Bridging the Gap: Evidence from externally hired CEOs[1]

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April 2017

Abstract

We investigate executive employment gaps (hereafter, gaps) between the appointment of an external CEO at a public firm and the individual's prior executive position at a public company. These gaps cannot be reliably obtained from common databases. We hand collect data for externally hired CEOs at public companies from 1992-2014. These CEOs represent approximately 40% of the 5,095 CEO successions and have a mean gap of 1.9 years. The gap increases to 3.2 years for the subset new hires with a gap. We hypothesize that labor market frictions and executive skillsets contribute to the existence and length of these gaps. Using theories from labor economics, we predict (equilibrium) associations between two measures of "fit" (executive compensation and long-term match quality) and gaps (both existence and length). Finally, we provide descriptive evidence on what executives do (e.g., sit on boards, work for private consulting companies, or consume leisure) during their gaps.

[1] We thank an anonymous referee, Christian Leuz (editor) and workshop participants at the Leeds School of Business Summer Brownbag Series for their comments and suggestions. We also thank Leah Baer, Paige Patrick, Andrea Pawliczek, and Nikki Skinner for programming assistance and Michael Arney, Nicole Buggy, Kevin Choe, Brett Eliasen, Andre Kaiser, Ramona Kimmett, Colin Mierau, Madelyn Mohr, Addison Overholt, Pinran Pan, and Shazad Sahak for data collection. Yonca Ertimur gratefully acknowledges financial support provided by the KPMG Accounting Faculty Fellowship; Caleb Rawson gratefully acknowledges financial support provided by the Gerald Hart Research Fellowship; and Sarah Zechman gratefully acknowledges financial support provided by the EKS&H Faculty Fellowship.

1. Introduction

We investigate executive employment gaps (hereafter, gaps) between the appointment of an external CEO at a public firm and the individual's prior executive position at a public company. Specifically, by augmenting several common databases with extensive hand-collected data on 2,036 externally hired CEOs, we examine the impact of labor market frictions and executive skillsets on the existence and length of gaps. We also explore whether the existence and length of gaps are associated with executive compensation and long-term match quality (two measures of executive-firm "fit").

An increasing trend in hiring external executives (Vancil [1987]; Parrino [1997]; Huson, Parrino, and Starks [2001]; Frydman [2007]; Murphy and Zábojník [2007]), reductions in CEO tenure (Kaplan and Minton [2012]), labor market frictions such as noncompete constraints (Garmaise [2011]) and anecdotal evidence (Lublin [2010a], Feintzeig [2014]) suggest that transitions into CEO positions are frequently preceded by an employment gap.[2] Prior to the main analysis, we conducted a pilot study with hand-collected dates for 50 randomly selected new CEOs to better understand the frequency and magnitudes of the gaps and evaluate available data sources. In this small sample, external CEO succession is very common.[3] For 25 of the 27 external hires, we could calculate a gap. Eight of these had gaps of less than 30 days, which we label as effectively having no gap.[4] The mean (median) gap for this sample of 25 is 2.26 (1.65) years. This seems economically large considering average tenure (also called time-to-turnover) for CEOs between

[2] Descriptive statistics in Fee and Hadlock (2003) suggest that some of their "nonraided" sample had gaps. In Table 3, they split their sample between 101 "raid" observations, defined as "cases where the individual jumped immediately from a prior public employer to the new position" (page 1333), and 31 "nonraid" observations.

[3] The 95% confidence interval for the proportion of externally hired CEO is 40.2% to 67.8%.

[4] We recognize that minor timing differences (due to physical moves, vacations, etc.) can arise even when a new CEO accepts the position before leaving another firm. Unfortunately, offer acceptance is not necessarily observable. As a result, we will classify any employment gap of less than 30 days as "no gap" or zero. Given the arbitrary nature of this cutoff, we also report results in Online Appendix A using a 60- and a 90-day cutoff.

1992 and 2007 is only seven years (Kaplan and Minton [2012]).[5] To the extent that executives do not find supplemental income sources during the gap, a gap of 2.26 years represents $10.8 million foregone compensation for an average CEO at an S&P 1500 firm over the 1992 – 2014 period; this average varies between a low of $3.8 million and a high of $15.8 million based on total compensation in 1992 and 2000, respectively (calculation based on TDC1 in Execucomp).

Understanding employment gaps is important for several reasons. First, *ex ante* it is not clear whether gaps benefit or harm executives. On one hand, sitting out of the job market can result in lost wages and deterioration of executives' skills. On the other hand, gaps may allow executives to recharge and invest in the search process, resulting in better CEO-firm matches. Second, gaps may have implications for firms and the economy as a whole. As key decision-makers in firms, CEOs can significantly impact economic growth (Bertrand [2009]). In fact, several studies suggest that *individual* CEOs affect firm outcomes (e.g., Hambrick and Mason [1984]; Johnson, Magee, Nagarajan, and Newman [1985]; Hayes and Schaefer [1999]; Bertrand and Schoar [2003]). In addition, talented CEOs are a scarce resource—one strand of executive compensation literature argues that the high levels and growth in executive pay result from competition for this scarce resource (Murphy [1999]; Edmans, Gabaix, and Landier [2008]; Gabaix and Landier [2008]; Terviö [2008]; Murphy [2013]). To the extent that gaps lead to deterioration of executives' skills or impact the CEO-firm matching process, gaps will affect the allocation of scarce resources in the economy, with potential implications on growth and social welfare.

Our study focuses on the gaps of externally hired CEOs coming from executive positions at other public companies. Given, to the best of our knowledge, this is the first study to examine employment gaps at the executive level, we provide descriptive evidence about executive

[5] We define employment gap based on not working as an executive for a public company. Some of these executives will probably pursue alternative income streams (e.g., consulting, non-executive directorships) during this period.

employment gaps. Our extensive hand-collected data on 2,036 externally hired CEOs includes details on the proportion of CEO hires preceded by a gap, gap lengths, and activities in which the executive engaged during the gap.

We then consider the effect of frictions resulting from noncompete constraints and executive skillsets on the existence and length of gaps.[6] Noncompete agreements can mitigate potential damages to the firm caused by departing employees (Garmaise [2011]; Bishara, Martin, and Thomas [2015]), but are not always enforceable (e.g., in California), so simply signing one does not necessarily constrain behavior.[7] As a result, we view an executive to be constrained when he or she signs an agreement and that agreement's enforceability is relatively high. We use the term noncompete constraint (or NCC) to describe the existence of a noncompete agreement (or "noncompete") when *combined* with a high level of enforceability.

As for executive skillsets, the increasing importance of general managerial skills and the decrease in the importance of firm- or industry-specific expertise is arguably the most salient trend related to CEOs' educational and professional backgrounds over recent decades (Frydman, [2007]; Murphy and Zábojník [2007]; Bertand [2009]; Custódio, Ferreira, and Matos [2013]). Therefore, we begin by examining generalist versus specialist executives and the differential role that NCCs are likely to play for these two executive types. First, we hypothesize that NCCs are less binding for generalists. Specifically, we predict that NCCs affect specialists' gaps (both the existence and length) more than those of generalists. We formally develop our hypotheses in Section 2.

[6] Labor economics literature examines "contemporaneous unemployment spells" among rank-and-file employees, which is analogous to our notion of gaps (Section 2 briefly discusses this literature). Yet the insights from this research may not apply to executive employment gaps. For example, with respect to determinants and with the aim to inform the policy debate on unemployment levels, labor economists typically focus on the effects of unemployment insurance on the duration of unemployment spells, an issue that is not relevant to the executive labor market.

[7] In Section 4 we provide a detailed description of how we operationalize noncompete constraints.

Next, we turn our attention to the relation between gaps and match quality (or fit). Conventional wisdom and prior literature in labor economics suggest that gaps for rank-and-file employees are red flags to prospective employers resulting in fewer interviews, fewer offers, and lower pay (e.g., Kroft, Lange, and Notowidigdo [2013]). Gaps exist for a variety of reasons (e.g., illness, factory closings, or voluntary career changes). Potential employers likely view some of these reasons as innocuous and others as worrisome. In rank-and-file employment, there is likely some pooling across gap reasons because applicants with innocuous reasons cannot fully separate themselves from applicants with worrisome ones. Furthermore, skills (or the value of skills) tend to deteriorate over time if not used and updated (Edin and Gustavsson [2008]). Thus, there is often a stigma attached to periods of unemployment that affects recruiting decisions of employers (Vishwanath [1989]; Gibbons and Katz [1991]; Ruhm [1991]; Arulampalam [2001]; Gangl [2006]; Kroft, Lange, and Notowidigdo [2013]).

While the conventional wisdom may accurately describe rank-and-file labor markets, our setting differs in several ways. First, the number of senior executives is relatively low, and the information about each potential match is relatively high. Using the terminology of Lazear (1986), the "visibility" of executives' marginal product is relatively high. We thus assume that firms searching for new CEOs are more likely to know about candidates' skillsets and why gaps exist, reducing the signaling role of gaps. Second, executives are more likely to have financial resources to use the gaps to assess their career goals and conduct due diligence to evaluate potential matches (Lublin [2010a]). Finally, our analysis is predicated on the executive becoming a CEO, an unlikely outcome for poor performers (Harris and Holmstrom [1982]). Thus, it is not obvious that the existence and duration of executive employment gaps lead to negative career consequences.

The above discussion suggests that a key assumption behind signaling models, the existence of substantial information asymmetries, is mitigated in the CEO labor market. However, another class of models, search-theoretic models, may provide insights. These models rely on the notion that market frictions are material − "it takes time and other resources for a worker to land a job, especially a good job at a good wage, and for a firm to fill a vacancy" (Rogerson, Shimer, and White [2005], p. 960).[8] We expect these market frictions to be significant in our setting, where the "worker" of interest is external CEO hires. For example, hiring a new CEO can be politically sensitive for both the hiring firm and potential candidates. Firms often consider both internal and external candidates, and external candidates may desire anonymity during the interview process, especially if employed elsewhere.[9] In some cases, potential candidates with the requisite skillset and interests will not be identified, or will require alternate recruiting methods, due to the desire for anonymity. Finally, it is difficult to predict the arrival of CEO opportunities.

Using search-theoretic models of the labor market as a framework as well as the theory of employee raids, we formulate two sets of hypotheses that we expect to apply to both generalists and specialists. First, these models suggest that (i) employed workers have higher reservation wages than unemployed workers and (ii) the best workers are more likely to be raided, resulting in job transitions without an employment gap. Therefore, we expect that externally hired CEOs with gaps have a worse CEO-firm match than those without gaps. Second, conditional on experiencing a gap, job search models show an unemployed worker's reservation wage is negatively associated with his or her discount rate (e.g., candidates with lower discounts rates,

[8] This notion is also supported by the popular press. Lublin (2010b) writes: "Quitting a high-powered job at a time when the unemployment rate hangs at 9.5% sounds crazy. Yet, some C-level executives are leaving these days before they line up another plum management spot. Most resign specifically so they'll have time to job hunt."

[9] This desire for anonymity exists in many labor markets including academic markets. Methods for recruiting experienced faculty or senior administrators are often shaped by such demands.

patient candidates, will turn down "poor" offers leading to higher utility through either higher wages or a better fit). This leads us to expect that, conditional upon having a gap, executives with longer gaps have a better match). In other words, these hypotheses predict a "U" shaped relation between gaps and CEO-firm fit, with better matches at no gap and at relatively longer gaps, where the executive has the ability to perform sufficient due diligence to achieve a better match.

We use two measures of executive-firm match quality. In search-theoretic models of the labor market, an employee's best match is the company that allows him or her to be most productive and receive the highest compensation (an *ex ante* measure of fit). These models assume that employment offers are completely characterized by the wage offered (e.g., Jones [1988]). In reality, employment utility is not only determined by wages, and so we supplement total compensation tests with an *ex post* measure of fit: time-to-turnover at the new firm. We expect good matches will last longer than poor ones.

We examine these hypotheses using ordinary least squares, logit, multinomial logit, and hazard models. We acknowledge that our study suffers from the potential for correlated omitted variables. In particular, the potential effect of unobservable factors on the existence and duration of gaps as well as their consequences is a significant concern in our analysis. For example, gaps may be driven either by (lack of) managerial talent, accumulated wealth, or simply desiring more time with family, but none of these are independently observable. In addition, because we focus on successful outcomes (i.e., individuals subsequently hired into a CEO position at an S&P 1500 firm), our results may not be generalizable to individuals with other outcomes (e.g., those hired as a CEO at a non-S&P 1500 firm, hired in a non-CEO position, etc.).

Our analyses yield a number of key results. First, employment gaps are prevalent among external CEO hires. 57.5% of the CEOs in our sample experience a gap and the average gap lasts

676 days (1.85 years). Second, we find mixed evidence on whether non-compete constraints are more binding for specialist CEOs. On one hand, when faced with non-compete constraints, specialists are more likely to experience a gap than generalists. On the other hand, among CEOs that experience a gap, gap lengths do not vary with director skillsets in the presence of non-competes. Third, externally hired CEOs with gaps have lower compensation. We also find, albeit only in univariate analysis, that these CEOs also experience shorter time-to-turnover. Thus, our results are consistent with the prediction that gaps are associated with worse CEO-firm matches. Finally, contrary to our expectations but similar in spirit to the rank-and-file literature, executives with longer gaps receive lower compensation and have shorter time-to-turnover.

Three prior studies are particularly relevant to our project. First, building on the theory of raids proposed by Lazaer (1986), Hayes and Schaefer (1999) argue that the average ability of managers who leave a firm for a similar position at another should be high. Using a clever research design, Hayes and Schaefer (1999) find that firms whose managers are raided (i.e., hired away) experience an average abnormal return of –1.51% compared to 3.82% abnormal returns for random departures (i.e., sudden managerial deaths). We draw from some of the same labor market theories in our hypotheses development. To focus on raids, Hayes and Schaefer (1999) effectively drop observations with meaningful employment gaps.[10] Furthermore, they are interested in the market's assessment of CEO ability.[11] In contrast, we are interested in causes of gaps and what these gaps imply about fit as measured by CEO compensation and employment time-to-turnover. Second, Fee and Hadlock (2003) examine the movement of executive talent across firms. Their evidence indicates that superior stock price performance increases the likelihood that an executive will

[10] Hayes and Schaefer (1999) specify that a new CEO is only included if he was "employed in the six months prior to his appointment by another publicly traded firm" (p. 133), suggesting nonzero employment gaps exist.
[11] Motivated by their study, we collect hire announcements to report market responses in Section 5.5.

obtain a CEO position elsewhere and is associated with larger hiring grants (e.g., stock options and restricted stock) for CEOs. While they include a small sample of executives with employment gaps in the analysis, Fee and Hadlock (2003) do not consider the factors underlying these gaps or their consequences for match quality. Finally, Allgood and Farrell (2003) find evidence of job-match heterogeneity in the CEO labor market—i.e., the productivity of a manager at a given firm varies with both the manager's ability and how well the manager's skills match the firm (e.g., Jovanovic [1979]). Allgood and Farrell (2003) show that, consistent with models of job match where manager-firm pairs learn about the quality of the match over time, the hazard of CEO turnover increases in the first five years of tenure and then decreases. They also document that CEO age, firm performance during the CEO's tenure, and some characteristics of the previous CEO are associated with match quality.

Our study makes several contributions. First, it contributes to the literature on executive labor markets. Prior studies in accounting and finance typically take a firm-centric view and focus on the role of firm performance and monitoring mechanisms in (forced) CEO turnover. We adopt a more executive-centric view and add to the few studies on voluntary job changes, movement of executive talent across firms, and CEO-firm match quality (Hayes and Schaefer [1999]: Fee and Hadlock [2003]; Allgood and Farrel [2003]).

Second, and more generally, our paper contributes to the literature on contemporaneous unemployment spells by focusing on an important component of the labor market: executives. This literature has historically focused on rank-and-file employees. Given the differences between rank-and-file employees and executives, we expect the insights from our study to complement the literature. Relatedly, an advantage of our setting is that voluntary and mandatory disclosures regarding CEO appointments, backgrounds and compensation allow us to construct a rich dataset

on executive gaps.[12] In contrast, the literature on rank-and-file employees relies on data from surveys (e.g., Current Population Survey), administrative records (e.g., unemployment insurance) and field experiments. Our dataset enables us to address questions (e.g., how employment gaps relate to employee-employer "fit") that would be difficult to examine in the rank-and-file setting.

Third, our study adds to the debate on noncompetes and the literature on executive skillsets by exploring the interplay between the two. Fourth, we provide descriptive evidence on the existence and length of executive employment gaps, as well as the activities during, as little is known about employment gaps beyond the rank-and-file worker level.

