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Essays on Human Capital and Executive Compensation

A DISSERTATION PRESENTED
BY
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TO
BORIS GROYSBERG
PAUL HEALY
GEORGE SERAFEIM
PIAN SHU

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS
FOR THE DEGREE OF
DOCTOR OF BUSINESS ADMINISTRATION
IN THE SUBJECT OF
TECHNOLOGY AND OPERATIONS MANAGEMENT

HARVARD UNIVERSITY
CAMBRIDGE, MASSACHUSETTS
MAY 2015

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Essays on Human Capital and Executive Compensation

Abstract

The contemporary executive career looks different from the "company man" era of post World War II. At that time, executives rose almost exclusively within a single firm, learning the business over many loyal years of service. Since the 1970s, firms have progressively relied more on external markets for filling its leadership ranks. As a result, the value of executives has become increasingly defined by capabilities portable across organizational settings. External markets have less information about executive abilities compared to incumbent employers, which strengthens the influence of externally observable signals of quality on executive career opportunities and compensation. Across three studies, this dissertation empirically explores how external markets value executive human capital attributes. In particular, this work focuses on how external markets differ from incumbent employers and explores implications for executives building their careers across multiple organizations.

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Chapter 2 was co-authored with Boris Groysberg and George Serafeim.

Chapter 3 was co-authored with Paul Healy and Boris Groysberg.

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For Karen, Maddox, and Marlena. Your patience and faith in me gave me the courage and conviction to complete this undertaking. Thank you for inspiring me to be the best I can be and for supporting me through my greatest challenges. I look forward to a lifetime of exciting adventures together.

Acknowledgments

My years as a doctoral student have not been easy, but they have been rewarding. It has been a privilege to work with and learn from an amazing group of colleagues. No amount of space here is enough to completely convey my gratitude.

I am grateful to my advisor, Boris Groysberg, for showing tremendous patience, daring, and generosity in taking me on, despite the circumstances of working remotely. I could not have accomplished this without your flexibility and your advocacy. Thanks for believing that I could do this in the face of such challenges. I have learned so much from your instruction, your counsel, and your example.

To Paul Healy, thank you for your persistent encouragement to improve my work and for giving your time so generously. I am fortunate to have you among my committee members and as a collaborator.

To George Serafeim, thank you for being an incredible collaborator. The craft of research is acquired in the doing, and I derive so much energy from your enthusiasm, your encouragement, and the opportunity to work side by side with you. I wish we could have crossed paths earlier, and I look forward to continuing to work with you in the years that lie ahead.

To Pian Shu, thank you for pushing the empirics and for wonderful guidance on shaping results into compelling messages for multiple audiences. Your advice was uncannily prescient. I cannot count the number of times that the shifts in direction of my work headed in the direction of something you mentioned a couple of weeks earlier. With practice, I hope that I can one day develop instincts as sharp as yours.

As a remote scholar, I could not have coordinated the many meetings, conference calls, and feedback cycles without the help of my committee's expert assistants: Kate Connolly, Emma Caldwell, Katherine Talbot, Elizabeth Connolly, Peter Simms, and Kyle Chalker. Thank you for tracking every critical document, recommendation letter, and scheduling detail. Most of all, thanks for helping me create access to my committee. You all do an amazing job.

To Lee Fleming, my first advisor, thank you for encouraging me to sharpen my methodological training and for providing me with my first taste of research. I learned a great deal from you, and I hope our paths cross again in the near future.

To Ethan and Maly Bernstein; Ryan Buell and Tricia Multhaupt-Buell; Nathan and Emily Craig; and Hise Gibson and Nicole Gilmore. Thank you for opening up your homes to me during my many trips to Boston.

To Xiang Ao, Adam Ramey, Bill Simpson, and Sarah Woolverton in the Division of Research at HBS, thank you for providing me advice and support in quantitative methods. You have helped me become a more careful and more efficient researcher. To Sarah Eriksen and Christine Rivera in HBS Knowledge and Library Services, thank you for your support and consultations in navigating data resources.

A special thanks to Rachael Jones, who provided me with tireless research support in preparing data and advising me on designing work for Amazon Mechanical Turk. Your incredible attention to detail and quality are unmatched. To Samantha Molineaux-Graham, thank you for providing copyediting support in the preparation of this manuscript.

To John Abele, Gerry Davis, Claudio Fernández-Aráoz , Mark Jaffe, Tom Moran, Sarah Naude, Krishnan Rajagopalan, Johannes Schmettow, Andy Smith, and Matt Stanley, thank you for helping me better understand the nuances of the executive search world. Our conversations were invaluable in shaping these research questions and insights.

The doctoral student community has been a tremendous source of feedback and support. My deep appreciation goes to Ryan Buell, Nathan Craig, Hise Gibson, Andrew Hill, and Everett Spain. Thank you for your friendship and your feedback on my work through the years.

I am grateful to my parents, Horng-Jau and Guey-Meei Lin, and my sister, Bonnie Boster, for their love and support. I also express thanks to Mark and Hilde Willis, and the entire Willis and Siegmann family, for supporting my work and family.

Finally, I want to thank my wife, Karen Willis, for her support, love, and patience as I have pursued this path of scholarship. I could not have done this without you. Our children, Maddox and Marlena, also deserve acknowledgment here, too. Each day they remind me that to take joy in learning one must be fearless in the face of mistakes and unstoppable in making the next, more informed attempt. You both inspire me through your example. I love you all so much.

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Dissertation overview

This dissertation examines how individual attributes influence executive compensation and the ways in which such influences differ between internal and external labor markets.

The first paper (with Boris Groysberg and George Serafeim) explores the extent to which a firm's misconduct can affect the compensation of former managers who were not even at the scandal firm at the time of misdeeds and not involved in the scandal. Reported results suggest that the contagion of stigma may influence compensation of former managers, even in cases where their departure from

the scandalized firm predates such misconduct. The decrease in compensation is more pronounced in countries with stronger rule of law, indicating a congruence between market behavior and legal practices. Reported scandal effects are stronger for individuals who were functionally proximate to the misconduct and for individuals who held senior positions. These findings suggest that labor markets may be interpreting past associations with scandal firms to inform more than determining direct involvement in wrongdoing. The stigma from financial misconduct may extend beyond the confines of potential legal responsibility to a broader interpretation based on association with a tarnished firm.

The second paper (with Boris Groysberg and Paul Healy) investigates how external mobility influences the gender compensation gap among job-switching executives. Using proprietary data from a global search firm capturing 2034 executive placements, reported results indicate job switching narrows the gender gap by 6%. This paper explores the contention that the relative scarcity of women among the pool of talent qualified for such positions may drive higher compensation for women moving on the external market. Effects are intensified in contexts where women are scarcer, such as for high-rank placements and in industries with fewer women in manager roles. This research suggests that scarcity may create demand for women and that mobility may present an opportunity to narrow the gender pay gap.

The third paper examines the importance of externally observable individual qualifications in predicting executive compensation and how this differs between external and internal markets. It provides evidence that information asymmetries between internal and external markets drive stronger reliance on credentials such as education and work experience for determining compensation in external markets. Salaries prior to and after an external job placement for executive job switchers are modeled, and results show that credentials are nearly twice as important on the external market, explaining 9% of salary variance, compared to a contribution of only 5% in prior job settings. Additionally, this paper examines differences in importance by type of qualification and type

of executive. Experience evincing general capabilities is found to be more important than experience aligned with the industry and function of the position. The increase in importance of credentials in the external market setting is more pronounced among senior placements, where information asymmetries are higher. In addition to standard regression techniques, this paper introduces variance decomposition methods that are more congruent with the question of comparing predictor importance. Results are robust to multiple approaches.

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Scandal and Stigma: Does Corporate Misconduct Affect the Future Compensation of Bystander Managers?

2.1 Introduction

This paper explores whether or not job-seeking managers who were employed by firms involved in a financial scandal are punished by labor markets, even when such individuals were not implicated in the scandal. Results suggest that organizational stigma may reach beyond those employed at the time misdeeds are committed and may extend to former managers as well, even in cases where their departure predates the events of the scandal. These findings suggest that markets may cast a broader net around individuals connected with firms affected by scandals, extending the stigma of financial misconduct beyond potential individual responsibility to mere association with a tarnished firm.

The current paper builds on the theory of stigma as developed by Goffman in 1963 and carried forward by psychologists, sociologists, and organizational researchers. A stigma is "an attribute that

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is deeply discrediting to the individual who possesses it" (Goffman, 1963, pp. 3-99). Specifically, a stigma undermines an individual's credibility in the social role he or she is attempting to fulfill. Goffman refers to this as a discrepancy between the actual identity—the person's true self, including the stigmatized aspect—and the virtual identity, or the role being attempted.

A stigma is not an unchanging attribute but represents a relationship among the perceiver, the stigmatized, and the social roles and institutions they are engaged in. The extent to which a particular attribute is stigmatizing changes over time (Goffman's midcentury examples of stigmas include Jewishness and being a woman with an advanced degree). Even in a particular historical moment, a condition that is stigmatizing in one situation may not be so in another. Stigmas are not inherently bad, but inherently discrediting. Physical beauty or personal wealth, for example, while clearly desirable attributes, may stigmatize public figures or performing artists if their beauty or money are believed to have bought them their place in the spotlight.

Goffman identified three distinct, though sometimes overlapping, types of stigmas: physical stigmas such as disfigurement, disability, and illness; tribal stigmas such as race and religion; and flaws of character, also identified as conduct stigma (Page, 1984). Extensive research, both experimental and based on real-world outcomes, provides evidence that physical and tribal stigmas negatively affect an individual's chances in the labor market (see Hebl and Kleck, 2002; Bertrand and Mullainathan, 2004; King, Shapiro, Hebl, Singletary, and Turner, 2006; Swami, Chan, Wong, Furnham, and Tovée, 2008; Derous, Nguyen, and Ryan, 2009; Park, Park, and Kim, 2009; Moore, 2010; Agerström and Rooth, 2011; Widner and Chicoine, 2011; Derous, Ryan, and Nguyen, 2012; Ghumman and Ryan, 2013). Despite lingering and possibly unconscious prejudices, however, employment discrimination based on a person's race or physical appearance is widely considered to be morally wrong and antithetical to the practice of good business. Laws against such discrimination exist in many countries, such as the United States' Civil Rights Act of 1964 and Americans with Disabilities Act.

No such dissonance exists in the case of conduct stigma. As the term implies, conduct stigma refers to the commission of stigmatized behaviors (e.g., drug abuse, criminal activity, sexual misconduct). A crucial element of conduct stigma is that it is seen as the individual's responsibility in a way that other stigmas are not. One key element of the stigma process is the degree of control the stigmatized individual is believed to have over his or her condition. The greater the controllability, fault, blame, agency, or responsibility, the greater the stigma. Physical and tribal stigmas may generate pity, ameliorative legislation, or a search for a cure. Conduct stigmas are generally considered the fault of the actor, and the common reaction is to punish and shun.

2.1.1 Stigma in the Business Context

Bankruptcy and business failure can function as conduct stigmas. This is unsurprising given that Goffman's primary metaphor for stigma is financial: "discrediting." Stigma functions as a kind of bankruptcy of image, in which the actual self cannot make good on the implicit promises of the virtual self. Even if there is no moral wrongdoing, the responsibility of presiding over a financial failure would clearly damage a person's credibility in the role of business leader (Sousa, 2013; Simmons, Wiklund, and Levie, 2014). Sutton and Callahan find that the stigma of bankruptcy can be so strong that even executives who join a firm after the Chapter 11 filing suffer the stigma (1987).

What does it mean to "suffer stigma" in the business context? According to Link and Phelan (2001), stigma involves five core processes: labeling, stereotyping, separation, status loss, and discrimination. The stigmatized are defined as a discrete group (labeling) and assigned negative attributes (stereotyping), on the basis of which they are informally and formally segregated and denigrated. Sutton and Callahan's 1987 study of four computer firms that had filed for bankruptcy found that reactions to the managers of the firms was consistent with Goffman's analysis of more personal kinds of stigma. Managers from the bankrupt firms found that suppliers, customers, personal friends, and other business contacts were likely to disengage with the relationship partly or fully.

Business contacts often took advantage of the vulnerability to bargain for better terms. Stigmatized managers were the target of rumors (motivated both by malice and innocent attempts to make sense of the complex situation) and direct insults. Along similar lines, Molinaro describes the mood at a leadership development pilot program that launched right after a high-profile scandal had hit the news:

> A few years ago, my team and I were launching a leadership development program for a client. Just before we launched the pilot program, the company was hit with a high-profile scandal....Many of the participants admitted they were now embarrassed to work for the company. Others said they were once so proud of where they worked that they now felt empty inside. There was another group of leaders who felt a real sense of resentment; they feared that the reputation of all of the company's leaders was negatively tainted. They were already seeing a change in relationships with customers and vendors. Trust had eroded. (2014)

The dynamics of stigma do not end with the travails of the stigmatized individual. Stigma can be contagious from one individual to another, a process Goffman calls courtesy stigma. Sutton and Callahan describe how the families of managers of bankrupt companies suffer courtesy stigma: "The president of one company reported that his wife felt shunned by friends because of the bankruptcy. Similarly, the president of another bankrupt firm...was upset because his children were being teased by other children in the neighborhood about the bankruptcy" (1987, p. 294).

Courtesy stigma has been found across many different types of stigmas and many different types of interpersonal relationships. Even casual and nonmeaningful associations can evoke a courtesy stigma: People photographed with undesirable others are often judged, themselves, as less desirable (Kulik, Bainbridge, and Cregan, 2008; Pryor, Reeder, and Monroe, 2012). Such stigma may work on a reflexive fear of contagion; for example, health and service providers to stigmatized groups can

themselves suffer a courtesy stigma (Phillips, Benoit, Hallgrimsdottir, and Vallance, 2012). Kinship and group membership are even more likely to lead to courtesy stigma, as there can be rational assumption that family members share similar nature and nurture, and that voluntary associations reflect values and priorities (Pryor et al., 2012).

In this paper, we examine evidence for the extent to which misconduct on the organizational level can lead to courtesy stigmatization of the organization's employees, regardless of whether those particular individuals were involved in the scandal or not. Do markets assign blame to executives with compelling evidence of being uninvolved? We test this by examining a population of job-switching executives with work histories at firms that committed financial misrepresentation; these executives' employment clearly predates any misdeeds and they were never legally implicated in the scandal. We find that such executives nevertheless suffer detriment in compensation when moving to another position. We also introduce a moderating variable that proxies for the likelihood of labor markets punishing a manager for past fraud in the organization. Specifically, we expect that in countries with strong rule of law, the effect will be larger as these countries have stronger regulatory enforcement, better corporate governance practices, higher accounting quality systems, and better information intermediation that allows for the dissemination of information. Consistent with our hypothesis, we find that the effect on compensation is stronger in countries with stronger rule of law.

These results are consistent with at least two hypotheses. The first one suggests that labor markets naively punish all individuals who have been associated with a firm involved in a scandal. The second suggests that labor markets process more granular information and are more likely to punish individuals who were not legally implicated in the financial misconduct but who potentially had managerial responsibility over such actions. To separate the two potential explanations, we introduce two moderating variables: the function (finance or nonfinance) and the seniority of the executive. If markets process information with a certain degree of granularity, we expect that future compensation will be influenced more for finance and senior executives. Our analysis is consistent

with this. While the economic effect is nearly 6% in the overall sample, for senior executives this estimate increases to 7.3%. For finance executives, the estimate is much larger at 15.1%.

These results have important practical and theoretical implications. Career-building managers may find that their investment of human capital in firms that were reputable at the time is vulnerable to subsequent events over which they had no control. On the theoretical level, this paper examines whether labor markets assign a broader notion of stigma than Goffman's (1963) concept of conduct-based stigma allows for. Markets may apply a notion of organizational stigma that works on the principle of guilt by association, assigning negative attributes of an organization even to individuals who are likely uninvolved with misdeeds.

2.2 Prior Literature and Hypotheses

2.2.1 Organizational Stigma in a Societal Context

A major purpose of stigma of all types and levels is to define, reinforce, and police social norms. In maintaining these norms for large organizations, the stakes are high and the questions are complex. The general public is reliant on experts to translate the nature of the scandal, apportion blame, and punish wrongdoers. Wiesenfeld, Wurthmann, and Hambrick (2008) identify three such groups of "arbiters" of organizational stigma.

Social arbiters are opinion makers with the soft power of authority and social capital, and include academics, reporters, watchdog and consumer groups, professional associations, activists, and the like. Legal arbiters, including regulatory agencies and the criminal and civil court systems, determine the laws and regulations governing businesses and judge and punish malfeasance. Economic arbiters are the gatekeepers of commerce:

> Often members of the business elite themselves, economic arbiters include principal employers and directors of the failing firm, executives and directors at other firms that

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may be potential employers of elites, members of executive search firms, and other exchange partners such as venture capitalists (Wiesenfeld et al., 2008, p. 234)

Differences in the functioning of these arbiters affect how stigma plays out in different industries and nations. For example, the regulatory and media environment, as well as cultural norms around the stigma of bankruptcy, can make it more or less likely that a failed entrepreneur will start a new business (Simmons et al., 2014).

Because these arbiters' own credibility is at stake in their decisions, words, and actions, arbiters are naturally motivated to be conservative. Defending the stigmatized, as we have seen, can lead to being stigmatized oneself. In addition, the arbiters' expert pronouncements are not necessarily intended for an expert audience. Lawyers must make arguments juries can understand, reporters must write stories readers will find plausible, and politicians must present ideas that voters find palatable. This leads to what Wiesenfeld, Wurthmann, and Hambrick (2008) call "constituent-minded sensemaking," in which arbiters' judgments are based not only on their expertise but on their beliefs about what their own constituents will and will not find understandable, believable, and in line with their preferences (see also Shapiro and Neuberg, 2008).

Economic arbiters, in particular, have reason to fear loss of credibility through courtesy stigma:

> Hiring a visibly tarnished person as an executive or director would clearly be at odds with the requirement for legitimacy, and few firms would be willing to take that risk. Therefore, even if a company believed that a person had been unfairly or excessively stigmatized (and perhaps that it could secure his or her capable services for a modest wage), the firm would feel extreme social pressure not to engage that individual, especially in any leadership role. (Wiesenfeld et al., 2008, p. 243)

Hence, stigma perpetuates itself. Arbiters and other stakeholders are implicitly aware of the reality of courtesy stigma. Hence, they are quick to shun and punish the stigmatized, for fear of being

categorized along with them (Pryor et al., 2012).

Because of this vicious cycle, individuals employed by scandal companies—even if they themselves are completely innocent—are vulnerable to courtesy stigmas. Relationship asymmetries increase the vulnerability of workers at scandal companies. Organizations have motive to shift blame to individuals (Warren, 2007). Recently, experiments by Sawaoka and Monin (2014) indicate that organizational stigma is more damaging when it is the leaders of the company who have done wrong. Unethical leadership taints the reputation of the entire group more strongly than wrongdoing by individuals lower in the hierarchy, and affects the career prospects of the employees of the stigmatized organization.

2.2.2   PRIOR WORK ON FINANCIAL SCANDAL EFFECTS

Prior work finds evidence that economic arbiters do indeed experience pressure to penalize wrongdoers, particularly when accountability is clear. In the case of financial misconduct, firms captured in the GAO's[*] database of financial misstatements are more likely to replace their managers (Arthaud-Day, Certo, Dalton, and Dalton, 2006; Desai, Hogan, and Wilkins, 2006). Using U.S. Securities and Exchange Commission (SEC) Accounting and Auditing Enforcement Releases (AAERs) as an alternative measure of financial misconduct, more than 70% of firms cited in such regulatory action fired at least one manager (Feroz, Park, and Pastena, 1991). In work that carefully traces regulatory enforcement actions to named persons, penalties were found to be severe and consistent, with more than 90% of named executives removed within the regulatory enforcement period (Karpoff, Scott Lee, and Martin, 2008).

Financial misrepresentation strikes at the heart of public trust in the economic system. The institution of publicly traded firms relies on trust not in the individual virtue of the participants, but in the system of governance and oversight that includes internal fiduciary guardians, regulatory agen-

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[*]Government Accounting Office, now the Government Accountability Office.

cies, and professional standards. A failure to trace misconduct to its perpetrators and penalize them discredits the entire enterprise, so there is ample incentive to prosecute wrongdoing severely. Because the market for public securities depends on accurate financial information to make efficient capital allocation decisions, misstating financial performance is particularly damaging, cheating direct investors as well as other firms who might be more deserving of scarce capital.

Within the labor market, Fama (1980) suggests that executives with a record of poor performance or misdeeds receive fewer or less attractive job opportunities. The idea that misbehaving executives are held accountable by the labor market has empirical support. Firms associated with publicized accounting restatements (Morgan and Arthur, 2005; Arthaud-Day et al., 2006; Desai et al., 2006), citations in SEC AAERs (Feroz et al., 1991), bankruptcies (Sutton and Callahan, 1987), and business failure (Cannella Jr, Fraser, and Lee, 1995) have been linked to higher instances of manager turnover and/or subsequent career setbacks for involved individuals.

However, because organizational decisions are frequently the result of many individual actions, appropriate assignment of wrongdoing is often unclear. Regulatory investigations involving legal arbiters require protracted deliberations, yet still come up short in resolving all accountability; it stands to reason that economic arbiters in the market may not incorporate the full knowledge of who did what. Rather, they may rely on interpretations of scandal information in a way that is not "equally efficient or rational" (Fama, 1980, p. 297). Such interpretations may represent attribution that is unbiased but based on incomplete knowledge. Nevertheless, under efficient-markets assumptions, markets act in a way that detect and punish wrongdoing, though they may not catch everyone all of the time.

2.2.3   The Role of Incomplete Information

Under incomplete information, markets can err in the opposite way as well—that is, markets may mistakenly punish those who did no wrong. Evidence suggests that external labor markets may have

trouble differentiating organizational performance from managerial performance. Work investigating the fates of executives presiding over failed companies report that a very small proportion of employed executives could find subsequent comparable work (Gilson, 1989; Houston and James, 1993), including those without clear responsibility for commercial demise. Sutton and Callahan (1987) found that managers of firms going through Chapter 11 were all negatively affected, regardless of their involvement in strategy decisions. Among executive-placement professionals, the effect of firm scandal attribution may be an accepted, albeit unfair, reality, as *The Wall Street Journal* reports:

> "Unless a client tells us to look outside the envelope, we're going to show them the best of the best. And the best of the best usually don't have major entanglements in companies that have been in the spotlight in a negative way," says Michael Boxberger, Korn/Ferry International's managing director for North America. "It's guilt by association. It's unfair and it's unfortunate. That's life, too." (Rigdon, 1993)

In the absence of additional qualifying information, markets may be acting conservatively by punishing all executives who are connected with the firm. For example, while executives named in AAERs received the strongest sanctions, executives who were present at scandal firms, but not named in AAERs, were still removed with higher frequency compared to firms without AAERs (Karpoff et al., 2008). Under this logic, markets are not failing to use information; rather they use it to make inferences in a different way.

But how discerning is the market in making judgments about potentially culpable executives? Events of misconduct often unfold in ways that make the clear assignment of culpability difficult and inexact. Misconduct is often not the act of a single person, but rather a complex web of misdeeds, ignorance, and willful neglect. When, as is typical, there is confusion or ambiguity about where responsibility for scandal lies, it is unclear how the labor market reacts.

There is evidence that labor markets discriminate among levels of responsibility. Cannella, Fraser, and Lee (1995) find that managers presiding over banks succumbing to insolvency faced more severe career repercussions than peer-level "innocent bystander" executives who lost their jobs merely because they were affiliated with a member bank in a failed holding company. Karpoff, Lee, and Martin (2008) examine a sample of SEC and Department of Justice (DOJ) enforcement actions that directly named individuals charged and find evidence that such individuals faced firings, job losses, and limitations to future employment.

Managers who left companies prior to a high-profile scandal are interesting cases: They are associated with the firms connected with the scandal ("scandal firms"), however, the timing of their departure from the firm makes it highly unlikely that they have direct involvement with it. If markets penalize such managers, it would suggest that the damage created by financial scandals may extend further than originally thought. Beyond ascertaining individual conduct, markets may be using organizational stigma to attribute undesirable characteristics apart from any attribution of actual wrongdoing.

However, other work suggests that markets may not track culpability with precision. Beneish (1998) found that managers from firms that overstated performance did not suffer higher job loss rates than managers from firms that reported earnings accurately. Among firms mentioned in *The Wall Street Journal* in the "fraud" and "crimes" listings of the newspaper's index, executive turnover was not measurably higher after public disclosure (Agrawal, Jaffe, and Karpoff, 1999). Furthermore, an empirical study of bank failures in Texas suggests that executives who leave prior to business failure may be able to dodge the full brunt of the stigma of presiding over a financial collapse (Semadeni, Cannella Jr, Fraser, and Lee, 2008).

We argue that external markets may use organizational stigma to categorize and judge individual actors. These judgments may be based on something more rational than mere prejudice. Association with scandal firms may be thought to provide more information than the opportunity to commit a

specific act of misconduct. With regard to the minority of organizations that stumble in their fiduciary responsibility, external markets may use such events to question the culture of an organization in tolerating such behavior and attracting those who would commit it. Given that there are many candidates who are free of such blemishes on their records, external markets may find it efficient to use conservative practices to screen out potential wrongdoers. In such cases, former managers whose careers predate misconduct will experience negative impact on their careers, despite clear evidence precluding their involvement.

Information asymmetries in the way firms use available information to make inferences about the value of present and potential employees amplify the negative effect of association with tainted firms for executives seeking to change jobs. While employers hiring from within can observe an individual's record of prior performance and use such insight to infer fitness for new roles (Chatman, 1991; Edwards, 1991; O'Reilly, Chatman, and Caldwell, 1991), firms hiring from the external market must make decisions without such intimate knowledge. Employers know more about executives who they promote from within because they have the opportunity to observe the decisions of such managers in a variety of situations and evaluate their conduct in contexts that are deeply understood (Waldman, 1984; Greenwald, 1986). Performance ratings within firms have shown strong interperiod stability, suggesting that firms calibrate their assessment of performance with precision (Sturman, Cheramie, and Cashen, 2005). Additionally, the tools that are available to evaluate external candidates have been shown to have poor reliability among assessors (Arvey and Campion, 1982; Judge, Higgins, and Cable, 2000; Posthuma, Morgeson, and Campion, 2002). Job interviews have been shown to be strongly influenced by assessor bias based on physical appearance (Pingitore, Dugoni, Tindale, and Spring, 1994) and nonverbal cues such as eye contact and facial expressions (Dipboye, 1992).

Beyond subjective interviews and reference checks, hiring externally relies on objective, externally observable signals of quality, such as work experience, education, and formal training (Spence, 1973;

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Grannovetter, 1981). In the absence of reliable first-hand insight from prior employment relationships, external hiring has been shown to put more weight on such externally observable information such as work experience and education (Bidwell, 2011).

If work experience and academic credentials represent externally observable positive signals, association with a scandal firm can be thought to represent an externally observable negative signal. Compounding the salience effects of externally observable signals is the question of whether the information is positive or negative. Hiring firms may also disproportionally consider the import of negative signals. There is evidence that evaluators assign greater importance to negative information: Rowe's (1989) study of job interview assessments finds that the ratio of positive information required to reverse the bad impression from a single piece of negative information is greater than 2 : 1.

Over the course of an employment relationship, employees from scandal firms may be able to diminish the effects of tarnished first impressions by revealing their true attributes through their behavior. As employers learn more about the employee through direct observation of performance, the firm's evaluation of the worker's merit begins to draw more on such proprietary insights (Chatman, 1991; O'Reilly et al., 1991), becoming less reliant on externally observed credentials or stigma-driven assumptions. As such, prior employers can learn to align a manager's value with finer calibration to observed performance. Switching employers, however, can expose executives once again to being weighed by their most salient characteristics with the anticipated biases of negative information. For this reason, we predict that the effect of association with a scandal firm will surface during moves in the external labor market.

