Executive Networks and Firm Policies: Evidence from the Random Assignment of MBA Peers

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Abstract

Using the historical random assignment of MBA students to sections at Harvard Business School, I explore how executive peer networks can affect managerial decision-making and firm policies. Within an HBS class, firm outcomes are significantly more similar among graduates from the same section than among graduates from different sections, with the strongest effects in executive compensation and acquisitions strategy. Both compensation and acquisitions propensities have elasticities of 10-20% with respect to the mean characteristics of section peers. I demonstrate the important role of ongoing social interactions by showing that peer effects are more than twice as strong in the year immediately following staggered alumni reunions. A variety of other tests suggest that peer influence can operate through direct reactions to peer outcomes in ways that do not necessarily contribute to firm productivity. JEL: D71, M12, G34

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1 Introduction

Recent empirical findings suggest that executives hold significant discretionary powers over a range of firm policies, including the allocation of capital through M&A activities and even the design of their own compensation schemes (Bertrand and Schoar, 2003; Frank and Goyal, 2007; Bennedsen, Perez-Gonzalez and Wolfenzon, 2010; and Graham, Li and Qiu, 2011). If executives can indeed meaningfully affect firm policies, how do they go about making decisions? An emerging literature answers this question by looking at the relationship between corporate outcomes and executive personal characteristics such as optimism, risk-aversion, ability, and resoluteness (e.g., Malmendier and Tate, 2008; Graham et al., 2009; and Kaplan et al. 2011). However, executives are extremely networked and social agents. In addition to being guided by their own innate preferences and beliefs, executives are likely to be strongly influenced by their social experiences. Studying executive social networks has the potential to add to our understanding of the determinants of managerial decision-making. Meaningful social influence among executives would imply that executives matter for corporate outcomes in systematic and predictable ways that can lead to correlated behavior across firms.

Peer interactions could affect managerial decision-making because information and beliefs travel through social networks. For example, Cohen, Frazzini and Malloy (2008) show that mutual fund managers gain informational advantages when investing in firms managed by those in their education networks. These word-of-mouth effects have also been highlighted in theoretical work by Ellison and Fudenberg (1995) and DeMarzo, Vayanos and Zwiebel (2003) and shown in financial contexts by Davis and Greve (1997) and Hong, Kubik and Stein (2005). Alternatively, peer interactions may induce executives to "keep up with the Joneses" in terms of compensation and acquisitions. For example, Frank (1985), Luttmer (2005), and Card et al. (2011) show in general contexts that individuals value relative earnings, while Goel and Thakor (2010) develop a model of envy-motivated mergers.

Estimation of peer effects faces the twin identification challenges of selection and common shocks (Manski, 1993).[1] Selection occurs when executives with similar unobserved characteristics select into peer groups. Common shocks occur when group members, by virtue of their association, experience

[1]Recent work has called into question whether previous estimates of peer effects were biased due to selection and common shocks. Sacerdote (2001), Kling, Liebman and Katz (2007), Lyle (2007), and Guryan, Kroft and Notowidigdo (2009) find in non-executive contexts that peer effects may be weaker or more nuanced than expected. Meanwhile, Cohen, Frazzini and Malloy (2008), Carrell, Fullerton and West (2009), Ahern, Duchin and Shumway (2011) and Buchardi and Hassan (2011) show that peer effects can be large in the contexts of insider trading, academic performance among Air Force cadets, transmission of economic attitudes, and income per capita following German reunification, respectively.

group-level unobserved shocks. These biases imply, for example, that one cannot attribute a merger wave within the auto industry to industry-level peer effects because empire-loving CEOs may have selected into that industry and because the auto industry may have experienced an unobserved common shock to the determinants of optimal firm size. Similarly, it is difficult to measure peer effects within an executive educational network, e.g., a particular Wharton cohort, if certain types of students select into each Wharton class and if Wharton indoctrinates each class of students with specific management philosophies.

This paper identifies the causal effect of peers on executive decision-making using a natural experiment involving randomly assigned peer groups and shocks to those peers over time. Starting with the class of 1949, Harvard Business School (HBS) began randomly assigning all entering MBA students to sections.[2] I refer to students who graduated from the *same* section in the *same* class year as section peers and students who graduated from *different* sections in the *same* class year as class peers. HBS attempts to foster strong and long-lasting social bonds among section peers – all first-year students take the same non-elective curriculum with their section peers and sections remain organizational focal points during alumni reunions and contribution campaigns. While many unobserved selective forces may affect the composition of each HBS class, within that class, randomized treatment determines whether any two students are section peers or class peers. To control for any remaining bias from section-level common shocks, I also examine changes in executive behavior following shocks over time to section peer outcomes as well as shocks to the strength of peer bonds.

I follow the subset of HBS alumni who become top executives at S&P 1500 companies. HBS provides an ideal empirical setting because it has historically been a major producer of executives, accounting for over six percent of all executives in the ExecuComp database. In addition, randomized HBS executive peer groups offer an identified laboratory in which to study peer influence among executives more generally, e.g., peers connected through common industries, regions, board relationships, civic organizations, and trade associations. This paper's use of the HBS empirical setting builds upon previous work by Lerner and Malmendier (2011), which uses HBS sections to identify peer effects in the decision to become an entrepreneur. Aside from focusing on executive decision-making rather than entrepreneurship, this paper also uses a longer historical panel of

[2]HBS attempts to create sections that are balanced in terms of variables observed by the Registrar, such as gender, marital status, undergraduate institution, and previous industry experience. Assignment is random conditional on Registrar observables. Balanced section assignment does not pose a problem for the empirical strategy, because it creates a small bias *against* findings of positive peer effects. This intuition is formalized in Appendix A and empirically supported in Section 5.1.