Finally, our study produces a novel and rich dataset on executive transitions as well as an expanded dataset of NCCs. Future research can draw on this data to address important questions. For example, how do executives' activities during gaps impact their eventual labor market outcomes? How do gaps and executives' activities during gaps affect the future productivity of the hiring firms (e.g., innovation and performance)? What is the impact on the overall economy of leaving top talent out of the labor market for extended periods? In addition, future research that explores outcomes for executive unemployment spells can use our data as a basis to extend our study. For example, we only examine executives with good outcomes (i.e., those who become CEOs). We do not provide insights on executives that take lower level jobs. We also do not incorporate internal promotions or separately identify interim CEOs, both of which may have different implications for the employee and the firm. Furthermore, while we take a more executive-

[12] While most disclosure requirements also apply to the five most remunerated employees at public companies, there is often not enough information regarding the employment histories of the other high-level employees (see Section 4). Furthermore, our study requires substantial hand-collection, which is unduly costly for a substantially larger sample.

centric view, one could adopt a firm-centric focus by examining searches for a new CEO and trying to identify how firms choose from a list of likely candidates.[13]

2. Hypothesis Development

2.1 Determinants of CEO gaps

Efficient human capital deployment is an important driver of economic growth and a central concern for policy makers. An extensive literature examines the determinants and consequences of employment gaps (i.e., unemployment spells) in rank-and-file labor markets. One strand of this literature focuses on the role of unemployment insurance level and duration on the duration of unemployment spells (e.g., Moffitt [1985], Meyer [1990], Katz and Meyer [1990], Card, Chetty and Weber [2007], Card, Johnston, Leung, Mas and Pei [2015]; also see Krueger and Meyer [2002] for a review). While these studies inform the policy debate on the relation between unemployment insurance and unemployment levels, their insights are not relevant for the executive labor market.

Another strand of the labor economics literature examines whether unemployment spells damage workers' future labor market careers. These studies are interested in whether long-term spells of contemporary unemployment have adverse effects on re-employment probabilities ("duration dependence") and whether past spells of unemployment have a negative effect on current employment probabilities as well as compensation ("scarring") (e.g., Ruhm [1991], Kletzer [1998], Arulampalam [2001], Imbens and Lynch [2006], Kroft, Lange and Notowidigdo [2013], Eriksson and Rooth [2014]; see also Machin and Manning [1999] for a review). Several studies demonstrate a negative relation between unemployment duration and subsequent wages (e.g., see

[13] Press articles often refer to likely candidates. For example: "…the formation of a search committee suggests the board is looking outside. The best-known banking figure currently out of work is former Citigroup executive Jamie Dimon. Other well-regarded bank executives include Firstar Corp. CEO Jerry Grundhofer and Leslie Biller, chief operating officer of Wells Fargo & Co." (Cahill [1999]).

Addison and Portugal [1989]; Farber [1993]; Kroft, Lange, and Notowidigdo [2013]; Eriksson and Rooth [2014]), though establishing causality has been difficult (see Eriksson and Rooth [2014] for a discussion).

In contrast to the rich literature in rank-and-file employment gaps, studies on the executive labor market tend to examine the drivers of turnover, which do not, in and of themselves, imply a gap.[14] We first focus on how executive labor market frictions resulting from NCCs interact with executive skillsets to affect the existence and duration of gaps.

Employees have access to sensitive information about their firms' products, costs, customers, current R&D projects, and strategy, among other things. Firms also invest in their employees' human capital on an ongoing basis. Thus employee departures, particularly to competitors, can damage firms. Noncompetes have emerged as a way to constrain employees and mitigate these costs and they pervade the executive labor market.[15] Garmaise (2011), for example, finds evidence of noncompetes between the firm and its top executives at 70.2% of a random sample of 500 Execucomp firms (i.e., firms that are in the S&P 1500). In a more recent study, Bishara, Martin, and Thomas (2015) examine 847 CEO employment contracts from a sample of S&P 500 firms from 1996–2010 and find that 80% include noncompetes.[16] While U.S. states vary in the enforceability of noncompetes, a common requirement for enforcement is that the agreement must not be overly restrictive (i.e., it should apply to a reasonable geographic area and have a reasonable definition of competing firms). Prior studies show that the existence and enforceability

[14] Examples include the work of Brickley, Linck, and Coles (1999), DeFond and Park (1999), Hayes and Schaefer (1999), Engel, Hayes, and Wang (2003), and Bushman, Dai, and Wang (2010).

[15] More broadly, firms and executives enter into other agreements that may include restrictions on employee conduct such as confidentiality and nondisclosure agreements and nonsolicitation agreements prohibiting the recruiting of the firm's current employees. Consistent with prior research (Garmaise [2011]; Bishara, Martin, and Thomas [2015]), we focus on noncompetes given their prominence and potential impacts on a CEO's post-employment activity.

[16] Kaplan and Strömberg (2003) find that noncompetes are used in approximately 70% of contracts between entrepreneurs and venture capitalists.

of noncompetes affect executive movements. Specifically, Garmaise (2011) documents that enforceability reduces executive mobility, particularly within a given industry. Similarly, Marx, Strumsky, and Fleming (2009) find that enforcement decreases mobility, especially for employees with greater firm-specific human capital. However, Fee and Hadlock (2003) show that only "26.34% of outside CEO hires from other public firms come from a firm with the same two-digit SIC code, implying that the majority actually come from other industries" (p. 1328). Thus, for new CEOs who are changing industries, noncompetes are unlikely to be the sole cause of gaps. [17]

Obviously, enforcement only matters in the presence of a covenant, so our construct of interest is NCCs (noncompete constraints) or the combination of the existence and enforcement of noncompetes. We expect that NCCs increase the likelihood and length of employment gaps. Consider an executive who is interested in an external CEO position but has signed a noncompete in a state with high enforceability (e.g., Florida after 1997). This executive can avoid penalties for violation by finding a CEO position at a firm that does not compete with his or her current employer or by resigning and waiting the required period for the noncompete to expire. In the former case, the executive may not experience an employment gap. In the latter case, a gap is certain. Alternatively, an executive who is not constrained (i.e., who has not signed a noncompete or signed in a state with no enforcement, like California) faces these same options plus the additional possibility of immediately taking a job at any competitor. Given each executive can choose from a menu of options, it is not obvious how NCCs will affect employment gaps in

[17] Due to issues of noncompete enforceability, firms in England started paying former employees their full salaries "during the period in which they are restrained from competing" (Lembrich, [2002], p. 2291), a practice known as "garden leaves." Historically, these provisions were uncommon in the US, and US courts have not consistently enforced such provisions when they do exist. See *Baxter International, Inc. v. Morris* 976 F.2d 1189, 1197 (8th Cir. 1992); *Maltby v. Harlow Meyer Savage, Inc*. 633 N.Y.S.2d 926, 930 (Sup. Ct. 1995) and *Lumex, Inc. v. Highsmith* 919 F. Supp. 624, 629 (E.D.N.Y. 1996). Our sample is dominated by US-based firms, therefore garden leaves are unlikely to be material for our sample.

equilibrium. Nevertheless, we expect that the expanded choice set in the absence of NCCs results in a smaller likelihood of experiencing an employment gap.

Conditional upon changing firms, we expect that NCCs will increase the average time it takes for an executive to start a new CEO position. After all, relative to executives who are not constrained, those with NCCs face a more limited set of employment options. We do not view the predictions on the relation between NCCs and the existence and length of employments gaps as having sufficient tension to justify formal hypotheses. We use these arguments to motivate an interactive effect (see below).

Under the optimal contracting view of executive compensation, firms compete for managerial talent in a competitive labor market, resulting in large compensation packages for high quality executives perceived to have the right skills.[18] Murphy and Zábojník (2004) argue that executive pay depends on the portion of CEO skills that is transferable across firms and industries and that the importance of general skills has increased over time. Custódio, Ferreira, and Matos (2013) provide compelling empirical evidence along these lines. They proxy for managerial skillsets using aspects of lagged employment histories. In particular, generalist CEOs have held more positions and have worked at more firms and industries.[19] That is, generalists have demonstrated both an ability and willingness to change jobs, companies, and industries. The high demand for their skills combined with their revealed preferences suggests generalists can more easily avoid triggering noncompetes (i.e., NCCs are less binding).[20] These arguments lead to the following hypotheses (stated in alternative form):

[18] Murphy (1999), Murphy (2013), Edmans and Gabaix (2015), among others, survey the executive compensation literature and discuss the optimal contracting view in addition to the rent extraction view.
[19] They are also somewhat more likely to have been a prior CEO and worked at a conglomerate.
[20] Note that we do not have a prediction for the main effect of generalist/specialist on the existence or magnitude employment gaps.

H1a: *When faced with NCCs, specialists are more likely than generalists to experience a gap.*

H1b: *When faced with NCCs, specialist employment gaps (when they exist) will be longer than generalist employment gaps.*

We acknowledge that, to the extent that NCCs are not binding, results may be insignificant. In addition, to the extent that moral obligations or reputation concerns are at play, executives with non-compete clauses in jurisdictions where enforcement is limited may still experience a gap prior to beginning employment at a new firm. This would bias against finding results consistent with *H1a* and *H1b* as we have included these executives in the no-NCC sample.[21]

2.2 Consequences of CEO gaps − match quality

The next set of hypotheses focus on the consequences associated with the existence and duration of employment gaps. Note that our predictions are based on equilibrium arguments and, therefore, are not expected to be causal. Furthermore, these predictions do not depend on NCCs or executive type (i.e., generalist and specialist). Instead, we use theories from labor market economics to structure our thinking.

Since the 1930s, labor economists have developed theories on the matching of workers and firms and, relatedly, on unemployment (see Petrongolo and Pissarides, [2001] for a brief history). These theories focus on challenges faced by typical (rank-and-file) workers (see, Mortensen and Pissarides [1999]; Petrongolo and Pissarides [2001] and Rogerson, Shimer, and Wright [2005] for reviews of these models). While modeling assumptions and equilibrium concepts vary across papers, some results are fairly general and likely apply to the executive labor market.

[21] As an example, a California firm, Indymac Bancorp, stated the following in their 2007 Proxy Statement: "Although a non-competition provision could not be included in the employment agreement under applicable law, Mr. Perry recognizes that he should and does have a moral and ethical obligation to Indymac, its shareholders and its employees not to compete with Indymac within one year after any resignation from his position. https://www.sec.gov/Archives/edgar/data/773468/000095012407001708/v27709ddef14a.htm

We begin by considering executive transitions without employment gaps vis-à-vis those with gaps. Search-theoretic models of the labor market provide a framework for an executive's decision to accept an external job offer. Burdett (1978) derives two intuitive results. First, workers have greater reservation wages when employed (say, Y) than when not employed (say, X), such that Y>X. That is, a given worker will accept any offer as great as X when unemployed but will continue to look for a job when employed until an offer emerges that is at least as great as Y. Second, employed workers will reject external offers that are lower than their current wages and accept external offers only if they are (sufficiently) higher than the current wage.[22] These results suggest that executives who transition without an employment gap will receive higher compensation from their new employers relative to those who transition with a gap.

Adopting the hiring firm's perspective leads to a similar prediction. In a setting where a workers' productivity at a given firm depends both on their ability and how well their skills match the needs of the firm, Lazear (1986) analytically demonstrates that the best workers are more likely to be raided, resulting in job transitions without an employment gap. Consistent with Lazear's theory, Hayes and Schaefer (1999) empirically show that firms whose executives are raided for an external CEO position experience, on average, –1.51% abnormal returns (compared to average abnormal returns of 3.82% for firms whose managers die suddenly). Based on these two distinct but consistent views, we hypothesize that:

H2a: *Externally hired CEOs with gaps have lower compensation than those without gaps.*

Search-theoretic models are couched in terms of reservation wages, which the workers earn until retirement or, in the case of models that allow for on-the-job search, until they leave for

[22] Note that, if the cost of job hunting while employed is sufficiently higher relative to the cost when unemployed, workers will not engage in on-the-job search (Burdett [1978]). In the executive labor market, this may be the case for executives for whom anonymity is an important concern.

another job. In these models, workers maximize their expected discounted income; this is equivalent to maximizing expected utility under certain conditions (Rogerson, Shimer, and Wright [2005]). Because what ultimately matters is the utility offered by the new job versus the utility offered by the old one, the expected time-to-turnover is also important (i.e., expected "fit"). Better matches result in longer employment relationships (e.g., a better fit implies a longer time-to-turnover), while poor quality matches end early. Thus, we hypothesize that:

H2b: *Externally hired CEOs with gaps have shorter time-to-turnover than externally hired CEOs without gaps.*

H2a and H2b imply that *ex ante* compensation and *ex post* employment duration are positively correlated for raided executives. While the predictions reflect logical interpretations of existing theories, there are arguments to be made for a negative correlation between *ex ante* compensation and *ex post* employment duration. For example, an executive who is being lured to a new firm may expect the new job to be very risky, resulting in a high expectation of turnover in the early learning periods (Jovanovic [1979]). Furthermore, firms are more likely to hire external CEOs when the firms' performance has been poor (e.g., Fee and Hadlock [2003]). As a result, executives that accept riskier jobs may require higher pay (in the cross-section). In this world, one would expect to find results consistent with H2a but inconsistent with H2b. In addition, to the extent that transitions without a gap (e.g., from raids) leave less opportunity for due diligence on the part of the executive, the resulting matches may be of worse quality leading to insignificant or opposite results.

2.3 Consequences of gap length – match quality

We next turn our attention to the subset of executives with positive gaps. We assume that they were not raided (i.e., that they left their jobs for other reasons) and face the basic problem of

searching for a job while unemployed. Intuitively, unemployed workers will turn down job offers when they are sufficiently patient and expect future offers to be better. Simple job search models (e.g., Rogerson, Shimer, and White [2005]) formalize this intuition by showing that an unemployed worker's reservation wage is negatively (positively) associated with his or her discount rate (discount factor).[23] CEO candidates can be patient (have low discount rates) if they have sufficient liquidity to cover expenses until the right offer arrives.[24] Because we cannot measure executive patience or liquidity directly, we provide the reduced form prediction that reservation wages are positively associated with gaps.

H3a: *For executives who experience a gap, those with longer gaps will receive higher compensation (a proxy for reservation wage).*

Relying on the same reasoning as in H2b we also predict:

H3b: *For executives who experience a gap, those with longer gaps will have longer time-to-turnover (a proxy for ex post fit).*

Broadly speaking, H3a contrasts with empirical findings for rank-and-file employees. As previously mentioned, numerous studies find a negative relation between unemployment duration and subsequent wages. These findings seem to suggest that signaling models are more descriptive than search-theoretic models for rank-and-file markets. As we discussed in the introduction, we expect the opposite to be true in the executive market. To the extent that CEOs are more similar to rank-and-file employees than we expect (e.g., if executives learn about their type over time, skills diminish over time, or there is similar level of information asymmetry in the executive labor

[23] Jones (1988) also derives conditions under which reservation wages are positively associated with unemployment duration.

[24] People who become CEOs of large firms likely have a history of high paying jobs. But as Charles A. Jaffe puts it, "it's not your salary that makes you rich, it's your spending habits." Indeed, executive extravagance in their personal life has become the subject of recent research (e.g., Davidson, Dey, and Smith [2014, 2015]).

market as in the rank-and-file labor market) we could find opposite results. To the extent that CEOs differ from rank-and-file employees on some dimensions, but not others, we might find insignificant results. Furthermore, if employment gaps are entirely driven by noncompetes, we will not find support for H3a and H3b.

Taken together, H2a (H2b) and H3a (H3b) predict a nonmonotonic relationship between employment gaps and compensation (CEO time-to-turnover). Overall, based on raid and search theories, we expect pay (future CEO time-to-turnover) to be highest in when gaps are effectively zero and when gaps are relatively long.

3. Research Design

We test H1a–H3b for a comprehensive sample of external CEOs hired at S&P 1500 firms (see Section 4 for details of sample construction). Our tests are parametric or semiparametric and therefore sensitive to outliers. To mitigate the influential observation problem, we winsorize (truncate) all continuous independent (dependent) variables at the 1% and 99% levels.

3.1 The Role of Executive Skillset and Noncompete Constraints – H1a and H1b

Our first two hypotheses focus on the relation between executive skillsets and the existence (H1a) and duration (H1b) of gaps in the presence of NCCs. We estimate the following logistic regression to test the prediction that, in the presence of NCCs, generalists are less likely than specialists to experience gaps (H1a):

$$\textit{Gap Indicator} = \alpha_0 + \alpha_1 \textit{Generalist} + \alpha_2 \textit{NCC\_Generalist} + \alpha_3 \textit{NCC\_Specialist} + \textit{Controls} + \varepsilon \quad (1)$$

Section 4 provides detailed descriptions of these variables as well as the underlying data sources, and Appendix A provides formal variable definitions. The dependent variable, *Gap Indicator*, is an indicator variable equal to one if the incoming CEO experiences a period of unemployment (i.e., he or she is unemployed for at least 30 days) before beginning a new position.

Even when executives smoothly transition from one job to another, they may incidentally experience a gap.[25] Therefore, we define an employment gap as at least 30 days of unemployment.[26] We include an indicator variable for whether the executive has a general skillset (*Generalist*). The variable *NCC* is the measure of noncompete enforceability (based on Garmaise [2011]) when an executive likely signed a noncompete and zero otherwise (because enforceability only matters with an agreement). For ease of interpretation, NCC enters the model separately for generalists and specialists (*NCC_Generalist* and *NCC_Specialist*). H1a predicts that NCCs are less binding for generalists than specialists. Hence we expect the coefficient on *NCC_Generalist* to be smaller than the coefficient on *NCC_Specialist* (i.e., $\alpha_2 < \alpha_3$). Though not explicitly part of our hypothesis, we also expect that α_1 will be negative, while α_2 and α_3 will be positive.