HYPOTHESIS 1 (H1): *When moving jobs, executives who have worked at firms tarnished by financial scandals yet have temporally demonstrated no direct involvement will have lower compensation than those who have not worked at scandal firms.*

Countries differ significantly in their use of managerial accountability mechanisms. Countries with strong accountability mechanisms have a full complement of legal, social, and economic arbiters including regulatory enforcement mechanisms; well-developed accounting and auditing systems to detect fraud and faithfully represent corporate performance; strong corporate governance systems; and information intermediaries that allow the efficient dissemination of information. These mechanisms indicate a high level of cultural belief in the stigma of corporate wrongdoing, as well as providing means to uncover and publicize wrongdoing. One would expect that the effect of organizational stigma on individual executives would be more severe in countries with stronger accountability mechanisms.

HYPOTHESIS 2 (H2): *Among executive job switchers placed in the external market, the negative effects from working at firms tarnished by financial scandals will be more pronounced in countries with stronger managerial accountability systems.*

As previously noted, while there is considerable pressure to be conservative in judgment, economic arbiters and the labor market do attempt to discriminate between levels of wrongdoing rather than shunning all employees of a scandal firm equally. For financial scandals, executives in finance roles may be thought to be more culpable, because they are trained to detect such misstatements and are responsible for preventing them. Even when there is temporal evidence exonerating executives, firms that stigmatize these executives would be less likely to do so if the functional background of a candidate were nonfinancial. Additionally, they would have less reason to be concerned that executives placed in jobs outside finance and accounting could perpetrate similar types of misconduct in the future.

HYPOTHESIS 3 (H3): *Among executive job switchers placed in the external market, negative effects from working at firms tarnished by financial scandals will be more pronounced among those working*

in financial roles.

Responsibility for wrongdoing accrues not only with functional responsibility, but with general oversight responsibility as well. Executives in more senior posts may reasonably be thought to have greater control over, and therefore greater ultimate accountability for, wrongdoing. Junior executives with less control may be thought less accountable, and accordingly suffer less severe career penalties (Cannella Jr et al., 1995).

Because the effectiveness of senior executives relies heavily on leading and inspiring others, a clean reputation matters for those who must credibly command a followership. Senior leaders are thought to be more representative of the firms they lead (Hambrick and Mason, 1984; Hambrick, Cho, and Chen, 1996), and hiring firms may draw stronger parallels between firms' attributes and the characters of their leaders as an executive's rank increases (Sawaoka and Monin, 2014).

Hypothesis 4 (H4): *Among executive job switchers placed in the external market, negative effects from working at firms tarnished by financial scandals will be more pronounced among senior placements.*

2.3 Methods

2.3.1 Setting

We examine this research question in the context of executives placed through a global executive search firm. Search firms play an increasingly active role in the executive labor market (McCool, 2008), and researchers have recently begun to gain access to data from such intermediaries (Hamori and Koyuncu, 2011). These data provide an opportunity to understand how such markets work and the role that these firms play. Additionally, they are a rich source of compensation data spanning diverse firms, industries, functions, and reporting ranks. Such firms have long-standing involvement

with executive placements (Hamori, 2010). More recently, a rising share of executive placement at lower levels in the hierarchy is being brokered by such intermediaries. A survey by the International Association for Corporate and Professional Recruitment reports that 54% of positions with an annual salary of $150, 000 or more are placed by executive search firms (Hamori and Koyuncu, 2011).

Because search consultants primarily view firms, rather than job seekers, as their clients, they have interests in shortlisting high-quality candidates with strong records of demonstrated achievement. If work histories with tarnished firms can cast doubt on potential candidates, search consultants may pass over such candidates in favor of others with less blemished records. As such, using search firm placement data may select out candidates with unattractive work records, creating a stronger test for uncovering a scandal effect among those strong enough to be shortlisted and placed despite tarnished work histories.

2.3.2 SAMPLE

Our data are from a leading global executive search firm, capturing 2034 executive placements across multiple functions, industries, seniority levels, and geographies from 2004 to 2011. Our data capture geography, position title, industry, company names, individual attributes such as gender and education, and compensation levels from both prior and placed positions. That these data capture two compensation levels per person is a particular strength of the data set as it allows us to observe scandal effects while keeping gender, education, and other largely individual-invariant characteristics constant through individual fixed effects. To these data, we add details of individual career histories, capturing years of employment and job title information of prior positions for each placement. These data were coded for functional classification and job rank based on reported job titles.

2.3.3 Variables

Dependent variable The outcome variable for all regression models is total cash compensation, including salary and bonus (*total.comp*). We transform compensation levels by taking the natural log to diminish the distortion caused by outliers.

Independent variables We draw on two sources to indicate whether an executive in our data has an association with a firm with a record of misdeeds. First, we draw from firms listed in the GAO database for restatements. Second, we use the list of firms cited with AAERs. Both of these sources have been used in research in the past (see Karpoff, Lee, Koester, and Martin, 2014, for examples by data source). We operationalize the condition of an executive being treated with an association to a scandal firm by flagging executives who have worked at scandal firms, but who have left to other employers prior to the firm's misconduct.

We indicate executives as treated with this condition with a binary variable, *scandal*, taking the value of 1 when the following criteria are met:

1. A firm from either the GAO database or the AAER database appears on the executive's work history.

2. The scandal takes place prior to the year of observed placement.

3. The period of the executive's employment takes place prior to the year of the observed placement.

4. The executive leaves the scandal firm prior to its misconduct.

In robustness checks, we also test *scandal* measures using only one source, either the GAO database or the AAER database (*scandal$_{GAO}$* and *scandal$_{AAER}$*).

We are careful to limit *scandal* to those who have worked for a scandal firm but who have left prior to the misconduct. This allows us to measure the impact of the negative association in cases where the individual is arguably clear of direct involvement.

Control Variables   Each executive appears twice in the data, once for their prior position and again for their placed position. An indicator variable differentiates between prior and placed positions, set to 1 for observations representing placed positions (*placed*).

We include a set of contextual controls at the firm, individual, and job level. At the job level, we include controls for the job type based on job title classification and the circumstances around the job change. As executives attain higher reporting levels, the complexity of their tasks increases, which can be reflected in higher compensation levels (Agarwal, 1981). We use job rank (*job.rank*) as well as functional association (*job.funct*) to capture effects of occupational differences. The variable *job.rank* refers to reporting level in the vertical management hierarchy. Executive job titles are coded by keywords and classified by categories including "manager," "director," "vice president," and so on. We coarsen the classification into a binary division of senior versus junior rank, with those at the C-suite level, president, and vice president levels considered senior positions (*job.rank* = 0), and the other positions considered junior (*job.rank* = 1). The variable *job.funct* refers to the standard classification of departments by business function. Executive job titles are coded by function in this horizontal classification into categories including finance, human resources, IT, marketing, operations, and so on (Figure 2.1 for a list of functions and share represented in the data).

At the firm level, we control for the firm's industry (*industry*) using the 2012 two-digit North American Industry Classification System (NAICS) code (see Figure 2.2 for a list of industries and share represented in the data). We control for the firm's size (*firm.size*) by the number of employees, measured as a category variable capturing size classifications of less than 500, 501 to 1000, 1001 to 5000, 5001 to 10,000, and greater than 10,000. We also control for whether or not the company is
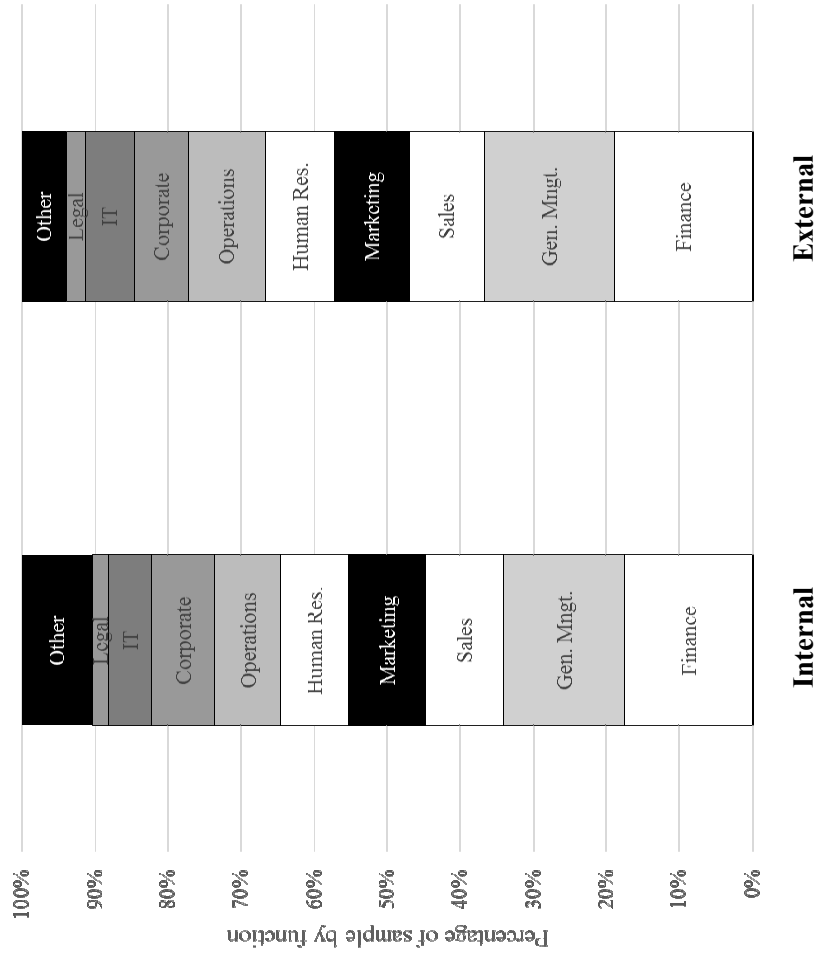
Figure 2.1: Function breakdown of the sample

publicly traded (*public*).

A common feature of executive pay is that a sizable portion may be contingent on performance. Such practices can impact pay levels, as greater personal risk generally demands higher compensation (Bonin, Dohmen, Falk, Huffman, and Sunde, 2007). We control for performance-based compensation practices (*perform.comp*) by measuring the proportion of annual cash compensation assigned to bonuses.

Because these data are global, we also control for the region (*region*) of the job placement. *region* groups job placements into Africa, Asia, Europe, North America, South America, and Australia/New Zealand. Finally, to control for macroeconomic trends, we also include dummies to capture year effects (*year*), from 2004 to 2011.

2.3.4   Model Specification

To control for individual differences, we use a fixed effects specification:

$$
\begin{aligned}
log(total.comp)_{it} \;=\; & \alpha_i + \beta_1 placed_t + \beta_2 (scandal_i \times placed_t) \\
& + \beta_3 performance.comp_{it} + \beta_4 job.rank_{it} \\
& + \beta_5 job.funct_{it} + \beta_6 firm.size_{it} + \beta_7 public_{it} \\
& + \beta_8 industry_{it} + \beta_9 region_{it} + \gamma_i + \varepsilon_{it}
\end{aligned}
\tag{2.1}
$$

By interacting *scandal* with *placed*, we can estimate how *scandal* affects compensation during observed job placements relative to its effect in the placement position. Because *scandal* is invariant with time, we cannot estimate it directly in fixed effects regression, but only the difference that it creates during a job change (Allison, 2009); this difference is captured by the *scandal* × *placed* interaction term. Standard errors are clustered at the individual level to mitigate serial correlation within individuals.
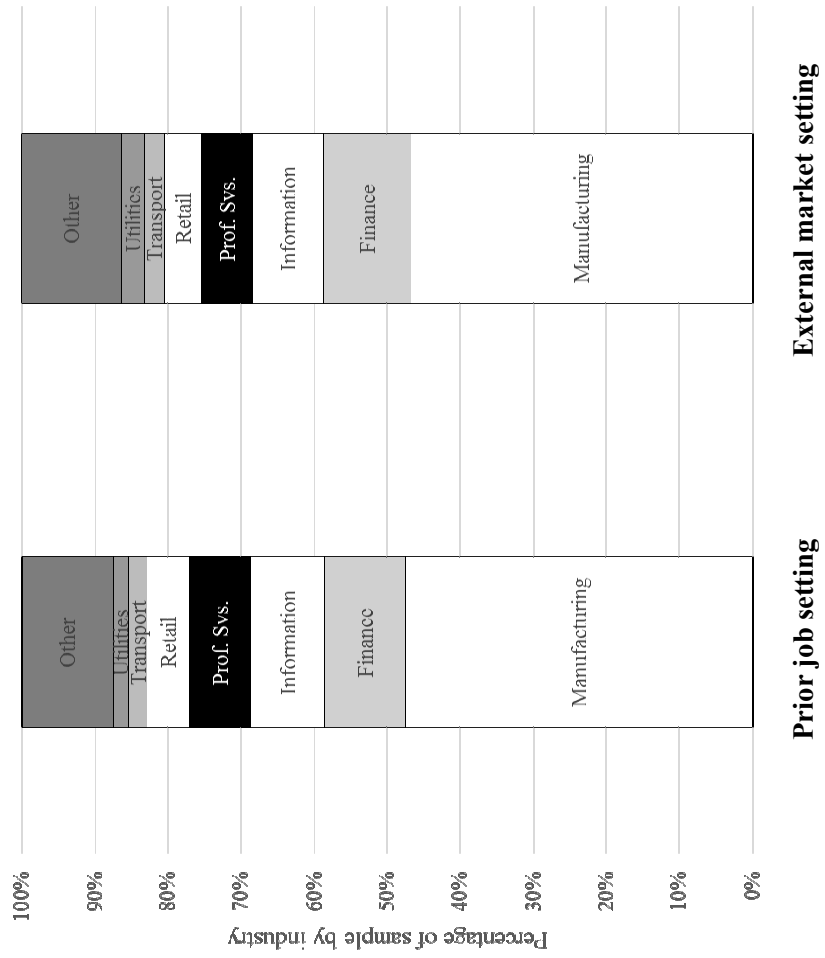
Figure 2.2: Industry breakdown of the sample

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2.4 Results

Summary statistics and bivariate correlations for numerical and binary variables are reported in Table 2.1. The log of *total.comp* is characterized by the expected bell-shaped distributions. Mean total compensation for prior and placed job is $294,000 and $331,000, respectively. The average bonus as a percentage of total compensation is 18% for prior jobs and 21% for placed jobs. The average executive has had almost 19 years of professional experience. Of the placed executives, 18% are women, a notably higher proportion compared to data sets relying exclusively on C-level executives. Nearly half the placements are for positions where executives are not C-level, presidents, or vice presidents, indicated by the mean value on *job.rank* of 0.48. A total of 18% of the observed executives have worked at a firm marked by misdeeds captured in either the GAO database or the AAER database. The GAO database captures only restatements and the AAER database captures SEC enforcement actions that have been flagged with an AAER designation (Karpoff et al., 2014). These events are distinct, and capture different firms. In our data, 205 unique firms have AAER transgressions and 147 have been identified in the GAO database for restatements. Only 44 firms appear in both sets of data. A total of 308 firms in our data appear on one or both of these lists, with 359 executives flagged with a scandal firm in their work history.

Table 2.1: Summary statistics and bivariate correlations

	var. no	Obs.	Mean	SD	var. 1	var. 2	var. 3	var. 4	var. 5	var. 6	var. 7	var. 8
total.comp – placed	1	2034.00	330.90	259.97								
total.comp – prior	2	2034.00	293.93	388.03	0.34***							
scandal	3	2034.00	0.18	0.38	0.01	0.02						
perform.comp – placed	4	2034.00	0.21	0.15	0.44***	0.19***	0.06**					
perform.comp – prior	5	2034.00	0.18	0.17	0.18***	0.19***	0.05*	0.28***				
job.rank – placed	6	2034.00	0.48	0.50	-0.22***	-0.16***	-0.06**	-0.16***	-0.11***			
job.rank – prior	7	2034.00	0.58	0.49	-0.23***	-0.16***	-0.04*	-0.15***	-0.17***	0.48***		
public – placed	8	2034.00	0.53	0.50	-0.01	0.00	0.04	0.03	0.00	0.06**	0.08***	
public – prior	9	2034.00	0.60	0.49	0.03	-0.02	0.02	0.08***	0.05*	-0.04	0.03	0.24***

2.4.1   Main Effects From Associations With Scandal Firms

Table 2.2 uses the fixed effects specification described in Equation 2.1. Estimates of the impact of *scandal* on compensation for job placements are consistent and significant at the $p < 0.05$ level. Model 1 includes a variable for *placed* and the interaction term, *scandal* × *placed*. The *scandal* variable, invariant with time, is not explicitly included, as it is absorbed into the intercept, leaving the coefficient of *scandal* × *placed* to estimate the incremental compensation effect of *scandal* in the placement condition over the prior condition. The inclusion of individual fixed effects means that stable individual traits, such as gender, education, and prior professional experience are accounted for in all of Table 2.2. In Model 1, the −0.054 coefficient of *scandal* × *placed* is significant, representing a 5.4% negative impact on placement compensation for those with scandal firms in their work history. This estimate is unchanged by the inclusion of *region* dummies in Model 2. The inclusion of *industry* dummies in Model 3 and firm level controls (*firm.size*, *public*) in Model 4 increases the magnitude of the effect slightly, to a 6.0% negative impact. The coefficient *scandal* × *placed* in Model 5 and 6 is −0.058, materially unchanged by the final introduction of job-level controls in Model 5 (*job.rank*, *job.funct*) and Model 6's addition of the *perform.comp* control.

2.4.2   Are Effects More Pronounced in Countries With Stronger Regulation Systems?

To test how *scandal* may differ by country, we include a country-level measure of the strength of accountability systems. We use a measure published by the World Bank in its Worldwide Governance Indicator metrics: the percentile rank score for the measured score on rule of law, capturing "perceptions of the extent to which agents have confidence in and abide by the rules of society, and in particular the quality of contract" (World Bank, 2015). The variable *strong.reg* is set to 1 if the country score for both the prior and placed job is above the mean score, and 0 if below the mean. Table

Table 2.2: Fixed effects regressions

	(1) $log(total.comp)$	(2) $log(total.comp)$	(3) $log(total.comp)$	(4) $log(total.comp)$	(5) $log(total.comp)$	(6) $log(total.comp)$
$scandal \times placed$	-0.054*	-0.054*	-0.058*	-0.060*	-0.058*	-0.058*
	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)	(0.024)
$placed$	0.200***	0.201***	0.203***	0.207***	0.204***	0.178***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)
$perform.comp$	No	No	No	No	No	Yes
$job.rank$	No	No	No	No	Yes	Yes
$job.funct$	No	No	No	No	Yes	Yes
$firm.size$	No	No	No	Yes	Yes	Yes
$public$	No	No	No	Yes	Yes	Yes
$industry$	No	No	Yes	Yes	Yes	Yes
$region$	No	Yes	Yes	Yes	Yes	Yes
Observations	4068	4068	4068	4068	4068	4068
R^2	0.132	0.135	0.150	0.160	0.170	0.280

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

2.3 reports results that test H2's contention that executives will face greater penalties for their associations with scandal firms in countries with stronger accountability systems. Model 1 (from Table 2.3) uses the same fixed effects specification from Model 6 in Table 2.2 on the partition of data representing placements in countries with average rule of law percentile scores higher than the mean score (*strong.reg* = 1). In this model, the coefficient of *scandal* × *placed* indicates a slightly higher penalty of 6.8% compared to the pooled regression reported in Model 6 (Table 2.2) of 5.8%. The coefficient is statistically distinct from zero. In Model 2, we run the regression on the partition of placements among those with weaker rule of law. Here, the coefficient of *scandal* × *placed* is −0.02, not statistically significant at the $p < 0.05$ level. Job switches representing moves across countries that differ by regulatory strength are excluded from both Model 1 and Model 2. These findings support H2's contention that scandal penalties are more pronounced for placements in countries with stronger regulatory systems.

2.4.3 ARE EFFECTS MORE PRONOUNCED AMONG FINANCIAL PLACEMENTS?

Table 2.4 reports results that address H3's contention that executives in financial roles will face greater penalties for their associations with scandal firms. Because our measure of misdeeds focus on misrepresentation of financial disclosures and auditing and accounting violations, those with careers in finance are likely to exhibit stronger negative associations with our measure of *scandal*. Model 1 runs the fixed effects model on those job switches that are among financial jobs and Model 2 replicates the model for that partition but captures moves across functions outside of finance. Job switches between finance positions and nonfinance positions are excluded from both of these models. The coefficient (−0.15) of *scandal* × *placed* in Model 1 is more than twice that of the overall estimate. Among finance-oriented executives, those having a scandal firm in their work history had an average of 15% lower placed compensation compared to those who did not. Those executives with scandal firms on their resumes moving across other functions were less affected; however, Model

Table 2.3: Fixed effects regressions, comparing placements among stronger rule-of-law countries versus weaker rule of law countries

	(1) "Stronger rule-of-law-country" $log(total.comp)$	(2) "Weaker rule-of-law-country" $log(total.comp)$
$scandal \times placed$	-0.068**	-0.021
	(0.021)	(0.093)
$placed$	0.132***	0.340***
	(0.011)	(0.041)
$perform.comp$	Yes	Yes
$job.rank$	Yes	Yes
$job.funct$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	3130	744
R^2	0.368	0.325

Standard errors in parentheses

$^{*}\ p < 0.05,\ ^{**}\ p < 0.01,\ ^{***}\ p < 0.001$

2 still reports statistically significant negative impact for this population as well (−0.059), a 5.9% penalty. These results suggest that the damages from financial scandals are sharply pronounced among those in finance-related careers; however, those outside of this functional area are not immune to negative association with such misdeeds.

Table 2.4: Fixed effects regressions, comparing placements among finance versus non-finance positions

	(1) "Finance execs" $log(total.comp)$	(2) "Non-finance execs" $log(total.comp)$
$scandal \times placed$	-0.151* (0.062)	-0.059* (0.025)
$placed$	0.207*** (0.028)	0.175*** (0.014)
$perform.comp$	Yes	Yes
$job.rank$	Yes	Yes
$job.funct$	No	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	664	3258
R^2	0.398	0.304

Standard errors in parentheses

$^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

2.4.4 Are Effects More Pronounced Among Senior Executive Placements?

Table 2.5 reports results that test H4's contention that executives placed in more senior roles will face greater penalties for their associations with scandal firms. Job switches between junior positions and senior positions are excluded from both Model 1 and Model 2. Model 1 uses the same fixed effects specification on the partition of data representing placements for junior executives (those with job titles below the rank of C-level, president, or vice president). In this model, the coefficient of *scandal* × *placed* is 0.01; the coefficient is not statistically distinct from zero. In Model 2, we run the regression on the partition of placements among those at senior ranks. Here, the coefficient of *scandal* × *placed* is −0.073 significant at the $p < 0.05$ level. This represents a more negative effect compared to the pooled model. These findings support H4's contention that among those whose positions warrant greater scrutiny as exemplars for other employees, the burden of past associations with scandal companies may be greater.

2.4.5 Additional Analyses

In this paper, we limit scandals to financial scandals, specifically those captured in either the GAO or AAER databases. Both databases have been used in prior research of corporate misdeeds examining other outcomes. These databases capture misdeeds of specific types. The GAO database captures only financial restatements, while the AAER database captures only SEC enforcement actions that also are designated by the AAER classification (Karpoff et al., 2014). Moreover, firms enter these databases in differ ways. AAERs are officially filed by the SEC, providing a reliable record of time for both filing and period of financial misstatements. This creates precision around the timing of misdeeds; however, lags between the misconduct itself, the public revelation of the misconduct, and the final filing by the SEC may obscure the timing at which the scandal becomes public enough to affect executive compensation. This study design requires that scandal events are known by hiring firms,

Table 2.5: Fixed effects regressions, comparing placements among junior versus senior placements

	(1) "Junior exec" $log(total.comp)$	(2) "Senior execs" $log(total.comp)$
scandal × *placed*	0.010	-0.073*
	(0.053)	(0.032)
placed	0.226***	0.098***
	(0.023)	(0.014)
perform.comp	Yes	Yes
job.funct	Yes	Yes
firm.size	Yes	Yes
public	Yes	Yes
industry	Yes	Yes
region	Yes	Yes
Observations	1624	1368
R^2	0.270	0.364

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

which means that misdeeds must not only have occurred, but also have been publicly recognized. In this sense, the GAO database may provide a cleaner indication of scandal. All events were captured using LexisNexis keyword and word stem searches for mentions of "restatements" in press releases and periodicals. The GAO report documents that these events were identified from public sources by 2002 (D'Agostino, 2003), well in advance of all observed job placements in our sample. While the use of the GAO database methodology introduces reporting biases (i.e., more newsworthy cases are more likely to be included), this bias is aligned with our research question. Firms identified from the GAO database are reliably historical and reasonably public at the time of all job placements we observe.

As a robustness test, we run previous models using *scandal* measured using the GAO or the AAER firms measures independently (tables available upon request). In both cases, fixed effects models for Hypothesis 1 return results consistent with the combined measure. Model 6 (from Table 2.2) attributes a negative 5.8% impact of *scandal*. When using only GAO-identified firms, the *scandal* impact is 8%. For AAER database firms, the *scandal* impact is 5%.

For differential effects of *scandal* between country type, using only GAO firms or only AAER database firms to define *scandal* both reflect a more severe penalty in countries with stronger rule of law. The average impact measured by the coefficient on *scandal* × *placed* is higher in both cases, 9% and 6% for GAO and AAER firms, respectively.

For differential effects of *scandal* on financial careers, *scandal* measured using only GAO firms reflects a steeper penalty among executives in finance functions. With a negative coefficient for *scandal* × *placed* of −0.23, executives in finance career tracks with scandal firms in their work record have a 23% penalty. Similar to the combined measure, executives moving outside of the finance function suffer less from associations with scandal firms (−6%). This −6% impact is replicated using a measure of *scandal* based exclusively on the AAER database as well. However, the small sample of finance professionals plagued by *scandal* using the AAER database does not permit precise esti-

mation in this segment to support H3's claims at the $p < 0.05$ level.

The disproportionate effect of *scandal* on senior executives is replicated using the GAO measure of *scandal*, again with higher severity. The impact of *scandal* on moving senior executives is estimated at -14%, twice the effect measured using the combined measure. These results are not replicated, however, using the AAER measure of *scandal*.

Overall, the main effects of *scandal* reported in our results are robust to different measures. However, for tests examining conditions that moderate the main effect of *scandal*, results are more sensitive to how *scandal* is measured.

For simplicity, this paper uses a binary measure of *scandal*. This approach to measuring does not incorporate how the passing of time influences the scandal effect. Scandals that occur more recently are more salient and may have arguably more negative impact compared to those that are more dated. Another type of timing difference is the time elapsed from the point at which the executive leaves the scandal firm until the time that the scandal actually takes place. Those who have departed scandal firms in the two or three years leading up to the scandal may incur greater negative effects if temporal proximity creates a stronger linkage between organization-level misdeeds and the individual. In the measures reported here, scandal effects are treated equally, regardless of the timing of their occurrence.

We conduct additional tests to examine the influence of these two types of timing differences. Using the GAO database indicators of *scandal*, we first modify the scandal variable with temporal restrictions, comparing scandals that occurred in the distant past versus those that occur more recently. The median time since scandal is 8 years. Model 1 from Table 2.6 reports results for the *scandal* effect where *scandal* includes only those scandals that occurred at least 8 years prior to the time of placement. The coefficient for *scandal* $\times$ *placed* (-0.04) is not statistically distinct from zero at the $p < 0.05$ level. Model 2 defines *scandal* to include only those scandals that occurred within the last 7 years from the time of placement. The coefficient for *scandal* $\times$ *placed* is -0.12, statisti-

cally significant at the $p < 0.05$ level and twice that of the average effect. These results suggest that recency of the scandal matters to the individual-level impact on compensation.