HBS alumni data, which necessitates a different empirical approach from that used in Lerner and Malmendier (discussed in detail in Section 3).

I explore the impact of peer interactions on a variety of firm policies including executive compensation, acquisitions strategy, investment, and financial policy. While existing work has linked executive networks to firm policies (e.g., Bizjak, Lemmon and Whitby, 2005; Fracassi, 2008; Hwang and Kim, 2009; Barnea and Guedj, 2010; Ishii and Xuan, 2010; Leary and Roberts (2010); Butler and Gurun, 2011; and Fracassi and Tate, 2011), most of the literature has focused on institutionalized links such as connections between the CEO and board members, industry bonds, or overlapping board membership, the strength of which may be enhanced through common educational backgrounds. In contrast, I focus on the non-institutionalized, but potentially powerful, social bonds among executives in firms that generally lack formal linkages. In addition, most of the financial networks literature[3] does not use randomly assigned peer groups (indeed, some of the literature is particularly interested in the endogenous formation of firm networks). This paper complements the literature by using a random assignment context to establish causality, and also by exploring the magnitudes, mechanisms, and timing behind peer influence.

Peer influence can occur if individuals react to the mean of group behavior, follow group leaders, or adopt a group norm. Regardless of exactly how peer influence occurs, we expect that section-based peer interactions will lead section peers to become more similar than class peers.[4] I find the strongest evidence of section peer similarities with respect to executive compensation and acquisitions. Relative to class peers, section peers receive significantly more similar compensation and are more likely to pursue similar acquisitions strategies. The variation in compensation and levels of acquisition activity among section peers is around 10 percent less than the variation among class peers. Under further structural assumptions developed in a Linear-in-Means Model of social interactions, I estimate a lower bound for the elasticity of the individual response to mean section peer characteristics of 10 to 20 percent. While the effect sizes are largest for compensation and acquisitions, I also find evidence of significant peer effects in investment, leverage, interest coverage,

[3]This paper also builds upon the financial networks literature, which studies both informal and institutional links. Hallock (1997), Kirchmaier and Stathopoulos (2008), Hwang and Kim (2009), Barnea and Guedj (2010), and Engelberg, Gao and Parsons (2010) find that the quality and size of an executive's network are predictive of compensation and firm performance. Peer influence has also been shown in broader financial contexts, such as analyst forecasts (Cohen, Frazzini and Malloy, 2010), fund voting (Matvos and Ostrovsky, 2010), bankruptcy (Cohen-Cole and Burcu, 2009), stock market participation (Brown et al., 2008 and Kaustia and Knupfer, 2011), internal capital markets (Duchin and Sosyura, 2011), and mutual fund and venture capital networks (Hochberg, Ljungqvist and Lu, 2007 and Kuhnen, 2009).

[4]Peer influence can also lead to a decrease in group similarity if individuals seek to become outliers. The empirical model and analysis will allow for both types of peer effects.

and cash policy.

To investigate underlying mechanisms and rule out potential biases, I begin by differentiating between similarities in executive behavior that are the result of "contemporaneous" rather than "past" interactions. Establishing the timing of peer interactions is important because the empirical analysis focuses on the subset of HBS graduates who become top executives. Given the initial random assignment of students to sections, selection of students into this executive subsample and/or into similar types of firms can be an important peer effect operating through past interactions. I find that past interactions are indeed important determinants of executive career profiles – relative to class peers, section peers are around 20 percent more likely to choose the same industries and geographical locations, and to overlap in firm affiliations. In contrast to past interactions, which are informative of early career trajectories, contemporaneous interactions describe ongoing interactions that occur while executives manage firms and are more informative of the impact of executive networks on firm policies.

I establish the importance of contemporaneous interactions using the natural experiment of HBS alumni reunions, which occur every five years after each executive's graduation year. Staggered reunions introduce exogenously-timed shocks to the strength of peer bonds. Because reunions cover the same time period as firm outcome measures, reunions should only affect estimates if contemporaneous interactions are important drivers of peer similarities. I find that peer similarities in compensation and acquisition activity are more than twice as strong in the year immediately following reunions relative to other years.

Evidence of contemporaneous interactions show that the results are not driven by bias from section-specific common shocks, e.g., a professor who indoctrinates her section with a particular management philosophy. Common shocks such as influential professors are unlikely to generate peer similarities that vary according to the staggered reunion schedule. The marginal increase in peer similarities following reunions serves as a lower bound for true peer effects. Evidence of contemporaneous interactions also illustrates the very persistent effects of peers on high-stakes decision-making among executives; peer groups formed in business school affect executive decision-making at S&P 1500 firms several decades after graduation.

Using an additional test of "pay for friend's luck" that builds upon the methodology in Bertrand and Mullainathan (2001), I find that executive compensation responds to lucky shocks to the pay of peers. Here, "lucky pay" is defined as the part of each executive's compensation that can be predicted using her mean industry returns (over which she has, arguably, minimal impact). The analysis is restricted to peers working in distant industries with minimal trading relationships to reduce the likelihood that shocks to peers in different industries will have significant *direct* unobserved effects on executives. I find that individual changes in compensation are significantly more responsive to section peers' lucky pay than to class peers' lucky pay, even after the introduction of numerous controls for own firm and industry performance. Like reunions, lucky industry shocks occur in the same time period as executive outcomes. Therefore, pay for friend's luck again highlights the importance of contemporaneous interactions. Evidence of pay for friend's luck further offers a check on bias from common shocks (e.g., professors) which are unlikely to generate behavior that varies over time with lucky industry shocks to peers.