We are not aware of any prior studies that examine the existence and length of executive employment gaps, but there is a vast empirical literature in labor economics on unemployment spells for rank-and-file employees (e.g., Lancaster [1979]; Nickell [1979]; Meyer [1990]; Lockwood [1991]; Lalive [2008]; also see Devine and Kiefer [1991] for an overview). These studies identify personal and demographic characteristics, demand conditions in the labor market, and the level and duration of unemployment benefits as important factors in determining the existence and length of these gaps. We rely on this literature for control variables. In particular, we control for the age of the executive (*Age*), whether the executive is close to retirement age (*Age > 60*), and the executive's gender (*Female*). We include year-quarter fixed effects to control for demand conditions in the labor market.[27]

[25] For example, Kenneth Klein, COO of Mercury Interactive Corp., left his position on December 31, 2003, to become chairman, CEO, and president at Wind River Systems but did not begin his new job until January 5, 2004 (per http://www.bloomberg.com/Research/stocks/private/person.asp?personId=328098&privcapId=58963875).
[26] For robustness, we alter this definition to 60 and 90 days – results in Online Appendix A.
[27] Specifically, the fixed effects are generated using the date the new CEO left his/her former public employer.

As we discuss in the introduction, we expect there to be relatively more information available about executives compared to the rank-and-file employees. This is likely to be most pronounced for the CEO. Therefore, we include an indicator variable that captures whether the incoming CEO held a CEO position at the prior employer, *Previous Position–CEO*. In addition, we include the performance of the prior employer (*Abnormal Returns–Prior Employer*), which proxies for both the executive's quality and the reason behind the employment gap—higher returns suggest a better executive and a less "problematic" reason underlying the employment gap. This is, of course, a noisy measure, particularly for lower level executives for whom the correlation between individual effort and firm performance is likely to be low. To account for the variation in the informativeness of this proxy, we interact *Abnormal Returns–Prior Employer* with *Previous Position–CEO*.

Note that H1a is generated using causal arguments. Obviously, the above model (even after adding control variables) is ill-suited for addressing causality. If we had enough random shocks to NCCs (given that generalist versus specialist is predetermined at the time of the shocks), then the case for causation would be stronger. Garmaise (2011) documents that several states (Texas, Florida, and Louisiana) change the strength of their enforcement. He exploits this fact to provide better identification (through the use of firm fixed effects). Our sample covers a substantially longer period than his, allowing for the possibility of more changes.[28]

Related to our duration hypothesis (H1b), time to event analysis (i.e., survival analysis) is common in many fields. As far back as Lancaster (1979) and Nickell (1979), labor economists have used a class of hazard models designed for the challenges that unemployment spell data (an

[28] Our initial intention was to use these shocks as instruments. However, after collecting and analyzing the data, we have only 18 observations in the first year of a new regime and 14 in the second year. Given the limited number of observations, we forgo an analysis using the shocks.

example of time-to-event data) presents. This data is typically nonnormal and censored. For example, at the time data collection ends, some individuals would not have completed their unemployment spells, resulting in right censoring (Keifer [1988]). Even when the researcher observes complete spells, hazard models are preferable to static models because they incorporate potential duration dependence into the estimation (Keifer [1988], Shumway [2001]).[29]

In the spirit of this literature, we estimate the Cox semiparametric proportional hazard model to test the H1b prediction that NCCs are less binding for generalists (resulting in shorter gaps) than for specialists. Our sample contains "complete unemployment spells" for executives—we know the start date and the end date of their employment gaps such that our data is not censored.[30] *Gap Length*, the time the executive spends "unemployed" when transitioning between positions, is our measure of time to failure in the hazard model. The proportional hazard model relates the explanatory variables to the hazard of the executive exiting the gap (i.e., becoming employed as a CEO). We estimate the model with the same set of independent variables as in Equation (1). Our hypothesis predicts the coefficient on *NCC_Specialist* to be lower than the coefficient on *NCC_Generalist* (i.e., in the presence of a non-compete agreement, specialists have a lower hazard rate of exiting the gap and becoming employed than generalists). We also expect both of these coefficients to be negative (i.e., non-compete agreements reduce the hazard rate of exiting the gap) and the coefficient on *Generalist* (an indicator variable) to be positive. We estimate this regression for all new CEOs with gaps.

[29] These models also allow for inclusion of covariates that change over time, which is crucial for policy analysis but not required for our analysis. Our covariates for testing H1b will be time-constant.

[30] The standard hazard model assumes "single-spell data," which is consistent with the data available from most unemployment surveys (i.e., each person can only be unemployed a single time). Excluding CEOs with multiple spells has a minimal effect on our tabulated H1b results, slightly weakening the significance due to a loss of power (the sample drops from 738 to 653; refer to Online Appendix A).

3.2 The Role of Executive Gaps for Match Quality – H2a and H2b

To examine whether externally hired CEOs with employment gaps have lower compensation than those without employment gaps (Hypothesis 2a), we estimate the following regression for a comprehensive sample of external CEO hires:

$$\textit{Total Compensation} = \alpha_0 + \alpha_1 \textit{ Gap Indicator} + \textit{Controls} + \varepsilon \quad (2)$$

The dependent variable, *Total Compensation*, is the sum of salary, bonus, other annual total value of restricted stock granted, total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other compensation from the Execucomp database. We have no strong theoretical reason to either transform (e.g., log) this variable or not, so we present results both ways.[31]

Rajgopal, Taylor, and Venkatachalam (2012) find that the properties of first-year pay differ significantly from those of subsequent years and that the cross-sectional variation in first-year pay is largely influenced by the CEO's reservation wage. Theories of reservation wages assume workers earn their reservation wage until retirement or until they leave for another job. To better align with this notion, we estimate Equation (2) separately for the first and second years of the CEO's employment. By also testing second-year compensation, we examine whether results are driven by one-time items in first-year pay (e.g., signing bonuses or initial stock grants) or partial year employment.

[31] Some studies that examine the determinants of CEO compensation do not to transform this variable (Core, Holthausen, and Larcker [1999]; Grinstein and Hribar [2004]; Rajgopal, Taylor, and Venkatachalam [2012]; Custódio, Ferreira, and Matos [2013]), while other studies take the log (Core, Guay, and Larcker [2008] and Cadman, Carter, and Hillegeist [2010]).

The variable of interest, *Gap Indicator*, is an indicator variable that is equal to one if the executive experiences an employment gap before joining the firm as a CEO and zero otherwise. We expect the coefficient of *Gap Indicator* to be negative, $\alpha_1 < 0$.

It is not immediately clear whether it is appropriate to include any control variables in Equation (2). Including controls for managerial ability or search costs eliminates precisely the effects we want to capture with the existence of an employment gap. The challenge we face resembles that of Fee and Hadlock (2003), who examine whether CEO compensation packages at a new employer are driven by stock performance of the prior employer and the value of forfeited compensation. They focus on estimations that include only their variables of interest (i.e., stock performance of the prior employer and the value of forfeited compensation). They then estimate an alternate model with a parsimonious set of control variables (CEO age and a time trend) for completeness. We follow a similar strategy.

We start by estimating a regression where the only independent variable is the variable of interest, *Gap Indicator*. Next, we estimate a second specification where we include control variables that, in the context of labor market models we use as a framework, could lead to cross-sectional variation in CEO compensation. Mortensen and Pissarides (1999) argue that search and matching frictions generate match-specific rents and that wages divide these rents between the employee and the employer. For example, certain governance structures may give the CEO greater bargaining power, allowing him or her to extract a greater proportion of these rents from the firm. Therefore, similar to studies that take into account the "managerial power" view of executive compensation, we include a number of control variables that capture the governance of the firm. In particular, we control for *CEO-Chair Duality*, *Board Size*, *% Outside Directors*, *Blockholder Indicator*, and *% Institutional Ownership*. (See Core, Holthausen, and Larcker [1999]; Grinstein

and Hribar [2004]; Rajgopal, Taylor, and Venkatachalam [2012]).[32] In this expanded specification, we also control for performance of the prior employer (*Abnormal Returns–Prior Employer*) given its positive association with first-year compensation at the new employer (Fee and Hadlock [2003]). Macroeconomic conditions (e.g., overall growth in the economy, merger and acquisition waves, IPO waves, etc.) may result in supply/demand fluctuations for CEOs, which, in turn, affect CEO compensation. We include year-quarter fixed effects to capture these effects.

Studies of executive compensation typically control for the economic determinants of total pay when examining the relation between total compensation and the variable of interest (e.g., governance characteristics in Core, Holthausen, and Larcker [1999]; compensation consultants in Cadman, Carter, and Hillegeist [2010] and Armstrong, Ittner, and Larcker [2012]; talent agents in Rajgopal, Taylor, and Venkatachalam [2012]). The list of variables typically includes firm size (*Log*(*Sales*)), investment opportunities (*Book-to-Market*), performance (*Return on Assets* and *Abnormal Returns-Current Firm*), and firm risk (*SD(ROA)* and *SD(Returns)*). We believe that, in our context, controlling for the economic determinants of compensation should remove much of the effect of interest, so we tabulate (but not interpret) a specification that includes these variables. The primary data constraint for this test is the ability to measure the gap for externally hired CEOs. (We do not need measures of generalist/specialist, and we do not need to measure NCCs.)

We next examine whether externally hired CEOs with gaps experience shorter *ex post* employment duration than externally hired CEOs without gaps (Hypothesis 2b). Our analysis of employment duration is related to CEO turnover literature (see Murphy [1999] for an overview). These studies often focus on the firm performance-CEO turnover relation and the factors that affect

[32] One concern that is not unique to our study is that these variables, rather than proxying for the effectiveness of governance, proxy for CEO quality or the complexity of the CEO's job. If this is the case, including the governance structure variables as controls will remove (at least) some of the effect we intend to capture with our variable of interest.

this relation (e.g., monitoring by the board or institutional shareholders). While this literature has traditionally relied on logistic and multinomial logistic models (Weisbach [1988]; Parrino [1997]; Huson, Parrino, and Starks [2001]), a number of more recent studies use hazard models (Campbell, Gallmeyer, Johnson, Rutherford, and Stanley [2011]; Hazarika, Karpoff, and Nahata [2012]; Jenter and Kanaan [2015]). Similar to these more recent studies, and as in our tests of H1b (*Gap Length*), we employ the Cox semiparametric proportional hazard model in our estimation. *Time-to-Turnover*, the length of time that the executive spends employed as the CEO of the new firm, is our measure of time to failure. In this analysis, our data is right censored because the CEOs in office at the end of our sample period have not yet left their positions.

Studies of CEO turnover try to distinguish between voluntary and forced turnover, either focusing solely on forced turnover or estimating separate hazard functions for each. Because we are interested in match quality, we want to identify both forced and voluntary turnover events that indicate poor CEO-firm matches as our "failure" events. We identify turnover reasons representing both forced and voluntary–poor match quality based on the process described in Parrino (1997), Huson, Parrino, and Starks (2001), and Hazarika, Karpoff, and Nahata (2012). We outline the process for this partition in detail in Section 4. All other turnover events not classified as resulting from a bad match (e.g., death, merger or acquisition, or planned retirement) are treated as censored observations. In addition to estimating a hazard model where we combine forced and voluntary–poor match departures, we use a competing-risks hazard model to estimate separate coefficients for forced and voluntary–poor match departures while controlling for the other type of competing "failure" event.

The variable of interest in the duration of employment models is *Gap Indicator*, an indicator variable that is equal to one if the executive experiences a gap before appointment to the

CEO position. We expect the coefficient of this variable to be positive (i.e., gaps are associated with a higher rate of failure). The hazard models include control variables for the CEO's demographic characteristics (*Age*, *Female*), governance structure of the firm at the time the executive began as CEO (*CEO-Chair Duality*, *Board Size*, *% Outside Directors*, *Blockholder*, and *% Institutional Ownership*), and the performance of the firm over the 12-months preceding the executive's appointment to the CEO position (*Past Abnormal Returns-Current Firm*). Controlling for prior performance controls for systematic differences in time-to-turnover for CEOs that join firms after a period of poor performance, and, relatedly, for the possibility of greater match uncertainty after a turbulent period. We control for the governance structure because we expect firms with better monitoring to dismiss poor matches earlier.

In addition to the hazard models above, following Allgood and Farrell (2003), we estimate a multinomial logit model with three possible outcomes: 1. bad match that ends with forced turnover, 2. bad match that ends with voluntary turnover due to poor match quality, and 3. good match (the duration of employment is three years or longer and does not end in either bad match type).

3.3 The Role of Executive Gap Length for Match Quality – H3a and H3b

Tests of H3a and H3b closely follow the tests of H2a and H2b, but several important differences are worth highlighting. First, both H3a and H3b are about gap length (*Gap Length*), as opposed to the existence of a gap (*Gap Indicator*). Typically, one expects predictions related to existence to match predictions related to duration. For example, when examining wages and education, people who enrolled in college make more than those who did not, while people who graduated from college make more than those that simply enrolled.[33] Our predictions do not follow

[33] https://www.census.gov/hhes/www/income/data/earnings/call1usboth.html

this example. We expect the best matches for people who either have no gap or have a relatively long gap. As a result, the predicted signs on *Gap Length* (in H3a and H3b) are opposite of the predicted signs on *Gap Indicator* (in H2a and H2b). Second, the sample sizes for testing H3a and H3b are smaller, because we exclude executives without a gap from these models. Finally, when we log the dependent variable (as a robustness test), we also log the independent variable of interest (*Gap Length*).

4. Data Gathering Methods

4.1 Data gathering investments

To conduct the empirical analyses we describe in Section 3, we construct a comprehensive sample of external CEO hires at firms in Execucomp from 1992–2014.[34] Sample construction requires substantial hand-collection (see Online Appendices B–F). Our starting sample before hand-collection (as detailed inOnline Appendices G and H) is 5,095 new CEO/firm observations (with available data in both Execucomp and CRSP). The first step in this process is identifying all CEO transitions at S&P 1500 firms. To this end, we start with executives in Execucomp who became a CEO in or after 1992, relying on the *BECAMECEO* variable. We refer to this variable as *E_Becameceo*, where the prefix *E_* refers to Execucomp.[35] We then require the CEO's new employer to be in the CRSP database as of the *E_Becameceo* date. (See Online Appendix H for brief verbal summaries of the relevant SAS programs and Online Appendix I for the .log files.)

[34] The Execucomp database covers the S&P 1500, companies removed from the index that are still trading, and some client requests. Data collection on the S&P 1500 began in 1994, and data before 1994 is mostly for the S&P 500. (see "Overview of Executive Compensation" on WRDS). We loosely refer to our sample as the sample of external CEO hires at S&P 1500 firms.

[35] *E_Becameceo* is well populated in Execucomp. Only 3.5% of the CEO observations (based on the annual CEO flag, *CEOANN*) have missing values for this variable. Our preliminary assessment of machine-readable data, which we outline in this section, assumes that *E_Becameceo* is accurate when not missing. While conducting our pilot sample hand-collection, we have seen numerous examples where this date is not accurately captured. Nevertheless, for the purposes of evaluating available machine-readable data from the perspective of someone focusing on avoiding hand-collection, we take this variable as is. As we outline below, for our sample, we plan to hand-check all *E_Becameceo* dates.

The second step is isolating external hires, the focus of our study. Below we discuss the classification of succession types, the key variables, and provide detailed information on the required hand-collection. The key variables are *Gap Indicator* (source: hand-collect), *Gap Length* (source: hand-collect), *NCC* (source: hand-collect), *Generalist*/*Specialist* (source: BoardEx), *CEO Total Compensation* (source: Execucomp), and *Time-to-Turnover* (source: hand-collect). Appendix A presents the descriptions of all the variables we use (including control variables) and their sources.

4.2 Succession types and employment gaps

Given there is no prior research on executive gaps, we begin with a brief discussion of four high profile CEOs we have chosen as examples to illustrate succession types and gaps: Jamie Dimon (Bank One and JPMorgan Chase), James Kilts (Nabisco and Gillette), Marissa Mayer (Yahoo) and Meg Whitman (eBay and Hewlett Packard). For parsimony, we provide the most details for Jamie Dimon's appointment at Bank One. These examples are from very large firms and should not be viewed as representing the population of CEOs. Also, these examples are meant only to illustrate and not to support or refute any hypotheses.

With respect to succession type, we view cases where the executive has been with the company for more than two years before becoming CEO as internal. We consider executives who first start with a company as its CEO as well as executives who join the company as part of a succession plan, whereby their early employment overlaps with the existing CEO, as external hires. The latter group is often referred to in the literature as "heir apparent" (see Vancil [1987]).[36]

[36] Researchers typically proxy for this arrangement in one of two ways. The first method is based on how long the executive was at the firm before being promoted to CEO (Allgood and Farrell [2003]). Alternatively, researchers use executive titles to identify heirs (Berry, Bizjak, Lemmon, and Naveen [2006]). This method follows the discussion in Vancil (1987), arguing that an "heir apparent" is typically appointed to the president or COO position before becoming CEO. We combine these methods and classify someone as an external heir if he or she (*i*) started working for the company within a year of becoming CEO or (*ii*) started working for the company as a president/COO within

Thus, identifying external successions, our sample of interest, requires us to observe the date the executive joined the company and the date the CEO appointment became effective. We then determine when the new CEO left his or her former employer and compare it to the date he or she joined the firm to calculate the length of the gap.