In Table 2.7, we examine the influence of time elapsed from the point where the executive leaves the scandal firm to the year the scandal occurs. The median number of years that passes from the point of departure to the year of the scandal is 5. In Model 1, *scandal* is defined to include only those instances where the executive leaves the scandal firm at least 5 years before the scandal occurs. The coefficient for *scandal* × *placed* is −0.04, not statistically significant at the $p < 0.05$ level. In Model 2, we restrict the measure of scandal to include only those instances where the executive leaves the scandal firm within the last 4 years. In this case, the coefficient for *scandal* × *placed* is −0.10, statistically significant at the $p < 0.05$ level. These results indicate that the timing of when an executive worked at a scandal firm matters; as expected, markets draw tighter linkages to those whose history at troubled firms more closely coincides with the timing of misdeeds.

2.5 DISCUSSION

This paper finds that external labor markets penalize executives with discredited firms on their resumes through lower compensation, even in cases where such executives have temporal alibis that likely preclude their direct involvement in the scandal. Using proprietary data from a global executive placement firm, enhanced with detailed career histories, we empirically investigate how such executives might suffer negative impact to their compensation when moving jobs in the external market. We find support for this effect, suggesting the negative impact from stigma can occur even in the presence of exonerating evidence. For those on financial career tracks or in more senior positions, the burden of association with financial scandal is more pronounced. These findings support the notion that financial misconduct can have reputational impacts on individuals that are far reaching and not exclusively based on assignment of responsibility.

Table 2.6: Fixed effects regressions, comparing recency of scandal at the time of placement

	(1)	(2)
	$log(total.comp)$	$log(total.comp)$
$(scandal \times placed)_{\text{scandal over 7 years ago}}$	-0.039	
	(0.046)	
$(scandal \times placed)_{\text{scandal less than 8 years ago}}$		-0.123*
		(0.059)
$placed$	0.169***	0.171***
	(0.012)	(0.012)
$perform.comp$	Yes	Yes
$job.rank$	Yes	Yes
$job.funct$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	4068	4068
R^2	0.279	0.280

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2.7: Fixed effect regressions, comparing timing of executive departures from scandal firms

	(1) $log(total.comp)$	(2) $log(total.comp)$
$(scandal \times placed)_{\text{departed over 4 years ago}}$	-0.041 (0.059)	
$(scandal \times placed)_{\text{departed less than 5 years ago}}$		-0.101* (0.047)
$placed$	0.168*** (0.011)	0.172*** (0.012)
$perform.comp$	Yes	Yes
$job.rank$	Yes	Yes
$job.funct$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	4068	4068
R^2	0.279	0.280

Standard errors in parentheses

$^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

Contributing to an established line of inquiry linking how external labor markets incorporate information on financial misdeeds, we explore the scope of such effects, opening up the possibility that reputational effects may run deeper than how the market assigns responsibility for actions; instead, markets may also be using organizational associations to develop a broader evaluation.

Additionally, this paper highlights the possibility of expanding the scope with which social stigma theories may be applied to cases of financial misconduct. Organizational stigma appears to affect many members of an organization, beyond those who were implicated in the scandal. Further research should attempt to tease out the nuances of how organizational stigma plays out in the careers of employees of a scandal firm. Are there factors besides function and seniority (such as gender or industry) that affect the extent of individual stigma? Do the different groups of arbiters —social, legal, and economic —have different effects (e.g., what seems to lead to greater stigma, negative publicity, or court rulings?). Also, how do executives from scandal firms cope with their subsequent stigma? Much of Goffman's original work, as well as subsequent research in his tradition, addresses the coping mechanisms of stigmatized individuals, yet we know little about the tactics used by bearers of an organizational stigma.

2.5.1 Implications for Management Professionals

The current findings have important practical implications for career-building executives. That the value of one's human capital can be rewritten ex post is startling. It is troubling enough that leaders must be responsible for all those on their team; to extend the liability to those who are not even present suggests that careful investments in human capital and reputation are exposed to influences far beyond one's control. Managers and executives may wish to keep the following pieces of advice in mind.

Know the law and cultural climate, especially if you are changing industries or companies. Both legal regulations and cultural values differ across industries and companies. Before making a transi-

tion, executives should ensure that they are clear on what constitutes legal misconduct and socially sanctioned behavior though technically legal behavior in their new environment.

Develop strong external networks. An extensive network and other types of social capital can help mitigate the effects of organizational stigma (Pozner, 2008). Stigmas are most salient when judging unknown individuals; the stigma, and hence its effects, become less important as a relationship develops. A stigmatized manager can appear less of a risk in the job market if there are credible arbiters to vouch for his or her integrity.

Develop a reputation for integrity. It is not enough to have integrity; a person must be known by others to have integrity. If an individual's reputation is vulnerable to events over which that individual has no control (such as an ex-post scandal), even more effort should be put into controlling what can be controlled.

Consider a diverse resume. Having multiple jobs on a resume may diffuse the impact of the scandal firm in the minds of hiring managers and potential coworkers (Beyer and Hannah, 2002). However, there are risks to this strategy. Statistically, the more firms an individual works for, the greater chance one of these will be embroiled in a scandal. Also, an effect of stigma is to color all subsequent pieces of information learned about the stigmatized party. If a manager is already subject to an organizational stigma, a resume with evidence of frequent job changes may appear to be evidence of lack of commitment and loyalty.

Stay out of stigmatized industries. Companies in industries that are already stigmatized (e.g., tobacco) may suffer greater organizational stigma in the event of a scandal than companies in less controverial industries: "[I]n stigmatized industries, whose survival depends on the capacity to remain discreet, players attacked publicly are more likely to be scapegoated by their peers" (Vergne, 2012, p. 1047).

Pay close attention to ethics and research a company before joining. Most job seekers do not conduct enough research on the organizations they join (Groysberg and Abrahams, 2010). The fact that

a company's behavior can affect a manager's career even after leaving the organization makes it even more crucial to be careful when selecting an employer.

Keep a watch for developing scandals and consider whistleblowing or leaving before the scandal breaks. Leaving in advance of a scandal can help protect an individual's value in the labor market (Sutton and Callahan, 1987; Semadeni et al., 2008).

Don't assume that organizational distance from the problem will protect you. As our findings indicate, while managers in finance were punished more heavily for financial scandals than nonfinance workers, no one escaped the organizational stigma entirely. To preserve human capital, executives should treat a scandal in any department as seriously as if it were happening in their own department.

Address your emotional and private needs. Being stigmatized is a major stressor that can have serious social, emotional, and even physiological consequences. Learning to cope with a stigmatized identity is an ongoing process (Miller and Kaiser, 2001). Managers with an organizational stigma should seek out a safe place to work through any emotional difficulties the scandal might have caused. Without this, emotional issues can easily affect a job search or attempts to network.

Stereotype-congruent stigmas are harder to escape. Managers cannot change their face or race, but should be aware that both can affect them in the event of a corporate scandal. When behavior, including stigmatized behavior, is congruent with an existing stereotype, it is more memorable and cognitively salient. Considerable research has shown that members of ethnic groups stereotypically associated with criminal enterprises (for example, Latinos and the drug trade) are given harsher sentences for those crimes than nonstereotyped defendants (Jones and Kaplan, 2003; Harris, Evans, and Beckett, 2011). Baby-faced individuals are stereotyped as honest, naive, and somewhat less competent than adults with square jaws and strong features. Baby-faced defendants, congruent with this stereotype, are more likely to be judged guilty of negligence and less likely to be judged guilty of intentional wrongdoing (Zebrowitz and McDonald, 1991).

Consider distancing strategies. In the event of a corporate scandal, distancing oneself from the stigmatized organization is crucial. Depending on the nature of the scandal and the degree, if any, of the manager's involvement, this distancing can take different forms. Certainly the manager needs to distance him or herself rhetorically from the wrongdoing.

Emphasize cooperation over competition in cover letters and interviews. Intriguing recent research suggests that stigmatized individuals do better in job-selection situations when they emphasize a common identity with the hiring manager and their capacity for teamwork (as opposed to taking a more individualistic, self-promoting tack) (Schmader, Croft, Whitehead, and Stone, 2013). Because stigma operates by creating a sense of distance and "us versus them" between the target and the perceiver, this recommendation makes sense.

2.5.2 Limitations and Future Research

These data provide advantages in studying how the presence of scandal firms in a work history can impair the value of accumulated human capital for job switchers; the ability to observe prior and placed compensation enables empirical designs that focus sharply on how moves in the external job market can reveal the market value for different profiles. The rich set of context variables and multiple observations per individual enable methods that carefully control for unobserved individual differences.

At the same time, this empirical study has limitations that are important to address in future research. Selection issues appear at three levels: individual, firm, and process. At the individual level, the population captured by our data represent switchers, and the inference from this study is limited to such movers. This means that all the individuals we observe were amenable to considering new positions. Executives may decide to be receptive to job moves for a variety of reasons—family concerns, perceived limitations of internal promotion potential, spousal professional goals, and so on. Those executives with scandal firms on their resumes select to make the job switch, their reputations

notwithstanding.

Our study does not incorporate a model for how executives become more willing to consider job moves and how this might be different if burdened by scandal; the point of departure for this work is to examine the effect of scandal among executives who have already chosen to participate in the external labor market. This introduces selection bias, in that scandal-plagued executives might opt out of such searches or exit the executive labor market entirely and thus not appear in our sample (see Heslin, Bell, and Fletcher, 2012, on the factors that lead stigmatized workers to exit the labor force). We contend that such selection bias works to make a significant finding less likely, as the executives we observe arguably have strong enough profiles to survive and secure employment in spite of their tainted resumes. That these results surface consistent and measurable negative influences from scandal for those moving in external markets suggests that the estimated effects may represent a lower bound.

At the firm level, selection concerns arise from the fact that these data all come from a single executive search firm. If the firm supplying these data has particularly strong access to candidates in certain industries or functions, this could bias the quality of accessible candidates. Clearly, financial scandals are related to the financial function; to the extent that scandals are correlated with industry, differences in market strength with respect to industry threatens the generalizability of results. The firm supplying our data represents one of the largest global search firms and has broad participation in service lines across multiple geographies, industries, and functions. To limit the influence of function-driven differences in expertise, we control for function in both the prior and placed jobs. Nevertheless, while single-site research provides the benefit of detailed measures that are hard to find in data covering broader populations, generalizability of the findings will be enhanced by replication using data from other placement firms. As arbiters of executive human capital, they also have interest in steering clear of questionable characters. As such, we expect selection bias from the firm to operate in a way that would either avoid scandal-plagued candidates or include them only in light

of strong compensating strengths. Such selection would create a bias toward a null finding; that we find measurable effects in light of this indicates the presence of results subject to a harder test.

Risk of selection bias also arises out of the placement process itself. Search firms may disqualify those with scandal firms on their records, removing them from consideration early in the process; only those with substantial compensating merits might warrant being considered. Because we observe final placements, we do not capture the impact of scandal for all those who are weeded out by the search firm or by the hiring firm. As in the individual selection case, we contend that selection works in a direction that biases against a measurable finding. The fact that measurable results surface consistently in light of likely selection suggests that detrimental effects could be even higher if prior selection was accounted for.

Like all empirical studies linking experience measures to compensation outcomes, there remains the threat of omitted variable bias. In particular, executives may have underlying differences in ability that firms perceive that are correlated with scandal and compensation, but not observed in the data. Unaddressed, such abilities stand as an alternative explanation for the results. We endeavor to account for such abilities using qualification control variables that are commonly used by both scholars and practitioners (e.g., education, experience, prior role, etc.), as well as fixed effects models to control for time-invariant individual attributes. It is important to note limiting conditions in these models. Controlling for such individual differences in this way assumes that such underlying attributes are valued in consistent ways by both the prior and placement firm. For stable personality traits (e.g., likability, executive presence, communication skills, etc.) to create systematic bias in the results, they would have to be not only correlated with the scandal treatment, but also systematically valued differently across prior and placed firms. Because the scandal treatment is determined after the executive has already left the scandal firm, executives have no ability to anticipate working at a scandal firm and shape career decisions around this eventuality. Of course, such executives can take action after reputation-damaging scandals come to light, to mitigate the personal impact (Sutton

and Callahan, 1987; Semadeni et al., 2008). However, to the extent that they are successful, these actions would again bias against measurable findings.

Moreover, we observe only the salary and bonus part of the compensation but not any long-term incentive plans (LTIPs) provided as part of the new placement. Such data were not provided to us, as they were not readily available. To the extent that LTIPs for individuals that have worked for a firm affected by a scandal are also lower compared to other individuals who switch jobs, our results would extend to LTIPs. If LTIPs are unaffected by the presence of a scandal then it is the cash part of the compensation package that is affected by the presence of a scandal. Lastly, if LTIPs are actually higher for individuals that have worked for a firm affected by a scandal, then one could conclude that future employers are transferring risk to employees by substituting cash compensation with LTIPs that are contingent on future performance.

Finally, we note that our individual fixed effects models implicitly use executives as their own control. For those executives historically employed at scandal firms, we deliberately exclude those who are fleeing embattled firms, since this circumstance could simultaneously drive the decision to move under duress and to settle for lower pay. This means that prior employers are also arguably aware of tarnished job histories and may impose implicit compensation penalties as well. If such discounts were applied in the prior firm, this would bias against finding a negative impact from scandal during external job moves in our analyses, as baseline compensation would be diminished by the same influence. That we find significant results suggests the quantification of the scandal effect may be understated. It is important to note, however, that individuals have the opportunity to clear their names over an employment relationship at their prior employers. Such executives may take actions that reveal that negative stigmas are not indicative of their true character in the light of their demonstrated performance. If the compensation after leaving the prior employer is inflated due to zealous efforts by individuals to throw off the effect of stigma, this might explain lower pay outcomes from moving to another employer. In this case, the negative effect on these executives may

manifest in the form of effort cost associated with such additional striving to inure themselves from the damage of their prior associations.

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Out and Up: The Effect of External Job Switches on the Gender Compensation Gap among Executives

3.1 Introduction

Moving across organizational boundaries offers the potential of career advancement. If progression is blocked at a current employer, career builders whose skills have value in multiple contexts can use external markets to upgrade their returns to human capital(Arthur and Rousseau, 1996). Well-timed moves on the external market can result in promotions and pay increases that are considerably larger than the incremental changes from internal career tracks (Pfeffer and Baron, 1988; Sonnenfeld, Peiperl, and Kotter, 1988; Sonnenfeld and Peiperl, 1988). Underutilized and underpaid talent can seek opportunities among firms that have not sufficiently developed leaders internally (Herzenberg, Alic, and Wial, 1998). As such, mobility may represent a means by which under-

appreciated human capital can command higher compensation.

This paper examines the potential for mobility to alleviate pay disparities for women in an arena with high compensation and intense competition for human capital: the external executive labor market. Women are underrepresented in the executive ranks and awareness of discrimination is currently high. Because of this, individual women may be able to use the gap in representation to mitigate, for themselves, the gap in pay.

Analyzing 2034 executive placements, we find job switching may be a mechanism by which female executives can reduce gender-based compensation differences that persist even after controlling for contextual and occupational factors. We argue that while traditionally structured career paths may have limited the number of women executives, the resulting scarcity of talent, combined with increasing pressure to demonstrate progress on leadership diversity, may have created a higher willingness to pay for seasoned female executives hired from the outside.

Our results build on work that demonstrates seemingly contrary results: that executive mobility exacerbates gender pay differences, favoring men over women (Brett and Stroh, 1997; Dreher and Cox, 2000; Lam and Dreher, 2004). These cross-sectional studies compare pay premiums associated with the job-switching population relative to the population of stayers, and how gender moderates these effects. This paper contributes to the gender gap literature by focusing instead on the job-switching event, comparing prior and placed compensation levels for the same individual movers. Our results suggest that after controlling for individual attributes, the act of job switching may diminish the wage gap.

This paper employs data obtained from a global executive placement firm that captures contextual details for each move, including prior and placed compensation, at the individual level. Observing prior and placed compensation enables empirical designs that can address two persistent issues in prior work. First, research designs relying on cross-sectional data estimate average differences in compensation between distinct populations of stayers versus leavers, measuring gender effects by

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comparing the differences between men and women within these populations (Brett and Stroh, 1997; Dreher and Cox, 2000; Lam and Dreher, 2004). Such designs are vulnerable to bias driven by unobserved heterogeneity between movers and stayers. This is important, because attributes that separate movers from stayers may differ between men and women in ways that are related to human capital or other drivers of pay. Beyond individual differences correlated with switching propensity, there is the issue of controlling for individual differences more generally, as stronger ability may drive both mobility opportunities and higher compensation. Individuals who have attained executive leadership positions have exceptional talents, and controlling for individual abilities marks an important contribution in understanding the impact of gender on compensation. This paper uses panel designs to control for such unobserved, time-invariant individual attributes.

Second, prior work examining the effects of external job moves on the gender gap rely on survey data, asking respondents to report job switches at any time over the stipulated period of study. This approach associates switching with reported differences in pay that many not be temporally well matched, since pay is reported in the year of data collection, but switching can occur over a range of time. By measuring compensation changes at the moment of the switch, this paper calibrates the effect of mobility on the gender gap with greater precision.

This paper tests the effect of job switching on the gender gap among a population of job switchers. We begin by decomposing the gender gap in prior jobs as well as in the new position, identifying key variables that contribute to explaining the persistent difference. Next, we examine how the average gender gap changes in the context of job switching. We introduce individual fixed effects regression models to distill the gender effect from individual differences in ability. Finally, we test conditions that moderate the level of scarcity and explore how context influences the switch effect on compensation.

3.2   Prior Literature and Hypotheses

3.2.1   The Gender Pay Gap Persists despite Some Progress

The gender gap in wages has long captured the interest of scholars and policymakers. One of the earliest mentions of the pay gap in the United States was a letter to the editor in *The New York Times* in 1869 which read, in part:

> Whatever arguments must be urged, with more or less force against the theory of
> women's political equality with man, very few persons deny the justice of the principle
> that equal work should command equal pay without regard to the sex of the laborer.
> But it is one thing to acknowledge the right of a principle and another to practice
> it. Women are known to be as good printers, teachers, telegraphers, clerks, &c. as
> men. But as fewer occupations are open to them, their necessity for employment is
> greater, and therefore their services can be obtained for less. ("Women as Government
> Clerks")

This letter is of interest not only for the basic principle of fairness that it espouses, but also for its implicit understanding of the relationship between the pay gap and the representation gap. Women were overrepresented in the jobs they were permitted to do, and there was no social pressure to hire women (quite possibly the reverse); hence, there was no mechanism by which an individual woman might better her situation.

"Equal Pay Day" is the day that represents the date to which women needed to work in a given year to achieve pay parity with men from the previous year. On Equal Pay Day in 2015, *The New York Times* published an editorial entitled "Women Still Earn a Lot Less Than Men," addressing the persisting gender gap in pay and urging congressional Republicans to pass "the Paycheck Fairness Act, a bill supported by President Obama that would have extended pay-equity rules that apply to

federal contractors to the entire American work force, in addition to making needed updates to the Equal Pay Act" (p. A22).

Between the 1869 letter and the 2015 editorial, both the representation gap and the pay gap have declined. Both world wars brought significant gains to women, as they were needed to take over the jobs men left behind. In both wars, this led to equal pay being the norm in more workplaces. The Equal Pay Act of 1963, which prohibits wage discrimination on the basis of sex, and the Civil Rights Act of 1964 continued to level the playing field. In 2009, the Lilly Ledbetter Fair Pay Act was written into law, which extended the amount of time a person has to file a discrimination claim. More symbolically, Senator Jeanne Shaheen of New Hampshire introduced the Women on the Twenty Act on Equal Pay Day 2015, legislation that proposes replacing Andrew Jackson on the $20 bill with a woman (Chandler, 2015).

In 2014, the Census Bureau calculated that women earned 77 cents for every dollar that men earned. The Pew Research Center, which accounts for both full- and part-time workers, calculates the female wage gap as 84 cents on the male dollar (Patten, 2015). Women out-earn men in only three categories of the 111 occupations listed in the Bureau of Labor Statistics 2013 wage data: bakers; wholesale and retail buyers; and "computer occupations, all others" (Kurtzleben, 2014). The United States Bureau of Labor Statistics reports that in 2012, women earned 81 percent of the median earnings of male workers[*] (U.S. Bureau of Labor Statistics, 2013). The pay gap is part of an overall wealth gap between men and women. The White House noted that "Women are the primary breadwinners in 40 percent of U.S. households but are bringing home 23 percent less than their male counterparts —which means less for families' everyday needs, less for investments in our children's futures, and, when added over a lifetime of work, substantially less for retirement" (The White House, Office of the Press Secretary, 2014). Recent surveys in several countries show that

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[*]This represents the difference in median wages, and these figures are before adjustments for differences in part-time work and job classifications.

boys get larger allowances, and more money for doing household chores, than girls (Osborne, 2013; Allstate Foundation, 2014). At the other end of life, owners of Individual Retirement Accounts (IRAs) are more likely to be male, and the individual average and median IRA balances are higher for men than women (Copeland, 2014).

3.2.2   Prior Work "Explaining" the Gender Gap

The reasons for the gender gap in pay are complex and not simply a question of women being paid less for identical work. Such straightforward sex discrimination in pay was officially abolished in the U.S. in 1963 by the Equal Pay Act. A significant portion of the gender wage gap closes when considering factors such as number of hours worked (women are less likely to work more than 40 hours per week), education (men are more likely to study subjects, such as engineering, that are associated with higher-paying jobs), industry (women are less likely to work in some of the highest paying fields), and family status (women are more likely to spend time out of the workforce caring for children) (Perry and Biggs, 2014). When these factors are taken into account, the gap becomes much smaller, though it never entirely disappears: single, never-married women earned 96% of what men earned in 2012, according to the Bureau of Labor Statistics. Research firm Catalyst has found that women MBAs are paid less in their first jobs than men, even after taking into account years of prior experience, time since MBA, job level, global region, industry, and parenthood (Carter and Silva, 2010). Other research has found a pay gap between male and female CEOs (Hodgson and Ruel, 2008).

Cha and Weeden (2014) suggest that the gender pay gap, particularly among high-level employees, may be driven by the rise of "overwork," or working more than 50 hours a week, a trend that has increased notably, particularly in professional and managerial jobs, in the past 30 years. Women are less willing than men to engage in overwork, and Cha and Weeden argue that this difference has slowed down convergence on the gender gap. They note the complexity of ameliorating this kind of

inequality at the workplace level:

> Employers do not specify separate work hour expectations for their male and female
> employees, nor do they systematically reward men who overwork more than women
> who overwork, relative to their full-time counterparts. Nevertheless, overwork rests
> on a social foundation that is itself highly gendered: employees who work long hours
> can only do so with the support of other household members, usually women, who
> shoulder the lion's share of unpaid-work obligations. (Cha and Weeden, 2014, p. 478)

In a recent paper for the Federal Reserve Bank, Albanesi, Olivetti, and Prados (2015) suggest that
the gender gap in executive pay is due to female executives receiving less incentive pay or other forms
of firm-specific wealth (e.g., stock options) as compensation. Though they found no difference in
performance, female executives "reap a lower gain in the case of positive firm performance relative
to male executives, and experience larger declines in firm-pecific wealth in the case of declines in firm
performance" (p. 24). They argue that corporate boards are not acting as impartial agents of the
shareholders, but that they are influenced and influenceable by "the incentive to be re-elected, infor-
mal networks linking them to CEOs, cognitive dissonance, and ratcheting" (p. 2). Male executives,
who are likely to be more entrenched, are more likely to get good compensation packages including
hidden forms of compensation.

With all the rhetoric surrounding the debate of how large the gender pay gap is and how to ad-
dress it, careful analysis that illuminates contextual factors that impact pay disparities helps inform
both scholars and policymakers on this important debate. Recent research questions whether or
not laws intended to ameliorate the gender pay gap actually remedy deep-seated issues of gender
inequality (Bertrand, Black, Jensen, and Lleras-Muney, 2014). In addition, it is difficult to know
the extent to which women's greater likelihood to be in lower-paying occupations is the result of
individual choices shaped by social norms or by discriminatory factors (e.g., a hostile environment

toward women in a university's computer science department might deter women from majoring in that field). Researchers and policymakers do not agree about the role gender discrimination may or may not play in shaping women's and men's different earnings, and some scholars argue that lower-paying professions tend to be lower paying because of their high concentration of women (Reskin and Roos, 1990).

Gender-gap research has examined differences in contextual factors as well as differences between men and women in how they think about careers and pay (Catalyst, 2014). Prior work has documented the importance of controlling for industry, firm, and job attributes when measuring gender pay differences that lie outside of occupational differences (Bertrand and Hallock, 2001; Bell, 2005; Selody, 2010; Bugeja, Matolcsy, and Spiropoulos, 2012; Gayle, Golan, and Miller, 2012).

3.2.3   The Gender Gap among Executive Ranks

In addition to the gender gap in pay, there is a gender gap in representation, particularly at the top corporate levels, an issue that has also received considerable attention in the United States. In 2012, *The Atlantic* published "Why Women Still Can't Have it All" by Anne-Marie Slaughter, in which she analyzed the barriers remaining to women's equality and recommended increasing women's representation in the top leadership of business and government, noting that "We may need to put a woman in the White House before we are able to change the conditions of the women working at Walmart" (2012, p. 102). Sheryl Sandberg, COO of Facebook, published *Lean In: Women, Work, and the Will to Lead* in 2013, which both reflected and generated interest in the progress of women at the top corporate ranks.

In Europe, an increasing number of countries are in the process of following Norway's landmark legislation passed in 2003, prescribing quotas for female representation on boards for publicly listed companies. Since then, Belgium, Iceland, Italy, the Netherlands, and Spain have passed similar laws. In November 2013, the European parliament supported a proposed draft law that would require

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board members in nearly 5000 listed companies to have 40% female representation by 2020, with state-owned enterprises moving more quickly to align with the target by 2018.

Top managers are highly visible and are often considered emblematic of the values of their organizations (Hambrick and Mason, 1984). Firms demonstrating that female employees can rise to positions of leadership are celebrated as progressive, while those that do not risk drawing criticism and suspicion. Anecdotal evidence suggests that organizations may appoint women to high-level positions as part of a reputation-repair effort. American Apparel, a company with a strong brand plagued by "loose corporate operations, financial losses and sexual harassment allegations" brought in a female CEO, Paula Schneider, after the ouster of Dov Charney: "Beyond toning down the over-sexed image, Schneider aims to install a more grown-up structure and culture" (Li, 2015, p. C4).

Announcing the appointment of women on boards can be a good thing for corporate reputation (Brammer, Millington, and Pavelin, 2009) and stock prices (Cook and Glass, 2011). Being recognized as a good employer for women because of diversity or work-life initiatives also boosts share prices (Hannon and Milkovich, 1996; Arthur and Cook, 2003; Arthur, 2003; McMillan-Capehart, Aaron, and Cline, 2010; Cook and Glass, 2014).

3.2.4 Company Actions and Implications for Women Executives

Companies seem to be heeding the signs. Google, a company that is two-thirds male, has engaged in a multiyear project to study unconscious bias and use data to create new workplace practices to reduce discrimination.

> [The initiative] has made big changes in the company, like removing the requirement
> that employees nominate themselves for promotion (women were much less likely to
> self-nominate than men). And it's made subtle ones too, like naming more conference
> rooms after female scientists upon noticing that all but one of the 15 conference rooms

on a certain floor bore the names of inspirational men. (Pinsker, 2015)

The biotech firm Biogen, with 3 out of 10 female board members, has started a program to get more women onto other corporate boards, as well (Leung, 2015, A15).