The channels through which peer influence operate can further be divided into two broad categories: reactions to peer fundamentals and reactions to peer outcomes. Reactions to peer fundamentals occur if the fundamental skills, beliefs, or information driving managerial decisions are transferred through peer networks. For example, executive compensation may be similar within a peer group because executives transfer managerial skills to one another, leading to similar levels of firm productivity which then lead to similar compensation. In contrast, reactions to peer outcomes can occur if executives respond directly to the actions of peers, e.g., if executives seek to match or exceed friends' compensation or acquisition levels or if a change in peers' compensation affects executives' outside options. Distinguishing between reactions to fundamentals and outcomes is important for our understanding of policy interventions, e.g., industry-level executive pay caps or anti-takeover regulations, which affect the actions of peers while leaving their fundamentals unchanged. For these policy interventions, only peer influence operating through reactions to peer outcomes will generate a peer multiplier (Glaeser, Sacerdote and Scheinkman, 2003). Further, reactions to fundamentals can improve firm productivity if information that is useful for the firm is passed through executive networks. In contrast, reactions to peer outcomes may weakly lower firm productivity (e.g., if executives blindly mimic each other's acquisitions activity).

Most likely, both forces are present and it is not the goal of this paper is to claim that peer effects operate only through one channel. Moreover, the empirical tests cannot rule out that reactions to peer fundamentals, e.g., information sharing, are important drivers of peer influence. However, a variety of tests suggest that peer influence also operates through reactions to peer outcomes in ways that do not contribute to firm productivity. Evidence of pay for friend's luck suggests that peer similarities in compensation are partly driven by direct reactions to peer compensation rather than by the sharing of underlying fundamentals such managerial skills or insight: pay responds to lucky shocks to friends, even after controlling for own firm and industry performance. This can occur if relative income directly enters into each executive's utility function or if peer's lucky shocks alter the outside options of executives. I also present exploratory evidence showing that peer similarities in acquisitions can operate through channels other than socially optimal information sharing. While these results suggest that peer influence can lead to less efficient acquisitions, important caveats to this analysis are discussed in Section 6.3.

Finally, this paper provides evidence concerning the aggregate implications of social interactions among executives. Strong social interactions imply a large peer multiplier: the aggregate response to a change in the fundamental determinants of compensation or acquisition activity will be larger than the direct response because of contagion among connected agents (Glaeser, Sacerdote and Scheinkman, 2003). I estimate a substantial peer multiplier of 10 to 20 percent, e.g., the aggregate increase in acquisition activity following a fundamental shock may be 10 to 20 percent larger than any individual firm's direct response to the shock. Peer influence can also contribute to clustered financial activity. Numerous papers document the large variation in mean executive compensation and merger activity across time, industries, and regions (Murphy, 1999 and Andrade et al., 2001). A variety of explanations emphasizing fundamental differences across groups have been offered for the clustering of pay and acquisitions (e.g., Harford, 2005; Jovanovic and Rousseau, 2002; and Shleifer and Vishny, 2003). This paper presents a complementary mechanism: differences in fundamentals across groups or over time can be amplified through peer interactions. Along these lines, Demarzo, Kaniel and Kremer (2007) and Goel and Thakor (2010) present financial herding models in which envy among peers leads to financial bubbles and merger waves. This paper explicitly shows that peer influence can lead to strongly clustered activity among section peers using the natural experiment of HBS sections. I find that the ratio of the between- to within-section variance in these firm policies is 20 to 40 percent larger than expected under the null hypothesis of no peer effects.

The remainder of the paper is structured as follows: Section 2 offers an illustrative example of peer influence among the HBS class of 1949. Section 3 describes HBS sectioning and the executive data. Section 4 presents the empirical model and the estimation methodology. Section 5 shows the main results, Section 6 presents extensions, and Section 7 concludes.

2 Peer Influence Among the HBS Class of 1949

Before delving into the empirical analysis, I begin with a motivating example of peer interactions among the HBS Class of 1949. Within the class, there were the usual social cliques. Here, I focus on one closely-knit clique, playfully dubbed "The Group" by their wives and girlfriends. The Group consisted of the eleven men listed in Table 1 who shared tastes for boisterous gallivanting and casual gambling. Group members were extremely successful and include the CEOs and founders of Bristol-Myers, Cap Cities/ABC, General Housewares, Johnson & Johnson, Resorts International, and Xerox.[5]

The Group remained in close contact after business school. After graduation, Group members lived in a cluster of apartments in New York City. During the 1950s and 1960s, The Group partied at informal reunions held at the grand Presidential Suite in Greenbrier Resorts, hosted by Group member and Greenbrier President, Jack Lanahan. In the 1970s, The Group launched "Operation Snowflake," an exclusive ski retreat. Operation Snowflake was such a success that it became an annual tradition lasting over a quarter of a century.

The composition of The Group is indicative of the key identifying assumption of this paper, that randomly assigned section peers tend to share stronger social bonds than class peers. Membership in the eleven-person group is disproportionately dominated by seven members from section C and three members from section A. While social bonds among class peers do exist – Group members don't all belong to the same section – bonds within sections are stronger in expectation. For all empirical estimates, I will focus on the marginal increase in peer influence among section peers relative to class peers, because that is the part that can be plausibly identified. To the extent that peer influence exists among class peers, my estimates represent lower bounds for the true magnitudes of peer influence.

Group members were instrumental in shaping each other's career trajectories. Peter McColough (CEO of Xerox) found his first job by sending his resume through Winslow Martin. Frank Mayers (President of Bristol-Myers) and Jack Davis (founder of Resorts International) developed their marketing skills together selling a new laxative called Prunex. At Tom Murphy's (CEO of Cap Cities/ABC) wedding, Davis was introduced to Murphy's brother-in-law, James Crosby. Davis and Crosby later purchased Paradise Island in the Bahamas and established a gambling resort empire. Jack Muller started General Housewares on the advice of Murphy, who told him to "take a leap and get into business on his own."[6] Murphy would later earn handsome profits through General Housewares thanks to a $1.6 million initial investment funded in part by Group members Burke, Mayers, and Murphy.