Table 1 summarizes the key dates, succession types, and gap length for Jamie Dimon, James Kilts, Marissa Mayer, and Meg Whitman. In the case of Jamie Dimon, public documents indicate that he left Citigroup on Nov. 2, 1998[37], became the CEO of Bank One on March 27, 2000,[38] and did not work at another public firm between the two. That is, he was not employed as an executive for a public company for approximately 511 days (*Gap Length*=511). JP Morgan Chase and Bank One merged on July 1, 2004.[39] When the companies combined, Dimon became the president and chief operating officer of the merged entity. His employment agreement with JP Morgan Chase (dated Jan. 14, 2004—before the completion of the merger) makes it clear that he was expected to become CEO (satisfying Vancil's notion of an "heir").[40] A year and a half later, he became CEO.[41]

Our second example is James Kilts. On March 24, 1997, he resigned his position (Executive Vice President, Worldwide Food) at Philip Morris (now Altria Group). On Jan.1, 1998, he joined RJR Nabisco as its CEO. He was an external hire with a gap of approximately 283 days.

two years of becoming CEO. To ensure method (*ii*) is accurately implemented, we hand-collect starting titles for anyone who joined the firm between one and two years of becoming CEO.

[37] http://www.sec.gov/Archives/edgar/data/831001/0001047469-99-008885.txt

[38] http://www.sec.gov/Archives/edgar/data/1067092/000095013100002400/0000950131-00-002400-d2.pdf

[39] http://www.sec.gov/Archives/edgar/data/19617/000119312504112906/d8k.htm

[40] "(i) During the Initial Period, the Executive shall serve as the President and Chief Operating Officer of the Company, and, during the Second Period, the Executive shall serve as the President and Chief Executive Officer of the Company"; http://www.sec.gov/Archives/edgar/data/19617/000095012304002100/y93320exv10w1.htm

[41] This example highlights the potential importance of business combinations. When equals merge, we eliminate these observations (Wulf [2004]). In situations where a larger firm and smaller firm combine, we keep the observation only if the CEO of the smaller firm becomes the CEO of the merged entity, and we consider that person to be an external hire (e.g., Jamie Dimon becoming CEO of JPMorgan Chase).

In 1999, RJR Nabisco spun off its tobacco business and became Nabisco Holdings Corp. On Oct. 20, 1999, Nabisco Group Holdings Corp, announced that Kilts would become its new CEO (effective Jan. 1, 2000) while remaining the CEO of Nabisco Holdings. From an executive turnover perceptive, this is akin to an internal hire, and we exclude this type of (primarily) technical employment status change. On June 26, 2000, Philip Morris announced that it was acquiring Nabisco Holdings, and Kilts was not offered a job at Philip Morris (Deogun, Fairclough, and Branch, 2000). The acquisition was completed on Dec. 11, 2000. On Jan. 22, 2001, Gillette announced Kilts would be its new CEO, starting on Feb. 12, 2001 (per Dow Jones Business News).[42] Kilts was an external hire for Gillette, and his gap was two months (63 days). In total, Kilts has had three separate CEO appointments, two of which are external appointments and therefore relevant to our study.

Our third example is Marissa Mayer, who became CEO of Yahoo effective July 17, 2012 (announced on July 16, 2012). This release also served as notice of her resignation as Google's vice president, local, maps & location services. She was an external hire with no gap.

Our final example is Margaret "Meg" Whitman, who was a top five executive at Stride Right, eBay, and Hewlett-Packard (HP). Her first transition was to leave Stride Rite and join eBay as its CEO. However, this occurred while eBay was still a private firm. Since accurate dates and compensation details are not available for private companies, we exclude these types of observations for the sample. She then left eBay (after it became public), effective March 31, 2008. After leaving eBay, Whitman joined the board of HP (effective Jan. 21, 2011) and subsequently became the CEO of HP (effective Sept. 22, 2011). Her gap before becoming an employee of HP (i.e., CEO) was 1,270 days. While she was not an executive at a public company for this extended

[42] Factiva document number: Document djon000020010711dx1m009jm

period, she was active. Most notably, she announced her candidacy for governor of California in February 2009. She won the Republican primary but lost the general election (on Nov. 2, 2010).

These examples highlight the key information we need to determine succession types and calculate gap length. To assess the extent of "enhanced investment" required to complete this project, we hand-collected this key information for the pilot study of 50 CEOs (from the sample of 5,095 CEO successions over the 1992–2014 period) and compared it to information from three machine-readable databases: Execucomp, BoardEx, and Thomson Financial. The details of this pilot study are provided in Online Appendix B.

The majority of papers on executive compensation and turnover rely exclusively on Execucomp. Several recent studies supplement (or replace) Execucomp with the BoardEx database (e.g., Custódio, Ferreira, and Matos [2013]). While less typically used, Rogers and Van Buskirk (2009) demonstrate that (at least in some cases) executive turnover can be inferred from Thomson Financial insider trading database.[43] Ignoring their result would likely understate the value of common databases and overstate the need for hand-collection. To conservatively estimate the need for hand-collection, we used Thomson to develop proxies for when an executive started with and left public companies. Because not all executives are required to file insider trading reports[44] and are certainly not required to trade on the day they start working for a new firm or the day they leave an old firm, these dates are noisy proxies.

Two key takeaways emerge from the analysis of the 50 random cases of CEO succession. First, Execucomp, BoardEx, and Thomson often lack the key information to classify succession types into internal and external. Second, the machine-readable databases perform even worse when

[43] On p. 152 the authors state: "To validate this procedure, [they] compare the insider trading inferred turnover to turnover based on the Execucomp data for firms with sufficient data in both Execucomp and the insider trading database. The comparison indicates that the two procedures agree approximately 94% of the time."
[44] Only officers, directors, and beneficial owners of more than 10% are required to file with the SEC.

it comes to identifying gaps for external successions. Not only do the databases lack the key information to determine the gap in a significant fraction of external successions (40%–88% depending on the database), but they also introduce measurement error by classifying executives incorrectly as having a gap when they do not and vice versa.[45] Of course, given the small sample size, it is difficult to arrive at firm conclusions with respect to measurement error.

Overall, our analysis of the 50 random cases as well as the systematic overview of machine-readable databases underscore the importance of hand-collection for both succession type and for the existence and length of gaps. We therefore proceed as follows. For each observation, we hand-collect succession type unless at least two machine-readable databases indicate that the succession type is internal (in which case we automatically exclude the observation from our sample). If the succession type is external, (*i*) we verify the date the executive became the CEO, (*ii*) hand-collect the date the executive joined the company, and (*iii*) hand-collect the date the executive left his or her prior company. Finally, because during the required hand-collection we have to read relevant proxy statements, we also collect information about the executives' activities during their gap, when disclosed.

4.3 Noncompete constraints (NCC)

An ideal measure of NCCs would reflect (*i*) whether a specific executive has signed a noncompete agreement and (*ii*) the degree to which the terms of that agreement can be enforced. When we think a noncompete likely exists, our NCC variable captures the state-level

[45] Our study is not the first to document problems with the machine-readable databases that collect data from proxy statements. For example, Cadman, Campbell, and Klasa (2015) find errors and omissions related to severance agreement details in Execucomp relative to their hand-collected data. Furthermore, a key source of the problems they note is when information must be extracted from text rather than tabular formats (i.e., the data in Execucomp deteriorates markedly when the vendor must extract information from text, which is exactly where our information on dates of employment is likely to be found).

enforceability of such agreements. When we fail to find evidence of such an agreement, our NCC variable is set to zero.

As discussed in Online Appendix D, we believe it is not possible to accurately assess whether an executive who joins a given firm as a CEO or an external heir signed a noncompete with his or her prior firm. The two main problems that arise are 1) firms do not always provide individual contracts (employment agreements, severance agreements, etc.) even for CEOs and 2) the majority of our sample comes from non-CEO positions.[46] As a result, we proxy for whether a new CEO had previously signed a noncompete using whether his or her prior employer uses noncompetes in any employment or severance agreements for any employee. To determine whether a company uses the agreements, we download all relevant EDGAR documents and use Python to identify likely agreements and potentially relevant subsections of these agreements. We then read the subsections to determine whether the company actually uses these covenants. (A more complete description is available in Online Appendix D.)

There is substantial variation across states in the enforceability of noncompetes. At one extreme is California, where no aspects of noncompetes are enforceable (Bishara, Martin, and Thomas [2015]).[47] At the opposite extreme is Florida (since 1997). To capture this variation in enforceability, we use the index developed by Garmaise (2011). Garmaise published summary scores by state and year for 1992–2004.[48], We hired three law student research assistants to extend this index to cover 1980–2013. We note this window includes years preceding Execucomp, which

[46] Custódio, Ferreira, and Matos (2013) investigate have a sample of 21,909 CEO-firm-years. For this pooled sample, they report that 35.4% of these observations are for individuals with CEO experience.

[47] Despite the lack of enforcement, about 65% of contracts for CEOs in California still contain noncompetes (Bishara, Martin, and Thomas [2015]). Similarly, descriptive statistics provided by Kaplan and Strömberg (2003) suggest that approximately 60% of contracts between entrepreneurs (in California) and venture capitalists contain noncompetes.

[48] Professor Garmaise has graciously shared the answers to the individual questions that provided the basis for the index. This should greatly enhance to accuracy and consistency of our collection efforts. We would have to get permission from Professor Garmaise before any public distribution of these details.

we use as a starting point to create our sample. We decided on this large window to minimize the chances that we would need to recollect this data (details in Online Appendix E).

To apply this index to new CEOs, we also must determine the location of their prior employer's headquarters. Compustat and BoardEx only include the current state and backfill prior observations with current state information. In Garmaise's sample, about 5% of firms move their headquarters. As a result, when available, we use the addresses from SEC filings (specifically insider filings in Thomson) to determine the location of headquarters around the time the executive leaves his/her prior employer. In cases where we are not able to identify an insider filing, we rely on the current location of headquarters per Compustat.

Returning to the example of Jamie Dimon, non-EDGAR sources indicate that he signed a separation agreement, which contained noncompete clauses, with Citibank. Unfortunately, we could not find a copy of this agreement on EDGAR, but we did find reference to it in the Citigroup proxy statement (DEF 14A) dated March 8, 1999. This reference did not explicitly mention noncompetes (as defined by Garmaise [2011]). In this same proxy statement, Citigroup does reference a "noncompetition agreement" for its subsequent CEO (Sanford Weill).[49] Based on this observation, we conclude that Dimon was also likely to have had a noncompete and set the value of NCC equal to three—the enforceability index score of New York in 1999, where Citigroup is headquartered, per Garmaise (2011). Note that, while Bank One (CRSP SICCD=6172 HSICCD=6021 as of Oct. 30, 1998) and Citigroup are both financial services firms (CRSP SICCD=6331 HSICCD=6211 as of Oct. 30, 1998), they are not in the same two-digit SIC code. We document whether new CEOs came from competitors in the same 2, 3 or 4 digit SIC code (for descriptive purposes only).

[49] "During such period of continuing payments and stock option vesting and exercise, Mr. Weill would be subject to a noncompetition agreement in favor of the Company."

4.4 Generalist/Specialist measure

Economists have been interested in general versus specific skill investment for decades (e.g., Becker [1962]). As discussed in the research design section, several of our tests rely on measuring whether a new CEO is more of a generalist or a specialist. We capture this construct using the procedures summarized in Custódio, Ferreira, and Matos (2013). Specifically, we use machine readable data to proxy for five aspects of a CEO's "professional career." These five variables are used to create an index that is bifurcated to create an indicator variable (*Generalist*), which equals one for generalists and zero for specialists. The estimation is detailed in Online Appendix G.

4.5 CEO total compensation

The testing of hypotheses 2a and 3a requires us to measure *Total Compensation* for the first and second year of the CEO's employment. As has become standard in the literature, we define *Total Compensation* as the sum of salary, bonus, other annual total value of restricted stock granted, total value of stock options granted (using Black-Scholes), long-term incentive payouts, and all other total from the Execucomp database. We complement missing observations for the first year of the CEO's tenure at the new employer by using information from the corresponding proxy statement.

4.6 CEO time-to-turnover and reason for CEO departure

The testing of hypotheses 2b and 3b requires us to determine how long the CEO stayed with the hiring firm and, in the case of turnover, the reason for the departure. Execucomp serves as our starting point to identify potential turnover events (i.e., cases where a different executive becomes the CEO). We then search SEC filings and press releases/business press articles to verify turnover event.

We include all turnover events in our analyses, though our focus is on those suggesting a poor firm-executive match (i.e., forced turnovers, which we assume stem from poor matches, and voluntary turnovers that indicate a poor match). We identify these departures based on the process used in Parrino (1997), Huson et al. (2001), and Hazarika et al. (2012). Specifically, we identify forced turnovers if the CEO is fired, forced from the position, or departed due to policy differences. We also classify departures as forced turnovers if the (*i*) the departing CEO is under the age of 60 and the reason for the departure is not listed as involving death, poor health, or the acceptance of another position (i.e., a board membership within the firm or a full time executive position elsewhere or within the firm); or (*ii*) the departing CEO is under the age of 60 and announcement is fewer than six months before the succession. We classify turnover events as voluntary−poor match if the departing CEO takes a comparable position elsewhere (i.e., a full time executive position at a different public firm) or departs for previously undisclosed personal or business reasons that are unrelated to the firm's activities. All other turnover events are treated as censored and are assumed to be for reasons other than a poor firm-executive match.

5. Results

Our sample starts with 5,095 new CEO-firm pairs in Execucomp and CRSP over the 1992-2014 period. We exclude 1,520 CEOs classified as an internal hire by at least two of three machine-readable databases we initially rely on (ExecuComp, BoardEx, and Thomson Insider) and not classified as an external hire by any. Of the remaining 3,575 CEO hires, we exclude 1,329 identified as internal hires by our hand collection. We then drop 160 observations where the prior public firm was a related firm or the same firm or where the executive founded the firm. We drop another 50 observations as errors (e.g., the executive became CEO before 1992, the employment spell could not be verified, or the employment spell was in Execucomp more than once). This

results in 2,036 external CEO hires, of which 1,707 were previously employed by a public firm. We lose another 18 observations where we are not able to obtain a CRSP identifier at the time of the hire, resulting in a sample of 1,689 external CEO hires. Our final sample is smaller than anticipated for two main reasons. First, we ran into several unexpected data complications (e.g. the prior public firm being the same firm or a non-cross listed foreign firm) that resulted in attrition. Second, the percent of external CEOs in our sample with a calculable employment gap was noticeably lower than in our pilot study (84% compared to 93%). As we explain below, our samples sizes vary across the various analyses because of additional data requirements.

5.1 Descriptive evidence

Table 2 reports descriptive statistics for the old employers at the time of departure, the new employers at the time of arrival, and the pooled S&P 1500 sample over 1992–2014.[50] The old employers are significantly larger (as captured by *Assets* and *Sales*), perform better (in terms of *ROA* and, to a lesser extent, *Abnormal Returns(FYR)*), and are less volatile (with lower *SD(ROA)* and *SD(Returns)*) than their new employers. Old employers also have significantly smaller *Book-to-Market* ratios. These results are consistent with the descriptive statistics on external CEO hires Fee and Hadlock [2003] report and suggest that executives, many of whom do not hold CEO titles at their old employers, obtain CEO positions at smaller and less established firms.

[50] We have 1,689 executive-new employer-year observations in our sample. The number of observations for new firms in Table 2 (1,565) is smaller because (i) some firms have more than one CEO transition in a given year, reducing the sample size to 1,630 new employer-year observations, and (ii) data requirements further reduce the sample size. The number of observations for old firms in Table 2 (1,408) is smaller because (i) we are unable to obtain GVKEY for 25 observations reducing the sample to 1,664, (ii) we are unable to obtain COMPUSTAT data for another 19 observations either because the data for the respective fiscal year is missing or because the firm reports in Canadian dollars, reducing the sample to 1,645, (iii) some employers hire more than one CEO in a given year, reducing the sample into 1,554 old employer-year observations, and (iv) data requirements further reduce the sample. Note that, in the case of both old and new employers, the sample sizes are smaller for governance characteristics. This is due to limitations in the coverage of RiskMetrics and BoardEx.

With respect to governance characteristics, old employers have larger boards and greater institutional ownership than new firms, both possibly driven by the old employers' relatively larger size. New employers are less likely to have a CEO-Chairman and more likely to have a blockholder. Finally, while statistically different, old and new employers have similar board composition in terms of the percentage of outside directors.

Turning our attention to the comparison between new employers, all of which are S&P 1500 firms, and the S&P 1500 universe, we note that at the time of the executives' arrivals, new employers are smaller, perform worse and are more volatile than the pooled S&P 1500 sample. New employers also have smaller boards and lower institutional ownership than the S&P 1500 sample and are less likely to have a CEO-Chairman. New employers are quite similar to the S&P 1500 sample in terms of board composition and are equally likely to have a blockholder.

Table 3 reports descriptive statistics for the gap observations in our sample. We focus on the set of variables that we use in later tests as dependent or independent variables and on the 1,538 gap observations for which the key data are available. Panel A shows that 57.5% of the incoming external CEO hires experience a gap (i.e., 30 days or more between the date the executive left the old employer and the date he/she joined the new employer). The average gap is 628 days while the median is 88 days, suggesting that the distribution of *GAP Length* is highly skewed.

Panel B presents characteristics of these incoming CEOs at the time of the hire. About half of the CEOs in our sample are generalists.[51] The average CEO is 53 years old when he/she joins the new employer and only 12% of the CEOs in our sample are older than 60. Approximately 19% held CEO positions at the old employer. Only 3.4% of the CEOs in our sample are female.