Given the rewards for placing women in high-profile leadership roles, and the increasing pressure on organizations to do so, underrepresentation of women as a group might, in certain ways, benefit women as individuals when there is increased social pressure for women in leadership. These two gender gaps —in pay and in representation —may interact to provide a chance for already-successful women to leverage their way into greater pay equity with men. While the recent legislative and social pressures to address gender differences highlight systematic issues at all levels of organizations, they may create opportunities for women executives who have already achieved success in the current environment. Firms eager to demonstrate commitment and progress toward greater diversity may use high-rank appointments to signal a more progressive stance toward diversity. Because established practices have systematically reduced the population of available women groomed to take executive posts (Gayle et al., 2012), those women willing to switch employers may find more opportunities to upgrade their pay with firms that want to show immediate, visible progress on gender diversity.

3.2.5   Mobility and Negotiated Pay

Social psychologists have accumulated a substantial body of research relevant to predicting gendered effects on pay around career discontinuities. Changing employers invokes a reassessment of human capital by external evaluators who lack the insights of longstanding employment relationships. In such situations, differences between women and men in attitudes toward negotiation, competition, and risk may influence pay levels, particularly among executives. Generally, the evidence from experimental work supports predictions that mobility will widen the gap between men and women. However, results from this work have had mixed results when tested in real labor market

settings where contextual cues and individual differences are important determinants of outcomes (Bertrand, 2011). The external job market is also more stereotype dependent because it does not have as much good information to rely on, making stereotypes more influential in this arena (Arvey and Campion, 1982; Dipboye, 1992; Judge et al., 2000; Posthuma et al., 2002).

Gender differences in attitudes toward negotiation have received substantial attention in the social psychology literature. There is an overriding stereotype that women do not negotiate, or do not negotiate well. Interim eddit CEO Ellen Pao banned salary negotiations in job offers, to equalize men's and women's starting compensation (Elder, 2015). Descriptive field studies comparing male and female graduates of professional school programs have found that the share of men negotiating initial job offers is higher than the share of women doing so (Babcock and Laschever, 2003; Babcock, Gelfand, Small, and Stayn, 2006). Scholars have theorized that women may find negotiating socially undesirable (Small, Gelfand, Babcock, and Gettman, 2007), particularly when it is ambiguous whether or not the situation calls for bargaining (Bowles, Babcock, and McGinn, 2005) or in cases where they believe negotiation is self-serving (Rudman, 1998; Rudman and Fairchild, 2004).

Women may believe that they will be punished for negotiating, expecting negative social consequences for driving a hard bargain (Small et al., 2007; Amanatullah and Morris, 2010), and experimental research suggest that this concern is a legitimate one: women are judged more negatively for negotiating than men are (Bowles, Babcock, and Lai, 2007). In short, the context and framing of the exchange strongly influence whether gender differences appear. Simply focusing on making women "better negotiators" or banning negotiation entirely may not be the best approach. Women's reluctance to behave assertively in negotiations may in fact be a way of mitigating backlash and preventing their negotiating partners from viewing them negatively. Because in real-world contexts, negotiating partners are often people who will continue to be coworkers, supervisors, or colleagues, this strategy has value even if it also means achieving, say, a lower salary.

Empirically, studies examining gender-specific attitudes toward negotiation have yielded mixed

results (Rubin and Brown, 1975) and meta-analyses have highlighted the importance of situational cues in defining gender differences in negotiation behavior (Stuhlmacher and Walters, 2006). Such cues have been shown to attenuate or even reverse gender-specific negotiation behaviors (Bertrand, 2011). External hiring for executives represents a clear negotiation situation: in addition to being primed to negotiate for their skills, executives are often trained bargainers. Such circumstantial factors motivate skepticism in the ability to generalize from the body of experimental literature on gendered negotiation to the executive labor market.

Scholars have suggested that negotiation differences may stem from more fundamental attitudes toward competition (Bertrand, 2011). Lab studies examining differences in preferences toward competitive pay schemes suggest that women have a weaker affinity for competitive compensation (Gneezy, Niederle, and Rustichini, 2003), even in cases where their abilities are higher than men (Niederle and Vesterlund, 2007).

While findings indicating that women shy away from competitive environments have been consistently demonstrated in the lab, replications in field studies show mixed results. Vandegrift and Yavas (2009) show that gender effects are attenuated in field studies that introduce higher stakes and where ability is revealed through repetitive trials. In the real world, executive careers are long, with multiple opportunities to get feedback on abilities; seldom do they resemble the simple games designed to abstract away context-specific mastery. All executives, including women, make job-switching decisions with established beliefs in their abilities, conjectures of what new jobs require, and an assessment of their potential to succeed. As such, the impact of competitive attitude differences cannot be separated from contextual factors that influence executives' confidence in their skills and their ability to convince markets to pay for them.

Closely connected to differences in confidence is the notion of gender differences in attitudes toward risk. Experimental evidence suggests women are generally more risk averse than men (see Eckel and Grossman, 2008; Croson and Gneezy, 2009, for a survey of experimental studies). While

most of this evidence is based on college students selecting among hypothetical or low-stakes wagers, some progress has been made in studying differences in risk preferences within the general population (Dohmen, Falk, Huffman, and Sunde, 2010). These findings have implications for attitudes toward competition discussed previously. They are also particularly relevant in the setting of changing employers, as individual performance outcomes are riskier for those moving across organizational settings. While there has been more consistency in substantiating gendered attitudes towards risk, stronger risk aversion does not provide clear predications of pay differences in external job moves. Greater conservatism may make women negotiators less confident about their abilities and less willing to demand higher pay. On the other hand, risk aversion could result in higher required premiums to induce a move to an unknown situation.

Overall, while the experimental literature examining relevant gender differences supports predictions that mobility may work against female executives, studies translating such findings into realistic labor market settings are inconclusive. Because lab participants may not provide representative analogs for executive behavior, there is reason to be cautious about generalizing such findings to executive labor markets. Even if lab subjects do provide reasonable proxies for the population as a whole, executives represent a select group of labor-market actors. Executives often receive formal training in evaluating decisions in a business context. Further, the professions of executive management selects those who have traits and preferences that are amenable to the pursuit of profits and commercial advantage. Such training and traits may diminish the observed tendencies of generalized traits.

3.2.6 Job Switching and Scarcity Effects

Complementing research that examines individual differences and their effect on how men and women make career decisions, we motivate our hypotheses by examining the other side of the transaction how markets assign value to human capital. Specifically, we argue that markets may be re-

flecting a recognition of the need to demonstrate stronger female representation among executive leadership.

Skilled professionals, such as executives, are characterized by increasing observed mobility (Cappelli, 1999b; Arthur, Khapova, and Wilderom, 2005). Pay raises in internal labor markets largely follow firm-specific structures that rationalize seniority and pay levels (Doeringer and Piore, 1971). However, executives putting their human capital on the external market have the opportunity to be evaluated afresh for positions that could represent a jump in reporting level and pay (Pfeffer and Baron, 1988; Sonnenfeld et al., 1988; Sonnenfeld and Peiperl, 1988).

Why might women executives be able to reduce the gender gap by switching? With growing pressure to show diversity in the leadership ranks, firms may collectively face the problem of perceived scarcity of qualified women executives (Clark, 2010). As executives move up the career ladder, women exit management at higher rates than men (Becker-Blease, Elkinawy, and Stater, 2010; Gayle et al., 2012), creating a limited supply of experienced talent (Ahern and Dittmar, 2012). Firms seeking to address diversity goals may not be able to wait for comprehensive solutions that mend the entire career track within their institutions. If larger qualified talent pools can only be accessed from the outside, then women willing to move jobs may be able to capture a premium for their talent and for the current difficulty firms face in developing female managers internally.

For those firms operating in environments where legislative pressures directly create the demand for female representation among leadership, the pay premiums could be higher. Amid vocal opposition by businesses lobbying against Norway's reforms (Criscione, 2002), predictions suggested that firms would lower the bar for women's qualifications in cynical deference to the requirements (Coate and Loury, 1993), perhaps leading to overall lower firm performance (Ahern and Dittmar, 2012). Concerns of lower qualifications for women have not materialized: the gap on observable qualifications between women and men has actually diminished since the passing of the quota (Bertrand et al., 2014). Maintaining high standards benefits women in forestalling accusations of

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being unqualified; it may also create stronger demand and premiums for those women who have such qualifications.

HYPOTHESIS 1 (H1): *Among executives changing jobs on the external market, women will receive higher pay raises compared to men, holding all else equal.*

3.2.7 ARE EFFECTS MORE PRONOUNCED AMONG HIGHER RANK PLACEMENTS?

If scarcity of women has an effect on pay, we expect to find that greater scarcity should magnify this effect. Moving up corporate ranks, the representation of women dwindles. A key driver of the measured gender gap among executives in publicly traded firms is that women exit the profession at higher rates, resulting in smaller representation among the highest-paying jobs (Clark, 2010). If scarcity lowers the wage gap for women job switchers, we can expect that these effects will be more pronounced among higher-ranked positions. Because capabilities are often evaluated using years of relevant experience (Mintzberg, 1973; Kotter, 1982b), the problem of fewer women with such experience is compounded by the inability to produce such talent quickly. For these reasons, we expect to see scarcity-driven pay premiums to be particularly acute among high-ranking positions.

HYPOTHESIS 2 (H2): *For job switchers, pay raises for women versus men will be greater in placements among senior placements compared to that of junior placements.*

3.2.8 ARE EFFECTS MORE PRONOUNCED IN INDUSTRIES AND FUNCTIONS WITH GREATER-SCARCITY OF WOMEN?

Relative scarcity can be driven by industry-related differences as well. Specific to executive managers, women MBA graduates are more likely than men to enter industries such as education, health care, nonprofits, and consumer goods (Goudreau, 2010). Given the key role that industry experi-

ence can play in assessing executive capabilities (Kotter, 1982a; Castanias and Helfat, 1991; Harris and Helfat, 1997; Sturman, Walsh, and Cheramie, 2008), industries attracting fewer women will face scarcity constraint more sharply. Industries that have enjoyed higher representation of women include health care/social assistance (80%), education (70%), and leisure and hospitality (54%). These industries also enjoy higher representation of women in managerial positions[†]: 70%, 57%, and 45%, respectively. Industries with lower representation of women —including manufacturing (30%), utilities and transportation (25%), and construction (10%) —may face stronger constraints within their respective labor pools; women representation among managerial positions in these industries is 23%, 27%, and 12%, respectively (Sherrill, 2010). Attracting executives across industry boundaries may require additional monetary inducements due to the risks associated with working in a new environment (Harris and Helfat, 1997); industries with fewer women may face an additional challenge for attracting women to work in environments that put their career advancement at greater risk. Fewer women can be equated with higher barriers to accessing informal workplace networks and greater challenges in finding mentorship relationships, both of which have been linked to advancement (Worklifepolicy, 2011; Catalyst, 2014).

Hypothesis 3 (H3):   *For job switchers, pay raises for women versus men will be greater in placements among industries where women are scarce compared to those industries with higher representation of women.*

The growing complexity of business has resulted in an increasing presence of functional management expertise. As the average size of executive teams reporting directly to the CEO has doubled from the mid-1980s to the mid-2000s from 5 to 10, 75% of that growth is driven by functional specialist (Guadalupe, Li, and Wulf, 2014). Like industry expertise, functional experience represents a scarce asset that is important to defining the relevant knowledge among executive leaders. The rep-

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[†]"Managerial" is based on the definition of "manager" from the U.S. Census Bureau's American Community Survey (ACS).

Table 3.1: Prevalence of women by function for Fortune 500 (top 20 positions)

Function	Prevalence of women in top 20 positions	Prevalence category
Human resources	48%	High
Marketing	28%	High
Legal	26%	High
Corporate	26%	High
Information technology	18%	High
Finance	17%	Low
Sales	17%	Low
Other	17%	Low
Operations	10%	Low
General management	10%	Low
Research and development	9%	Low

resentation of women differs significantly across business functions. Among the top 20 executives as reported in the S&P Capital IQ database as of December, 2014, women are strongly represented in human resources (48%) and marketing (28%), and are far less prominent in functions such as operations (10%), general management (10%), and research and development (9%) (see Table 3.1). For external hiring, functions with fewer women will confront a more constrained pool of female talent with relevant capabilities. Function human capital accumulation is costly, and attracting executives across functional boundaries may entail additional inducements to move away from their demonstrated strengths. For these reasons, we expect placements in functions with fewer women will require higher pay for placements of women.

HYPOTHESIS 4 (H4):   *For job switchers, pay raises for women versus men will be greater among functions where women are scarce compared to those functions with higher representation of women.*

3.2.9   ARE EFFECTS MORE PRONOUNCED IN GEOGRAPHIES TAKING A MORE PROGRESSIVE STANCE ON ECONOMIC OPPORTUNITIES FOR WOMEN?

Scarcity is affected not only by supply-side influences but also by demand-side ones as well. Countries differ in the attention and appetite they have toward addressing gender pay differences. In societies where pro-equality attitudes create pressure to demonstrate visible progress, stronger motivation to show gains on gender diversity in corporate leadership can create demand-side influences that intensify scarcity premiums of experienced female executives.

Countries exhibiting greater economic opportunity for women in the form of stronger labor market participation and higher representation in legislative positions will place higher visibility on disparities. As such, firms operating in such countries will experience stronger urgency to demonstrate progress on pay parity.

HYPOTHESIS 5 (H5):   *For job switchers, pay raises for women versus men will be greater among countries with greater economic opportunities for women compared to those countries with less.*

3.3   METHODS

3.3.1   SETTING

To study gender-specific compensation effects of external job switching, we examine a population of job switchers for whom we can observe prior and placed compensation outcomes. This allows us to measure the contemporaneous change in compensation at the time of the switch for a given individual. We use placement data from a global executive search firm. In addition to being a rich source of

placements across a variety of ranks, functions, and industries, placements intermediated by search firms represent an important segment of the executive labor market, and in recent years, researchers have gained access to data from such intermediaries (Hamori, Bonet, and Cappelli, 2011). Such firms have had longstanding involvement with C-level placements (Hamori, 2010). More recently, a rising share of executives at lower levels in the hierarchy are being brokered by such intermediaries. A survey from the International Association for Corporate and Professional Recruitment reports that 54% of positions with an annual salary of $150, 000 or more are placed by executive search firms (Hamori and Koyuncu, 2011).

In addition to representing a sizable channel through which executives change employers, data from such intermediaries provide the additional benefit of capturing job moves among executives below the C-suite. Prior work examining the gender compensation gap among executives has relied mostly on mandatory compensation disclosures for the highest-paid executives in publicly traded companies. Within this elite population, the representation of women is quite low; in studies from the last two years examining the top-compensated executives (Demerjian, Lev, and McVay, 2012; Gayle et al., 2012; Guthrie, Sokolowsky, and Wan, 2012), women hold less than 6% of such positions, limiting the number of variables that can be precisely tested. Additionally, attainment of such noteworthy posts introduces a selection bias, because all individuals captured by these thresholds have demonstrated exceptional abilities, regardless of gender. Prior work warns against generalizing gender pay gaps from patterns created by such an elite group (Bertrand and Hallock, 2001; Gayle et al., 2012). Because our data includes lower-level placements, the representation of women is notably higher (18%) and arguably more representative of the majority of women executives.

Data limited to disclosing the compensation for only the five highest paid executives also make it impossible to separate the effect of job rank from job function. Job rank in a reporting hierarchy is a measure of professional attainment and is strongly correlated to pay levels. However, functional role (such as finance, human resources, IT, marketing, and operations) also captures differences in work

tasks that can impact compensation through occupational segregation (Anker, 1997; Blackburn, Browne, Brooks, and Jarman, 2002). For such functional specialization, human-capital investment decisions are often made at the beginning of a career; such decisions can have persistent effects on career paths and consequent compensation outcomes (Bielby and Baron, 1984; Bielby and Baron, 1986b; Bielby and Baron, 1986a). Our data allow us to examine the effect of function on the gender gap and to control for such differences in our analysis.

Finally, having multiple compensation observations per individual allows us to introduce models using individual fixed effects; these designs help address concerns for unobserved ability differences among this elite group of executives.

3.3.2 SAMPLE

Our data are from a leading global executive search firm, capturing 2034 executive placements across multiple functions, industries, seniority levels, and geographies from 2004 to 2011. Our data capture geography, position title, industry, company names, and compensation levels from both prior and placed positions. Individual attribute data include gender and education. To these data, we add details on individual career histories. We also include data from Government Accountability Office (GAO) reports based on Bureau of Labor Statistics reporting of the prevalence of female managers by industry.

3.3.3 VARIABLES

DEPENDENT VARIABLE   The outcome variable for all regression models is total compensation (*total.comp*). We transform compensation levels by taking the natural log to diminish the distortion from outliers.

Independent variable   The variable *gender* takes the value of 1 when the executive is female, 0 when male. Each executive appears two times in the data, once for their prior position and again for their placed position. Because we are focused on the effect of gender on compensation in the context of job moves, the quantity of interest is captured by the interaction of *gender* with the binary variable, *placed*, which takes the value of 1 for jobs representing a placement position, 0 if a prior position.

Moderator variables   To further test the scarcity effect, we introduce two variables that moderate the severity of scarcity for women executives. We use job rank (*job.rank*) to study how the scarcity effect differs at different levels of seniority. This variable refers to reporting level in the vertical management hierarchy. Executive job titles are coded by keywords and classified by categories including "manager," "director," "vice president," and so on. We coarsen the classification into a binary division of senior versus junior rank, with those at C-suite, president, and vice-president levels considered senior positions (*job.rank* = 0), and other positions considered junior (*job.rank* = 1).

Scarcity of women also varies by industry. To partition industries by high and low prevalence, we use the proportion of women in manager positions in the year 2000 for industries defined by the two-digit North American Industry Classification System (NAICS) classification. The median value is 47%. We use this reference point to partition our data between industries with fewer than 47% women managers (lower prevalence of women) and those with 47% or greater (higher prevalence of women). The variable *fem.industry* takes the value of 1 for higher-prevalence industries and 0 for lower-prevalence industries. To partition function by high and low prevalence of women, we calculate gender representation by function based on aggregated data captured by the top 20 officers of the Fortune 500 firms as reported by Capital IQ on December, 2014. Prevalence of women by function are detailed in Table 3.1. We classify those functions in the top half of women representation —human resources, marketing, legal, corporate, and information technology —as high

prevalence. The variable *fem.function* takes the value of 1 for higher-prevalence functions and 0 for lower-prevalence functions.

To partition countries by their progressiveness on providing economic opportunity for women, we use measures published by the World Economic Forum (WEF) capturing a country-level index combining labor market participation, representation in legislative positions and professional roles, and income equality. In nations with higher scores, such as the Nordic and North American countries, firms are arguably under more pressure to demonstrate a progressive stance toward gender diversity. Among countries with lower scores, such pressures may be less intense. The variable *fem.country* takes the value of 1 for countries above the median (0.78) and 0 for countries below the median.

CONTROL VARIABLES   Human-capital theory links compensation with distinctive capabilities gained through experience and investments in education. We control for years of professional experience (*total.experience*) and investments in education. We use highest degree attained (*highest.deg*) to control for investments in formal education. Executives fall into one of three groupings: (1) "pre-bachelors," (2) "bachelors," and (3) "postgraduate" for those attaining the equivalent of a master's degree or higher, including all post-baccalaureate professional schools. Education backgrounds including the prestigious Ivy League institutions and MBA degrees have been connected with perceived executive performance (Bertrand and Schoar, 2003; Miller, Xu, and Mehrotra, 2015). We also include binary variables that indicate whether the executive has attended an Ivy League school (*ivy.league*) and if he or she has an MBA degree (*mba*).

At the job level, we include controls for the job type based on job title classification. As executives attain higher reporting levels, the complexity of their tasks increases, which can be reflected in higher compensation levels (Agarwal, 1981). We use job function (*job.funct*) in addition to job rank (*job.rank*) to capture effects from occupational differences. The variable *job.funct* refers to the stan-

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dard classification of departments by business function. Executive job titles are coded by function into categories that include finance, human resources, IT, marketing, operations, and so on. Function names and their breakdown of representation in the data are illustrated in Figure 2.1 on page 22.

We include a set of contextual controls at the firm, individual, and job level. At the firm level, we control for the firm's industry (*industry*) using the 2012 two-digit NAICS code (see Figure 2.2 on page 24 for a breakdown of representation in the data). We control for the firm's size (*firm.size*) by the number of employees, measured as a category variable capturing size classifications of less than 500, 501 to 1000, 1001 to 5000, 5001 to 10,000, and greater than 10,000. We also control for whether or not the company is publicly traded (*public*).

Because these data are global, we also control for the region (*region*) of the job placement, grouping job placements into Africa, Asia, Europe, North America, South America, and Australia/New Zealand. Finally, to control for macroeconomic trends, we also include dummies to capture year effects (*year*), from 2004 to 2011, inclusive.

3.3.4 MODEL SPECIFICATION

We test our hypotheses using the following ordinary least squared (OLS) regressions to test hypothesis 1:

$$
\begin{aligned}
log(total.comp)_{it} \;=\; & \alpha_i + \beta_1 gender_i + \beta_2 placed_t \\
& + \beta_3 (gender_i \times placed_t) + \beta_4 total.experience_i \\
& + \beta_5 highest.deg_i + \beta_6 mba_i + \beta_7 ivy.league_i \\
& + \beta_8 job.rank_{it} + \beta_9 job.funct_{it} + \beta_{10} firm.size_{it}
\end{aligned}
$$

$$+ \beta_{11} public_{it} + \beta_{12} industry_{it} + \beta_{13} year_i$$

$$+ \beta_{14} region_{it} + \varepsilon_i \qquad (3.1)$$

The coefficient on the interaction term *gender* $\times$ *placed* measures the quantity of interest —the effect of being female on compensation in the placement in the external market. Standard errors are clustered at the *industry* $\times$ *job.rank* level to account for pay-level differences by industry and rank of executive title.

To control for individual differences, we use a fixed effects specification:

$$log(total.comp)_{it} \quad = \quad \alpha_i + \beta_1 placed_t + \beta_2(gender_i \times placed_t)$$

$$+ \beta_3 job.rank_{it} + \beta_4 job.funct_{it} + \beta_5 firm.size_{it}$$

$$+ \beta_6 public_{it} + \beta_7 industry_{it} + \beta_8 region_{it} + \varepsilon_{it} \qquad (3.2)$$

total.experience, *highest.deg*, *ivy.league*, and *mba* are absorbed into the individual fixed effects. By interacting *gender* with *placed*, we can estimate how *gender* affects compensation during observed job placements relative to its effect in the placement position. Because *gender* is invariant with time, we cannot estimate it directly in fixed effects regression models, but only the difference that it creates during a job change. Standard errors for fixed effects regressions are clustered at the individual level.

3.4   Results

Summary statistics and bivariate correlations for numerical and binary variables are reported in Table 3.2. Mean total compensation for prior and placed jobs are approximately \$294K and \$331K, respectively. The average executive has had almost 19 years of professional experience. Nearly half the placements are for positions where executives are not C-level, presidents, or vice-presidents, indicated by the mean value of 0.48 for *job.rank* of placed positions.

Table 3.2: Summary statistics and bivariate correlations

	var. no	Obs.	Mean	SD	var. 1	var. 2	var. 3	var. 4	var. 5	var. 6	var. 7	var. 8	var. 9	var. 10	var. 11	var. 12
total.comp – placed	1	2034	330.90	259.97												
total.comp – prior	2	2034	293.93	388.03	0.34***											
total.experience	3	2034	18.87	6.40	0.16***	0.10***										
job.rank – placed	4	2034	0.48	0.50	-0.22***	-0.16***	-0.24***									
job.rank – prior	5	2034	0.58	0.49	-0.23***	-0.16***	-0.22***	0.48***								
gender	6	2034	0.18	0.38	-0.06**	-0.07**	-0.06**	0.11***	0.06**							
ivy.league	7	2034	0.05	0.22	0.08***	0.04	0.04	-0.11***	-0.06**	-0.02						
mba	8	2034	0.12	0.33	0.04	0.00	-0.03	-0.01	0.00	0.00	0.24***					
public – placed	9	2034	0.53	0.50	-0.01	0.00	-0.04	0.06**	0.08***	0.04	-0.01	0.02				
public – prior	10	2034	0.60	0.49	0.03	-0.02	0.00	-0.04	0.03	-0.02	0.01	0.00	0.24***			
fem.industry	11	2034	0.45	0.50	0.08***	0.04*	-0.01	-0.04	-0.07***	0.06**	0.09***	0.03	-0.12***	-0.06**		
fem.function	12	2034	0.36	0.48	-0.08***	-0.05*	-0.05*	0.07**	0.10***	0.23***	0.01	-0.01	0.09***	0.03	0.05*	
fem.country	13	2034	0.48	0.50	0.14***	0.10***	0.22***	-0.46***	-0.38***	0.01	0.16***	0.00	-0.04	0.07**	0.06**	0.02

Table 3.3: Comparisons of means between male and female executives

Statistic	Men			Women			t-statistic
	Obs.	Mean	SD	Obs.	Mean	SD	
total.comp − placed ('000)	1,670	338.07	249.76	364	298.01	300.64	2.37
total.comp − prior ('000)	1,670	306.08	422.03	364	238.20	143.37	5.31
total.experience	1,670	19.05	6.34	364	18.06	6.62	2.60
ivy.league	1,670	0.05	0.22	364	0.04	0.20	0.93
mba	1,670	0.12	0.33	364	0.12	0.33	0.04
public	1,670	0.53	0.50	364	0.57	0.50	1.61

Of the placed executives, 18% are women, a notably higher proportion compared to data sets relying exclusively on C-level executives. Table 3.3 provides a comparison of average compensation and experience levels between men and women. Among the executives represented in the data, men make more than women do, both in salary and total compensation, and in both prior and placed jobs. Men on average have 19 years of professional experience, while women have 18.

In the following reported results, we begin by measuring the gender gap in compensation among the observed population. We decompose the gender gap with the stepwise introduction of control variables and find that measurable gender differences in pay unexplained by our controls still exist. We observe a difference in observed gender gap between prior and placed jobs, with a smaller gap in the placement condition. Next, we move to examining how the job move affects the gender gap by studying interactions between *gender* and the *placed* binary variable, first in pooled OLS models, then in individual fixed effects specifications to control for individual differences. We find evidence for reduced gender pay gaps among job switchers. Finally, we explore circumstances that moderate this switch effect on the pay gap by comparing models across partitions of the population that allow us to test the moderating hypotheses (H2 through H5).

3.4.1   Persistent Gender Gaps in Prior and Placed Compensation

Prior to exploring our hypotheses, our first order of business is to compare the average compensation level for men and women in both prior and external market settings. The mean compensation in the prior job setting for men is $306K and for women is 238K; in the external market setting, the mean compensation for men is $338K and $298K for women. The gap in pay in the prior job setting is 22%; in the external market setting, this gap drops to 12%.

Next, we use multivariate regressions to study the gender gap while controlling for additional contextual factors. We decompose the gender gap through the stepwise introduction of contextual control variables, first among compensation levels in prior positions (Table 3.4) then among placed positions (Table 3.5). The influence of control variables on the pay gap is illustrated in Figure 3.1 and Figure 3.2. These decompositions reveal three consistent patterns:

- The gender gap shrinks with the addition of controls, but there remains a measurable gap after all controls are included.

- The gender gap measured in the external market setting is lower. The raw gender gap in the prior job setting is 20%; in the external market setting it is 14%. This is indicated by the coefficients on gender of approximately -0.20 and -0.14 in Model 1 for both Table 3.4 and Table 3.5 respectively. Loading in a full complement on controls in Model 9 of both tables, the gaps shrink to approximately 11% and 6%, respectively.