After establishing themselves among the corporate elite, Group members were not above some

[5] Marilyn Wellermeyer, "The Class the Dollars Fell On," *Fortune*, May 1974, p. 227.

[6] David Callahan, *Kindred Spirits* (Hoboken, New Jersey: John Wiley & Sons, 2002), p. 133.

friendly competition, especially with regard to executive compensation:

> The members of The Group love to tease one another about their accomplishments, and Baldwin is one of the most adept at it ... [H]e and Burke set out to bait Tom Murphy. First Baldwin pretended to compliment Murphy for having accumulated more than anyone else in the class – close to $3 million, Baldwin surmised. Then, as Baldwin tells it, while Murphy beamed, Burke mentioned that he had just seen McColough, who, he guessed, must be worth four times that. "Tom looked like he had had a cardiac arrest," says Baldwin.[7]

Stories surrounding the 49ers also motivate the use of HBS alumni reunions as exogenous shocks to the strength of peer bonds. Like every other HBS class, the 49ers hold reunions every five years after their graduation year. Reunions provide formal and informal peer bonding opportunities. For example, following the 49ers' 40th reunion gala, members of The Group retreated upstairs to a more exclusive cocktail party in a private suite.[8] Reunions are also a time for friends to promulgate their views of corporate strategy. For example, the 40th reunion for the 49ers included a telling debate on takeovers and corporate ethics:

> The panel discussion is spirited, but ... a rough consensus emerges: In takeover situations, managers' fiduciary duty to shareholders is not paramount; it should be weighted against the needs of employees and others. Led by Burke and McColough, most of the panelists argue vigorously that companies often should resist takeovers even when they can't produce as much money for shareholders as can the outside bidder. Panelists speak out for poison pills and golden parachutes.[9]

The Group provides just one example of how peer influence can powerfully affect executives. In the remainder of this paper, I investigate executive peer effects using data from the HBS class of 1949 to the present.

3 Data

3.1 Section Assignment at Harvard Business School: 1949 - Present

Starting with the class of 1949, HBS began assigning all entering MBA students to sections of roughly 90 students. Section assignment continues into the present day. Initially, there were seven

[7] Wellermeyer, p. 344.
[8] Stratford P. Sherman, "You're Invited to the CEO's Ball," *Fortune*, January 15, 1990, p. 140.
[9] Sherman, p. 140.

sections: A, B, C, D, E, F, and G; sections H, I, and J were added over time. Sections foster close social bonds that last well beyond graduation. Section members take the complete non-elective first-year sequence of courses together in the same classroom. In the second year, students take elective courses, so classrooms contain a mix of students from different sections. However, second-year students still refer to themselves as "old A" or "old G." Despite the random assignment to sections, section peers develop a strong sense of group identity. Throughout the two-year MBA program, athletic competitions and student organizations are organized around sections. After graduation, reunions are organized around section parties, and alumni contribution campaigns are similarly organized by sections.

Numerous studies of student life at HBS describe the strong peer interactions within sections. In his study of section norms, Orth[10] describes the typical life of a first-year student:

> He speaks mostly to other men in his section who happen to live in rooms near his. When he sits down at the lunch table ... it is almost always with a group of men from his section. When ... he joins a study group, he finds that the members of the group are likely to be men in his own section.

In an autobiographical memoir, Henry[11] recalls that sections fostered both conformity and competition:

> On the surface we were still Section D, loyal to each other and supportive of our individual differences. But under this there was a great deal of teasing and a lot of jockeying for position. We were competing with each other and at the same developing a fierce conformity, as though if we conformed we'd be in a better position to compete.

Section assignment is random conditional on student characteristics observed by the HBS administration. School administrators balance sections on the following observables: race, ethnicity, nationality, industry background, undergraduate institution, geographical origin, and marital status. Balanced section assignment does not pose a problem for the validity of the empirical methodology. In the absence of peer effects, balanced sectioning implies that two randomly selected class peers are actually more likely to have similar characteristics than two section peers. Thus, balanced assignment generates a small negative bias *against* findings of positive peer effects. This intuition is formalized in Appendix A and demonstrated empirically in Table 4.

[10] Orth, Charles D., III, *Social Structure and Learning Climate: The First Year at the Harvard Business School*, Boston, Division of Research, Graduate School of Business Administration (Harvard University, 1963), p. 4.

[11] Henry, Fran Worden, *Toughing it Out at Harvard: The Making of a Woman MB*A (New York: McGraw-Hill Book Company, 1983), p. 130.

The empirical context of HBS sections used in this paper is most similar to the context used in Lerner and Malmendier (2011), hereafter referred to as LM. LM measures the effect of entrepreneurial peers on subsequent entrepreneurship rates. This paper's methodology differs from LM in three ways. First, this paper uses a longer historical sample (alumni records from 1949 to 2008) while LM's sample spans 1997 to 2004. Second, LM identifies peer effects using the cross-sectional relationship between peer ex-ante entrepreneurship experience and an individual's ex-post decision to become an entrepreneur. In contrast, this paper uses data that contains detailed panel coverage of individual ex-post outcomes. However, the data contains limited coverage of individual ex-ante characteristics (due to the nature of the longer historical sample). Therefore, this paper's methodology identifies peer effects using the distribution of ex-post outcomes within and across sections for each class year, and tests how the distribution of ex-post outcomes in the panel data changes with shocks to the strength of peer bonds over time (e.g., reunions). Third, LM focuses on entrepreneurial entry decisions shortly after graduation while this paper explores how peers affect executive decision-making and firm policies in the long term (several decades after graduation).