[51] Note that we are able to define *Generalist* for 1,251 of the 1,689 incoming CEOs because of limitations in BoardEx coverage.

Panel C focuses on compensation and time-to-turnover, the two measures of CEO-firm fit that we investigate. The mean (median) compensation in the first year the executive becomes CEO at the new employer is $5.8 million ($3.1 million). The compensation in the subsequent year is lower (mean and median of $3.6 and $2.1 million, respectively), possibly because the first year compensation includes hiring grants (note, however, that we have second year compensation data for 1,275 observations compared to 1,504 observations for the first year). The average *Time-to-Turnover* is 1,746 days (4.78 years). When we differentiate between *Time-to-Turnover* for failure versus non-failure events, we observe that mean and median *Time-to-Turnover* is significantly lower for failure events (e.g., mean (median) of 1,377 (1,176) days for *Failure* compared to 1,909 (1,656) for *Non-Failure*).

Panel D reports the characteristics of the new employers at the time of the hire. Not surprisingly, these descriptives are very similar to those in Table 2; the small differences arise from slightly different sizes, that are, in turn, due to the difference in unit of observation across the two tables (new employer-year in Table 2 versus executive-new employer-firm in Table 3).

Finally, Panel E presents the characteristics of the old employers at the time of departure. The mean (median) abnormal returns of the employer over the 12-month period preceding the executives' departure is 0.69% (-2.04%).[52] The distribution of *NCA_Signed* indicates that 85% of the CEOs in our sample come from firms that use non-compete agreements.

Before we comment on the distribution of *NCC* in Panel E, we briefly discuss Table 4, which reports the scores for the noncompetition enforceability index developed by Garmaise

[52] Note that the mean and median abnormal returns for the old employer are somewhat different from the abnormal returns for the old firm in Table 2. This stems from differences in the (i) samples. with 1,408 old employer-year observations in Table 2 and 1,538 executive-new employer-year observations in Table 3, and (ii) the measurement windows, with the returns focusing on the most recent 12-month fiscal period prior to the executives' departure date in Table 2 and on the 12-month period prior to the executives' departure date in Table 3.

(2011) over the 1980–2013 period. We obtain the scores for the 1992–2004 period from Garmaise (2011) and extend the time period following the process detailed in Online Appendix E. Higher scores indicate stricter enforcement of non-compete agreements. The enforceability score ranges from a low of zero (California for the entire period, Louisiana over 2002–2003, North Dakota over the entire time period) to a high of nine (Florida over 1997–2013).

Turning our attention back to Table 3, Panel E, we observe that the mean (median) *NCC* is 2.9 (5) (*NCC* is equal to zero where the firm does not employ signed non-competes and is equal to the state enforceability index from Table 4 otherwise). Note that there is considerable variation in the distribution of *NCC* even when we limit the sample to 1,280 observations where the executive likely signed an NCA, with 25th percentile of 3 and 75th percentile of 5. Thus, the degree to which executives who sign NCAs are constrained by those agreements varies in our sample.

5.2 Determinants of CEO gaps

In this section we report the results for tests of H1a and H1b, which consider the effect of NCCs and CEO skillsets on the existence and length of employment gaps. We limit our analysis to the 1,251 observations for which we are able to define *Generalist*/*Specialist* per Custódio et al. [2013]. We begin by documenting the joint distribution of non-compete constraints and CEO skillset. With respect to non-compete constraints, we first partition executives into two groups based on whether they likely signed an NCA (*NCA_Signed* = 1 and *NCA_Signed* = 0). We further partition executives who likely signed an NCA into three groups based on the Garmaise [2011] enforceability index: no enforcement (*NCC* = 0), low enforcement (*NCC* = 1 – 4) and high enforcement (*NCC* = 5 – 9). With respect to CEO skillset, we partition executives into two groups: *Generalist* and *Specialist*.

Table 5, Panel A reports the resulting 4x2 joint distribution. Our sample executives are roughly evenly split between generalists (627 CEOs) and specialists (624 CEOs) by construction as we bifurcate the factor score at the median. About one third of each group of CEOs effectively do not face non-compete constraints—191 (212) generalists (specialists) fall either in the *NCA_Signed* = 0 or *NCC* = 0 groups. The remaining generalist and specialist CEOs are fairly evenly distributed across the *NCC* = 1– 4 and *NCC* = 5 – 9 groups.

Panel B focuses on the 738 executives that experience an employment gap. The proportion of generalists experiencing a gap is not statistically different from that of specialists (p-value from a χ^2 test is 0.26). Two key insights emerge. First, NCCs do not seem to affect the likelihood of employment gaps. Among generalists, approximately 62% (=(33+86)/(63+128)) in the *NCA_Signed* = 0 or *NCC* = 0 groups experience a gap compared to about 55% (=(117+124)/(202+234)) in the *NCC* = 1– 4 and *NCC* = 5 – 9 groups. The percentage of specialists experiencing a gap is about 60% both in the *NCA_Signed* = 0 or *NCC* = 0 groups and the *NCC* = 1– 4 and *NCC* = 5 – 9 groups. Second, consistent with H1a, when faced with NCCs, specialists are more likely than generalists to experience a gap. For example, in the *NCC* = 5 – 9, ~60% of the specialists experience a gap compared to ~53% of generalists.

Panels C and D show that, consistent with H1b, among executives facing NCCs > 0, gap length is longer for specialists than for generalists (all means and medians significantly different from each other [at the 10% level] except the medians for *NCC* = 5– 9 group). In addition, in untabulated results, the median gap length for specialist CEOs facing NCCs (1,248 for the *NCC* = 1– 4 group and 701 for the *NCC* = 5 – 9 group) is higher than for those that do not face NCCs (694 for the *NCA_Signed* = 0 group and 647 for the *NCC* = 0 group).

Table 6 presents the results from the estimation of Equation (1), where we test H1a—that, in the presence of NCCs, generalists are less likely than specialists to experience gaps. Column (1) reports the results for the benchmark model without control variables. In columns (2) and (3) we include controls for CEO characteristics and the executives' prior position, respectively. In column (4) we estimate a full model with all the control variables.[53] We find some evidence consistent with H1a. Specifically, in columns (2) and (4) the coefficient of *NCC_Generalist* is significantly lower than the coefficient of *NCC_Specialist* (see χ^2 test). That is, conditional on the presence of NCCs, generalists are somewhat less likely to experience gaps than specialists. Contrary to our expectations, the coefficient of *Generalist* is not different from zero and the coefficient of *NCC_Generalist* is negative and significant. Thus, generalists subject to NCCs are less likely than non-constrained generalists to experience a gap, a surprising result. As for the control variables, we find that older executives are more likely to experience a gap as well as executives who were CEOs at their prior employer.

Table 7 provides the results of tests of H1b, which predicts specialist employment gaps will be longer than generalist employment gaps given the presence of non-compete constraints. To test this, we use a Cox semiparametric proportional hazard model, as discussed in Section 3.1. The Cox model predicts the hazard rate of failure (i.e., exiting the gap and gaining employment). We expect the coefficient of *Generalist* to be positive—generalists have a shorter gap or higher hazard rate of exiting the gap and gaining employment—and the coefficients of *NCC_Generalist* and *NCC_Specialist* to be negative—NCCs constrain both generalists and specialists, decreasing the hazard rate of exiting the gap for both types. Most importantly, per H1b we expect that

[53] Note that the number of observations in Table 6 is 1,209 compared to 1,251 observations in Table 5 with non-missing gap information. The attrition results from the year-quarter fixed effects perfectly predicting the outcome (gap or no gap) in 42 cases.

specialists are more constrained in the presence of NCCs resulting in the coefficient of *NCC_Specialist* being smaller than that of *NCC_Generalist*.

A key assumption of the Cox model is that of proportional hazards. We begin by examining whether the proportionality assumption is violated for any intended predictors using two different methods. First, we run each of the four models adding time-dependent covariates in the model as significance of a time-dependent covariate suggests a potential violation for the respective predictor.[54] Second, we test the proportionality assumption using the Schoenfeld residuals after fitting the model. The only variable that indicates a violation of the proportionality assumption across both tests is *Age > 60* in Column 2. When we exclude this variable from Column 2, we find qualitatively consistent results (unreported).

In Columns (1) – (3) of Table 7, the coefficient on *Generalist* is significantly positive, as expected. However, there is no evidence that *NCC_Generalist* or *NCC_Specialist* are negative. These results, combined with results in Table 6, suggest that generalists have shorter gaps (i.e., have a higher hazard rate of exiting the gap and gaining employment) but, when facing non-compete constraints, are less likely to have a gap. Finally, contrary to H1b, χ^2 tests of the difference in coefficients suggest that the coefficient on *NCC_Specialist* is not statistically different from that on *NCC_Generalist*.

Overall, the results provide modest evidence consistent with our H1 predictions. We find evidence consistent with the prediction that, when faced with NCCs, specialists are more likely to have a gap (p-values from χ^2 tests of differences range from 0.06-0.11 in Table 6). With respect to gap length, we find univariate support that gaps are longer for specialists when facing NCCs (Table 5, Panels C and D), but no multivariate support (p-values from χ^2 tests of differences are

[54] Specifically, we interact our predictors with the logarithm of our measure of time.

not significant in Table 7). The only control variables that are consistently significant are *Age* and *Age > 60*, which suggest older executives are more likely to have a gap and, given they do, the gaps are longer (i.e., they have a lower hazard rate of exiting the gap and gaining employment).

5.3 Consequences of CEO gaps – match quality

In this section, we shift our attention to the relation between experiencing an employment gap and the CEO-new employer match quality. Recall that we predict externally hired CEOs with gaps have lower compensation (H2a) and shorter time-to-turnover (H2b) than those without gaps.

We start by providing descriptive statistics for the variables of interest as well as control variables we utilize in this analysis separately for executive with and without gaps. Table 8, Panel A shows that, consistent with H2a, CEOs with a gap earn lower compensation in the first and second years of their employment. Further, consistent with H2b, CEOs with a gap experience shorter time-to-turnover than CEOs without a gap, both for *Failure* events (i.e., when employment ends due to forced turnover or voluntary turnover resulting from a bad match) and *Non-Failure* events). We observe some differences in control variables across the two groups of executives. Specifically, CEOs with gaps are older and join smaller firms with higher book-to-market ratios, and with poorer and more volatile performance. Firms that hire CEOs with gaps also have smaller boards and larger institutional ownership.

Panel B presents the Spearman and Pearson correlation coefficients between gap length and measures of match quality. Gap length is negatively correlated with *Time-to-Turnover*, *Total Compensation*$_t$ and *Total Compensation*$_{t+1}$ (significant at 5% level), contrary to H3a and H3b. Finally, Panel C provides information on fit type based on whether the executive experiences turnover at his/her new employer during our sample period and, for those that do turn over, whether the turnover is forced or stems from a bad match. Among the 722 external CEO hires that are

classified as a *Good* fit, 53% had experienced a gap. In the *Bad* fit category, the proportion with and without gaps is very similar at 50%. Finally, mean and median gap length is shortest in the *Good* category and highest in the *Bad-Forced* category, with those in the *Bad-Poor Match* category falling in the middle.

Next, we conduct multivariate analyses for the relation between CEO compensation and whether the individual experienced a gap prior to joining the company. Table 9, Panel A reports the results using compensation for the first fiscal year of employment. We estimate a benchmark model without controls except for year-quarter fixed effects (column 1) as well as a series of models with controls for governance structure of the new employer, past performance of the prior employer, CEO characteristics, and characteristics of the new employer. We estimate these models alternatively with *Total Compensation*$_t$ and *Log(Total Compensation*$_t$*)* as the dependent variable. The coefficient on *Gap Indicator* is negative and significant (at the 1% level) across all models. These results lend support to the hypothesis that externally hired CEOs with gaps have lower compensation than those without gaps (H2a). The results are economically significant as well. Based on the reported coefficients in column (4), total compensation for executives with a gap is, on average, a $2.35 million lower (recall that mean total compensation at new employers is $5.8 million, per Table 2).

Panel B repeats the same analysis for compensation in the second year of the CEO's employment. The sample size for this analysis is smaller, partially because some CEOs do not stay at the new employer beyond the first year. We continue to find a negative and significant coefficient on *GAP Indicator* in columns (1) – (3) and (7) – (10). However, the economic magnitude of the effect is smaller and the statistical significance is weaker than using year one compensation. In the case of *Total Compensation*$_{t+1}$ as the dependent variable, once we control for

CEO characteristics, the coefficient of *Gap Indicator* becomes insignificant. Further, in models where we control for the economic determinants of compensation (columns (5), (6), (11) and (12)), we no longer find a relation between experiencing an employment gap and CEO pay. However, as we discuss in Section 3.2, because including the economic determinants of pay (i.e., firm controls) should remove much of the effect of interest, we do not interpret these specifications.

As for the control variables, results in columns (2) – (4) and (8) – (10) suggest that governance structures play a role in the determination of compensation levels. For example, the association between *CEO-Chair Duality* and *Total Compensation*$_t$ is positive, consistent with CEOs being able to extract greater rents when the CEO and Chairman positions are combined. In contrast, CEO compensation in lower when the percentage of outside directors on the board is greater, perhaps because CEOs have a lower ability to extract rents when the board is more independent. Contrary to the results in Fee and Hadlock (2003), there is no relation between performance at the prior firm and compensation. When we include the economic determinants of total pay, the relation between variables capturing the governance structure of the firm and compensation largely disappears, as expected (refer to discussion in Section 3.2). The coefficients of the economic determinants are generally consistent with prior literature: pay is higher at larger firms, firms with more volatile performance and growth firms. One unexpected result is the negative and significant coefficient on *ROA*.

We test whether gaps are associated with the quality of the match using two different methods in Table 10. In Panel A, we perform a survival analysis using the length of employment at the new firm (*Time-to-Turnover*) as the time to event and define a "failure" event as either forced or voluntary-poor match turnover. In Panel B, we use a multinomial logit with three categories of match quality using a combination of departure reasons and length of employment at the new firm.

Match Quality is considered "bad" if there is a forced turnover or if it is voluntary-poor match turnover and "good" if there is not forced or voluntary-poor match turnover and the *Time-to-Turnover* is longer than three years. Overall, the results from both panels suggest there is no relation between CEO employment gaps and the subsequent quality of the CEO-firm match.

Panel A of Table 10 presents the results using hazard models. Columns 1 and 2 use a competing-risk hazard model that allows us to obtain coefficients separately for forced and voluntary−poor match turnover events. Column 3 uses a Cox proportional semi-parametric hazard model and defines a "failure" event as both forced and voluntary−poor match turnover events. For each column, we estimate the hazard model with and without controls for CEO characteristics, governance, and past performance. We expect the coefficient of *Gap Indicator* to be positive—H2b predicts that CEOs with gaps will have a shorter time-to-turnover (or a higher hazard rate of a failure event).

Similar to the method used for Table 7, we begin by examining whether the proportionality assumption is violated for any predictors using the two separate methods. The tests suggest *% Institutional Ownership* and *Age* violate the proportionality assumption. As such, Panel A shows the results excluding these two variables. Contrary to our prediction in H2b, the results across all iterations consistently indicate that there is no association between the existence of an employment gap and the quality of the subsequent employment CEO-firm employment match.

In Panel B, the multinomial logit model uses three categories representing good matches and bad matches defined as resulting in either forced or voluntary-poor match turnover. In Column 1, the model regresses the match type on an indicator for whether there is a gap prior to employment. Column 2 adds controls for CEO characteristics and Column 3 further adds controls for governance and past firm performance. Consistent with Panel A, but contrary to H2b, we find

no evidence suggesting an employment gap is associated with the subsequent CEO-firm match quality. The only predictor that is consistently associated with future match quality is *Age*, suggesting older executives have a better match quality (i.e., are more likely to have a good match and less likely to have either type of bad match).

5.4 Consequences of CEO gap length – match quality

In this section, we focus on the subset of executives who experience a gap and examine the relation between gap length and the quality of the subsequent CEO-firm match quality. Specifically, we provide tests of our prediction that, for executives who experience a gap, those with longer gaps will have a better match quality (i.e., receive higher compensation (H3a) and have longer time-to-turnover (H3b)).

Table 11 presents the results for tests of H3a. Similar to our approach in testing H2a, we estimate a series of models with $Total\ Compensation_t$ and, alternatively, $Log(Total\ Compensation_t)$ as the dependent variable. With respect to compensation in the first year of the CEO's tenure, the coefficient of *Gap Length* and *Log(Gap Length)* are negative and significant across all models in Panel A (with the exception of the model presented in column (6), where the coefficient is insignificant). This suggests that the longer the employment gap, the lower the compensation the CEO receives in the first year of his/her employment. Panel B repeats the analysis for compensation in the second year. With two exceptions, the coefficients of *Gap Length* and *Log(Gap Length)* are not different from zero. The results for the control variables are generally similar to the results in Table 9. Overall, the results are inconsistent with H3a, which predicts higher compensation with longer gaps.

We next test whether the gap length is associated with match quality (H3b) the same way we tested the relation between the existence of a gap and match quality (H2b) in Section 5.3. In

particular, we test for relation using both hazard models (including a competing-risk hazard model to obtain separate coefficients for forced and voluntary-poor match turnover events and a Cox proportional semi-parametric hazard model where the two turnover events are combined) and a multinomial logit model. We similarly define failure events and match quality as discussed in Section 5.3.