- Control variables that have the largest impact of explaining the gender gap are at the job level. Controlling for *job.rank* and *job.funct* explain approximately one-third of the raw gap in the prior job setting and more than half of the gap in the external market setting.

Model 1 from Table 3.4 starts with a measured coefficient on gender of -0.20. This is equated with 20% lower pay associated with being female. The introduction of *year* dummies in Model 2

Table 3.4: Gender gap for prior position: pooled OLS

	$DV = log(total.comp)$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Constant	12.406***	12.324***	12.013***	11.803***	11.840***	11.811***	12.086***	12.136***	11.761***
	(0.141)	(0.000)	(0.167)	(0.323)	(0.327)	(0.274)	(0.409)	(0.396)	(0.405)
gender	-0.195***	-0.196***	-0.187***	-0.191***	-0.191***	-0.153***	-0.124***	-0.120***	-0.111***
	(0.045)	(0.046)	(0.049)	(0.035)	(0.035)	(0.027)	(0.027)	(0.028)	(0.026)
total.experience									0.015***
									(0.002)
highest.deg	No	No	No	No	No	No	No	Yes	Yes
mba	No	No	No	No	No	No	No	Yes	Yes
ivy.league	No	No	No	No	No	No	No	Yes	Yes
job.rank	No	No	No	No	No	No	Yes	Yes	Yes
job.funct	No	No	No	No	No	Yes	Yes	Yes	Yes
firm.size	No	No	No	No	Yes	Yes	Yes	Yes	Yes
public	No	No	No	No	Yes	Yes	Yes	Yes	Yes
industry	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
region	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2034	2034	2034	2034	2034	2034	2034	2034	2034
Adjusted R^2	0.012	0.014	0.069	0.090	0.093	0.147	0.227	0.230	0.247

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.5: Gender gap for placed positions: pooled OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$DV = log(total.comp)$								
Constant	12.587***	12.612***	12.434***	12.486***	12.450***	12.275***	12.434***	12.215***	11.887***
	(0.100)	(0.000)	(0.137)	(0.239)	(0.237)	(0.295)	(0.231)	(0.181)	(0.200)
gender	-0.140***	-0.142***	-0.136***	-0.150***	-0.150***	-0.104***	-0.070*	-0.068*	-0.061*
	(0.024)	(0.024)	(0.021)	(0.023)	(0.024)	(0.026)	(0.028)	(0.028)	(0.029)
total.experience									0.011***
									(0.002)
highest.deg	No	No	No	No	No	No	No	Yes	Yes
mba	No	No	No	No	No	No	No	Yes	Yes
ivy.league	No	No	No	No	No	No	No	Yes	Yes
job.rank	No	No	No	No	No	No	Yes	Yes	Yes
job.funct	No	No	No	No	No	Yes	Yes	Yes	Yes
firm.size	No	No	No	No	Yes	Yes	Yes	Yes	Yes
public	No	No	No	No	Yes	Yes	Yes	Yes	Yes
industry	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
region	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2034	2034	2034	2034	2034	2034	2034	2034	2034
Adjusted R^2	0.010	0.017	0.105	0.131	0.142	0.210	0.269	0.270	0.289

Standard errors in parentheses

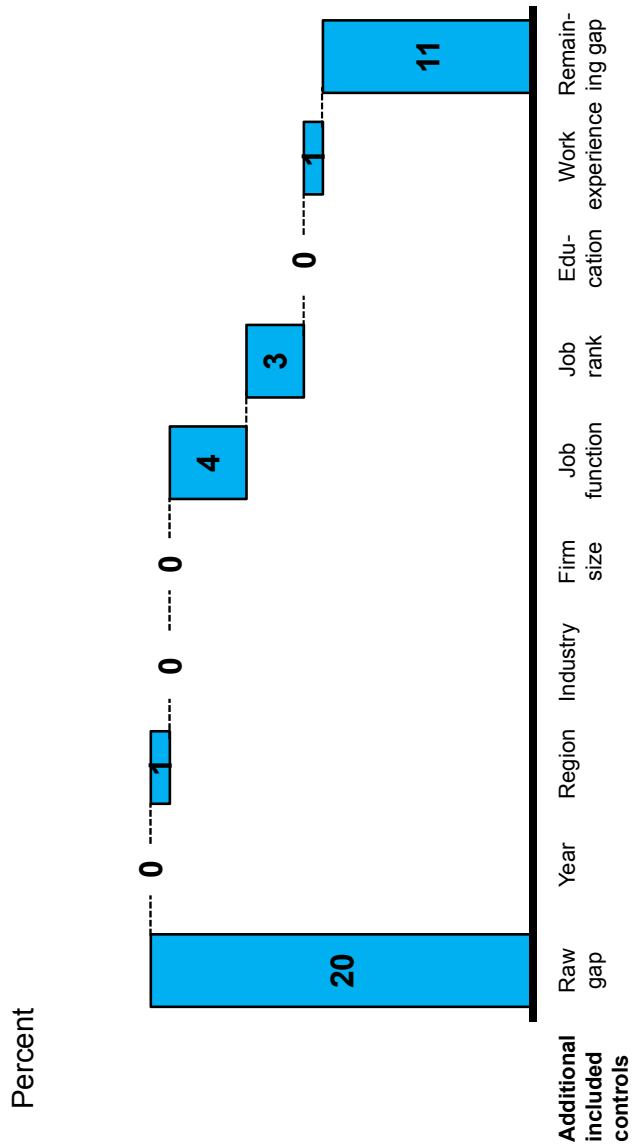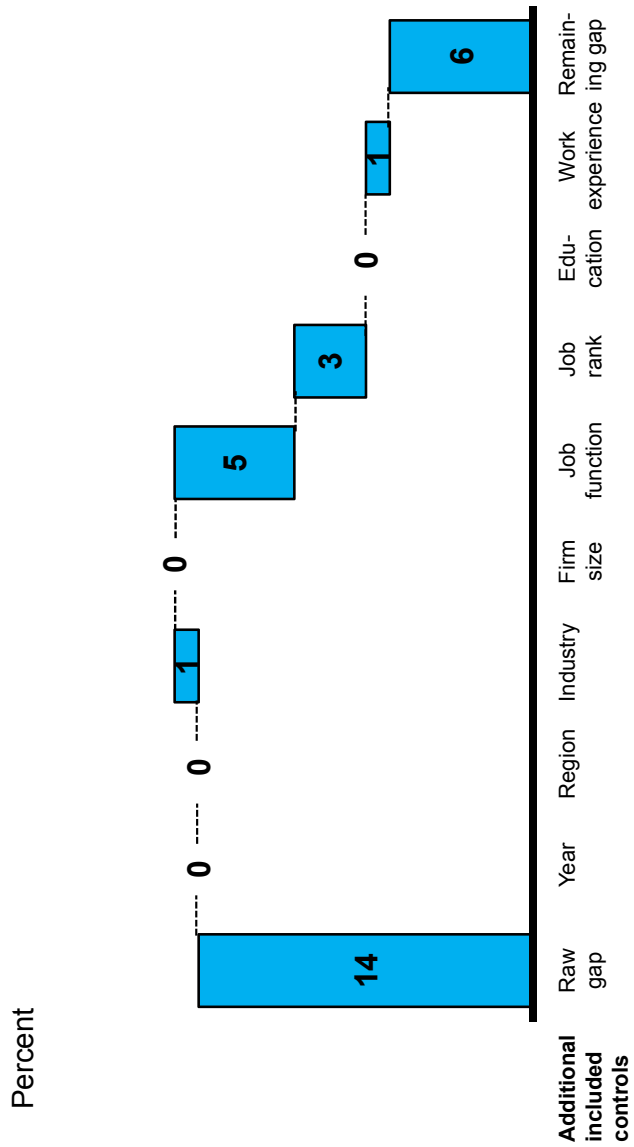* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Figure 3.1: Impact of controls on gender gap for prior job setting

Figure 3.2: Impact of controls on gender gap for external market setting

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leaves this estimate unchanged, and *region* dummies included in Model 3 lower the coefficient on *gender* slightly to −0.19. Controlling for *industry* in Model 4 and firm-level covariates (*firm.size* and *public*) in Model 5 do not materially change the gender gap. Models 6 and 7 introduce job-level covariates *job.rank* and *job.funct*, and represent the first material movement in the measured gender gap. The gap shrinks by about a third, from −19% to −12%. Introducing education controls in Model 8 does not change the estimate. Finally, Model 9 controls for number of years of professional experience, reducing the gender gap by 1 percentage point to −11%. Table 3.5 replicates the analysis from Table 3.4 on the data, capturing placements in the external market setting. The relative effects of the stepwise inclusion of controls are similar to Table 3.4; however, the initial raw gap and the final gap as reported in Model 1 and Model 9 differ. Among placements, the gender gap starts at 14% and ends at 6%.

Consistent findings in wage decompositions across these two settings provide calibration of how controls explain the gap. However, the sizable difference in the measured gap between prior job and external market settings motivates exploring how the job change event itself influences the gender gap.

3.4.2   Does Job Switching Result in Higher Premiums for Women?

Table 3.6 uses both the prior and placed data to study the switch effect on the gap by interacting *gender* with *placed* over a series of regressions that introduces control variables in an identical way to Table 3.4. Consistent with the commonly held belief that substantial compensation increases among management executives are effected through job moves, we observe that the coefficient on *placed* is consistently 18 –19% until the *job.rank* control is introduced; this control absorbs promotion effects for those advancing to higher-ranking job titles. Even after controlling for such promotion effects, the average premium for job switchers stands at 16%. The coefficient on *gender* × *placed* quantifies any incremental effect for job switching that is specific to women. This tests if job switching mea-

surably affects the gender gap. In none of these specifications is the coefficient on *gender × placed* significant at the $p < 0.05$ level.

Table 3.6: Gender interacted with placed: pooled OLS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	$DV = log(total.comp)$										
Constant	12.402***	12.406***	12.377***	12.123***	12.010***	12.045***	11.922***	12.148***	12.054***	12.074***	11.721***
	(0.139)	(0.141)	(0.021)	(0.099)	(0.216)	(0.192)	(0.179)	(0.234)	(0.209)	(0.208)	(0.221)
gender	-0.167***	-0.195***	-0.196***	-0.188***	-0.196***	-0.196***	-0.155***	-0.131***	-0.130***	-0.128***	-0.119***
	(0.030)	(0.045)	(0.045)	(0.047)	(0.036)	(0.038)	(0.032)	(0.030)	(0.030)	(0.031)	(0.029)
placed	0.191***	0.181***	0.181***	0.182***	0.181***	0.181***	0.184***	0.155***	0.155***	0.155***	0.158***
	(0.040)	(0.043)	(0.042)	(0.046)	(0.042)	(0.042)	(0.040)	(0.038)	(0.038)	(0.038)	(0.039)
gender × placed		0.054	0.054	0.053	0.050	0.053	0.054	0.073	0.073	0.073	0.073
		(0.042)	(0.043)	(0.046)	(0.043)	(0.044)	(0.047)	(0.046)	(0.046)	(0.047)	(0.047)
total.experience											0.013***
											(0.002)
highest.deg	No	No	No	No	No	No	No	No	Yes	Yes	Yes
mba	No	No	No	No	No	No	No	No	No	Yes	Yes
ivy.league	No	No	No	No	No	No	No	No	No	Yes	Yes
job.rank	No	No	No	No	No	No	No	Yes	Yes	Yes	Yes
job.funct	No	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes
firm.size	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
public	No	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes
industry	No	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
region	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
year	No	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4068	4068	4068	4068	4068	4068	4068	4068	4068	4068	4068
Adjusted R^2	0.035	0.035	0.040	0.104	0.127	0.132	0.188	0.257	0.258	0.259	0.276

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Until this point, all regression models have used pooled OLS specifications. Though controls measuring individual ability differences are included, unobserved individual differences are not addressed using such models. To control for these differences, we exploit multiple observations per

executive captured in our data using individual fixed effects models.

Because gender is invariant with time, the standard setup of individual fixed effects regressions is unable to provide estimates. Fixed effects models can estimate the effect of stable attributes over different periods by interacting them with time-period variables, because such interactions vary with time (Allison, 2009). The interaction term *gender* × *placed* can be estimated with individual fixed effects, but the coefficient on *gender* is absorbed into the intercept. The interpretation on *gender* × *placed* is the incremental effect of mobility on the gender gap beyond the effect of gender in the prior condition. This is precisely the quantity we seek to estimate.

Fixed effects regressions based on Equation 3.2 are reported in Table 3.7. Similar to prior tables, we introduce time-varying control variables incrementally. Model 1 reports that the coefficient on *gender* × *placed* is 0.05, just outside conventional parameters of statistical significance with a *p*-value of 0.06. The introduction of regional dummies in Model 2 bring the *gender* × *placed* coefficient (0.06) into the range of statistical significance at the $p < 0.05$ level, suggesting that being female is linked to higher compensation outcomes when moving. Subsequent introductions of *industry* (Model 3), firm attributes (Model 4), and job-level attributes (Model 5) do not materially change the estimate on *gender* × *placed*. In Model 5, controlling for both time-variant contextual variables and time-invariant individual characteristics, the coefficient of 0.06 on *gender* × *placed* translates into a 6% reduction in the gender pay gap among movers. These findings support H1.

3.4.3 Are Gender-Specific Premiums Higher among High-Ranking Executives?

H2 explores moderating conditions of this main result by seniority. If scarcity drives higher pay for women, conditions that intensify scarcity should be equated with even higher compensation. We test H2's contention that the switch effect on narrowing the gender gap will be more pronounced among higher-ranked executives, as the talent pool of women at this level is more exclusive. We use fixed effects models to test switch effects on the gender gap for junior and senior placements. Results

Table 3.7: Gender interacted with placed, fixed effects regressions

			$DV = log(total.comp)$		
	(1)	(2)	(3)	(4)	(5)
gender $\times$ placed	0.054	0.056*	0.057*	0.059*	0.060*
	(0.028)	(0.028)	(0.028)	(0.028)	(0.029)
placed	0.181***	0.181***	0.182***	0.186***	0.180***
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)
job.funct	No	No	No	No	Yes
job.rank	No	No	No	No	Yes
firm.size	No	No	No	Yes	Yes
public	No	No	No	Yes	Yes
industry	No	No	Yes	Yes	Yes
region	No	Yes	Yes	Yes	Yes
Observations	4068	4068	4068	4068	4068
R^2	0.132	0.135	0.150	0.160	0.176

Standard errors in parentheses

$^*\, p < 0.05$, $^{**}\, p < 0.01$, $^{***}\, p < 0.001$

are reported in Table 3.8. Model 1 replicates the fixed effects specification from Model 5 of Table 3.7 on the subset of observations for placements at the junior *job.rank* level. All placements here are from executives that are coming from and placed into junior positions. The interaction on *gender* × *placed* is not statistically significant. Model 2 replicates this model using only senior placements (*job.rank* = 0). Here, the *gender* × *placed* coefficient is 0.14, statistically significant at the $p < 0.001$ level. This is more than twice as high as the pooled sample of Model 5 from Table 3.7, indicating that the switching effect on the gender gap is largely driven by moves at the senior *job.rank* level. Model 3 pools the observations from Model 1 and Model 2 in an interaction model to test if the switch effect on the gender gap among senior placements is statistically distinct from that of junior placements. The coefficient on *gender* × *placed* measures the switch effect in the placement job among senior executives. At 0.13, it is statistically significant and positive, similar to that of Model 2. The *gender* × *placed* × *job.rank* coefficient measures the switch effect in the placement job among junior executives. It is −0.14; the *placed* × *job.rank* coefficient is 0.16, indicating that junior men have a positive effect of switching. To measure the overall compensation impact for junior switchers that are women requires combining the coefficients on *gender* × *placed*, *placed* × *job.rank*, and *gender* × *placed* × *job.rank*. Examining these coefficients, we can make comparisons of the effect of switching for men versus women, conditional on whether they are senior or junior placements. That job switching does not have a gap-narrowing effect for junior women stems from the fact that junior male executives get a higher pay raise for moving (16%), a moving premium that does not happen for junior women incremental to their 13.2% pay raise over senior men. Junior women are observed to have comparable pay raises to that of senior women. However, this increase does not have a gap-narrowing effect for them because junior men are observed to command slightly higher raises than junior women.

Table 3.8: Fixed effects regressions, comparing gender effects among junior executives versus senior executives

	(1)	(2)	(3)
	"Junior execs"	"Senior execs"	"Interaction model"
	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	-0.013	0.139***	0.132**
	(0.046)	(0.042)	(0.042)
$placed$	0.253***	0.086***	0.097***
	(0.024)	(0.014)	(0.015)
$gender \times placed \times job.rank$			-0.143*
			(0.061)
$placed \times job.rank$			0.159***
			(0.027)
$job.funct$	Yes	Yes	Yes
$firm.size$	Yes	Yes	Yes
$public$	Yes	Yes	Yes
$industry$	Yes	Yes	Yes
$region$	Yes	Yes	Yes
Observations	1624	1368	2992
R^2	0.218	0.182	0.182

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.4.4 Are Gender-Specific Premiums Higher among Industries and Functions with Lower Prevalence of Women Managers?

Scarcity of women may be driven not only by sparse representation higher up the corporate ladder, but also among industries that have had less success in developing women among its management ranks.

We test H3's prediction of stronger scarcity effects in industries with less female representation using individual fixed effects regression models reported in Table 3.9. Model 1 replicates the fixed effects specification from Model 5 in Table 3.7 on the subset of observations for placements within industries with lower prevalence of women. All placements here are from executives that are coming from and placed into lower-prevalence industries (*fem.industry* $= 0$). For this population, the coefficient on *gender* $\times$ *placed* is 0.13, statistically significant at the $p < 0.05$ level. This is equated with a 13% incremental pay increase over that of men. Model 2 reports the results for industries with higher representation of women in manager roles (*fem.industry* $= 1$). The coefficient on *gender* $\times$ *placed* is not distinct from zero. Comparing results from Model 1 and Model 2 provides initial support for H3.

Model 3 pools the observations from Model 1 and Model 2 in an interaction model to test if the switch effect on the gender gap in industries with lower prevalence of women is statistically distinct from that of industries with higher prevalence of women. The coefficient on *gender* $\times$ *placed* measures the gender effect in placement for lower-prevalence industries. At 0.13, it is statistically significant and has a positive effect, similar to that of Model 1. The *gender* $\times$ *placed* $\times$ *fem.industry* coefficient measures the gender effect in the placement job for higher-prevalence industries. Though the negative coefficient is congruent with expectations, the precision attenuates to the $p < 0.10$ level, providing weaker evidence that the impact of job switching on the gender gap between lower-prevalence and higher-prevalence industries is statistically distinct.

The *placed × fem.industry* coefficient is −0.08, indicating that male executives moving among industries with higher prevalence of women have 8% lower pay raises compared to men moving in industries with lower prevalence of women. Both men and women in industries with higher prevalence of women have lower pay raises. Contrary to the case comparing junior and senior women, women moving in industries with lower prevalence of female managers do in fact command higher raises than women moving in industries with higher prevalence of female managers.

We test H4's prediction of stronger scarcity effects in functions with less female representation. We calculate gender representation by function based on aggregated data capturing the top 20 officers of the Fortune 500 firms as reported by Capital IQ in December, 2014. The top half of functions by female representation are classified as "high prevalence" (*fem.function* = 1) and the bottom half are classified as "low prevalence" (*fem.function* = 0) (see Table 3.1 on page 63). Individual fixed effects regression models are reported in Table 3.10. Model 1 replicates the fixed effects specification from Model 5 in Table 3.7 on the subset of observations for placements within functions with lower prevalence of women. All placements in Model 1 of Table 3.10 are from executives that are coming from and placed into lower-prevalence functions. For this population, the coefficient on *gender × placed* is 0.12, statistically significant at the $p < 0.05$ level. This is equated with a 12% higher pay for job-switching women compared to job-switching men, holding all else equal. Model 2 reports results for job moves among functions with higher prevalence of women. The coefficient on *gender × placed* (0.05) is not statistically distinct from zero. Comparing results from Model 1 and Model 2 provides initial support for H4.

3.4.5 Are Gender-Specific Premiums Higher among Countries with Greater Economic Opportunities for Women?

We compare placements in countries scoring in the upper half of the World Economic Forum scores (*fem.country* = 1) to those in the bottom half (*fem.country* = 0). Model 1 in Table 3.11 reports

Table 3.9: Fixed effects regressions, comparing industries with lower prevalence of women managers versus industries with higher prevalence of women

	(1)	(2)	(3)
	"Lower prevalence"	"Higher prevalence"	"Interaction model"
	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	0.132*	0.022	0.133*
	(0.061)	(0.036)	(0.060)
$placed \times fem.industry$			-0.076**
			(0.027)
$gender \times placed \times fem.industry$			-0.113
			(0.070)
$placed$	0.208***	0.132***	0.207***
	(0.019)	(0.022)	(0.018)
$job.rank$	Yes	Yes	Yes
$job.funct$	Yes	Yes	Yes
$firm.size$	Yes	Yes	Yes
$public$	Yes	Yes	Yes
$region$	Yes	Yes	Yes
Observations	1718	1324	3042
R^2	0.201	0.137	0.167

Standard errors in parentheses

$^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

Table 3.10: Fixed effects regressions, comparing functions with lower prevalence of women versus functions with higher prevalence of women

	(1)	(2)
	"Lower prevalence"	"Higher prevalence"
	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	0.118^*	0.051
	(0.057)	(0.043)
$placed$	0.178^{***}	0.173^{***}
	(0.016)	(0.027)
$job.funct$	Yes	Yes
$job.rank$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	2272	1176
R^2	0.197	0.218

Standard errors in parentheses

$^* \, p < 0.05, \, ^{**} \, p < 0.01, \, ^{***} \, p < 0.001$

results among countries with higher scores on economic opportunity for women, and Model 2 captures results for countries with lower scores. The coefficient on *gender × placed* measures the compensation impact of moving for women distinct from that of men. Among countries with stronger economic opportunities for women, Model 1 reports a coefficient of 0.07, significant at the $p < 0.05$ level. In Model 2, capturing countries with lower scores, the coefficient is smaller (0.06) and not statistically significant.

Such results may simply reflect greater opportunities for women in general in high-scoring countries, rather than a demand-side effect. To further calibrate our analyses, we combine supply and demand influences by comparing the *gender × placed* coefficients between high and low prevalence industries (H3) in higher economic opportunity countries versus lower economic opportunity countries in Table 3.12. Only among countries with higher economic opportunity for women do we observe the scarcity effects for women switchers among low-prevalence industries. The positive coefficient on *gender × placed* in industries with fewer women managers is 0.10, significant at the $p < 0.05$ level. This is twice as high as the coefficient for industries with higher prevalence of women managers (0.05), which is not significant at the $p < 0.05$ level. This distinction by industry type does not surface in lower economic opportunity countries. The coefficient on *gender × placed* for both industry types are not statistically distinct from zero among countries with low WEF scores. Table 3.13 replicates this analysis for lower and higher prevalence functions (H4). Results are similar to Table 3.12. Only among countries with higher WEF scores do we observe the scarcity effects for women switchers among low-prevalence functions. The positive coefficient on *gender × placed* in functions with lower prevalence of women is 0.12, significant at the $p < 0.05$ level. This is twice as high as the coefficient for functions with higher prevalence of women (0.06), which is not significant at the $p < 0.05$ level. As with industry, this distinction by function does not surface in countries with low WEF scores.

Table 3.11: Fixed effects regressions, comparing countries with higher economic opportunity for women versus lower economic opportunity for women

	(1)	(2)
	"Higher economic opportunity"	"Lower economic opportunity"
	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	0.073[**]	0.056
	(0.026)	(0.053)
$placed$	0.117[***]	0.232[***]
	(0.015)	(0.021)
$job.funct$	Yes	Yes
$job.rank$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
Observations	1882	2004
R^2	0.213	0.199

Standard errors in parentheses

[*] $p < 0.05$, [**] $p < 0.01$, [***] $p < 0.001$

Table 3.12: Fixed effects regressions, comparing industries with lower prevalence of women versus industries with higher prevalence of women by country type

	"Higher economic opportunity country"		"Lower economic opportunity country"	
	(1)	(2)	(3)	(4)
	"Lower prevalence"	"Higher prevalence"	"Lower prevalence"	"Higher prevalence"
	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$
gender × placed	0.098*	0.052	0.182	-0.033
	(0.049)	(0.043)	(0.102)	(0.066)
placed	0.135***	0.106***	0.255***	0.166***
	(0.019)	(0.030)	(0.030)	(0.033)
job.funct	Yes	Yes	Yes	Yes
job.rank	Yes	Yes	Yes	Yes
firm.size	Yes	Yes	Yes	Yes
public	Yes	Yes	Yes	Yes
Observations	736	700	884	578
R^2	0.316	0.183	0.229	0.213

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.13: Fixed effects regressions, comparing functions with lower prevalence of women versus functions with higher prevalence of women by country type

	"Higher economic opportunity country"		"Lower economic opportunity country"	
	(1)	(2)	(3)	(4)
	"Lower prevalence"	"Higher prevalence"	"Lower prevalence"	"Higher prevalence"
	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$	$log(total.comp)$
gender × *placed*	0.120**	0.058	0.138	0.046
	(0.044)	(0.035)	(0.102)	(0.080)
placed	0.116***	0.103***	0.226***	0.235***
	(0.021)	(0.024)	(0.026)	(0.048)
job.rank	Yes	Yes	Yes	Yes
firm.size	Yes	Yes	Yes	Yes
public	Yes	Yes	Yes	Yes
industry	Yes	Yes	Yes	Yes
Observations	1020	590	1146	548
R^2	0.223	0.261	0.213	0.263

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.4.6 Additional Analyses

Testing Selection on the Underpaid

The results reported here suggest mobility may be a means for women to make progress on narrowing pay disparities. Because the sample consists of observed job switchers, an alternative selection story may be at work: a population of job switchers may overrepresent those who think they are being underpaid for their qualifications. Given longstanding gender pay differences, women may be subject to a stronger sense of being underpaid and a longer period of tolerating such unfairness. By the time the job switch is observed, such women may be due for a more significant pay upgrade compared to men.

To explore this alternative, we partition our data into two populations: those who are "underpaid" and those who are not. To measure this, we use the following pooled OLS regression model:

$$
\begin{aligned}
log(placed.comp)_i \;=\; & \alpha_i + \beta_1 total.experience_i + \beta_2 total.experience_i^2 \\
& + \beta_3 function.experience_i + \beta_4 function.experience_i^2 \\
& + \beta_5 industry.experience_i + \beta_6 industry.experience_i^2 \\
& + \beta_7 gm.experience_i + \beta_8 gm.experience_i^2 \\
& + \beta_9 gender_i + \beta_{10} highest.deg_i + \beta_{11} ivy.league_i \\
& + \beta_{12} mba_i + \beta_{13} job.rank_i + \beta_{14} job.funct_i \\
& + \beta_{15} firm.size_i + \beta_{16} public_i + \beta_{17} industry_i \\
& + \beta_{18} region_i + \beta_{19} year_i + \varepsilon_i
\end{aligned}
\tag{3.3}
$$

Residuals from this model measure those who are underpaid, taking into account professional

qualifications (education and experience by type), job attributes, firm attributes, industry, region, and year. Negative residuals identify if executives are "underpaid" (*underpaid* $=$ 1), and positive residuals represent executives who are not underpaid. If selection on perceived pay dissatisfaction is driving results, we would expect to observe a stronger effect for women switchers in the underpaid population. Results are reported in Table 3.14. This model replicates the fixed effects model we use in Model 5 in Table 3.7. The coefficients on *gender* $\times$ *placed* are similar. The coefficient for Model 1 is 0.06 in the population where *underpaid* is set to 1. For Model 2, *underpaid* is set to 0, and the coefficient is 0.05. Only in the population that is not underpaid is the coefficient statistically significant. These results suggest that our results are not driven by selection bias of women who have endured pay disparity for too long.