3.2 Executive Data

The data sample begins with HBS alumni records from 1949 to 2008, which are matched to the CompuStat ExecuComp database, which covers the compensation of top executives at S&P 1500 firms from 1992 to 2009.[12] Acquisitions data comes from SDC Platinum. Biographical and employment history data comes from ExecuComp and is supplemented, if missing, with data from BoardEx. Firm measures come from CompuStat and CRSP, while industry returns come from CRSP and the Kenneth French data library. Observations in the panel data are uniquely identified at the executive $\times$ year level, where year corresponds to the fiscal year of the executive's firm and should not be confused with each executive's class year (the year she graduated from HBS).

Table 2 summarizes the data coverage of HBS MBA alumni who become top executives in S&P 1500 firms as reported in ExecuComp. Altogether there are 596 CEOs/CFOs and 1051 top executives (inclusive of CEOs and CFOs), resulting in 3071 CEO/CFO $\times$ year and 6413 top executive $\times$ year observations in the panel data. In the ExecuComp data covering the years 1992 to 2009, the median CEO/CFO has two section peers (same section and same class year) and 14 class peers (same class year, different sections), *excluding herself*, while the median top executive has three

[12] Matching between the two databases consists of two steps. I first match by name and age using the fuzzy string-match algorithm in the RecLink software package. All matches are then verified using HBS alumni records, BoardEx executive biographical records, political contributions, executive biographies, and firm annual reports.

section peers and 25 class peers. Note that while the alumni records cover all HBS MBA graduates from 1949 to the present, over 90% of the executive subsample graduated between 1960 and 1990 (this is because ExecuComp covers executives from 1992-2009 with a median age of 58).

Table 3 summarizes compensation, demographics, and firm policies for both the HBS CEO/CFO sample and the complete sample of CEOs/CFOs in ExecuComp. While non-HBS CEOs/CFOs are not directly relevant for the empirical analysis, their outcomes will be used in some specifications to control for industry and time trends. Executive compensation data takes two forms: direct compensation is the sum of salary and bonus while total compensation is the sum of direct and equity-linked compensation (defined as the sum of restricted stock grants and the Black-Scholes value of option grants and long term incentive plans as calculated by ExecuComp).[13] In general, the compensation of HBS CEOs/CFOs is slightly higher than that for the overall ExecuComp sample. HBS CEOs/CFOs are slightly more likely to be female and have median firm tenure of six years compared to eight years in the ExecuComp sample. Firm and industry (SIC3) fiscal year returns are very similar across the two samples, while HBS CEOs/CFOs tend to belong to firms 40 percent larger than the mean firm in ExecuComp (where firm size is measured by sales or assets).

Acquisition policy data comes from the SDC database which covers all private and public mergers that appear in SEC filings in which at least five percent of the ownership of a company is transferred. I present two measures for acquisitions. Completed acquisitions are successful acquisitions in which acquiring firms successfully gained 50 percent or greater stakes in the acquired entities. Attempted acquisitions include all recorded acquisitions in the SDC database and are inclusive of completed acquisitions. Attempted acquisitions are informative of peer influence even if the acquisitions ultimately fail (e.g., due to takeover defenses or regulatory restrictions) because they provide evidence of executives' intentions to acquire. All acquisitions are assigned to years t according to the date of the initial merger announcement. Acquisitions are common occurrences in the sample – the median HBS CEO/CFO attempts at least one acquisition per year. These acquisitions represent a significant change in firm's allocation of capital, with median and mean values of over \$100 million and \$1 billion, respectively.

[13]A change in SEC compensation disclosure rules went into effect in 2006. Among other changes, the new rules require that the value of equity-based compensation be reported as the grant date fair value according to FAS 123R standards. For continuity, I use the ExecuComp measures of equity-related compensation that follow the 1992 reporting format for all years. Table 10 presents evidence that results are robust to the 2006 change in reporting standards.

4 Empirical Model

4.1 A Linear-in-Means Model of Peer Influence

Peer influence can operate in a variety of ways. For example, individuals may follow leaders (see Section 2 of Graham, 2008) or develop a group norm (see the discussion of hierarchical models in Blume et al., 2010). In this section, I develop an empirical model that assumes that individuals react to the mean characteristics of their peer group. The initial setup of this model is similar to existing work on Linear-in-Means Models developed in Graham (2008) and Glaeser and Scheinkman (2001), with extensions as noted. However, the Linear-in-Means Model offers only a parsimonious approximation of how peer influence actually operates. Since most forms of peer effects predict that section peers will have more similar outcomes than class peers, the empirical results will offer both reduced-form comparisons of section versus class peer similarity as well as estimates of model parameters. Of course, social interactions can also lead to dissimilarity among group members if, for example, individuals choose to be mavericks. Both the reduced form estimates and model will also allow for this type of peer effect.

Consider individual i in section s in class year c. For simplicity, restrict attention to a single class year c and single year in the panel data of outcomes. Let section s represent each individual's relevant peer group within the class year (assume, conservatively, that across-section peer interactions are zero). Individual outcomes (e.g., compensation or acquisition policy) Y_{isc} are represented by the following linear function:

$$Y_{isc} = \theta \overline{Y}_{sc} + \phi \overline{v}_{sc} + \alpha_{sc} + \rho v_{isc}. \tag{1}$$

Exogenous student-level fundamentals (e.g., ex-ante managerial skills or private information) that affect outcomes are represented by v_{isc}. Following Graham (2008), I allow for two types of peer effects: responses θ to mean group outcomes $\overline{Y}_{sc}$ and responses ϕ to mean group fundamentals $\overline{v}_{sc}$ (initially labeled as endogenous/reflective and contextual/exogenous peer effects, respectively, by Manski, 1993). Responses $\theta \in (-1, 1)$ to mean group outcomes[14] occur when individuals react directly to $\overline{Y}_{sc}$, i.e., if peers' compensation or acquisitions outcomes directly impact individual compensation or acquisitions. Responses ϕ to mean group fundamentals occur when individuals react directly to $\overline{v}_{sc}$: responses to fundamentals might represent transfers of fundamental skills or information among peers. Both responses to peer outcomes and fundamentals are true peer effects. However, distinguishing between the two responses becomes important when evaluating

[14] θ is constrained to be between -1 and 1 in order to guarantee convergence of the harmonic series.

policy interventions or other shocks that change or limit outcomes $\overline{Y}_{sc}$ while leaving fundamentals $\overline{v}_{sc}$ unchanged. For such shocks, only responses θ to peer outcomes will generate a peer multiplier effect.