In Panel A of Table 12, we test the prediction that longer gaps have a more successful match (i.e., longer gaps are associated with a lower hazard rate of failure). We begin by examining whether the proportionality assumption is violated for any predictors using the two separate methods (as discussed in Section 5.2). The tests suggest that *Age* violates the assumption. As such, Panel A shows the results excluding *Age*. Contrary to H3b, the results indicate that longer gaps are associated with a higher hazard rate of failure, which is largely driven by forced turnover events (i.e., we find the results when defining a failure event as forced or both, but not for voluntary−poor match alone).

In Panel B, we use a multinomial logit model with three categories representing good matches and bad matches due to either forced or voluntary-poor match reasons. In Column 1, the model regresses the match quality indicator variable on gap length. Column 2 adds controls for CEO characteristics and Column 3 further adds controls for governance and past firm performance. Consistent with Panel A, but contrary to H3b, we find that longer gaps are less likely to be followed by good CEO-firm matches and more likely to be followed by forced turnover (when controls are included in the models). Again, there is no indication that gap length is associated with voluntary poor-match turnover events. The only predictor that is consistently associated with future match quality is *Age*, suggesting older executives have a better match quality (i.e., are more likely to have a good match and less likely to have either type of bad match).

5.5 Discussion of additional results

Table 13 provides information on the activities executives engage in during their employments gaps (see Online Appendix F for details about data collection). We group gap activities into five categories: *Board Membership*, *Consulting*, *Investing*, *Private Firm Employment,* and *Other*. In Panel A we focus on the primary gap activity, i.e. the single predominant activity in which each executive engaged. We are able to determine the primary activity for 719 of the 981 executives with a gap. The most common gap activity is *Board Membership* (28.2% of the sample), followed by *Private Firm Employment* (26.4%). *Consulting* (8.7%) and *Investing* (7.9%) are less common primary gap activities. We have 21 executives (2.1%) whose primary gap activity falls in the *Other* category.

Many executives engage in more than one activity during their employment gap. Panel B reports the number of executives that engage in each activity (for the subset of 719 executives where we were able to identify a primary gap activity). 436 executives served on boards during their gaps while 298 worked for a private firm, with fewer executives engaged in *Consulting* or *Investing* (125 and 121 executives, respectively). For the 719 executives, we were able to identify an average of 1.4 activities per gap.

Table 14 reports information on the number of executives whose prior employer is a competitor of the new employer, defined using two-, three- and four-digit SIC codes. Most executives move to an employer in a different SIC code. Not surprisingly, as we tighten the definition of the industry, the proportion of executives who move to a firm in the same SIC code decreases. For example, 36.1% of the executives move to a firm in the same two-digit SIC code while 20% of the executives move to a firm in the same four-digit SIC code.

To examine the market reaction to the announcements of CEO hires, we calculate cumulative returns for the new firm adjusted for Fama and French (1996) and Carhart (1997) factors over four alternative windows: (i) two-day window starting from the announcement date, (ii) three-day window starting from the announcement date, (iii) the three-day window starting from the day prior to the announcement date and, (iv) the four-day window starting from the day prior to the announcement date.[55] We then estimate the following ordinary least squares regression with robust standard errors:

$$CAR = \alpha_0\, No\ Gap + \alpha_1\, Gap\text{–}Short + \alpha_2\, Gap\text{–}Medium + \alpha_3\, Gap\text{–}Long + \varepsilon \qquad (3)$$

No Gap is an indicator variable that is equal to one if the firm appoints a CEO without an employment gap. *Gap–Short* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is shorter than 222 days (25th percentile of the *Gap Length* distribution). *Gap–Long* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is longer than 1,667 days (75th percentile of the *Gap Length* distribution). *Gap–Medium* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is longer than 222 days and shorter than 1,667 days.

Table 15 reports the results for the 1,579 observations where we are able to obtain the announcement date of the hire (see Online Appendix C). We find a positive and significant market reaction to the appointment of CEOs with no gaps (about 1% across the four models), with short gaps (between 2.3% and 2.6%, depending on the return measurement window) and, for all return windows except for the [-1,1] window with medium gaps (between 0.7% and 1.3%, depending on the return measurement window). The market reaction to the appointment of CEOs with long gaps is not significantly different from zero. Thus, investors view CEO appointments to be value-

[55] Results are qualitatively similar when we use size-adjusted returns.

increasing news, on average, except when the CEO has a long gap prior to employment. Somewhat surprisingly, the market reaction to the appointment of CEOs with short gaps is significantly more positive than the reaction to the appointment of CEOs with no employment gap.

6. Conclusion

In this study we examine the determinants and consequences of employment gaps preceding external CEO hires by public firms. Understanding employment gaps is important for several reasons. First, it is unclear whether gaps harm executives (e.g., through lost wages and deterioration of skills) or benefit them (e.g., by allowing greater investment in the search process leading to a better CEO-firm match). Second, gaps may have implications for firms and the economy as a whole given CEOs are key decision-makers in corporations and can significantly impact economic growth. To the extent they lead to the deterioration of executives' skills or impact the CEO-firm matching process, gaps will affect the allocation of scarce resources in the economy, with potential implications on growth and social welfare.

We first examine the impact of labor market frictions and executive skillsets on the existence and length of gaps. Consistent with our prediction, we find that, when faced with non-compete constraints, executives who are specialists are more likely to endure an employment gap than those who are generalists. This is consistent with non-compete constraints being more binding for specialists, perhaps because they are more constrained to peer firms for future employment. However, while the univariate evidence suggests specialists faced with non-compete constraints endure longer gaps than generalists, the multivariate analyses provide no such support—there is no evidence that generalists and specialists have different gap lengths in the presence of a non-compete.

We then examine the prediction that employment gaps are associated with worse CEO-firm matches, where we measure match quality using compensation (an *ex ante* proxy) and time-to-turnover (and *ex post* measure). Our prediction is based on search-theoretic models of the labor market as well as the theory of employee raids that suggest employed workers have higher reservation wages than unemployed workers and the best workers are more likely to be raided, resulting in job transitions without an employment gap. While our results are consistent with our prediction that CEOs hired with an employment gap have lower compensation in both univariate and multivariate analyses, there is only univariate evidence suggesting gaps precede worse CEO-firm matches based on time-to-turnover. The multivariate evidence based on both hazard and multinomial logit models provide no evidence of an association.

Our final set of analyses examines the prediction that longer employment gaps are associated with better CEO-firm matches, using the same measures of match quality. Our prediction is derived from job search models suggesting that an unemployed worker's reservation wage is negatively associated with his discount rate combined with our expectation that CEOs can be patient (have low discount rates) because they have sufficient resources to wait for a better CEO-firm match. However, our evidence consistently suggests the opposite—longer gaps are associated with lower compensation, higher hazard rates of failure events where failure is defined as turnover resulting from a bad match, lower probability of good matches (time-to-turnover > 3 years), and higher probability of bad match quality (i.e., forced or voluntary-poor match turnover).

Interestingly, executive age is significant throughout many of the tests. The results suggest that older executives are more likely to have employment gaps and the gaps are longer. However, given a gap occurs, they are more likely to have a good subsequent CEO-firm match (i.e., more

likely to have longer time-to-turnover and less likely to have a forced or voluntary-poor match turnover event).

Overall, our results suggest that the consequences of executive gaps are similar in spirit to those of rank-and-file employees. Consistent with the theory of employee raids, the existence of a gap suggests a worse *ex post* employment outcome while, contrary to search-theoretic models, longer gaps do not lead to more successful executive-firm matches.

Our paper contributes to the literature on executive labor markets, and more generally, to the literature on contemporaneous unemployment spells. We complement studies in labor economics, which typically focus on rank-and-file employees. We leave it to future research to explore why, in spite of the differences in the underlying labor markets, the consequences of longer gaps for executives seem to be similar in spirit to those for rank-and-file employees. Future research can also draw on our novel and rich dataset on executive transitions to address other important questions. For example, how do gaps and executives' activities during gaps affect the future productivity of the hiring firms (e.g., innovation and performance)? What is the impact on the overall economy of leaving top talent out of the labor market for extended periods? Finally future studies can adopt a firm-centric focus by examining searches for a new CEO and trying to identify how firms choose from a list of likely candidates, taking into consideration the role the existence and duration of gaps play in these decisions.

Appendix A: Variable Definitions

Dependent Variables

Gap Indicator	= an indicator variable equal to one if the executive has a gap between the last day of employment at the prior firm and first day of employment at the current firm, zero otherwise. We define a gap as at least 30 days between the last day of employment at the prior firm and the first day of employment at the current firm (source: hand-collect).
Gap Length	= the days between the last day of employment at the prior firm and the first day of employment at the new firm (source: hand-collect).
Total Compensation	= the sum of salary, bonus, other annual total value of restricted stock granted, total value of stock option granted (using Black-Scholes), long-term incentive payouts, and all other total compensation. We measure *Total Compensation* alternatively in the first and second year of employment as CEO in the current firm (source: TDC1 variable in Execucomp).
Time-to-Turnover	= the days that the executive spends employed as the CEO of the new firm. (source: hand-collect).

Independent Variables

Generalist	= an indicator variable equal to one if the executive has a general skillset per Custódio, Ferreira, and Matos (2013), and zero otherwise. Section 4 and Appendix G provide further details about this variable (source: BoardEx).
NCA_Signed	= an indicator variable equal to one if the executive is likely to have signed a noncompete, zero otherwise (source: hand-collect).
NCC	= the measure of noncompete enforceability (based on Garmaise [2011]) when an executive is likely to have signed a noncompete, zero otherwise (including when the executive comes from a foreign firm) (source: hand-collect).
NCC_Generalist	= the measure of noncompete enforceability (based on Garmaise [2011]) when a generalist executive (*Generalist*=1) is likely to have signed a noncompete, zero otherwise. Section 4 and Appendix G provide further details on the construction of this variable (source: hand-collect).

NCC_Specialist	= the measure of noncompete enforceability (based on Garmaise [2011]) when a specialist executive (*Generalist*=0) likely to have signed a noncompete, and zero otherwise. Section 4 and Appendix G provide further details about the construction of this variable (source: hand-collect).
Age	= the age of the executive. In tests of H1a and H1b, we measure *Age* at the beginning of the executive's gap. In tests of H2 and H3, we measure *Age* at the time the executive joins the firm as a CEO (source: Execucomp and BoardEx; hand-collect when not available on either).
Age>60	= an indicator variable equal to one if *Age* is greater than 60, zero otherwise (source: Execucomp and BoardEx; hand-collect when not available on either).
Female	= an indicator variable equal to one if the executive is a female, zero otherwise (source: Execucomp and BoardEx; hand-collect when not available on either).
Previous Position–CEO	= an indicator variable equal to one if the executive was the CEO at their prior firm, zero otherwise (source: Execucomp and BoardEx; hand-collect when not available on either).
Abnormal Returns–Prior Firm	= abnormal returns of the prior employer. Following Bhojraj, Hribar, Picconi and McInnis (2009), we define abnormal returns as the size- and book-to-market adjusted buy-and-hold returns over a 12-month performance period preceding the departure of the employee. As a robustness, following Fee and Hadlock (2003), we will use a 60-month period to measure buy-and-hold returns (source: CRSP and Compustat).
Abnormal Returns–Current Firm	= abnormal returns of the current employer. Following Bhojraj et al. (2009), we define abnormal returns as the size- and book-to-market adjusted buy-and-hold returns over the relevant fiscal year (source: CRSP and Compustat).
Past Abnormal Returns–Current Firm	= abnormal returns of the current employer prior to the CEO's tenure. Following Bhojraj et al. (2009), we define abnormal returns as the size- and book-to-market adjusted buy-and-hold returns over a 12-month performance measurement period preceding the executive joining the firm as the CEO (source: CRSP and Compustat).
CEO-Chair Duality	= an indicator variable equal to one if the CEO is also the Chair of the Board of Directors in the CEO's first year of

	employment, zero otherwise (source: ISS Directors Database).
Board Size	= number of individuals on the Board of Directors in the CEO's first year of employment (source: ISS Directors Database).
% Outside Directors	= percent of independent directors on the Board of Directors in the CEO's first year of employment (source: ISS Directors Database).
Blockholder Indicator	= an indicator variable equal to one if the firm has at least one institutional investor with at least 5% ownership in the CEO's first year of employment (source: Thomson Reuters).
% Institutional Ownership	= the percent of equity owned by institutions based on 13-F filings in the CEO's first year of employment (source: Thomson Reuters).
Log(*Sales*)	= natural logarithm of the firm's sales (Compustat item *sale*) in the relevant fiscal year (source: Compustat).
Book-to-Market	= book value of assets (Compustat item *at*) divided by by the sum of book value of liabilities (Compustat item *at* less Compustat item *ceq*) and market value of equity (Compustat item *prcc_f* multiplied by Compustat item *csho*) in the relevant fiscal year (source: Compustat).
Return on Assets	= operating income before depreciation (Compustat data item o*ibdp*) scaled by average total assets in the relevant fiscal year (source: Compustat).
SD(ROA)	= standard deviation of *Return on Assets* over the five fiscal years preceding the CEO's first year of employment (source: Compsutat).
SD(Returns)	= standard deviation of monthly returns of the firm over the five fiscal years preceding the CEO's first year of employment (source: CRSP).

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Table 1. Succession Type and Gap Length for Jamie Dimon, James Kilts, Marissa Mayer, and Margaret Whitman (Hand-collected)

	Transition #1	Transition #2	Transition #3
Jamie Dimon			
Company Name	BANK ONE CORP	JPMORGAN CHASE & CO	
Prior Company Name	CITIGROUP INC	BANK ONE CORP	
Became CEO	3/27/00	12/31/05	
Joined Company	3/27/00	7/1/04	
Left Prior Company	11/2/98	6/30/04	
Succession Type	external	external	
Gap Indicator	1	0	
Gap Length (days)	511	0	
James M. Kilts			
Company Name	NABISCO HOLDINGS CORP -CL A	NABISCO GROUP HOLDINGS CORP	GILLETTE CO
Prior Company Name	ALTRIA GROUP INC	NABISCO HOLDINGS CORP -CL A	NABISCO GROUP HOLDINGS CORP
Became CEO	1/1/98	(set to missing)	2/12/01
Joined Company	1/1/98	(set to missing)	2/12/01
Left Prior Company	3/24/97	(set to missing)	12/11/00
Succession Type	external	(set to missing)	external
Gap Indicator	1	(set to missing)	1
Gap Length (days)	283	(set to missing)	63
Marissa A. Mayer			
Company Name	YAHOO INC		
Prior Company Name	GOOGLE INC		
Became CEO	7/17/12		
Joined Company	7/17/12		
Left Prior Company	7/16/12		
Succession Type	external		
Gap Indicator	0		
Gap Length (days)	0		
Margaret C. Whitman			
Company Name	EBAY INC (PRIVATE)	HEWLETT-PACKARD CO	
Prior Company Name	STRIDE RITE	EBAY INC (PUBLIC)	
Became CEO	(set to missing)	9/22/11	
Joined Company	(set to missing)	9/22/11	
Left Prior Company	(set to missing)	3/31/08	
Succession Type	(set to missing)	external	
Gap Indicator	(set to missing)	1	
Gap Length (days)	(set to missing)	1,270	

Table 2. Firm descriptives

This table reports firm characteristics for the companies in our sample. Variable definitions are provided in Appendix A. Column 1 shows information about the prior (old) employer at the date a CEO left that firm. Column 2 shows information about the subsequent (new) employer at the date a CEO arrived at that firm. Column 3 shows information about all S&P 1500 firms pooled across all sample years from 1992-2014. Column 4 reports the p-values for the differences between columns 1 and 2. Column 5 reports the p-values for the differences between columns 2 and 3. P-values are from tests of differences in means (medians) between samples from a two-sided t-test (Wilcoxon rank-sum test). For binary variables, p-values are from a χ^2 test.

