Regular Season Game Outcomes

	(1)			(2)			(3)			(4)			(5)			(6)		
	DV: Margin			**DV**: Win			**DV**: Points Scored			**DV**: Margin			**DV**: Win			**DV**: Points Scored		
Head coach tenure*QB Injured										-1.217	(0.601)	*	-0.040	(0.018)	*	-0.315	(0.436)	
Head coach team tenure	-3.775	(0.945)	***	-0.088	(0.028)	**	-2.131	(0.512)	***	-3.964	(0.886)	***	-0.094	(0.026)	***	-2.180	(0.494)	***
Starting QB Injured?	-2.811	(2.419)		-0.184	(0.079)	*	-0.564	(1.400)		1.168	(2.215)		-0.053	(0.071)		0.466	(1.185)	
Head coach win %	2.808	(2.564)		0.071	(0.084)		3.505	(1.317)	**	2.429	(2.569)		0.059	(0.085)		3.407	(1.287)	**
Starting QB team tenure	-4.583	(1.083)	***	-0.148	(0.031)	***	-3.641	(0.693)	***	-4.922	(1.025)	***	-0.159	(0.029)	***	-3.729	(0.645)	***
Coach/QB co-tenure	4.697	(1.033)	***	0.103	(0.031)	***	2.349	(0.593)	***	4.938	(0.967)	***	0.111	(0.028)	***	2.411	(0.579)	***
Over-Under	-0.106	(0.125)		-0.003	(0.004)		0.274	(0.078)	***	-0.123	(0.124)		-0.004	(0.004)		0.270	(0.078)	***
Point spread	-0.697	(0.108)	***	-0.025	(0.003)	***	-0.229	(0.087)	**	-0.675	(0.107)	***	-0.024	(0.003)	***	-0.224	(0.086)	**
Home team?	2.278	(0.891)	*	0.014	(0.035)		1.727	(0.766)	*	2.323	(0.883)	**	0.016	(0.035)		1.739	(0.759)	*
# of other injured players	0.160	(0.358)		0.006	(0.012)		-0.023	(0.233)		0.186	(0.345)		0.007	(0.012)		-0.016	(0.232)	
Team total salary ($M)	0.135	(0.104)		-0.003	(0.003)		0.061	(0.054)		0.117	(0.104)		-0.003	(0.003)		0.056	(0.054)	
Opposition coach NFL wins	-0.025	(0.010)	*	0.000	(0.000)		-0.008	(0.007)		-0.025	(0.010)	*	0.000	(0.000)		-0.008	(0.007)	
Opposition total salary ($M)	-0.144	(0.061)	*	-0.001	(0.002)		-0.053	(0.050)		-0.150	(0.061)	*	-0.002	(0.002)		-0.055	(0.050)	
Opposition wins last year	-0.053	(0.182)		0.002	(0.006)		-0.048	(0.127)		-0.065	(0.184)		0.001	(0.006)		-0.051	(0.127)	
Week / Year / Day dummies?	YES			YES			YES			YES			YES			YES		
Team fixed effects?	YES			YES			YES			YES			YES			YES		
R-sq within	0.1669			0.1296			0.0942			0.1704			0.121			0.0793		

Note: Unit of analysis is team-game. 1,024 observations representing all 612 regular season games played in the National Football League in 2011 and 2012. Robust standard errors clustered by team are presented in parenthesis. †p<.10,* p<.05, **p<.01, ***p,.001 Two tailed test

Table 21: Robustness Checks – Team-Season Fixed Effects and Additional Interactions

	(1)			(2)			(3)			(4)			(5)			(6)		
	DV: Margin			**DV**: Win			**DV**: Points Scored			**DV**: Margin			**DV**: Win			**DV**: Points Scored		
Head coach tenure*QB Injured	-0.981	(0.761)		-0.032	(0.018)	†	-0.107	(0.498)		-1.464	(0.747)	†	-0.051	(0.023)	*	-0.459	(0.546)	
Coach/QB co-tenure*QB Injured										2.626	(1.083)	*	0.080	(0.036)	*	1.497	(0.704)	*
Coach win % * QB Injured										-12.706	(10.392)		-0.187	(0.363)		-6.910	(6.654)	
QB tenure * QB Injured										-0.299	(0.557)		-0.026	(0.012)	*	-0.228	(0.341)	
Head coach team tenure										0.947	(0.252)	***	0.017	(0.008)	*	0.217	(0.233)	
Starting QB Injured?	-1.179	(3.575)		-0.113	(0.097)		-1.633	(2.038)		4.869	(3.896)		0.014	(0.160)		2.560	(2.569)	
Head coach win %										3.192	(3.060)		0.065	(0.102)		3.810	(1.498)	*
Starting QB team tenure										0.183	(1.394)		-0.049	(0.028)		-1.231	(0.683)	
Coach/QB co-tenure										-4.900	(0.949)	***	-0.113	(0.026)	***	-2.398	(0.574)	***
Over-Under	-0.179	(0.150)		-0.004	(0.005)		0.199	(0.093)	*	-0.108	(0.126)		-0.004	(0.004)		0.278	(0.079)	***
Point spread	-0.545	(0.116)	***	-0.021	(0.004)	***	-0.147	(0.084)		-0.680	(0.109)	***	-0.024	(0.003)	***	-0.226	(0.087)	**
Home team?	2.935	(0.905)	**	0.030	(0.035)		2.084	(0.764)	**	2.343	(0.882)	**	0.016	(0.035)		1.751	(0.760)	*
# of other injured players	0.264	(0.343)		0.007	(0.012)		-0.042	(0.221)		0.154	(0.352)		0.006	(0.012)		-0.034	(0.236)	
Team total salary ($M)										0.121	(0.108)		-0.003	(0.003)		0.058	(0.057)	
Opposition coach NFL wins	-0.027	(0.009)	**	0.000	(0.000)		-0.010	(0.006)		-0.025	(0.010)	*	0.000	(0.000)		-0.008	(0.007)	
Opposition total salary ($M)	-0.163	(0.061)	**	-0.002	(0.002)		-0.058	(0.044)		-0.153	(0.061)	*	-0.002	(0.002)		-0.056	(0.050)	
Opposition wins last year	-0.135	(0.164)		0.000	(0.006)		-0.084	(0.118)		-0.071	(0.185)		0.001	(0.006)		-0.054	(0.128)	
Week / Year / Day dummies?	YES			YES			YES			YES			YES			YES		
Team fixed effects?	NO			NO			NO			YES			YES			YES		
Team-season fixed effects?	YES			YES			YES			NO			NO			NO		
R-sq within	0.1346			0.0972			0.0581			0.1669			0.1296			0.0942		

Note: Unit of analysis is team-game. 1,024 observations representing all 612 regular season games played in the National Football League in 2011 and 2012. Robust standard errors clustered by team are presented in parenthesis. †p<.10,* p<.05, **p<.01, ***p,.001 Two tailed test

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