Testing Differences in Risk Attitudes

Another potential explanation for the results we see comes from gender differences in risk aversion. As discussed earlier, prior work using both lab and archival designs indicates that women are more risk averse than men. If women are more risk averse, then it is possible that they require greater premiums to motivate a job move.

One measure of risk tolerance is the amount of pay at risk in the form of performance bonuses. Those who have historically aligned with compensation having a higher proportion of performance bonus reveal a higher tolerance for risk.

To explore this risk attitude explanation, we partition our data into two populations: those with stronger risk aversion versus those with weaker risk aversion. As a measure of risk aversion, we use residuals from the following pooled OLS regression model:

$$\frac{performance.comp_{prior}}{total.comp_{prior}} = \alpha_i + \beta_1 total.experience_i + \beta_2 highest.deg_i$$
$$+ \beta_3 ivy.league_i + \beta_4 mba_i + \beta_5 job.rank_i$$

Table 3.14: Fixed effects regressions, comparing gender effects among underpaid executives versus not underpaid executives

	(1)	(2)
	"Underpaid"	"Not underpaid"
	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	0.060	0.053[*]
	(0.053)	(0.025)
$placed$	0.318[***]	0.068[***]
	(0.025)	(0.012)
$job.funct$	Yes	Yes
$job.rank$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	1762	2306
R^2	0.300	0.153

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

$$+ \beta_6 job.funct_i + \beta_7 firm.size + \beta_8 public_i$$

$$+ \beta_9 industry_i + \beta_{10} region_i + \beta_{11} year_i + \varepsilon_i \qquad (3.4)$$

Positive residuals from this model indicate those who have been willing to put more of their compensation at risk, controlling for professional qualifications, job attributes, firm attributes, industry, region, and year. Negative residuals identify executives who have stronger risk aversion, with less of their cash compensation at risk ($riskaverse = 1$); positive residuals represent executives who have weaker risk aversion ($riskaverse = 0$).

Results reported in Table 3.15 suggest differences in risk preferences may contribute to observed positive effects on compensation for women switchers. Model 1 captures the population with stronger risk aversion. The coefficient on $gender \times placed$ for Model 1 is 0.09, statistically significant at the $p < 0.05$ level. This is a stronger effect compared to the total population. In the less risk averse population (Model 2), the $gender \times placed$ coefficient is small (0.002) and not statistically distinct from zero at the $p < 0.05$ level. These results suggest that differences in attitudes toward risk may help explain the conditions where we would expect to see gender differences in the compensation impact from switching. Within the population of executives with more risk aversion, risk aversion as measured by the residuals from the model in Equation 3.4 is statistically undifferentiated between women and men at the $p < 0.05$ level (t-statistic = 1.78). Matched on risk attitudes that favor conservatism, women are observed to capture higher pay in job switches compared to men. Among the population with higher revealed risk tolerance, men are observed to have higher tolerances compared to women that are statistically distinct (t-statistic = 2.86). As such, at higher risk profiles, there is greater ambiguity for how risk attitudes could influence compensation outcomes. Higher risk tolerance for women may dampen the necessary price to motivate a move. On the other hand, a stronger appetite for performance-based compensation could raise compensation levels for both genders. Because risk attitudes can affect both the willingness to move and the compensation level itself, fur-

ther work is necessary to fully disentangle how different attitudes toward risk may impact the pay differences by gender, particularly when risk tolerances are generally high for both sides.

3.5 Discussion

This paper examines how job switching among executives on the external labor market influences the gender pay gap. Using proprietary data from a global executive placement firm, we use the prior and placed compensation levels among a population of executive job switchers to estimate the switch effect, controlling for individual differences. We narrow our investigation sharply to the actual switch event, using fixed effects to control for individual attributes, including those that could influence propensity for job switching and individual ability. Contrary to prior work, our results support the potential for mobility to help close the gender pay gap among executives. Testing the job switching effect in a set of moderating conditions, our results are consistent with the argument that scarcity may narrow the gender pay gap among job switchers. Pay gaps are reduced more among senior executive placements compared to junior placements. Also, partitioning the data by industry, pay gap reductions were more pronounced in industries characterized by fewer women in managerial positions.

3.5.1 Implications for Researchers

This paper makes both an empirical and theoretical contribution to the growing body of research studying the gender compensation gap. Empirically, this work makes progress on isolating the switch effect on the gender gap distinct from individual attributes of switchers. The results reported here address longstanding empirical challenges of accounting for individual differences in ability. This is particularly germane in a setting where standard covariates only partially capture perceived abilities and where the overall evaluation of talent may be a complex function of how such compo-

Table 3.15: Fixed effects regressions, comparing gender effects among more risk averse versus less risk averse executives

	(1)	(2)
	"More risk averse"	"Less risk averse"
	$log(total.comp)$	$log(total.comp)$
$gender \times placed$	0.094^{*}	0.002
	(0.045)	(0.027)
$placed$	0.298^{***}	0.062^{***}
	(0.021)	(0.012)
$job.funct$	Yes	Yes
$job.rank$	Yes	Yes
$firm.size$	Yes	Yes
$public$	Yes	Yes
$industry$	Yes	Yes
$region$	Yes	Yes
Observations	2034	2034
R^2	0.308	0.096

Standard errors in parentheses

$^{*} p < 0.05,$ $^{**} p < 0.01,$ $^{***} p < 0.001$

nent attributes are combined.

Our findings sound a cautionary note regarding the care with which gender gap research must proceed. Our findings follow a stream of replicated empirical work suggesting that mobility has a deleterious effect on the gender gap. In light of the preponderance of evidence contrary to our findings, replication of our results in other settings is critical to building out the most informed perspective on how job switching affects the gender gap in executive compensation. That said, this work highlights the importance of accounting for individual differences in empirical studies of compensation. As new data become available that capture compensation contemporaneously with events of interest, embracing empirical approaches that exploit panel methods can provide traction on issues of unmeasured characteristics.

Theoretically, we argue and empirically support that while scarcity of women executives may represent an undesirable social outcome, it may also motivate a greater demand for women with backgrounds comparable to their male contemporaries. While persistent gender pay gaps suggest market may fail to appropriately value human capital, these findings highlight conditions where markets may in fact assign premiums, motivating further theory development of how economic incentives may be used alongside mandates to advance the fullest utilization of the total workforce.

In addition to the mechanism of scarcity plus demand, two other mechanisms explaining the finding should be examined: that women at the executive level are more qualified than their male peers, and/or that women are better at switching jobs.

Some evidence suggests that star women may outperform star men. Lyness and Heilman, in a study of upper-middle and senior managers at a large financial services company, found that promoted women had received higher performance ratings than men who had received similar promotions, suggesting that women had to meet a higher bar to advance (Lyness and Heilman, 2006). Groysberg (2010) in a study of star equities analysts, found that women ranked as All-Stars by *Institutional Investor* magazine had a higher group average rank than men, and that more women ranked

first than any other rank.

A more conservative hypothesis is that high-achieving women are better at changing jobs than men are. Groysberg (2008; 2010) found that female star analysts who changed jobs did not suffer a performance penalty for moving compared to their male counterparts. This was because women had more portable networks than men. They found it more difficult to build in-house relationships, and instead focused on clients, the media, and other external contacts. This was a form of social capital they could take with them when they left. Women also did more extensive research before joining a firm, making sure the culture of the firm was a good one for women and would promote their success. As Fred Fraenkel, a former research director, explained:

> They [women] are much more inquisitive about the culture of the firm ... Because
> you hear over and over and over again that, say, Goldman Sachs had this great culture—
> but it was a great culture for men. And so it wasn't enough to know if a firm had
> a good culture. You have to know how does a woman actually stack up? And, you
> know, "Here's our big-name women." Does that mean anything? Or does that mean
> that they're just showing you some big-name women? What does it mean for all
> women? It varies quite a bit.(Groysberg, 2010, pp. 182-183)

These tactics were largely defensive maneuvers on the part of the women, aimed at mitigating the difficulties they had in building in-house relationships, the likelihood of being hired as a token, the greater difficulty in finding a cultural fit in a male-dominated industry, and the higher likelihood of being laid off in a downturn. However, they nonetheless protected the portability of star women's performance. Further research may help in untangling these dimensions.

3.5.2   Implications for Management Professinals

The findings reported here have important practical implications for career-building executives and for hiring firms. For rising executives, mobility represents disruptions, costs, and career risk, and some argue that such costs could be disproportionally higher for women, particularly if such women are more likely than men to have spouses with high relocation costs. That said, mobility might come with disproportionate gains for women as well. Such gains may be related to scarcity. It is important to note that such career moves represent options that individuals can make independently. As such, mobility may be an equalizing lever that can be pulled despite the slow pace of social reform.

These findings indicate that firms should pay close attention to developed women leaders in executive ranks. If women become the target of poaching due to scarcity, replacement through internal development or external acquisition can become a costly proposition. The extent to which retention of women creates challenges is context specific; however, firms may be well advised to assess their exposure to poaching, their sources of either developing or replacing female talent, and the tactics that such retention efforts would look like. In industries such as retail trade in the United States, women have strong representation, representing nearly half the workforce; however, among managerial ranks, women comprise a little over a third of such roles. In such cases, firms may want to consider levers that focus on development of female leadership in addition to retention. The project of developing female leaders might, like many organizational–cultural initiatives, be susceptible to vicious- or virtuous-cycle dynamics. Pro-woman practices, policies, and cultural norms reinforce each other, as do practices, policies, and cultural norms that make an organization unwelcoming to women.

Underrepresentation of women at the top of an organization can make it difficult to develop women lower in the ranks. To develop an identity as a leader, one must be able to observe role models, "try on" those role models' leadership personae and adopt their techniques, and evaluate the results (Ibarra, 1999). Fewer women at the top means fewer potential role models to choose from,

as well as an implicit message that leadership and being a woman are to some extent incompatible (Ely, Ibarra, and Kolb, 2011). A study of law firms showed that in firms with few female partners, the female associates did not view those partners as positive role models and did not have mutually supportive relationships with other women in the firm. This was not the case in firms with a high proportion of female partners (Ely, 1994). In a forthcoming study, Dezso, Ross, and Uribe found that once one woman had been appointed to a firm's top management team, the likelihood of another woman being hired at a similar level by the firm dropped by around 50% (Dezsö and Uribe, Forthcoming). They hypothesize that companies have an invisible quota for women. The effect does not seem to be driven by competition between women, as when the elite woman hired onto the team is the CEO, the effect is mitigated. In fact, the quota effect is strongest the less power the "token" woman has (Duguid, 2011; UMD Right Now, 2015).

3.5.3 Limitations and Future Research

These data provide some advantages in studying the effect of mobility on the gender gap and make progress on separating switcher attribute effects from influences of the switch itself. At the same time, this empirical study has limitations that are important to address in future research. Selection issues that limit the generalizability of this work appears at three levels: individual, firm, and process.

The compensation effect of switching can only be directly measured among switchers. At the individual level, the population captured by our data represent successful switchers, and the inference from this work is limited to such movers. All individuals we observe were amenable to considering new positions and were successful at securing a new job on the external market. Although individual fixed effects allow us to control for unobserved, time-invariant attributes, our sampling frame lacks individuals who are not motivated to switch. Executives may be more or less receptive to job moves for a variety of reasons: family concerns, perceived limitations of internal promotion potential, spousal professional goals, and so on. Of course, nonswitchers may differ from switch-

ers on a variety of other dimensions that may affect compensation. Because we use fixed effects, the lack of nonswitchers in our data does not directly threaten our identification of the switch effect on the gender gap; it does, however, limit the extent to which we can generalize the findings. These results do not extrapolate to nonswitchers who might hypothetically consider putting their skills on the market. Prior studies that compare populations of switchers and nonswitchers can offer more generalizable insights that envelop the nonswitching population and address how differences might drive compensation effects. For example, Dreher, Lee, and Clerkin (2011) find that men are more likely to pursue external job search strategies than women and minorities are, and that these strategies are linked to higher compensation. For the purposes of this paper, we opt to focus sharply on estimating the switch effect on the gender gap, separate from the switcher effects. Future work modeling switching propensity can significantly inform this work; incorporating a model of switching propensity as a prior stage to market realization of skills and personal attributes such as gender would help to generalize (or qualify) these results.

At the firm level, selection concerns arise from the fact that the data all come from a single executive search firm, and these concerns also limit generalizability. If the firm supplying these data have particularly strong access to candidates in a certain industry or function, this could bias the quality of accessible candidates by function. To the extent that these uneven capabilities are correlated with gender, outcomes may reflect gender effects that mask an underlying condition that this firm happens to be particularly strong in industries and functions where the relative strength of women versus men is higher. The firm supplying our data represents one of the largest global search firms and has broad participation in service lines across multiple geographies, industries, and functions. According to a 2014 industry report published by the Association of Executive Search Consultants (AESC), the top five firms command nearly 30% of the global market. These firms have long been considered the industry leaders, each having global presence and broad service lines across industries and functions. In 2013, each of the top five had global revenues between $500 million and $1 billion.

Firms outside the top five are substantially smaller and less diversified. The same AESC report estimates that "medium"-sized firms generate $50 –150 million, and revenues from smaller boutique are $1 –10 million. Because the sample is drawn from a top-five firm, the clientele comprising the data for this study is more likely to reflect a broader cross section of preferences. Nevertheless, while single-site research provides the benefit of detailed measures that are hard to find in data covering broader populations, generalizability of the findings will be enhanced by replication using data from other search firms.

Risk of selection bias also arises out of the placement process itself. We observe final placements, but not the shortlists of executives considered that the firms did not hire. Interviews with executive search consultants confirm that functional and industry qualifications play a significant role in the selection of the preliminary set of candidates. Women have been found to be underrepresented in placement firm databases (Judge et al., 2000; Dreher et al., 2011), suggesting that those women who do gain the attention of search firms may be quite exceptional. Further, we observe women who are not only presented by placement firms, but who are also ultimately hired by the client firm. Our observations are limited to executives who pass both screens, both women and men. Once again, this selection effect invokes caution in how we generalize these findings. Though we control for individual attributes within our sample, the magnitude of the gender effect may be different within other populations.

In summary, the limitations discussed here reflect a strong bias toward investing in identification of the switch effect on the gender gap at the cost of broader generalizability. The intense rhetoric and passion that characterizes the gender gap debate is fueled by conflicting findings with multiple interpretations. By applying a sharp focus in method and data selection to the question of job switching, this work aims to distill findings in a way that limits alternative explanations within this setting.

<div style="text-align: right; color: #8B0000; font-size: 3em; font-weight: bold;">4</div>

External Labor Markets and the Importance of Credentials to Executive Pay

4.1 Introduction

In the past, firms developed and promoted leadership primarily from within. Today, external markets are playing an increasingly important role in determining who is placed in senior positions. Between the 1970s and the mid-2000s, external hiring of top executives more than doubled from 15% to 33% (Murphy and Zábojník, 2007). External sourcing among lower-ranking executives is also on the rise (Hamori and Koyuncu, 2011; Bonet, Cappelli, and Hamori, 2013). This trend creates puzzles for human capital theorists, who have long contended that all else equal, insider candidates who have built firm-specific competencies are comparatively more productive (Becker, 1964)The trend toward greater reliance on external hires runs counter to mutual interests between executives and firms for exploiting investments in learning how to operate in a specific organization. Compounding this puzzle, the apparent abandonment of firm-specific human capital is not reflected in lower

compensation from anticipated lower productivity of outside hires; on average, external executive hiring is characterized by significant pay raises (Murphy and Zábojník, 2004; Bidwell, 2011; Aivazian, Lai, and Rahaman, 2013). These observations motivate a closer examination of what drives executive compensation and the differences between internal and external markets. This paper examines the importance of externally observable individual qualifications in predicting executive compensation and how this differs between external and internal markets. In particular, it investigates the potential role of information asymmetries in influencing compensation decisions.

Because the value of human capital is contextually defined, there are idiosyncratic reasons for why firms may value human capital differently, based on both firm needs and individual attributes (Mackey, Molloy, and Morris, 2014). However, there are also systematic differences between internal and external labor markets that can drive observable patterns. In particular, incumbent employers and external markets differ in the information available to evaluate human capital. In addition to work history, educational background, and reference checks, employers observe their managers in action and have a rich appreciation of the context in which they operate. Information asymmetries are important, because valuing human capital is far from an exact science. This is particularly true for executives, whose contributions to outcomes are obscured because they work primarily through the efforts of others. Though compensation should generally reflect individual capabilities, it is strongly influenced by the information that firms have to evaluate potential hires (Bidwell, 2011).

Because internal and external markets differ in available information, they likely also differ in how they use such information to determine compensation. Prior work argues that external markets rely more on "externally observable" markers of human capital (Spence, 1973; Grannovetter, 1981): credentials such as education and work experience. Subsequent work builds on this argument, suggesting that firms are willing to pay more for credentials for external hires (Bidwell, 2011). However, this important assumption has not been empirically tested.

Using proprietary data that captures both prior and placed compensation for 2034 job-switching

executives, this paper examines the importance of credentials to pay and how this differs between internally and externally determined compensation. Results reported here indicate that credentials such as education and work experience are more important predictors of pay in external markets —these qualifications explain 9% of the variance in pay, nearly twice as high as that for internally determined pay. Additionally, this paper finds that markers of general management capabilities are more important drivers of pay differences compared to matching experience in function or industry. Finally, this paper contrasts differences between internal and externally determined pay across circumstances where information asymmetries differ. Because the individual contributions of senior executives are more challenging to assess from the outside, this paper predicts that stronger information asymmetries will drive greater differences between internal and external markets in how human capital predictors are used. Results reported here indicate increased importance of credentials is more than three times higher (5.2 percentage points) among senior executive placements compared to junior ones (1.6 percentage points).

This paper uses multiple methods to measure how the importance of predictors differs for internally versus externally determined pay. Traditional regression analyses is complemented using variance decomposition methods for measuring predictor importance and results are robust across multiple methodologies.

4.2   Prior Literature and Hypotheses

4.2.1   Credentials Are Stronger Predictors of Pay in External Labor Markets

Firms presumably want to hire executives with capabilities to deliver organizational performance. But assessing such skills is challenging. Positive track records in similar roles provide an idea of potential, but they leave many unanswered questions about the connection between skills and firm outcomes. Prior success may not be an accurate indication of how a person will perform in a dif-

ferent setting. Executives' contributions are "causally ambiguous" because top managers work by directing the behaviors and decisions of others (Lippman and Rumelt, 1982). Organizational success is also influenced by chance (Levinthal, 1991; Blettner, Chaddad, and Bettis, 2012; Henderson, Raynor, and Ahmed, 2012), and it can be challenging to distill skill from luck based on outcomes alone. Additionally, outcomes themselves can be difficult to measure, particularly in dynamic, interdependent environments. Taken together, incomplete information creates obstacles for firms that can significantly influence organizational performance.

The information available to firms for making hiring decisions varies in quality and availability. "Externally observable" credentials, such as education and work experience (Spence, 1973; Grannovetter, 1981), are visible to everyone and have long been used as measures of human capital. Beyond these credentials, current employers have the opportunity to observe behaviors and decisions of employed managers in a variety of relevant contexts (Waldman, 1984; Greenwald, 1986). Because of this, current employers are in a better position to gather information connecting an executive's capabilities and firm outcomes compared to firms on the outside. This rich source of insight makes internal markets potentially less reliant on coarser proxies to evaluate human capital. In addition to better information on individuals, firms promoting from within are in a stronger position to assess fit between capabilities and subsequent roles (Chatman, 1991; Edwards, 1991; O'Reilly et al., 1991).

The information advantage of incumbent employers over external markets is substantiated by some empirical evidence. Performance ratings within firms have shown strong interperiod stability, suggesting that firms calibrate their assessment of performance with some precision (Baker, Gibbs, and Holmstrom, 1994; Sturman et al., 2005). In contrast, the tools that are available to evaluate external candidates have been shown to have poor reliability among assessors (Arvey and Campion, 1982; Judge et al., 2000; Posthuma et al., 2002). Job interviews are influenced by assessor bias based on physical appearance (Pingitore et al., 1994) and nonverbal cues such as eye contact and facial expressions (Dipboye, 1992). Given the lack of precise evaluation tools, firms hiring externally may rely

more on objective, externally visible signals of quality, such as work experience and education.

HYPOTHESIS 1 (H1):   *For executive job switchers, credentials are more important in explaining salary outcomes in external markets compared to internal ones.*

4.2.2   THE IMPORTANCE OF CREDENTIALS BY QUALIFICATION TYPE

If credentials matter more in external markets, which ones are the most important? Among executives, MBA degrees (Bertrand and Schoar, 2003) and graduation from prestigious Ivy League universities (Miller et al., 2015) have both been linked to higher valuations of human capital. Professional experience is also an important criterion for assessing fit and capability. The frequently used measure of total work years is a rough index of wisdom that comes with experience and age. This rough metric conflates a mix of experience types, some demonstrating general leadership capacity and others showing specific domain expertise. By parsing experience by both magnitude and type, greater insights can be gained on how external markets weigh the informativeness of different types of experience.

Because contextual familiarity is important for sound decision making, fit between experience and job may be a strong influence on compensation. Industry expertise has long been considered a critical element of executive human capital (Kotter, 1982a; Kotter, 1982b; Castanias and Helfat, 1991; Castanias and Helfat, 2001). The value of such expertise is constrained to settings where particular industry insight is relevant. Another potential dimension of fit is functional experience. Executives also specialize in particular business functions, such as finance, marketing, operations, and so on. For executives overseeing functional units, relevant functional experience may be an important driver of perceived value among hires.

In addition to fit between experience and job requirements, external markets may also consider signals of general leadership capabilities. General measures of management ability may be more

relevant in external markets (Becker, 1964; Murphy and Zábojník, 2004), because such skills are portable to the widest range of potential hirers. Custódio et al. (2013) find that "generalist" CEOs are awarded a 19% compensation premium over "specialist" CEOs, nearly $1 million more per year, and premiums are highest when generalists are hired from the external market to replace specialists. The general manager skill set may be valuable not only in general management roles, but also within functional positions. The growth of functional executives over the last 25 years (Guadalupe et al., 2014) and the flattening of executive hierarchies (Wulf, 2012) put greater pressure on functional executives to adopt the general management perspective to provide strategic insights to the business leaders they support (Groysberg, Kelly, and MacDonald, 2011; Dahlstrom, Davis, Hieronimus, and Singer, 2014). Such skills are also desirable because they enable functional leaders to work laterally among each other without the constant coordination of general management executives. Even among highly specialized functionalized roles such as the Chief Finance Officer, those with broader credentials receive higher pay compared to those with deep backgrounds in accounting and finance (Datta and Iskandar-Datta, 2014).

I argue that general management experience is a stronger predictor of pay compared to other measures of experience. As firms have moved to flatter reporting structures, that fact that there are fewer general management positions means that fewer executives have such credentials (Cappelli, 1999a; Cappelli, 1999b; Osterman, 2008). The relative scarcity of such experience makes it a stronger differentiating signal. This marker of capability is also more generally valued. While narrow domain expertise requires a matching set of circumstances to unlock its value, demonstrated ability to lead people is valued in any executive role, both in general management and increasingly within functional roles.

Hypothesis 2 (H2):   *For executive job switchers, credentials that show general management capabilities are more important in explaining salary outcomes relative to other observable qualifications.*

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4.2.3 Senior Placements Present Stronger Information Asymmetries between Internal and External Markets

As executives rise in seniority, their jobs become more complex (Agarwal, 1981; Kotter, 1982b), and evaluating their capabilities becomes more difficult. This may create even greater information asymmetries between incumbent employers and the external market for senior executive placements compared to junior ones.

There are several reasons why employers have stronger information advantages over external markets for senior hires. First, assessing how well senior executives have delivered on responsibilities is hard because their goals often encompass more dimensions compared to those of junior officers. The clearer mandates for junior executives make evaluating their performance against goals easier to verify from the outside. Second, because hiring firms are usually bringing in a single executive separate from his or her team, it is important to distill the individual contribution of the target recruit, as demonstrated effectiveness may not be replicated if supporting team members are not present (Groysberg, Lee, and Nanda, 2008). Senior executives are more dependent on the behaviors and decisions of subordinates, making their own contributions more ambiguous, particularly to outside hirers. Finally, as executives rise in seniority, their responsibilities move from executing projects to setting portfolio priorities. In such decisions, one important way executives contribute is by saying "no" to less promising projects. Though examples of such discretion are clear to internal coworkers, this is far less visible to outside observers.

In addition to being less visible to the external market, the skills of senior executives such as developing strong lieutenants, coordinating their actions, and making wise portfolio decisions are critical to performance in such roles. As a result, information asymmetries in what matters most increase as leaders move up the corporate ladder. Interviews with executive recruiters indicate that as placements rise in seniority, the focus of evaluation shifts from what they accomplished to the manner in

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which they accomplished it. For this reason, the relative information advantage of internal markets is higher for senior placements, suggesting the relative importance of credentials in external markets will be stronger.

HYPOTHESIS 3 (H3):   *The increase in importance of credentials in explaining salary are higher for senior placements than for junior placements.*

4.3   METHODS

4.3.1   SETTING

Executive search firms are becoming increasingly influential in the executive labor market (McCool, 2008), and in recent years research has focused more on how they work (Hamori et al., 2011). Such search firms are sources of recruiting expertise that manage the search and recruiting process (Britton and Ball, 1999; Finlay and Coverdill, 2007). While C-level posts have long been mediated by executive search firms (Hamori, 2010), executives lower in the hierarchy are increasingly brokered by such intermediaries. A survey from the International Association for Corporate and Professional Recruitment reports that 54% of positions with an annual salary of $150,000 or more are placed by executive search firms (Hamori and Koyuncu, 2011).

Beyond rising relevance of the intermediated external market for executives, this setting provides additional advantages for investigating the relationship between human capital and compensation. In the external market, a wider variety of experience profiles and job settings are observed than in the internal market; the external market also exhibits more diversity in matches between profiles and settings. Finally, the external market provides more generalizable estimates that are less biased by firm-specific career-track practices and compensation policies.

4.3.2 Sample

My data are from a leading global executive search firm, capturing executive placements across multiple functions, industries, seniority levels, and geographies. My sample captures detailed accounts of 2034 placements of executives from 2004 to 2011. These data contain descriptions of prior and placed positions, including geography, position title, industry, company names, and compensation levels. Individual attribute data include gender and education. I augment these data with detailed career histories. For each placement, I capture years of employment and job title information for prior positions. These data are coded for functional classification and job rank based on reported job titles.

4.3.3 Empirical Strategy —Aligning Methods with the Question

These hypotheses concern the importance of credentials as predictors for determining pay and how this differs between internal and external settings. It is key to align empirical methods with measuring importance, the quantity of interest. The importance of predictors is distinct from their effect sizes. Most often, empirical researchers use regression models to quantify associations between two variables, or in the case of causal models, to measure the effect of a unit change in predictor X on response variable Y. In these cases, regression coefficients for explanatory variables quantify a hypothesized relationship between the explanatory and the response variable, holding the effect of other variables constant. A key assumption in these studies is this *ceteris paribus* condition. To test the relationship between two variables, analysts assume stability for the remaining system of influences (McGahan and Porter, 2005). By contrast, the aim of measuring predictor importance is quantifying its ability to explain, predict, and therefore to understand observed outcomes in the presence of a system of other influences.

There are multiple ways to measure variable importance (Darlington, 1968); though there has

been much recent attention focused on variable importance measures (Soofi, Retzer, and Yasai-Ardekani, 2000; Whittaker, Fouladi, and Williams, 2002; Budescu and Azen, 2004; Johnson and LeBreton, 2004; Lebreton, Ployhart, and Ladd, 2004; Grömping, 2007), no universal definition exists (Azen and Budescu, 2003). I describe three commonly used measures, what is learned from them, and their vulnerabilities. Next, I introduce a fourth approach, the LMG model, named after its first mention by authors Lindeman, Merenda, and Gold (1980). This fourth approach uses an averaging method that integrates the other measures into a single metric, addressing several of the identified limitations inherent in the other three measures.