	Old Employers at Time of Departure (1)			**New Employers at Time of Arrival (2)**			**Pooled S&P 1500 (3)**			**Test of Diff. (p-value) (4) = (1) – (2)**		**(5) = (2) – (3)**	
Firm Characteristics	**N**	**Mean**	**Median**	**N**	**Mean**	**Median**	**N**	**Mean**	**Median**	**Mean**	**Median**	**Mean**	**Median**
Assets	1,408	38,316	4,741	1,565	5,663	1,004	31,601	10,847	1,916	< 0.01	< 0.01	< 0.01	< 0.01
Sales	1,408	15,399	4,168	1,565	3,369	907	31,601	5,300	1,456	< 0.01	< 0.01	< 0.01	< 0.01
Book-to-Market	1,408	0.666	0.662	1,565	0.706	0.719	31,601	0.666	0.676	< 0.01	< 0.01	< 0.01	< 0.01
ROA	1,408	0.125	0.128	1,565	0.089	0.097	31,601	0.140	0.133	< 0.01	< 0.01	< 0.01	< 0.01
SD(ROA)	1,408	0.043	0.026	1,565	0.057	0.038	31,601	0.038	0.025	< 0.01	< 0.01	< 0.01	< 0.01
Abnormal Returns (FYR)	1,408	-3.042	-5.098	1,565	-5.592	-13.130	31,601	0.726	-2.928	0.17	< 0.01	< 0.01	< 0.01
SD(Returns)	1,408	0.120	0.103	1,565	0.147	0.132	31,601	0.112	0.102	< 0.01	< 0.01	< 0.01	< 0.01
CEO-Chair Duality	918	0.643	1.000	1,302	0.351	0.000	27,050	0.589	1.000		< 0.01		< 0.01
Board Size	918	10.497	10.000	1,302	8.812	9.000	27,050	9.493	9.000	< 0.01	< 0.01	< 0.01	< 0.01
% Outside Directors	918	0.719	0.750	1,302	0.737	0.774	27,050	0.716	0.750	< 0.01	< 0.01	< 0.01	< 0.01
Blockholder Indicator	918	0.755	1.000	1,302	0.889	1.000	27,050	0.880	1.000		< 0.01		0.74
% Institutional Ownership	918	0.803	0.682	1,302	0.685	0.712	27,050	0.715	0.732	0.08	< 0.01	< 0.01	< 0.01

Table 3. Sample descriptives

This table reports descriptive statistics for the employment gap observations in our sample (i.e. there can be more than one observation per firm if there was more than one new CEO during the window). Panel A reports information on the employment gaps experienced by incoming (newly hired) CEOs in our sample. Panel B reports information about the incoming CEOs at the time of hire. Panel C reports compensation and turnover details surrounding incoming CEOs. Panel D reports firm characteristics for the hiring firm at the time of hire. *Time-to-Turnover* is shown for the full sample and the sample bifurcated by whether the turnover occurred due to a bad match (i.e., *Failure;* forced turnover or voluntary-bad match) or not (*Non-Failure)*. Panel E reports characteristics of the incoming CEOs' most recent prior firm at the time of departure. Variable definitions are provided in Appendix A.

Panel A: Gap Characteristics

	N	25th	Median	75th	Mean	Std. Dev.
Gap Indicator	1,538	0	1	1	0.575	0.495
Gap Length	1,538	1	92	866	676	1,140

Panel B: CEO Characteristics

	N	25th	Median	75th	Mean	Std. Dev.
Generalist	1,212	0	1	1	0.505	0.500
Age	1,538	48	52	57	52.78	6.844
Age > 60	1,538	0	0	0	0.122	0.327
Female	1,538	0	0	0	0.034	0.182
Previous Position-CEO	1,538	0	0	0	0.193	0.395

Panel C: New Employment Characteristics

	N	25th	Median	75th	Mean	Std. Dev.
Total Compensation$_t$	1,504	1,196	3,060	7,055	5,782	7,714
Total Compensation$_{t+1}$	1,275	1,017	2,126	4,672	3,588	3,918
Time-to-Turnover	1,526	755	1,492	2,439	1,746	1,278
Time-to-Turnover (Failure)	469	947	1,176	1,894	1,377	952
Time-to-Turnover (Non-Failure)	1,057	876	1,656	2,792	1,909	1,367

Panel D: New Firm Characteristics

	N	25th	Median	75th	Mean	Std. Dev.
Sales	1,538	292	926	2,723	3,451	7,415
Book-to-Market	1,538	0.496	0.712	0.913	0.702	0.270
ROA	1,538	0.033	0.097	0.158	0.090	0.132
SD(ROA)	1,538	0.019	0.037	0.073	0.056	0.057
Abnormal Returns	1,538	-38.586	-12.905	15.355	-5.801	55.560
SD(Returns)	1,538	0.094	0.129	0.176	0.143	0.068
CEO-Chair Duality	1,261	0	0	1	0.351	0.478
Board Size	1,261	7	9	10	8.800	2.279
% Outside Directors	1,261	0.667	0.778	0.857	0.741	0.153
Blockholder Indicator	1,538	1	1	1	0.860	0.348
% Institutional Ownership	1,538	0.478	0.693	0.863	0.697	0.345

Panel E: Old Firm Characteristics

	N	25th	Median	75th	Mean	Std. Dev.
Abnormal Returns-Prior Firm	1,538	-22.573	-2.042	17.465	0.694	42.209
NCA_Signed	1,514	1	1	1	0.845	0.362
NCC	1,514	0	3	5	2.963	2.383
NCC (if NCA_Signed = 1)	1,280	3	4	5	3.505	2.195

Table 4. Noncompete state enforceability index

This table reports the scores for the noncompetition enforceability index developed by Garmaise (2011) in our extended time period from 1980 to 2013. Scores from 1992 to 2004 were obtained from Garmaise (2011). Higher scores indicate stricter enforcement of noncompete agreements while lower scores indicate lower enforcement levels. The process used to extend the index is explained in detail in Online Appendix E.

State (Years)	Score	State (Years)	Score
Alabama (1980 – 2013)	5	Minnesota (1980 – 2013)	5
Alaska (1980 – 2013)	3	Mississippi (1980 – 2008)	4
Arizona (1980 – 2013)	3	Mississippi (2009 – 2013)	5
Arkansas (1980 – 2013)	5	Missouri (1980 – 2013)	7
California (1980 – 2013)	0	Montana (1980 – 2013)	2
Colorado (1980 – 1991)	1	Nebraska (1980 – 2013)	4
Colorado (1992 – 2011)	2	Nevada (1980 – 2013)	5
Colorado (2012 – 2013)	3	New Hampshire (1980 – 2013)	2
Connecticut (1980 – 1991)	4	New Jersey (1980 – 2013)	4
Connecticut (1992 – 2013)	3	New Mexico (1980 – 2013)	2
D.C. (1980 – 2013)	7	New York (1980 – 2013)	3
Delaware (1980 – 2013)	6	North Carolina (1980 – 2013)	4
Florida (1980 – 1996)	7	North Dakota (1980 – 2013)	0
Florida (1997 – 2013)	9	Ohio (1980 – 1991)	4
Georgia (1980 – 2004)	5	Ohio (1992 – 2013)	5
Georgia (2005 – 2013)	6	Oklahoma (1980 – 2013)	1
Hawaii (1980 – 2006)	3	Oregon (1980 – 2013)	6
Hawaii (2007 – 2013)	4	Pennsylvania (1980 – 2013)	6
Idaho (1980 – 1991)	5	Rhode Island (1980 – 2013)	3
Idaho (1992 – 2008)	6	South Carolina (1980 – 2013)	5
Idaho (2009 – 2013)	7	South Dakota (1980 – 2013)	5
Illinois (1980 – 2013)	5	Tennessee (1980 – 2013)	7
Indiana (1980 – 2013)	5	Texas (1980 – 1994)	5
Iowa (1980 – 2013)	6	Texas (1995 – 2013)	3
Kansas (1980 – 2007)	6	Utah (1980 – 2013)	6
Kansas (2008 – 2013)	7	Vermont (1980 – 2013)	5
Kentucky (1980 – 2013)	6	Virginia (1980 – 1991)	4
Louisiana (1980 – 1991)	2	Virginia (1992 – 2005)	3
Louisiana (1992 – 2001)	4	Virginia (2006 – 2013)	4
Louisiana (2002 – 2003)	0	Washington (1980 – 2013)	5
Louisiana (2004 – 2013)	4	West Virginia (1980 – 1983)	2
Maine (1980 – 2013)	4	West Virginia (1984 – 1991)	3
Maryland (1980 – 2013)	5	West Virginia (1992 – 2013)	2
Massachusetts (1980 – 2013)	6	Wisconsin (1980 – 2013)	3
Michigan (1980 – 2013)	5	Wyoming (1980 – 2013)	4

Table 5. Joint distribution of noncompete constraints and CEO skillset

This table reports information about employment gaps for generalist and specialist CEOs at firms that do and do not require the signing of non-compete agreements and in states with varying levels of enforcement. CEOs are generalists if the executive has a general skillset following Custódio, Ferreira, and Matos [2013] and specialists otherwise. *NCA_Signed* is an indicator variable equal to one if the executive is likely to have signed a noncompete agreement and zero otherwise. The *NCC* variable ranges from zero to nine for firms with *NCA_Signed* = 1, where zero indicates no enforcement of signed noncompetes and nine indicates the highest level of enforcement. Panel A reports the number of observations in our sample. Panel B reports the number (percent) of observations in our sample with employment gaps and the p-value for the difference in percentage of observations with employment gaps. Panels C and D report the mean and median employment gaps for CEOs with a gap in our sample with p-values from t-tests and Wilcoxon rank-sum tests, respectively.

Panel A: Number of Observations

	NCA_Signed = 0	*NCA_Signed* = 1			
		NCC = 0	*NCC* = 1 – 4	*NCC* = 5 – 9	Total
Generalist	63	128	202	234	627
Specialist	110	102	211	201	624
Total	173	230	413	435	1,251

Panel B: Number (percent) of Observations with Gap

	NCA_Signed = 0	*NCA_Signed* = 1			
		NCC = 0	*NCC* = 1 – 4	*NCC* = 5 – 9	Total
Generalist	33 (52.38%)	86 (67.19%)	117 (57.92%)	124 (52.99%)	360 (57.42%)
Specialist	70 (63.64%)	58 (56.86%)	129 (61.14%)	121 (60.20%)	378 (60.58%)
Total	103 (59.54%)	144 (62.61%)	246 (59.56%)	245 (56.32%)	738 (58.99%)
χ^2 Test of column and row independence (p-value)					< 0.01

Panel C: Mean Gap Length (days)

	NCA_Signed = 0	*NCA_Signed* = 1			
		NCC = 0	*NCC* = 1 – 4	*NCC* = 5 – 9	
Generalist	1,296	1,142	919	969	
Specialist	1,448	1,067	1,595	1,253	
Test of diff. (p-value)	0.63	0.74	< 0.01	0.06	

Panel D: Median Gap Length (days)

	NCA_Signed = 0	*NCA_Signed* = 1			
		NCC = 0	*NCC* = 1 – 4	*NCC* = 5 – 9	
Generalist	1,022	539	503	614	
Specialist	694	647	1,248	701	
Test of diff. (p-value)	0.66	0.78	< 0.01	0.31	

Table 6. Test for H1A

This table reports the results of our logit regressions examining the likelihood of CEOs experiencing an employment gap. The dependent variable *Gap Indicator* equals one if the executive experiences an employment gap. α_2 is the coefficient of *NCC_Generalist* and α_3 is the coefficient of *NCC_Specialist*. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

		Dependent Variable*: Gap Indicator*			
		Without Controls (1)	CEO Controls (2)	Prior Position Controls (3)	All Controls (4)
Proxies for Skillset and NCC					
Generalist	-	0.146	0.061	0.009	-0.011
NCC_Generalist	+	-0.084**	-0.112***	-0.085**	-0.112***
NCC_Specialist	+	-0.002	-0.012	-0.002	-0.012
CEO Controls					
Age			0.088***		0.086***
Age > 60			0.650**		0.584*
Female			0.365		0.370
Prior Position Controls					
Prior Position-CEO				0.795***	0.435**
Abnormal Returns-Prior Firm				-0.002	-0.002
*Prior Position-CEO*Abnormal Returns-Prior Firm*				0.001	0.002
χ^2 test of $\alpha_2 = \alpha_3$ (p-value)		0.11	0.06	0.11	0.07
Year-Quarter Fixed Effects		Yes	Yes	Yes	Yes
N		1,209	1,209	1,209	1,209
Pseudo R^2		0.5%	9.4%	2.1%	9.9%

Table 7. Test for H1B

This table reports the coefficients for the Cox proportional semi-parametric hazard model testing the prediction that NCCs are less binding for generalists than for specialists. The dependent variable *Gap Length* equals the length of the employment gap (in days). α_2 is the coefficient of *NCC_Generalist* and α_3 is the coefficient of *NCC_Specialist*. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

		Analysis Time: *Gap Length;* Failure Event = Exit Gap			
		Without Controls (1)	CEO Controls (2)	Prior Position Controls (3)	All Controls (4)
Proxies for Skillset and NCC					
Generalist	+	0.226*	0.214*	0.232*	0.172
NCC_Generalist	-	0.031	0.039*	0.032	0.039*
NCC_Specialist	-	0.012	0.005	0.013	0.004
CEO Controls					
Age			-0.036***		-0.037***
Age > 60			-0.312***		-0.343**
Female			-0.221		-0.240
Prior Position Controls					
Prior Position-CEO				-0.049	0.207**
Abnormal Returns-Prior Firm				-0.000	0.000
*Prior Position-CEO*Abnormal Returns – Prior Firm*				0.002	0.000
χ^2 test of $\alpha_2 = \alpha_3$ (p-value)		0.33	0.21	0.30	0.23
Year-Quarter Fixed Effects		Yes	Yes	Yes	Yes
N		738	738	738	738
Log-Likelihood Ratio		-4,133	-4,085	-4,132	-4,083

Table 8. Statistics for H2 & H3

Panel A reports information about our sample of CEOs partitioned by whether they experienced an employment gap. Column 3 provides p-values from tests of differences between the gap and no gap samples. P-values are from tests of differences in means (medians) between samples from a two-sided t-test (Wilcoxon rank-sum test). For binary variables, p-values are from a χ^2 test. Panel B reports correlations between key independent and dependent variables. Numbers above (below) the diagonal are Spearman (Pearson) correlations. Correlations significant at a 5% level are in bold. Panel C reports the distribution of CEOs with and without gaps, gap length (in days), and CEO-firm fit type. Variable definitions are provided in Appendix A.

Panel A: Univariate Statistics	**Gap (1)**			**No Gap (2)**			**Test of Diff. (p-value) (3)**	
	N	**Mean**	**Median**	**N**	**Mean**	**Median**	**Mean**	**Median**
Compensation & Turnover								
Total Compensation$_t$	866	4,658	2,498	638	7,308	4,046	< 0.01	< 0.01
Total Compensation$_{t+1}$	712	3,249	1,966	563	4,017	2,392	< 0.01	< 0.01
Time-to-Turnover	876	1,539	1,230	650	2,025	1,745	< 0.01	< 0.01
Time-to-Turnover (Failure)	235	1,269	1,025	234	1,485	1,289	0.01	< 0.01
Time-to-Turnover (Non-Failure)	641	1,637	1,324	416	2,328	2,030	< 0.01	< 0.01
Gap Length	884	1,174	670					
Governance Characteristics								
CEO-Chair Duality	754	0.321	0.000	507	0.396	0.000		< 0.01
Board Size	754	8.592	8.000	507	9.110	9.000	< 0.01	< 0.01
% Outside Directors	754	0.745	0.778	507	0.734	0.778	0.21	0.27
Blockholder Indicator	884	0.868	1.000	654	0.849	1.000		0.29
% Institutional Ownership	884	0.712	0.706	654	0.676	0.682	0.04	0.05
Past Performance								
Abnormal Returns-Prior Firm	884	0.641	-2.042	654	0.765	-2.075	0.95	0.83
CEO Characteristics								
Age	884	54.336	54.000	654	50.645	51.000	< 0.01	< 0.01
Age > 60	884	0.181	0.000	654	0.041	0.000		< 0.01
Female	884	0.040	0.000	654	0.028	0.000		0.20
Firm Characteristics								
Log(Sales)	884	6.652	6.604	654	7.033	7.003	< 0.01	< 0.01
Book-to-Market	884	0.715	0.724	654	0.686	0.695	0.04	0.05
ROA	884	0.081	0.088	654	0.102	0.109	< 0.01	< 0.01
Abnormal Returns	884	-7.302	-15.942	654	-3.772	-8.270	0.22	< 0.01
SD(ROA)	884	0.059	0.040	654	0.052	0.036	0.02	< 0.01
SD(Returns)	884	0.150	0.137	654	0.134	0.117	< 0.01	< 0.01

Table 8. Statistics for H2 & H2 (continued)

Panel B: Correlations: Spearman above (Pearson below)

	(1)	(2)	(3)	(4)
1. *Gap Length*	1.000	**-0.244**	**-0.257**	**-0.134**
2. *Time-to-Turnover*	**-0.232**	1.000	**0.205**	**0.220**
3. *Total Compensation*$_t$	**-0.194**	**0.072**	1.000	**0.561**
4. *Total Compensation*$_{t+1}$	**-0.091**	**0.065**	**0.440**	1.000

Panel C: Fit Type

	N	**Gap**	**No Gap**	**Gap Length (Days) Mean (Median)**
Good	722	382 (52.91%)	340 (47.09%)	921 (456)
Bad-Forced	292	146 (50.00%)	146 (50.00%)	1,053 (644)
Bad-Poor Match	177	89 (50.28%)	88 (49.72%)	984 (470)

Table 9. Test for H2A

This table reports the results of our regressions of whether CEOs with prior employment gaps have lower compensation. The dependent variable is total compensation (in millions) or log of total compensation. Panel A uses current year (t) compensation as the dependent variable and Panel B uses subsequent year (t+1) compensation as the dependent variable. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