α_{sc} (scaled to have mean zero, without loss of generality) represents section-specific common shocks, such as a professor who affects the outcomes of all students in her section. Following the intuition developed in herding cascade models (e.g., Banerjee, 1992 and Bikhchandani, Hirshleifer and Welch, 1992), $\rho \in (0, 1]$ represents the extent to which peer interactions can lead individuals to underweight their own fundamentals.[15] Note that, fundamentals v_{isc} are defined such that, in the absence of both common shocks ($\alpha_{sc} = 0$) and peer influence ($\rho = 1$, $\theta = \phi = 0$), individual outcomes are determined by individual fundamentals: $Y_{isc} = v_{isc}$.

Averaging over individuals in the same group yields the average of optimal group outcomes:

$$\overline{Y}_{sc} = \frac{\alpha_{sc}}{1-\theta} + \frac{\phi+\rho}{1-\theta}\overline{v}_{sc}. \tag{2}$$

For now, I assume that $\alpha_{sc} = 0$, i.e., that section peer groups do not experience section-specific common shocks such as a professor who indoctrinates her section with a certain management philosophy. Peer group common shocks are less likely to be significant in the context of HBS sections than in other contexts, e.g., industry peer groups, because HBS imposes the same curriculum across sections and attempts to promote equal access to educational services across sections. Nevertheless, I will explicitly present empirical evidence against common shocks bias.

Using equations (1) and (2), the individual optimal outcome can be expressed as a simple linear-in-means function of own and group fundamentals:

$$Y_{isc} = \tau\overline{v}_{sc} + \rho v_{isc} \quad , \quad \tau \equiv \frac{\phi+\theta\rho}{1-\theta}. \tag{3}$$

τ represents the effect of changes in mean group fundamentals $\overline{v}_{sc}$ on individual outcomes Y_{isc}. Note

[15] Existing Linear-in-Means Models usually use the form $Y_{isc} = \theta\overline{Y}_{sc} + \phi\overline{v}_{sc} + \alpha_{sc} + v_{isc}$ (see Graham, 2008 for a sample utility function that generates optimal outcomes of this form). Equation (1) is identical up to a rescaling of the fundamentals by ρ, which measures the extent to which individuals underweight their own fundamentals. The introduction of ρ allows for comparisons to the null case of no peer effects and no common shocks (when $Y_{isc} = v_{isc}$) and provides an explicit parameter through which within-group variance can approach zero (when ρ approaches zero). I thank John Campbell for highlighting this issue.

Equation (1) is also equivalent to a common alternative specification using "leave-out" means: $Y_{isc} = \tilde{\theta}\overline{Y}_{sc,-i} + \tilde{\phi}\overline{v}_{sc,-i} + \tilde{\alpha}_{sc} + \tilde{\rho}v_{isc}$ where, e.g., $\tilde{\theta} = \left(\frac{m-1}{m-\theta}\right)\theta$ and m is the number of individuals in the peer group. I use specification (1) because it allows for simple interpretations of the peer multiplier (see next section). Inclusion of i in the group mean does not bias results because Equation (1) is not estimated directly as a regression; it is estimated as described in Sections 4.2 and 4.3 which utilize the most conservative assumption for group size m.

that a change in $\overline{v}_{sc}$ can affect Y_{isc} in two ways: (1) if individual outcomes directly respond to peer fundamentals through the ϕ channel, or (2) if individual outcomes respond through the θ channel to mean peer outcomes which in turn respond to changes in own fundamentals through the ρ channel.

In baseline estimates, I do not distinguish between responses to outcomes θ, responses to fundamentals ϕ (later tests will differentiate between the effects), and the extent to which social interactions lead individuals to underweight their own fundamentals by ρ. Since θ, ϕ, and ρ all represent peer effects, I take an approach that is common in the literature[16] and estimate a joint peer influence parameter:

$$\text{Peer Elasticity} \quad \gamma \equiv \frac{\tau}{\rho}. \tag{4}$$

γ represents the elasticity of the individual response Y_{isc} to a unit change in mean group fundamentals $\overline{v}_{sc}$, scaled by the elasticity of the individual response to a unit change in own fundamentals. For example, $\gamma = 0.20$ implies that the response to a change in mean group fundamentals will be 20 percent as large as the response to a similarly sized change in own fundamentals. Note that $\gamma > 0$ implies positive peer effects. However, the model does not restrict peer effects to be positive.

Selection into the Executive Subsample

While the model described above applies to peer groups generally, the empirical analysis will focus on the subsample of HBS graduates who hold positions as CEOs/CFOs and top executives at S&P 1500 firms during the period covered by ExecuComp – roughly four and six percent, respectively, of all alumni. I focus on top executives because their outcomes are well-documented, and more importantly, because their actions have large consequences in the financial world. As noted in the Section 1, differential section-level selection into the executive data can be an important peer effect. Given the random assignment of all entering students to sections and assuming no common shocks, baseline measures of executive peer similarities will capture the joint effect of two types of peer effects: past interactions (peers selecting into the executive data and entering similar firms) and contemporaneous interactions (ongoing interactions among executives that represent the effect of executive networks on firm policies). Extensions of the model to account for selection into the ExecuComp subsample are presented in Appendix B and additional tests, presented in Sections 5.3 and 5.4, will isolate peer effects due to contemporaneous interactions.