I employ all four approaches to test the presented hypotheses and report results supporting the main finding consistently, regardless of method.

4.3.4   Four Approaches for Determining Predictor Importance

The four approaches detailed below are summarized in Table 4.1 on page 115.

ZERO-ORDER CORRELATION   In evaluating the importance of a predictor variable, analysts often first measure bivariate correlations between explanatory variables and the response variable. This measure indicates how well any one explanatory variable performs were it selected as the sole predictor. However, zero-order correlations do not account for other factors that could also have an impact on the response variable or influence the relationship between the focal explanatory predictor and the response variable. The absence of controls means that alternative explanations are not ruled out. Additionally, bivariate correlations are not useful for synthesizing a measure of how a set of variables jointly influences the outcome, because they represent pairwise correlations that cannot be intuitively aggregated. For this reason, empirical researchers often use zero-order correlations only as a first indication for relationships to investigate with more sophisticated, multivariate regression models.

Regression coefficients   The estimated coefficients of multivariate regressions are popular for measuring relationships between constructs. The additional benefit over zero-order correlations is that they allow the analyst to account for other explanations through the introduction of control variables. Regression coefficients index the effect on the response variable for a unit change in the explanatory variable[*], holding all other quantities constant (*ceteris paribus*). What regression coefficients tell us is the effect of an explanatory variable, holding other predictors at their average. The *ceteris paribus* assumption makes the most sense when explanatory variables are uncorrelated and theoretically unrelated. However, in observational studies using archival data, regressors are typically correlated (Grömping, 2006) and this obscures the meaningfulness of this assumption and the interpretation of coefficients. In the presence of credible theory describing relationships between regressors, a unit change in a focal variable will likely imply a change in other explanatory variables as well.

The introduction of control variables is often motivated by an interest in ruling out alternative explanations for tested effects. However, the addition of controls without considering their relationship to focal explanatory variables can obscure interpretation of the results. If the control variables are correlated with the explanatory variable, then introducing those controls may result in diffusing the actual influence of the explanatory variable across their coefficients. Given the number of contextual controls that are available to compensation scholars today and their likely correlation with human capital measures, regression coefficients may underestimate both effect sizes and variable importance.

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[*]Because analysts are often interested in measuring effect size of a variable of interest rather than relative importance among all explanatory variables, units for variables are often left in measures that allow for intuitive interpretation of what a unit change represents. For those analysts interested in measuring relative importance among variables, estimated coefficients are dependent on the units selected. Coefficients can still be used to index relative importance by normalizing all variables by their standard deviation.

Table 4.1: Summary table of approaches for determining predictor importance

Method	Description	What is learned	Limitations
Zero-order correlation	Bivariate correlation between predictor of interest and the dependent variable	The predictive performance of a predictor variable taken by itself	Cannot aggregate collective predictive performance over a set of predictors and does not account for individual predictor impact in the presence of other explanatory variables
Regression coefficients	Beta coefficients, standard output of multivariate regression analyses	The effect size on the response variable for a unit change in a predictor variable holding other predictors constant	Cannot aggregate collective predictive performance over a set of predictors, and *ceteris paribus* assumptions make interpreting coefficients challenging in the presence of correlation among explanatory variables
Semipartial correlation	Marginal increase in R^2 by including the predictor of interest after including a subset of other predictors	The incremental improvement in model prediction performance by the inclusion of the predictor of interest	In the presence of correlation among explanatory variables, allocation of R^2 is dependent on ordering of variables
LMG	R^2 decomposition by averaging allocations over the permutation of all possible variable orderings	Given the availability of a set of explanatory variables, results allocate the average contribution of variables to R^2; measures express explanatory power or "importance" of predictors	Though LMG is invariant to ordering, this statistic is relatively new; distribution properties are not yet completely known, so measures of precision are confined to nonparametric bootstrapping approaches

SEMIPARTIAL CORRELATION   In the framework of multivariate regression, the coefficient of determination, R^2, is a measure of how well a regression model explains the data; it quantifies the proportion of the variance in the dependent variable explained by the model. One intuitive measure of variable "importance" is to investigate how the introduction of a variable (or set of variables) impacts the R^2 of a model (Darlington, 1968). By comparing how the R^2 statistic changes, the explanatory contribution for each variable is measured by apportioning its share of R^2.

This approach to variance decomposition has a long history in strategy and management literature. It has most often been used to explain relative impact of year, industry, firm, and business unit on profits (Schmalensee, 1985; Rumelt, 1991; Roquebert, Phillips, and Westfall, 1996; McGahan and Porter, 1997; McGahan and Porter, 2002). In addition to the cited seminal works, other scholars have used variance decomposition to identify additional constructs that are important to predicting firm performance, including home geography of the corporate parent (Khanna and Rivkin, 2001; McGahan and Victer, 2010), subnational regional attributes of subsidiaries (Ma, Tong, and Fitza, 2013), business life cycles (Karniouchina, Carson, Short, and Ketchen, 2013), and strategic groups (Short, Ketchen, Palmer, and Hult, 2007). These techniques have included CEO variables to identify the contribution that the top officer makes to determining profits, after other factors have been accounted for (Lieberson and O'Connor, 1972; Wasserman, Anand, and Nohria, 2001; Crossland and Hambrick, 2007; Mackey, 2008).

Across these studies, a common challenge is accounting for correlation among explanatory variables. The earliest forms of decomposing R^2 followed a simple procedure: additional regressors were introduced sequentially, increasing the R^2. The additional increase in R^2 was attributed to the variable(s) introduced, and this continued until all variables were included in the model. This approach to parsing variance among regressors has been criticized because results are dependent on the order in which variables are introduced (Weiner, 1978; McGahan and Porter, 2002). By entering the variable(s) of interest last, the change in R^2 measures the semipartial correlation—the residual variance

explained after all other controls are accounted for. This is the most conservative estimate of a variable's contribution, as all variance shared with other explanatory variables will have been accounted for earlier (Wasserman et al., 2001).

Variable importance should account for "the contribution each variable makes to the prediction of a dependent variable considering *both* its unique contribution and its contribution when combined with other variables" (Johnson, 2000, italics added). The problem with these three measures of variable importance is that they each capture a portion of total variable importance. Zero-order correlations capture predictor performance individually, but not taken together with other potential predictors. Regression coefficients impart constraining assumptions on correlations among explanatory variables. Finally, the dependence of semipartial correlation methods on variable ordering requires justification of a particular sequencing for entering regressors.

LMG   Methods for measuring variable importance represent an active area in statistics research (Soofi et al., 2000; Whittaker et al., 2002) and applied methods (Azen and Budescu, 2003; Budescu and Azen, 2004; Johnson and LeBreton, 2004; Lebreton et al., 2004). One common approach to solving the order-dependency problem is to apply the heuristic of permuting over all possible orderings of regressors and averaging the portion of variance explained by component predictors over all possible orderings (Johnson, 2000). This was first introduced by Lindeman, Merenda, and Gold (1980, pp. 119-120), and this averaging approach is frequently referred to in the statistics literature as "LMG".

Though originally advanced as a heuristic for making variance decomposition results invariant to ordering, the LMG approach enjoys other properties that have increased its popularity (Grömping, 2015):

- Importance weighting accounts for the contribution of explanatory variables independently, as a marginal effect after accounting for all other variables, and conditional on any prior com-

bination of explanatory variable subsets. This is aligned with modern definitions of variable importance.

- LMG measures are invariant to linear transformations of explanatory variables.

- LMG is invariant to the addition of pure noise to any subset of variables.

- LMG is not affected by mutually exclusive, collectively exhaustive subgrouping. For example, if industry experience and functional experience variables are accounted for separately, the sum of their relative importance measures is identical to the approach of entering them as a single group.

- The importance measures of all predictors using the LMG approach always sum to the R^2 of the model.

The growing interest in relative importance methods has encouraged researchers to investigate the statistical properties of such measures to create stronger theoretical foundations for them (Grömping, 2007). At this time, measures of uncertainty such as standard errors and confidence intervals have not been analytically derived for LMG. To support comparisons among importance statistics between internal and external markets, I follow the suggested approach of using bootstrapped samples to non-parametrically generate distributions of LMG measures (Lipovetsky and Conklin, 2001; Azen and Budescu, 2003; Budescu and Azen, 2004).

4.3.5 Variables

Dependent variable   The outcome variable for all regression models is compensation measured by *salary*. Congruent with other studies using compensation as the dependent variable, I transform *salary* by taking the natural log to diminish the distortion from outliers.

Explanatory variables are separated into five groups: (1) individual credentials, (2) position attributes, (3) firm characteristics, (4) industry, and (5) year and region.

INDIVIDUAL CREDENTIALS   Individual externally observable attributes include gender, education, and work experience. *gender* is set to 1 if the executive is female, 0 if male. Education is characterized by three variables. I use highest degree attained (*highest.deg*) to control for investments in formal education. Executives fall into one of three groupings: (1) pre-bachelor's, (2) bachelor's, and (3) postgraduate for those attaining the equivalent of a master's degree or higher, including all post-baccalaureate professional schools. I also include binary variables that indicate whether the executive has attended one or more Ivy League schools (*ivy.league*) and if he or she has an MBA degree (*mba*). Work experience variables include different measures characterizing duration and type. Generally, compensation is linked to professional experience (Becker, 1964; Lazear, 1995) . To account for the total magnitude of experience, I control for total years of professional experience (*total.experience*). To capture individual-position fit measures, I measure functional experience and industry experience within the last 20 years from the point of hire. Function refers to the widely used classifications of business areas: finance, human resources, information technology (IT), marketing, operations, and so on. Function names and their breakdown of representation in the data are illustrated in Figure 2.1 on page 22. Relevant functional experience (*function.experience*) is the aggregated years of work experience in the functional area over the prior 20 years matching that of the job of placement. Relevant industry experience (*industry.experience*) captures the number of years of accumulated work experience in the industry matching the job over the last 20 years, defined by the North American Industry Classification System (NAICS) at the two-digit level (see Figure 2.2 on page 24 for a breakdown of representation in the data)jk. I measure years of general management experience in the last 20 years (*gm.experience*), indicating positions with general responsibility over geographies, product lines, or business units.

POSITION ATTRIBUTES   At the position level, I include controls for the job type based on job title classification. As executives attain higher reporting levels, the complexity of their tasks increases, which can be reflected in higher compensation levels (Agarwal, 1981). I use job rank (*job.rank*) as well as functional association (*job.funct*) to capture effects from occupational differences. *job.rank* refers to reporting level in the vertical management hierarchy. Executive job titles are coded by keywords and classified by categories including manager, director, vice president, and so on. I coarsen the classification into a binary division of senior versus junior rank, with those at the C-suite level, president, and vice-president levels considered senior positions (*job.rank* = 0), and the other positions considered junior (*job.rank* = 1). *job.funct* refers to the standard classification of departments by business function (see Figure 2.1 on page 22 for a list of function classifications and their representation in the data).

FIRM CHARACTERISTICS   I control for the firm's size (*firm.size*) by the number of employees, measured as a category variable capturing size classifications of less than 500, 501 to 1000, 1001 to 5000, 5001 to 10,000, and greater than 10,000. I also control for whether or not the company is publicly traded (*public*).

INDUSTRY   I control for the firm's industry (*industry*) using the 2012 two-digit NAICS code.

YEAR AND REGION   Because these data are global, I also control for the region (*region*) of the job placement. *region* groups job placements into Africa, Asia, Europe, North America, South America, and Australia/New Zealand. Finally, to control for macroeconomic trends, I also include dummies to capture year effects (*year*), from 2004 to 2011, inclusive.

Table 4.2 summarizes how variables are grouped.

Table 4.2: Variable groups

Variable group	Variable	Desciption
	total.experience	Total years of working experience
	industry.experience	Years of experience in industry experience matching job in the last 20 years prior to placement
Individual credentials	*functional.experience*	Years of experience in functional experience matching job in the last 20 years prior to placement
	gm.experience	Years of experience in general management positions in the last 20 years prior to placement
	highest.deg	Dummy variable capturing pre-bachelors, bachelors, and postgraduate
	mba	Set to 1 if MBA degree attained
	ivy.league	Set to 1 if attended an Ivy League school
	gender	Set to 1 if executive is female
Position	*job.rank*	Set to 0 if C-level, president, or vice-president; set to 1 otherwise
	job.funct	Dummy variables capturing standard functions (finance, marketing, operations, etc.)
Firm	*firm.size*	Dummy variables for number of employees (0–500, 501–1000, 1001–5000, 5000–10,000, 10,000+)
	public	Set to 1 if firm was publicly traded
Industry	*industry*	Dummy variables based on 2-digit NAICS classification
Year and region	*region*	Dummy variables (Africa, Asia, Europe, N. America, S. America, and Australia/New Zealand)
	year	Dummy variables 2004–2011 inclusive

4.3.6 Model Specification

$$
\begin{aligned}
log(salary)_i \ = \ & \alpha + \beta_1 total.experience_i + \beta_2 total.experience_i^2 \\
& + \beta_3 function.experience_i + \beta_4 function.experience_i^2 \\
& + \beta_5 industry.experience_i + \beta_6 industry.experience_i^2 \\
& + \beta_7 gm.experience_i + \beta_8 gm.experience_i^2 \\
& + \beta_9 gender_i + \beta_{10} highest.deg_i + \beta_{11} ivy.league_i \\
& + \beta_{12} mba_i + \beta_{13} job.rank_i + \beta_{14} job.funct_i \\
& + \beta_{15} firm.size_i + \beta_{16} public_i + \beta_{17} industry_i \\
& + \beta_{18} region_i + \beta_{19} year_i + \varepsilon_i
\end{aligned}
\tag{4.1}
$$

4.4 Results

4.4.1 Summary Statistics and Bivariate Correlations

Summary statistics and bivariate correlations are given for the prior job setting and the external market setting in Table 4.3 and Table 4.4 respectively. The mean salary in the prior job is \$227K, and the mean salary for the external market job is \$243K. The average executive in the sample has nearly 19 years of total experience. A total of 5% have degrees from Ivy League schools, and 12% have MBA degrees. About half of the observed executives are senior executives with placed titles such as "C-level," "president," or "vice-president." Women represent 18% of the placements. Breakdowns by function (Figure 2.1 on page 22) and industry classification (Figure 2.2 on page 24) show the distributions among function categories and industry are similarly represented between prior jobs and external market placements.

Bivariate or zero-order correlations with *salary* provide a measure of the strength of predictors

on their own for explaining variation in the dependent variable. None of the predictors have correlations with *salary* that are greater than 0.3. While such low correlations do not indicate a strong presence of any one factor, comparing the bivariate correlations with *salary* across Table 4.3 and Table 4.4 provides some initial insights. The zero-order correlation with *salary* for *total.experience* and *gm.experience* is more than twice as high in the external market job setting compared to the prior job setting; for *ivy.league*, it is more than three times as high. These comparative correlations offer some initial support that *total.experience*, *gm.experience*, and *ivy.league* may be more important predictors for pay in the external market job compared to the prior job setting. However, these bivariate correlations do not control for other important contextual factors. Additionally, it is not possible to aggregate the importance of personal qualifications taken as a set predictors.

Table 4.3: Summary statistics and bivariate correlations —prior job

	var.no	Obs	Mean	SD	var. 1	var. 2	var. 3	var. 4	var. 5	var. 6	var. 7	var. 8	var. 9
salary	1	2034	227.18	364.95									
total.experience	2	2034	18.87	6.40	0.07**								
function.experience	3	2034	9.43	5.65	0.02	0.30***							
industry.experience	4	2034	7.70	5.40	0.04	0.23***	0.30***						
gm.experience	5	2034	3.01	4.90	0.08***	0.24***	0.04	0.14***					
gender	6	2034	0.18	0.38	-0.05*	-0.06**	-0.01	-0.04	-0.11***				
ivy.league	7	2034	0.05	0.22	0.03	0.04	-0.01	0.03	0.08***	-0.02			
mba	8	2034	0.12	0.33	0.00	-0.03	-0.02	-0.04	0.08***	0.00	0.24***		
job.rank	9	2034	0.58	0.49	-0.11***	-0.22***	-0.14***	-0.05*	-0.16***	0.06**	-0.06**	0.00	
public	10	2034	0.60	0.49	-0.03	0.00	0.05*	0.04	-0.01	-0.02	0.01	0.00	0.03

Table 4.4: Summary statistics and bivariate correlations —placed job

	var.no	Obs	Mean	SD	var. 1	var. 2	var. 3	var. 4	var. 5	var. 6	var. 7	var. 8	var. 9
salary	1	2034	242.98	136.88									
total.experience	2	2034	18.87	6.40	0.19***								
function.experience	3	2034	9.42	6.27	0.04	0.24***							
industry.experience	4	2034	10.71	6.34	0.01	0.24***	0.15***						
gm.experience	5	2034	3.01	4.90	0.22***	0.24***	0.17***	0.01					
gender	6	2034	0.18	0.38	-0.09***	-0.06**	-0.06**	0.03	-0.11***				
ivy.league	7	2034	0.05	0.22	0.11***	0.04	0.04	0.01	0.08***	-0.02			
mba	8	2034	0.12	0.33	0.03	-0.03	0.04	0.00	0.08***	0.00	0.24***		
job.rank	9	2034	0.48	0.50	-0.25***	-0.24***	0.03	-0.12***	-0.11***	0.11***	-0.11***	-0.01	
public	10	2034	0.53	0.50	0.00	-0.04	-0.04	-0.01	-0.05*	0.04	-0.01	0.02	0.06**

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4.4.2 Multivariate Regression Results

Multivariate regression provides additional insight by controlling for the presence of other predictors.

Table 4.5 reports the regression results applying the specification from Equation 4.1 to the prior job setting, where compensation is determined within the employing firm. Model 1 includes only those explanatory variables capturing position attributes, firm characteristics, industry, year, and region. The R^2 from this model is 0.148. Model 2 includes only the individual externally observable predictors, including gender, education variables, and variables describing professional experience. This model reports results consistent with findings from zero-order correlations in Table 4.3; *total.experience*, *gm.experience*, and *ivy.league* have significant coefficients. The coefficient on *total.experience* is 0.04; this equates to 4% increase in salary for each year of experience subject to some diminishing returns, given the negative coefficient on the squared term for *total.experience*. One year of *gm.experience* translates to a 3% increase in salary. Among education variables, the coefficient on *ivy.league* is 0.17, equating to a 17% increase in salary for those attending these prestigious schools. Additionally, there is a significant coefficient on industry experience, equating a year of matching *industry.experience* with 2% increase in salary. Finally, Model 3 includes all predictor variables from Model 1 and Model 2. With the full model, the coefficient on *total.experience* attenuates by 0.01. *gm.experience* and *industry.experience* are no longer statistically significant from zero, and the measured effect from *ivy.league* attenuates to 0.14. The R^2 from Model 3 is 0.17, indicating that the full model explains 17% of the variance in the data.

Table 4.6 reports the regression results applying the specification from Equation 4.1 to the external market setting. Coefficients are similar to Table 4.5, but the explained variance as measured by R^2 is considerably higher. Model 1 includes only those variables capturing position attributes, firm characteristics, industry, year, and region. The R^2 from this model is 0.26. Model 2 includes only

Table 4.5: OLS Regression models - prior job setting

	Dependent variable:		
	log(salary)		
	(1)	(2)	(3)
Constant	12.073***	11.382***	11.582***
total.experience		0.040***	0.032***
total.experience^2		-0.001**	-0.0004***
functional.experience		0.002	0.004
functional.experience^2		-0.0001	-0.0003
gm.experience		0.028***	0.007
gm.experience^2		-0.001	0.0002
industry.experience		0.019*	0.005
industry.experience^2		-0.001	-0.0001
ivy.league		0.170***	0.140**
mba		0.015	0.031
gender	no	yes	yes
highest.deg	no	yes	yes
job.funct	yes	no	yes
job.rank	yes	no	yes
industry	yes	no	yes
firmsize	yes	no	yes
public	yes	no	yes
region	yes	no	yes
year	yes	no	yes
Observations	2,034	2,034	2,034
R^2	0.148	0.061	0.171
Notes		*p<0.1; **p<0.05; ***p<0.01	

the individual externally observable predictors, including gender, education variables, and variables describing professional experience. The coefficient on *total.experience* is 0.04; this equates to 4% increase in salary for each year of experience subject to some diminishing returns, given the negative coefficient on the squared term for *total.experience*. One year of *gm.experience* translates to a 3% increase in *salary*. Among education variables, the coefficient on *ivy.league* is 0.19, equating to a 19% increase in salary for those attending these prestigious schools. Model 3 includes all predictor variables from Model 1 and Model 2. With the full model, the coefficient on *total.experience* attenuates by 0.02. *gm.experience* attenuates, but not completely in this case; a coefficient of 0.02 indicates that the effect of a year of general management experience is reduced to 2%. The measured effect from *ivy.league* attenuates to 8%. The R^2 from Model 3 is 0.30, indicating that the full model explains 30% of the variance in the data.

Examining the coefficients from Table 4.5 and Table 4.6, the conditional effects of individual observable qualifications are quite similar between internal and external markets. However, effect sizes are not clear measures of importance. Furthermore, like bivariate correlations, coefficients do not offer a clear way to aggregate the collective effects of observable qualifications as a group into a measure of collective importance.

4.4.3   Semipartial Correlation Results

The coefficient of determination (R^2), measures the proportion of variance explained by the model. By allocating this measure over the model variables, this decomposition provides a measure of the explanatory contribution of component predictors.

Comparing model coefficients from the prior job and external market settings, credentials have similar conditional effect sizes; however, there are notable differences in the amount of variance these identical variables explain depending on the setting. Using semipartial correlation, the share of variance explained by individual credential predictors is higher in external market settings for

Table 4.6: OLS Regression models - placed job setting

	Dependent variable:		
	$\log(salary)$		
	(1)	(2)	(3)
Constant	12.649***	11.603***	12.172***
total.experience		0.041***	0.024***
total.experience^2		-0.001***	-0.0004***
functional.experience		-0.008	-0.008
functional.experience^2		0.0002	0.0003
gm.experience		0.029***	0.017***
gm.experience^2		-0.001**	-0.0003
industry.experience		-0.005	-0.002
industry.experience^2		0.0002	0.0001
ivy.league		0.187***	0.084**
mba		0.014	0.023
gender	no	yes	yes
highest.deg	no	yes	yes
job.funct	yes	no	yes
job.rank	yes	no	yes
industry	yes	no	yes
firmsize	yes	no	yes
public	yes	no	yes
region	yes	no	yes
year	yes	no	yes
Observations	2,034	2,034	2,034
R^2	0.255	0.124	0.297

Notes $\qquad\qquad$ *$p<0.1$; **$p<0.05$; ***$p<0.01$

all semipartial orderings. Figure 4.1 plots the incremental R^2 gained by the addition of credential predictors over all possible prior combinations of control variables. The solid line plots the contribution of credential variables in the prior job setting and the dashed line plots its contribution for the external market setting. The share of variance attributed to credentials is always higher —nearly twice as high —in the external market setting, supporting H1. The drop in variance contribution of credentials is most pronounced when position attributes (*job.rank* and *job.funct*) are already accounted for. This makes intuitive sense, since the path of connecting human capital differences with higher pay is typically through the channel of securing better-paid positions (Baker et al., 1994).

4.4.4   Use of LMG Predictor Importance Measures

Finally, I apply the LMG method of averaging R^2 allocations to variable groups across all possible permutations of ordering. This allocation of R^2 across predictors weighs all possible scenarios, providing a blended measure that accounts for the individual contribution of a predictor together with its contribution when combined with other regressors. LMG results confirm semipartial methods: credential predictors explain nearly twice as much variation in the external market (9%) compared to the prior job setting (5%). I calculate the LMG measures over 50 bootstrap samples where 2034 observations are drawn from the original sample with replacement[†]. Figure 4.2 compares the distributions of allocated variance to individual credentials between prior job and external market setting. *t*-tests confirm statistical distinction between these parameters, providing support for H1.

[†]Because these estimates are derived by averaging over 40 million permutations, the variation of the statistic using bootstrapping is quite narrow for LMG (Lipovetsky and Conklin, 2001). At 50 bootstrap samples, the difference in means apportioned to credential variables can be measured at the $p < 0.05$ level. Additional bootstrap samples sharpen the precision of the estimated parameters.
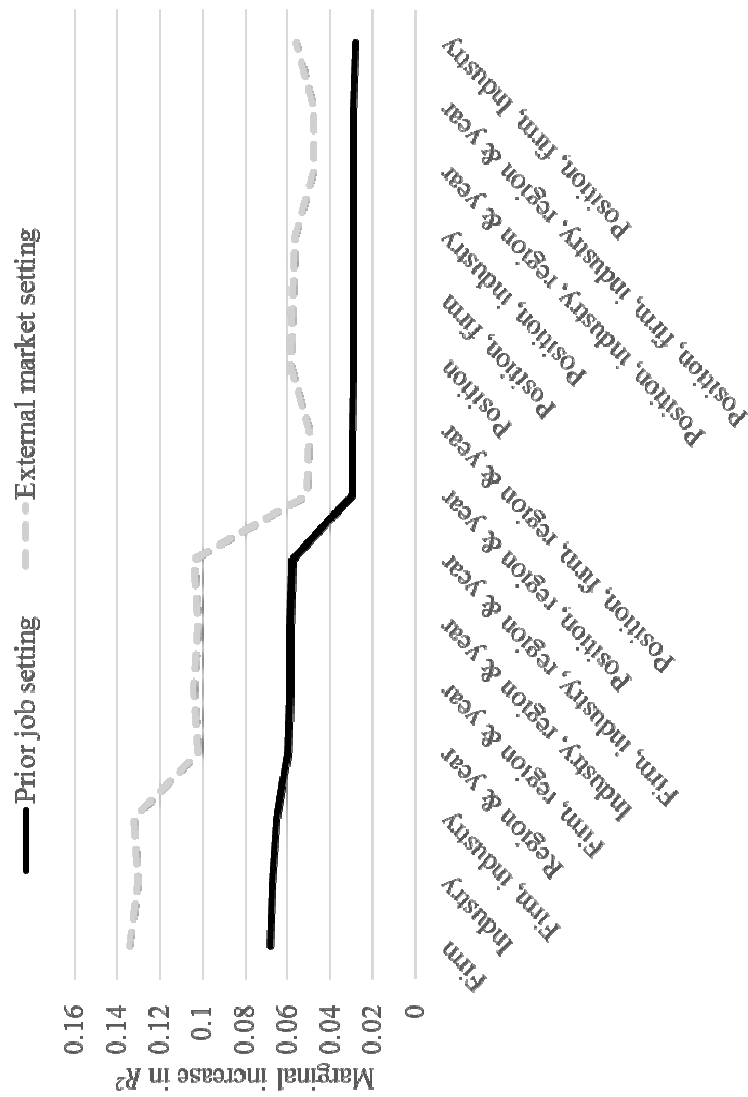
Figure 4.1: Marginal R^2 explained by the addition of credential variables.
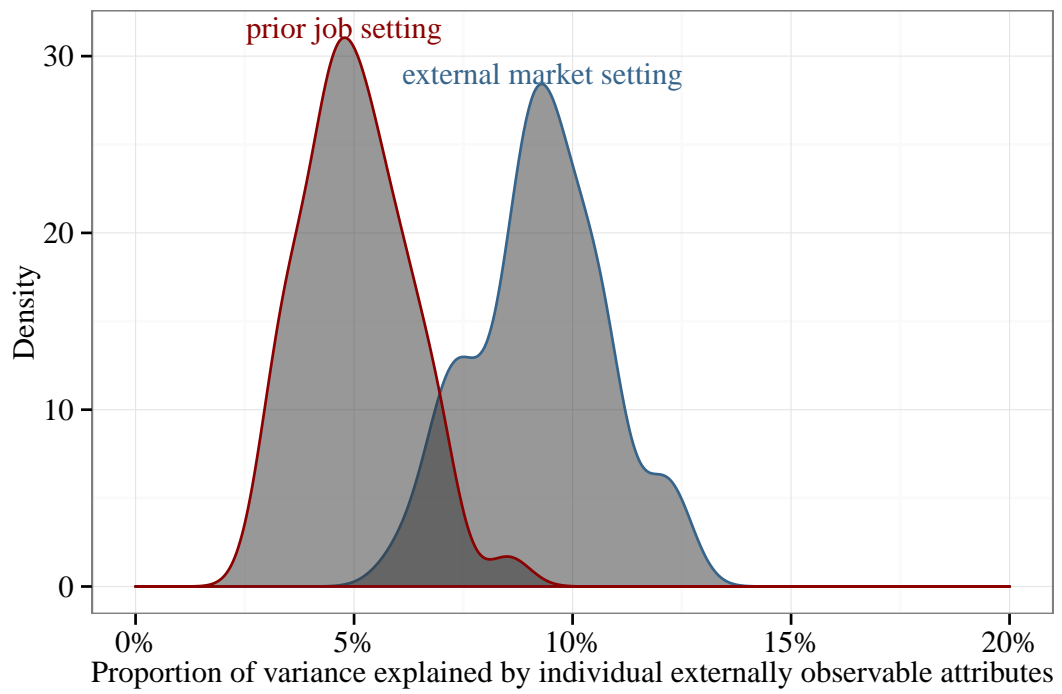
Figure 4.2: Comparing the importance of credentials between the prior job setting and the external market setting.