Panel A: Compensation in Period t	Dependent Variable:											
	Total Compensation$_t$						*Log(Total Compensation*$_t$)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Gap Indicator -	-2.563***	-2.848***	-2.848***	-2.351***	-2.005***	-1.775***	-0.497***	-0.632***	-0.630***	-0.503***	-0.389***	-0.398***
Governance												
CEO-Chair Duality		0.984*	0.985*	1.069**		0.036		0.141	0.140	0.157*		-0.021
Board Size		0.724***	0.724***	0.771***		0.057		0.097***	0.096***	0.107***		-0.012
% Outside Directors		-4.513***	-4.513***	-3.648**		-2.320		-0.878***	-0.869***	-0.630**		-0.371
Blockholder Indicator		-1.306	-1.306	-1.125		0.977		-0.082	-0.082	-0.040		0.324**
% Institutional Ownership		1.977**	1.977**	2.231***		-0.194		0.442***	0.442***	0.516***		0.059
Past Performance												
Abnormal Returns-Prior Firm			-0.000	0.000		-0.004			0.001	0.001		-0.000
CEO												
Age				-0.106**		-0.124***				-0.020**		-0.022***
Age > 60				-1.192		-1.268				-0.489***		-0.504***
Female				1.230		0.365				0.014		-0.115
Firm												
Log(Sales)					2.567***	2.853***					0.437***	0.484***
Book-to-Market					-6.200***	-6.592***					-1.471***	-1.434***
ROA					-8.186***	-10.155***					-0.933***	-1.469***
Abnormal Returns					0.007**	0.005					0.001	0.000
SD(ROA)					7.259*	6.398					0.620	0.147
SD(Returns)					12.659***	12.653***					2.899***	2.630***
Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,504	1,240	1,240	1,240	1,504	1,204	1,504	1,240	1,240	1,240	1,504	1,204
R^2	2.9%	8.9%	8.9%	10.7%	28.3%	31.9%	3.6%	9.1%	9.1%	13.3%	27.3%	33.4%

Table 9. Test for H2A (continued)

Panel B: Compensation in Period t+1	Dependent Variable*:*											
	Total Compensation$_{t+1}$						*Log(Total Compensation*$_{t+1}$)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Gap Indicator -	-0.578**	-0.440*	-0.438*	-0.365	-0.189	0.032	-0.190***	-0.166**	-0.166**	-0.117*	-0.076	-0.019
Governance												
CEO-Chair Duality		1.170***	1.158***	1.209***		0.351		0.288***	0.287***	0.303***		0.102
Board Size		0.532***	0.529***	0.541***		0.098*		0.165***	0.165***	0.170***		0.058***
% Outside Directors		-1.739*	-1.665*	-1.636*		-1.514*		-0.264	-0.263	-0.198		-0.156
Blockholder Indicator		-0.545	-0.534	-0.525		0.469		-0.177	-0.177	-0.174		0.062
% Institutional Ownership		1.597***	1.586***	1.608***		0.163		0.623***	0.623***	0.633***		0.217*
Past Performance												
Abnormal Returns-Prior Firm			0.003	0.003		0.002			0.000	0.000		-0.000
CEO												
Age				-0.029		-0.038*				-0.008		-0.009
Age > 60				0.194		-0.050				-0.234*		-0.308**
Female				0.663		-0.055				0.263		0.097
Firm												
Log(Sales)					1.400***	1.472***					0.372***	0.363***
Book-to-Market					-2.578***	-3.081***					-0.722***	-0.776***
ROA					-3.472***	-4.601***					-0.319	-0.497
Abnormal Returns					0.000	-0.001					0.000	-0.000
SD(ROA)					3.865*	5.204**					0.335	0.589
SD(Returns)					0.543	0.001					0.294	0.730
Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,285	1,114	1,114	1,114	1,285	1,114	1,285	1,114	1,114	1,114	1,285	1,114
R^2	1.0%	11.1%	11.2%	11.4%	33.0%	34.7%	1.3%	13.9%	13.9%	15.3%	30.8%	33.9%

Table 10. Test for H2B

This table reports the results of our tests examining whether employment gaps are associated with future CEO turnover. Panel A reports the coefficients from hazard models for the sample of executives with completed tenure at hiring firms. Columns (1) and (2) use a competing-risks hazard model to obtain the coefficients when a failure event is defined as forced turnover (Column 1) or voluntary-poor match turnover (Column 2), while controlling for the other failure category. Column 3 uses a Cox proportional semiparametric hazard model and measures a failure event as both forced and voluntary−poor match turnovers. All three columns show results both with and without control variables. Panel B reports the results from a multinomial logit model for the propensity of experiencing good and bad employment matches. Following Allgood and Farrell (2003), a match is classified as bad if the employment ends due to forced or voluntary-poor match turnover. We classify a match as good if it is not a bad match (as defined above) and tenure is greater than 3 years. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

Panel A: Cox Hazard Model

		Analysis Time: *Time-to-Turnover*					
		(1) Failure Event: Forced turnover		(2) Failure Event: Voluntary-poor match turnover		(3) Failure event: Both	
		No Controls	All Controls	No Controls	All Controls	No Controls	All Controls
Gap Indicator	+	-0.022	-0.057	-0.034	0.047	-0.008	-0.003
CEO							
Age > 60			-1.142**		-0.564		-0.910**
Female			0.019		-0.071		-0.003
Governance							
CEO-Chair Duality			-0.210		-0.237		-0.259**
Board Size			-0.022		-0.064*		-0.044*
% Outside Directors			0.372		-1.516***		-0.449
Blockholder Indicator			0.043		-0.246		-0.077
Past Performance							
Past Abnormal Returns-Current Firm			-0.000		-0.003		-0.002
Year-Quarter Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes
N		1,526	1,256	1,526	1,256	1,526	1,256
Log-Likelihood Ratio		-1,958	-1,535	-1,207	-946	-3,107	-2,437

Table 10. Test for H2B (continued)

Panel B: Multinomial Logit Model	Dependent Variable*: Match Quality*								
	(1) No Controls			(2) With CEO Controls			(3) With All Controls		
	Good	Bad Forced	Bad Voluntary	Good	Bad Forced	Bad Voluntary	Good	Bad Forced	Bad Voluntary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gap Indicator	0.027	-0.018	-0.009	-0.009	0.005	0.005	-0.007	-0.001	-0.008
CEO									
Age				0.017***	-0.010***	-0.006***	0.015***	-0.012***	-0.004*
Age > 60				0.087	-0.076	-0.011	0.063	-0.069	0.006
Female				0.023	0.002	-0.025	0.024	-0.014	-0.010
Governance									
CEO-Chair Duality							0.049	-0.023	-0.025
Board Size							0.012*	-0.003	-0.009*
% Outside Directors							0.193*	0.023	-0.216***
Blockholder Indicator							0.055	-0.028	-0.027
% Institutional Ownership							-0.039	0.070	-0.031
Past Performance									
Past Abnormal Returns-Current Firm							0.001	-0.000	-0.000*
Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	722	292	177	722	292	177	583	237	145
Pseudo R^2		0.0%			2.2%			3.6%	

Table 11. Test for H3A

This table reports the results of our regressions of whether, conditional on experiencing an employment gap, the length of a CEO's employment gap is associated with compensation. The dependent variable is total compensation or log of total compensation. Panel A uses current year (t) compensation as the dependent variable and panel B uses subsequent year (t+1) compensation as the dependent variable. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

Panel A: Compensation in Period t		Dependent Variable:											
		Total Compensation$_t$						*Log(Total Compensation*$_t$*)*					
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Gap Length	+	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.000						
Log(Gap Length)	+							-0.285***	-0.258***	-0.258***	-0.205***	-0.233***	-0.139***
Governance													
CEO-Chair Duality			0.849	0.857	0.869		0.111		0.071	0.071	0.077		-0.100
Board Size			0.678***	0.685***	0.712***		0.248*		0.107***	0.107***	0.118***		0.019
% Outside Directors			-5.637***	-5.699***	-5.348**		-4.148**		-1.264***	-1.263***	-1.088**		-0.853**
Blockholder Indicator			0.023	0.029	0.096		1.453		0.032	0.032	0.067		0.329*
% Institutional Ownership			0.825	0.822	0.969		-1.079		0.359*	0.359*	0.441**		-0.029
Past Performance													
Abnormal Returns-Prior Firm				-0.006	-0.007		-0.009			0.000	-0.000		-0.001
CEO													
Age					0.006		-0.008				-0.011		-0.015
Age > 60					-2.077**		-2.260**				-0.536**		-0.564***
Female					-0.005		-0.175				-0.229		-0.277
Firm													
Log(Sales)						2.074***	2.152***					0.381***	0.441***
Book-to-Market						-4.715***	-5.253***					-1.179***	-1.301***
ROA						-5.962***	-6.504***					-0.565	-0.994*
Abnormal Returns						0.007	0.008*					0.001	0.001
SD(ROA)						9.476**	8.106*					0.421	-0.080
SD(Returns)						6.433*	9.081**					2.443***	2.444***
Year-Quarter Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N		866	743	743	743	866	743	866	743	743	743	866	743
R^2		3.6%	7.8%	7.9%	8.8%	22.3%	24.8%	7.4%	9.4%	9.4%	12.4%	23.1%	24.2%

Table 11. Test for H3A (continued)

Panel B: Compensation in Period t+1		Dependent Variable:											
		Total Compensation$_{t+1}$						*Log(Total Compensation*$_{t+1}$)					
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Gap Length	+	-0.000	-0.000	-0.000	0.000	-0.000	0.000						
Log(Gap Length)	+							-0.097***	-0.061	-0.060	-0.035	-0.069**	-0.027
Governance													
CEO-Chair Duality			0.927***	0.933***	0.976***		0.352		0.160	0.165	0.198*		0.046
Board Size			0.494***	0.497***	0.501***		0.092		0.167***	0.170***	0.174***		0.058**
% Outside Directors			-1.369	-1.424	-1.271		-0.588		-0.122	-0.171	-0.021		0.159
Blockholder Indicator			-1.207**	-1.207**	-1.215**		0.186		-0.292	-0.292	-0.300		0.118
% Institutional Ownership			2.052***	2.055***	2.086***		0.650		0.655***	0.658***	0.658***		0.218
Past Performance													
Abnormal Returns-Prior Firm				-0.001	-0.001		-0.001			-0.001	-0.001		-0.001
CEO													
Age					0.007		0.011				-0.005		-0.003
Age > 60					-0.883		-1.291**				-0.421**		-0.555***
Female					0.243		-0.169				0.310		0.201
Firm													
Log(Sales)						1.205***	1.312***					0.350***	0.375***
Book-to-Market						-3.063***	-3.746***					-0.939***	-1.130***
ROA						-2.363**	-3.903***					-0.050	-0.508
Abnormal Returns						-0.003	-0.004					-0.000	-0.001
SD(ROA)						2.307	3.097					0.003	0.152
SD(Returns)						1.087	1.934					0.766	1.560**
Year-Quarter Fixed Effects		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N		714	633	633	633	714	633	714	633	633	633	714	633
R^2		1.1%	11.4%	11.5%	12.3%	30.3%	34.7%	2.8%	12.6%	12.9%	15.3%	27.3%	33.8%

Table 12. Test for H3B

This table reports results from our tests examining whether the length of employment gaps is associated with future CEO turnover. Panel A reports the coefficients from hazard models for the sample of executives with employment gaps. Columns (1) and (2) use a competing-risks hazard model to obtain the coefficients when a failure event is defined as forced turnover (Column 1) or voluntary-poor match turnover (Column 2), while controlling for the other failure type. Column 3 uses a Cox proportional semiparametric hazard model and measures a failure event as both forced and voluntary−poor match turnovers. All three columns show results both with and without control variables. Panel B reports the results from a multinomial logit model for the propensity of experiencing good and bad employment matches. Following Allgood and Farrell (2003), a match is classified as bad if the employment ends due to forced or voluntary−poor match turnover. We classify a match as good if it is not a bad match (as defined above) and tenure is greater than 3 years. We divide *Gap Length* by 365 so that significant coefficients have values different from 0. Significance levels are indicated by ***, **, * representing 1%, 5%, and 10% two-tailed, respectively. Variable definitions are provided in Appendix A.

Panel A: Cox Hazard Model

	Analysis Time: *Time-to-Turnover*					
	(1) Failure Event: Forced turnover		(2) Failure Event: Voluntary-Poor Match turnover		(3) Failure Event: Both	
	No Controls	All Controls	No Controls	All Controls	No Controls	All Controls
Gap Length / 365	0.032	0.062**	0.006	0.009	0.028	0.049**
CEO						
Age > 60		-1.422**		-0.384		-0.979**
Female		-0.242		-0.212		-0.238
Governance						
CEO-Chair Duality		-0.276		-0.316		-0.331**
Board Size		-0.042		-0.014		-0.033
% Outside Directors		0.265		-2.140***		-0.910*
Blockholder Indicator		-0.194		-0.202		-0.216
% Institutional Ownership		0.677**		-0.133		0.420*
Past Performance						
Past Abnormal Returns-Current Firm		-0.001		-0.002		-0.001
Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	876	750	876	750	876	750
Log-Likelihood Ratio	-889	-706	-550	-466	-1,412	-1,153

Table 12. Test for H3B (continued)

Panel B: Multinomial Logit Model	Dependent Variable: *Match Quality*								
	No Controls			With CEO Controls			With CEO and Governance Controls		
	Good	Bad Forced	Bad Voluntary	Good	Bad Forced	Bad Voluntary	Good	Bad Forced	Bad Voluntary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Gap Length / 365	-0.007	0.006	0.001	-0.015**	0.011**	0.003	-0.018**	0.015**	0.003
CEO									
Age				0.019***	-0.012***	-0.007***	0.019***	-0.014***	-0.005*
Age > 60				0.034	-0.067	0.033	0.027	-0.081	0.054
Female				0.078	-0.050	-0.029	0.084	-0.056	-0.028
Governance									
CEO-Chair Duality							0.078	-0.040	-0.038
Board Size							0.009	-0.007	-0.002
% Outside Directors							0.298*	0.018	-0.316***
Blockholder Indicator							0.065	-0.030	-0.034
% Institutional Ownership							-0.092	0.112*	-0.020
Past Performance									
Past Abnormal Returns-Current Firm							0.001	-0.000	-0.000
Year-Quarter Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	382	146	89	382	146	89	318	121	79
Pseudo R^2		0.1%			2.8%			5.3%	

Table 13. Gap Activities.

This table reports information on the gap activities of the executives in our sample. Panel A reports the primary gap activities of executives who had an employment gap. This was determined by looking at the number of months spent doing each activity. Following Fich and Shivdasani [2006], serving on three public boards is considered a full time gap activity except when the executive was also consulting for the firm. For 719 executives with a known primary gap activity, Panel B reports all activities engaged in during employment gaps (i.e., one executive can engage in multiple gap activities).

Panel A: Primary Gap Activity (1 Activity per Executive)

	N	Percent
Board Membership	277	28.2%
Consulting	85	8.7%
Investing	77	7.9%
Private Firm Employment	259	26.4%
Other	21	2.1%
Unknown	262	26.7%
Total	981	100.0%

Panel B: Gap Activities of Executives with a Primary Activity

	N	Percent (of 719 executives with known gap activities)
Board Membership	436	60.6%
Consulting	125	17.4%
Investing	121	16.8%
Private Firm Employment	298	41.4%
Other	30	4.2%
Total	1,010	140.5%

Table 14. Prior Firm Competitors.

This table reports information on the number of executives whose prior public firm was a competitor of the executive's new employer based on SIC codes.

Measure of Competitor	Old – New Same SIC		Old – New Different SIC	
	N	%	N	%
2-Digit SIC	572	36.1%	1,012	63.9%
3-Digit SIC	430	27.1%	1,154	72.9%
4-Digit SIC	317	20.0%	1,267	80.0%

Table 15. Announcement returns.

This table reports the results for the market reaction tests to the announcement of CEO appointments. The dependent variable is the cumulative abnormal returns for the appointing firm adjusted for Fama and French [1996] and Carhart [1997] factors over four announcement windows: two-day window starting from the announcement date in column (1), three-day window starting from the announcement date in column (2), the three-day window starting from the day prior to the announcement date in column (3) and the four-day window starting from the day prior to the announcement date in column (4). *No Gap* is an indicator variable that is equal to one if the firm appoints a CEO without an employment gap. *Gap–Short* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is shorter than or equal to 222 days (25th percentile of the *Gap Length* distribution). *Gap–Long* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is longer than or equal to 1,667 days (75th percentile of the *Gap Length* distribution). *Gap–Medium* is an indicator variable that is equal to one if the firm appoints a CEO who experiences a gap that is longer than 222 days and shorter than 1,667 days. t-statistics are reported in parentheses with robust standard errors. ***, **, * denote significance at the 1%, 5%, and 10% levels two-tailed, respectively.

	Dependent Variable:			
	CAR [0,1]	CAR [0,2]	CAR [-1,1]	CAR [-1,2]
	(1)	(2)	(3)	(4)
No Gap	0.010***	0.009***	0.011***	0.010***
Gap–Short	0.024***	0.023***	0.026***	0.025***
Gap–Medium	0.007*	0.011**	0.010	0.013**
Gap–Long	0.001	0.002	0.007	0.008
N	1,579	1,579	1,579	1,579
Adj. R^2	1.17%	0.99%	1.14%	1.07%
Tests of Differences				
No Gap vs. *Gap–Short*	-0.013**	-0.014*	-0.015**	-0.015**
No Gap vs. *Gap–Medium*	0.003	-0.002	0.002	-0.003
No Gap vs. *Gap–Long*	0.010	0.007	0.005	0.002
Gap–Short vs. *Gap–Medium*	0.016**	0.012	0.016**	0.012
Gap–Short vs. *Gap–Long*	0.023**	0.021*	0.019	0.017
Gap–Medium vs. *Gap–Long*	0.007	0.009	0.003	0.005