[16] Sacerdote (2001) contains a detailed discussion of the difficulties of disentangling responses to fundamentals and outcomes.

Multipliers in Aggregate Levels and Variance

A useful and important implication of the Linear-in-Means Model is that social interactions will lead to a multiplier in terms of the aggregate levels of outcomes. Using equation (3), ρ equals the direct response to a unit change in own fundamentals. Taking the mean of equation (3), $(1+\gamma)\,\rho$ equals the aggregate response to a unit change in mean fundamentals. Thus $(1+\gamma)$ is the multiplier, equal to the ratio of the full effect to direct effect of a change in the mean fundamental determinants of behavior $\overline{v}_{sc}$. If peer effects are positive ($\gamma > 0$), the aggregate response to a change in mean fundamentals will be larger than the direct effect by γ, the peer elasticity.[17]

A second implication of the Linear-in-Means Model is that social interactions will increase the amount of variation across groups relative to the amount of variation within groups. Formally, equation (3) implies that the ratio of the variance of mean outcomes across groups to the variance of outcomes within groups (scaled by group size m) is increasing in γ.

$$\text{Between Section Variance} = Var\left(\overline{Y}_{sc}\right) = (\tau+\rho)^2\, Var\left(\overline{v}_{sc}\right) \tag{5a}$$

$$\text{Within Section Variance} = Var\left(Y_{isc}|s\right) = \rho^2 Var\left(v_{isc}|s\right) \tag{5b}$$

$$\text{Variance Ratio} = \frac{m \cdot Var\left(\overline{Y}_{sc}\right)}{Var\left(Y_{isc}|s\right)} = (1+\gamma)^2\, \frac{m \cdot Var\left(\overline{v}_{sc}\right)}{Var\left(v_{isc}|s\right)} \tag{5c}$$

Under random assignment to peer groups ($v_{isc} \sim iid$ within a class year c), the ratio of the scaled between- to within-section variance of fundamentals is equal to unity ($m \cdot Var\left(\overline{v}_{sc}\right)/Var\left(v_{isc}|s\right) = 1$), so the variance ratio of outcomes reduces to the following:

$$\text{Variance Ratio (under random assignment)} = (1+\gamma)^2\,. \tag{6}$$

It is now clear how peer interactions can contribute to clustered outcomes across peer groups. Outcomes are clustered when the variation across groups is large relative to the variation within groups. In the case of random assignment, peer influence increases the variance ratio from unity to $(1+\gamma)^2$. More generally, if groups are not randomly assigned and there are existing fundamental

[17] If there is a change in fundamentals, the peer multiplier will operate regardless of whether peer influence acts through reactions to fundamentals ϕ or reactions to outcomes θ. However, amplification of shocks to outcomes $\overline{Y}_{sc}$ depends on the relative values of ϕ and θ. Consider shocks to outcomes that leave fundamentals unchanged, such as relaxation of anti-takover legislation. If $\theta = 0$, acquisition activity will directly respond to the regulatory change but there will be no multiplier effect. If $\phi = 0$, the aggregate response to the regulatory change will be larger than the direct response by the full $(1+\gamma)$. For intermediate cases in which ϕ and θ are both strictly positive, estimates of $(1+\gamma)$ provide an upper bound on the peer multiplier with respect to shocks to outcomes that leave fundamentals unchanged.

differences across groups, such that $\frac{m \cdot Var(\overline{v}_{sc})}{Var(v_{isc}|s)} > 1$, peer influence will amplify existing differences across groups by the factor $(1+\gamma)^2$. While this paper focuses on HBS sections to credibly identify peer effects, peer groups might be defined by region, industry, or even intertemporally. Peer influence can potentially contribute to the clustering of firm policies and financial activities across these other groups by amplifying fundamental differences across these groups.

The variance implications of equation (6) are also useful because they allow estimation of the peer elasticity γ even when individual fundamentals v_{isc} are unobserved. Under random assignment to sections, it is sufficient to measure the extent to which outcomes Y_{isc} are more similar within sections than across sections among graduates of a given class year. In the next two sections, I formalize these measures of group similarity.

4.2 The Pairs Distance Metric

The empirical analysis relies on two metrics, the Pairs Distance Metric and the Excess Variance Metric, each with trade-offs as discussed below. Both metrics offer reduced-form measures of section peer similarity that, under the additional structural assumptions of the Linear-in-Means Model, offer an estimate of the peer elasticity γ.

The Pairs Distance Metric measures whether the mean absolute distance in outcomes between two section peers is less than the distance between two class peers. Estimation follows a two-stage estimation procedure similar to that used in Fracassi (2008).[18]

$$\text{1st Stage}: Y_{it} = a_0 + a_1 X_{it} + \widetilde{Y}_{it} \tag{7a}$$

$$\text{2nd Stage - Levels}: \left|\widetilde{Y}_{it} - \widetilde{Y}_{jt}\right| = \beta_0 + \beta_1 \cdot I_{ij}^{\text{section peers}} + \varepsilon_{ijt} \tag{7b}$$

$$\text{2nd Stage - Changes}: \left|\left(\widetilde{Y}_{it} - \widetilde{Y}_{i,t-1}\right) - \left(\widetilde{Y}_{jt} - \widetilde{Y}_{j,t-1}\right)\right| = \beta_0 + \beta_1 I_{ij}^{\text{section peers}} + \varepsilon_{ijt} \tag{7c}$$