4.4.5 Comparisons among Different Types of Observable Qualifications

Hypothesis 2 asserts that credentials that are indicators of general capabilities are more important predictors of pay relative to fit in terms of measures of human capital. The first initial evidence of this is presented in the zero-order correlations from Table 4.3 and Table 4.4. *total.experience* and *gm.experience* have higher correlations with pay compared to *industry.experience* and *function.experience* in both the prior job setting and the external market setting. Moreover, the differences in their zero-order correlations with *salary* are larger in the external market. Regression coefficients in Model 3 in both Table 4.5 (prior job setting) and Table 4.6 (external market setting) also indicate that general capability measures such as *total.experience* and *gm.experience* have larger coefficients than *industry.experience* or *function.experience*. Figure 4.3 provides an LMG analysis of the components of individual credentials for both the prior job and external market settings—this allows for comparing the importance of measures within setting and across settings. In both settings, *total.experience* and *gm.experience* command the largest contribution. In the prior job setting, *total.experience* and *gm.experience* account for 2.0% and 1.4% of variance respectively; in the external market setting, *total.experience* and *gm.experience* account for 3.1% and 3.3%. These two types command at least 80% of the total variance allocated to individual credentials in both settings. Measures of fit between experience and job position such as *function.experience* and *industry.experience* account for much smaller shares of variance. Figure 4.3 shows that the difference in importance of credentials between prior job and external market settings is driven by *total.experience* and *gm.experience*. Distributions of these allocations based on bootstrapped sampling indicate that contributions from *total.experience* and *gm.experience* are higher than *function.experience* and *industry.experience*, confirming H2.
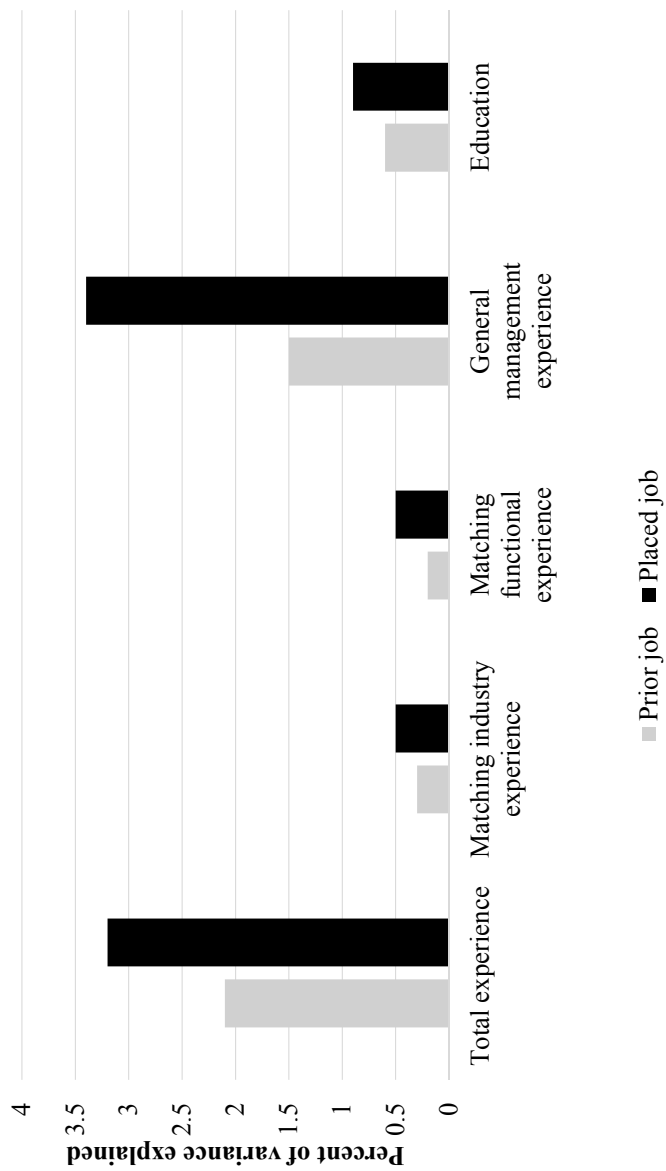
Figure 4.3: Comparing importance by type of observable qualification —prior job and placed job.

4.4.6 Comparing Junior and Senior Executives

To test H3, I replicate the LMG analysis separately across the population of junior executive placements and senior executive placements. Among junior executive placements, the variance allocated to credentials is 5.6% in the prior job setting, compared to 7.2% in the external market setting, a difference of 1.6 percentage points. Among senior executive placements, the variance allocated to observable qualifications moves from 4.3% to 9.5%, a difference of 5.2 percentage points. Figure 4.4 and Figure 4.5 depict bootstrapped distributions comparing the difference between prior job and external market settings. The stronger rise in the importance of credentials for senior executives supports H3's prediction. This result is aligned with the notion that information asymmetries are more severe among senior placements. Bootstrapping the LMG measures over 50 samples confirms that these estimated differences are all statistically distinct.

In addition to comparing differences between the prior job and external market settings across seniority, comparisons of observed levels are consistent with expectations. In the prior job setting, the importance of credentials are lower for senior executives, consistent with the idea that the valuation of their human capital is more dependent on unobserved insights derived from observations of conduct as an employee. The importance measure of credentials in the external market setting is higher for senior placement compared to junior placements, which is congruent with the intuition that their longer track records are more informative for assessing human capital.

4.4.7 Additional Analyses

Out-of-sample performance of predictors   Predictor importance using variance decomposition methods takes an observed quantity —variance in a response variable —and allocates it between unexplained variance (model error) and variance explained by the model. Next, it partitions the explained variance among the components that comprise the model as a measure of predictor
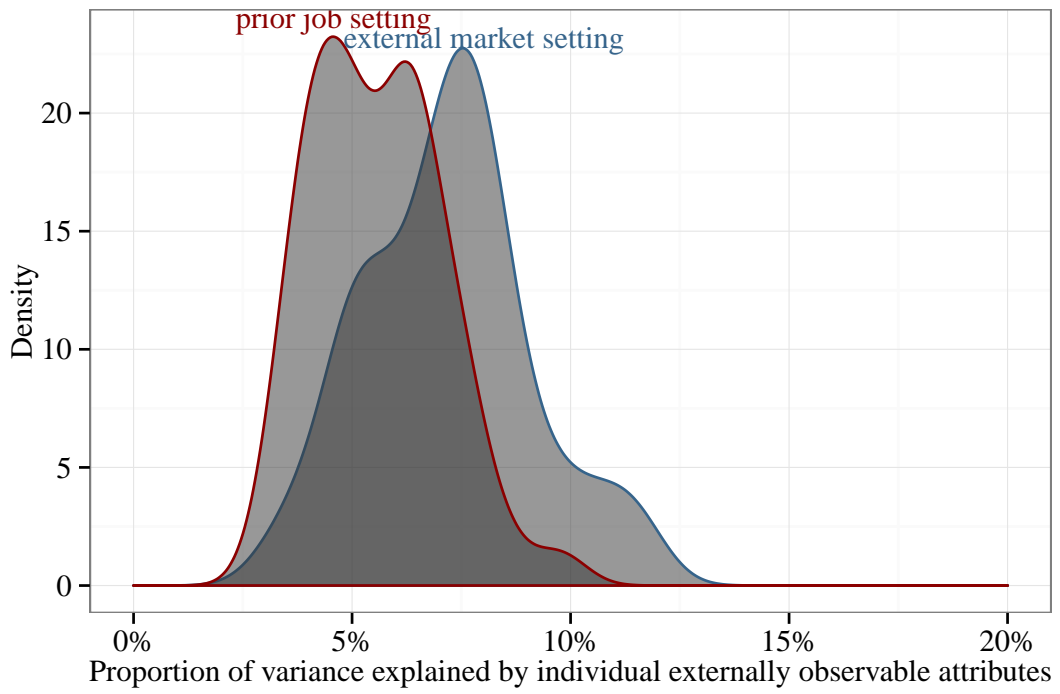
Figure 4.4: Comparing the importance of credentials between the prior job setting and the external market setting —junior executives.
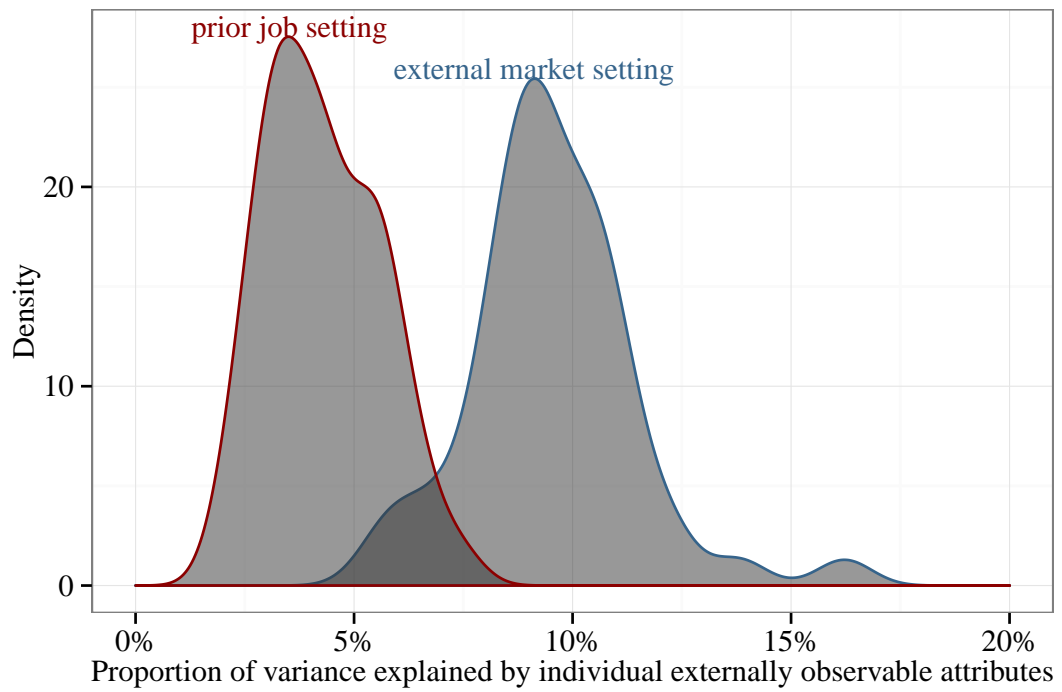
Figure 4.5: Comparing the importance of credentials between the prior job setting and the external market setting —senior executives.

importance. This paper compares the performance of an identical model across two settings for an identical population. As such, the predictor importance measures are based on a model derived from a given sample. Bootstrapping provides some tests of robustness to alternative samples, but importance measures are still based on fit between model and the data used to create it.

An alternative to comparing measures of predictor importance is evaluating its predictive performance out of sample through cross validation. As a robustness check, I perform k-fold cross validation for the regression models from both Table 4.5 and Table 4.6, where k is set to 10. In this procedure, the data is randomly divided into 10 mutually exclusive subsamples. The regression model is built using the data aggregating 9 of the 10 subsamples, and then tested on the holdout subsample k for predictive performance. This process is repeated 10 times, so each data subset is used 9 times to develop the model and 1 time to test model performance. The final performance is measured as the root mean squared error (RMSE) of the model applied to the testing data, averaged over each of the 10 samples. This cross-validation procedure can be used to evaluate the performance of variables in predicting the dependent variable in a sample distinct from that used to create the model in the first place.

Table 4.7 summarizes the results of cross validation. The first column compares model error (measured by RMSE) using only individual credentials as predictors for pay. In the prior model setting, the average RMSE is 0.71; in the external market setting, the average RMSE is 0.40. This confirms that credentials are much better predictors of pay in external markets compared to the prior job setting. In the external market setting, a model using only credentials as predictors has an average error of 35%. This performance is considerably better than in the prior job setting, where the average error is 51%. Column 2 adds position variables to the models and column 3 includes all predictors. The inclusion of other explanatory variables does not materially change these results. Using out-of-sample predictor performance as a measure of importance confirms results reported earlier.

Table 4.7: Root mean squared error across different model specifications to predict $log(salary)$ uusing k-fold cross validation

	Variables included		
	Individual credentials	Individual credentials and position attributes	All explanatory variables
Prior job setting	0.711	0.686	0.686
External market setting	0.427	0.402	0.392

SUBSET TESTS OF FUNCTIONAL CAREERS   One alternative explanation to the findings linking the importance of general management experience to pay is stickiness of role. Executives who are given general management responsibility arguably look for general management roles in their next job move; similarly, firms looking to hire for general management positions from the outside may have stronger preferences for those who have had bottom-line responsibility. If general managers are paid more, then external market pay and general management experience could be linked by general managers adhering to a general management career path. As a robustness check, I examine the subset of executives moving along functional paths. Results supporting the importance of markers of general management capabilities are similar to initial results: *total.experience* and *gm.experience* account for more than 70% of the variance explained by credentials in both prior job and external market settings. This is aligned with the recent notion that general management capabilities have become important differentiators for those with functional careers, too (Groysberg et al., 2011; Datta and Iskandar-Datta, 2014). These findings suggest that stickiness within general manager career paths is not driving the results supporting H2.

Testing selection on the underpaid   This paper advances information differences as one possible explanation of the observed higher importance of credentials in external market settings. Because the sample consists of observed job switchers, an alternative selection story may be occurring; a population of job switchers may have a higher prevalence of executives who think they are being underpaid for their qualifications. If the sample overrepresents those motivated to rectify perceived pay unfairness, the results may be driven by individuals selecting into positions with stronger linkages between compensation and credentials.

To explore this alternative, I use residuals from Model 3's regression in Table 4.5 as a measure of how much executives are "underpaid" at their prior job. The sign of the residuals identifies if executives are "more underpaid" (negative residuals) or "less underpaid" (positive residuals). If selection on perceived pay dissatisfaction is driving results, the more underpaid executives would be expected to show more pronounced changes in importance of credentials relative to the less underpaid.

The importance for credentials does have a more pronounced increase for the more underpaid executives compared to the average population —from 5% to 13% (compared to the average increase of 5% to 9%). This provides some support for selection on pay dissatisfaction. Interestingly, for the population of less underpaid executives, the importance of credentials drops from 13% to 9%. This reversal is not quite consistent with the pay dissatisfaction hypothesis. While those who feel they are more underpaid would have stronger motivation to seek jobs that link pay more tightly to credentials, less underpaid executives would have similar motivations, albeit with less severity. Attenuation of effects is expected, but it is not clear why those who are less underpaid would seek out the reverse.

Another selection story, one that is based on information differences, could help explain this reversal. For the previously discussed situation of selection on pay dissatisfaction, residuals are used as measures of how much firms may have inaccurately calibrated the value of an executive; this assumes that firms are poorly informed on the value of such talent. On the other hand, if one assumes that

employing firms are well informed about an executive's value, then residuals represent measures of unobserved strengths or weaknesses that are not captured by observable qualifications. In this case, positive residuals are evidence of more human capital beyond observed qualifications, and negative residuals are indications of less unobserved capabilities. From this viewpoint, prior compensation reveals information about unobserved capabilities, and this information sheds light on how selection might take place. Executives who have less capability than their credentials would indicate would be best served to seek out firms with stronger linkages between pay and observable qualifications, taking advantage of the information asymmetries (Akerlof, 1970). Such individuals could be identified as those with lower pay for their qualification levels. For those who have higher pay for their level of credentials, such high residuals may indicate greater levels of unobserved talent. As such, they have motivation to seek out employment with those who are willing to look outside externally observable credentials. Such selection would be consistent with this population seeking out hirers that place lower importance on observable credentials.

While both individual selection scenarios are consistent with why more underpaid executives might select into jobs with stronger linkages between qualifications and pay, the second selection situation driven by individuals acting on firm information asymmetries is also consistent with the observed direction change on the importance of observable credentials.

4.5   Discussion

4.5.1   Implications for Researchers

These findings provide empirical evidence of how markers for human capital among executives influence pay differently in the external market compared to internal ones. While this has been assumed in predicting higher qualification levels for externally hired managers (Baker et al., 1994; Bidwell, 2011), it has not been empirically measured. Additionally, this paper indicates that under-

standing different valuations of human capital between internal and external markets is informed not only by what attributes are observable, but also by what those attributes are; markers of general capability appear to be more important drivers of pay. Measures of match between domain expertise and prospective positions appear to matter less, suggesting that market remuneration for executive human capital may be less influenced by fit and more by a broader sense of demonstrated leadership capability.

This paper also highlights how other methodological approaches can be useful for shedding additional light on the connection between human capital and pay. The argument that differences in human capital should influence compensation is not controversial. Practitioners and scholars have long contended that executive human capital comes from the experiences and backgrounds of managers (Mintzberg, 1973; Kotter, 1982a; Kotter, 1982b; Mintzberg, 2004), with relevant qualifications being managerial experience and education (Hogan and McPheters, 1980). The idea that unique managerial capabilities should influence firm performance and pay (Becker, 1964; Andrews, 1971) is core to human capital explanations of compensation.

However, despite this broadly accepted idea, collective empirical results do not provide robust evidence connecting human capital to compensation (Finkelstein, Hambrick, and Cannella, 2009). The lack of robust linkages between measures of human capital and compensation is surprising given the intuition that credentials for executives are relevant measures of individual potential. This has motivated determined researchers to examine narrower circumstances, focusing on how fit between human capital with industry context (Castanias and Helfat, 1991; Harris and Helfat, 1997; Castanias and Helfat, 2001; Sturman, Walsh, and Cheramie, 2008), strategic posture (Carpenter, Sanders, and Gregersen, 2001) , and firm-specific assets (Coff, 1997; Coff and Kryscynski, 2011; Mackey et al., 2014) are critical to determining market value of executive talents. The sensitivity to contextual attributes has cast understandable doubt on the robustness of the relationship between human capital and executive pay. If hiring firms value executive experience, such effects should be

measurable without resorting to narrowly defined circumstances.

Finkelstein, Hambrick, and Cannella (2009) suggest that the absence of robust findings may stem from a "lack of sophistication" in methods and a greater need to appreciate contextual nuances that moderate the perceived value of an executive's background. This paper indicates that the introduction of new methods may provide insight in the service of answering broader questions, too. If the question of interest is understanding the importance of human capital to determining pay, methods should be matched to measuring importance, not effect size. By supplementing coefficients with additional measures that characterize predictor importance, scholars may be able to more fully explore the connection between human capital and compensation.

4.5.2   IMPLICATIONS FOR INDIVIDUAL EXECUTIVES

This paper has practical implications for executives moving through external labor markets. For individual executives, a career that spans multiple firms has become increasingly commonplace (Cappelli, 1999a; Cappelli, 1999b; Osterman, 2008). In additional to managing and developing their own human capital, executives need to be sensitive to what types of human capital will provide the strongest currency in the context of a move. Functional executives, in particular, need to think beyond their domain expertise (Groysberg et al., 2011) and demonstrate the ability to take a general management perspective. Not surprisingly, the scarcity of general management posts confers a credible signal of capability that markets value.

The influence of information asymmetries indicates that those building their career across organizations are well advised to alleviate information obstacles to capture the highest return on their human capital, both observed and hidden. Trusted person-to-person networks may represent an obvious source of removing information asymmetries; however, even in the absence of direct relationships, working for firms with strong "placement records" can make one a better-understood human capital commodity. Reference checking is a staple means of intelligence gathering, and interviews

with search firm consultants confirm their frequent use to capture the "back story" on candidates. Such references are often constrained by job seekers who wish to conceal their candidacy from current employers; while disclosure of exploring outside employment options is sometimes impossible, executive job seekers need to consider how information gaps can influence their perceived value. If current colleagues are too high a risk, executives may want to consider potential references among external relationships such as suppliers and clients. For senior executives, findings here also point to the need to cultivate relationships among subordinates who can speak directly to management styles from a different perspective. Positive mentee relationships with those who have themselves moved on successfully to other divisions or companies present multifaceted advantages; in addition to providing insight on management style, such references represent lower risk of disclosure and higher perceived independence by reference checkers.

4.5.3   Implications for Firms

For firms, the focus of external markets on general management experience warrants a reconsideration of what drives human capital portability. Prior received wisdom once connected general managers with high firm-specific human capital investments, particularly in the form of firm-specific interpersonal networks (Kotter, 1982a; Kotter, 1982b). This logic suggested that general management experience was less portable. Even if the particulars of knowledge may be firm-specific, mobility may not be hampered if markets interpret such experience as a strong signal for the willingness and capability to make such investments elsewhere (Morris, Alvarez, Barney, and Molloy, 2014).

For hiring firms, information asymmetries appear to be highly influential, and strategic hiring necessitates removing some of the information blindness. A part of this entails systematic learning through experience. Executive hires from different sources, such as consulting or industries with particularly strong functional reputations (e.g., packaged goods for marketing, high tech for innovation, etc.) may exhibit patterns that are revealed after they are brought on board. Hiring firms

serious about becoming smarter recruiters will take note not only of patterns that confirm or debunk prior expectations on true capabilities, but also observe where unanticipated strengths (or weaknesses) with respect to other capabilities surface. Hiring firms can also become more knowledgeable by asking better questions of the executive placement firms they use. Quality search firms study their placement outcomes to improve their understanding of different sources of candidates and to demonstrate the success of their placements. Search firms have the advantage of observing many placements and drawing inferences about different profiles on hidden talents and potential liabilities. Also, because executive search firms may cultivate long-standing relationships with those they place, they can offer insights on unfolding careers of similar profiles. This can help firms understand how to anticipate future career paths of those they hire and to anticipate potential warning signs along the way.

For hiring firms, the impact of information asymmetries suggests that strategies to retain talent may include making it harder for outsiders to determine the capabilities of poaching targets. Team-driven approaches to advancing initiatives have been advocated in cases where diverse viewpoints can create sounder solutions and where multiple stakeholder engagement can ease subsequent implementation. Such approaches can also make it more challenging for outsiders to discern individual-level contributions. Managing executives out and on to other positions can also be guided by reducing information asymmetries to markets in the form of deploying them in assignments with greater clarity in the transferability of their skills. In professional baseball, the Oakland Athletics became famous for their ability to use proprietary metrics to target recruits; less discussed were their practices for inflating metrics more commonly used in the labor market to trade away players at stronger premiums (Lewis, 2004). Although most firms do not capture such direct rents when employees move to other firms, some industries, such as professional services, rely on continued relationships among its alumni as a source of future clients and referrals.

4.5.4 Limitations and Future Research

The ability to observe both prior and placed compensation for the same executives enables empirical designs that contrast identical human capital profiles between two different contexts. Additionally, the rich set of context variables enable comparisons of how much credentials matter in the presence of other explanatory factors.

At the same time, this empirical study has limitations that are important to address in future research. Selection issues exist at the individual and firm levels. At the individual level, the population captured by our data represent switchers, and the inference from this work is limited to such movers. All observed individuals were amenable to considering new positions. Executives may be more receptive to job moves for a variety of reasons —family concerns, perceived limitations of internal promotion potential, spousal professional goals, and so on. Additionally, executives with higher investments in general skills may be more willing (and able) to move. However, to bias results, individuals would need to be more than just flight risks. Selection bias would exist if movers have preferences to be paid more *for their externally observable qualities only*. Executives who believe they are underpaid and want to upgrade their compensation are more likely to desire higher pay for the full portfolio of their talent. It is less plausible to imagine a situation where mobile executives move to advance return on observable measures of human capital to the exclusion of their other types of human capital. As discussed in the additional analyses section, robustness tests are more consistent with an information-driven selection story compared to an alternative explanation of selection on pay dissatisfaction. Also, preferences for pay versus other compensating benefits are heterogeneous (Goldin and Katz, 2008; Goldin and Katz, 2011), and may be correlated with human capital investment profiles. For example, preferences for those making more investments in general human capital could be correlated with a demand for higher pay when moving. However, if this type of individual selection is at work, such preferences would likely have influenced prior pay as

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well; because my data includes prior and placed compensation for the same individuals, stable individual preferences should not systematically bias the results.

Firm-level selection poses a more concerning bias risk. These data all come from a single executive search firm. If the firm supplying these data has clients that have strong tastes for observable credentials, then this would introduce bias into the reported results.

The firm supplying my data represents one of the top five firms globally. According to a 2014 industry report published by the Association of Executive Search Consultants (AESC), the top five firms command nearly 30% of the global market. These firms have long been considered the industry leaders, each having global presence and broad service lines across industries and functions. In 2013, each of the top five had global revenues between $500 million and $1 billion. Firms outside the top five are substantially smaller and less diversified. The same AESC report estimates that "medium-sized" firms generate $50 –150 million, and revenues from smaller boutique are $1 –10 million. Because the sample is drawn from a top-five firm, the clientele comprising the data for this study is more likely to reflect a broader cross section of preferences. Nevertheless, while single-site research provides the benefit of detailed measures that are hard to find in data covering broader populations, generalizability of the findings would be enhanced by replication using data from other search firms.

Like all empirical studies linking experience measures to compensation outcomes, there remains the threat of omitted variable bias. For standard regression coefficient interpretation, the inclusion of an omitted variable could change the magnitude, sign, and significance of an estimated effect. In measures of predictor importance, the concern with omitted variables is different.

Omitted variables that are uncorrelated with individual credentials do not pose a threat to bias. However, the inclusion of omitted variables that are correlated with credentials can only lower the explanatory power of such qualifications. If such omitted variables were included in the prior job setting, it could only lower the importance of externally observable qualifications, strengthening

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the reported effect. In fact, it is the presence of unobserved attributes (i.e., private information on abilities) that I argue drives the higher importance of externally observable qualifications in external markets. To confound the findings, omitted variables would have to be at play in the external market setting, but not in the prior job setting. From the point of view of information asymmetries, most of what is observed by external markets is also available to incumbent employers; such merits (or liabilities) for the executive are therefore likely to affect pay in the prior job if they affect the compensation offered in the external market. For omitted bias to be a threat to results reported here, it would have to be a trait that influences external market pay that is either unobserved or systematically disregarded by prior employers. Additionally, the omitted variable would have to have the explanatory importance of education, total professional experience, functional experience, industry experience, and general management experience combined.

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