Here, i indexes individuals and t indexes firm fiscal years in the panel data. Observations in the first stage are unique at the individual $\times$ fiscal year level. In the first stage, the executive outcome of interest Y_{it} is regressed on X_{it} which can consist of individual, firm, industry, and time controls. Residuals $\widetilde{Y}_{it}$ from the first stage regression measure the unexplained component of Y_{it} and are used in the second stage. The purpose of controls in the first stage is to allow estimation of "excess" peer influence, e.g., peer similarities in compensation beyond what can be explained

[18] Fracassi (2008) regresses the absolute distance between pairs of peers on a set of explanatory variables representing the strength of the relationship between the two individuals comprising each pair. The pairs distance metric used in this paper differs because it uses a random assignment setting to regress pairs distance on a dummy for whether peers are in the same peer group and adopts a Linear-in-Means framework to estimate an implied peer elasticity.

by observable selection into similar firms and industries (selection into similar firms can be a true peer effect operating through past interactions; tests of "excess" peer influence help to narrow the mechanism through which peer effects operate).

In the second stage, I create all possible pairs of executives who graduated in the same class year from HBS and exist in the same firm fiscal year. Note that executives graduating from different class years or working in different firm fiscal years t are never paired. The unit of observation in the second stage is a pair of executives in a given fiscal year. If we are interested in peer similarities in levels of outcomes, the dependent variable is equal to the absolute value of the pair difference in first stage residuals $\widetilde{Y}_{it}$. Alternatively, if we are interested in peer similarities in changes in outcomes, the dependent variable is the absolute value of the difference in changes in the first stage residual $\left(\widetilde{Y}_{it} - \widetilde{Y}_{i,t-1}\right)$. The pair absolute distance is then regressed on a dummy variable $I_{ij}^{\text{section peers}}$ for whether i and j are section peers (graduated from the same section in the same class year).

The identifying assumption is straightforward: whether a given pair of executives graduating in the same class year are section peers or class peers (different sections in the same class year) is exogenously determined by the random assignment of students to sections. β_0 is the mean distance between two class peers and $\beta_0 + \beta_1$ is the mean distance between two section peers. An informative reduced-form statistic is the distance ratio δ^{PDM} equal to the fractional difference in the expected distance between a pair of section peers and a pair of class peers:

$$\text{Distance Ratio} \quad \delta^{PDM} \equiv 1 - \frac{E\left[|Y_{isct} - Y_{jsct}|\right]}{E\left[\left|Y_{isct} - Y_{js'ct}\right|\right]} \tag{8}$$

$$\widehat{\delta}^{PDM} \equiv -\frac{\beta_1}{\beta_0} \tag{9}$$

A $\widehat{\delta}^{PDM}$ significantly greater than zero is evidence of positive peer effects. For example, $\widehat{\delta}^{PDM}$ equal to 0.10 implies that the average absolute distance between a pair of section peers is 10 percent less than the average distance between a pair of class peers. In other words, section peers are 10 percent more similar than class peers.

Assuming that peer interactions follow the Linear-in-Means Model described in Section 4.1, the distance ratio $\widehat{\delta}^{PDM}$ can be used to solve for the peer elasticity γ. Under random assignment of students to sections, fundamentals v_{isc} are distributed iid with variance σ_v^2 within a class year. Let m equal the peer group size. To derive analytic solutions, I impose an additional normality assumption that $v_{isc} \sim N\left(\mu, \sigma_v^2\right)$. This assumption will be relaxed in the next section. Using equation (3), the expected absolute distance between two section peers and two class peers is:[19]

$$E\left[|Y_{isct}-Y_{jsct}|\right]=\rho\sigma_v\frac{2}{\sqrt{\pi}}\quad,\quad i\neq j \tag{10a}$$

$$E\left[\left|Y_{isct}-Y_{js'ct}\right|\right]=\left\{\frac{1}{m}\left(\left(\gamma+1\right)^2-1\right)+1\right\}^{1/2}\rho\sigma_v\frac{2}{\sqrt{\pi}}\quad,\quad i\neq j\text{ and }s\neq s'. \tag{10b}$$

By rearranging terms in equations (10a) and (10b), γ can be expressed as a function of the distance ratio δ^{PDM}:

$$\gamma=\left\{m\left[\left(\frac{1}{1-\delta^{PDM}}\right)^2-1\right]+1\right\}^{1/2}-1. \tag{11}$$

Note that γ is strictly increasing in m, equal to the number of individuals in the section peer group that each individual responds to (m does not necessarily equal the number of executives in the data sample). For all empirical estimates, I assume a conservative value of $m=2$. This assumption reflects the fact that, on average, only three students per section become CEOs or CFOs who appear in the ExecuComp database. Thus, estimates of γ should be viewed as a conservative lower bound for the true γ.[20]

4.3 The Excess Variance Metric

Peer influence will tend to reduce the variance of outcomes within peer groups relative to the variance across groups. The Excess Variance Metric offers a reduced-form measure of the extent to which the across-section variance exceeds the between-section variance in each class year. Estimation follows a standard ANOVA decomposition of variance framework. The variance decomposition can be applied to raw outcomes Y_{isct} or residual outcomes $\widetilde{Y}_{isct}$ from a first stage regression of outcomes on a set of controls, as described in equation (7a). The scaled within-section sum of squares is defined as

[19] The solution uses the following result for the folded normal distribution: if $X\sim N\left(\mu,\sigma^2\right)$, then $E\left[|X|\right]=\sigma\sqrt{\frac{2}{\pi}}\exp\left(-\frac{\mu^2}{2\sigma^2}\right)+\mu\left[1-2\Phi\left(-\frac{\mu}{\sigma}\right)\right]$

[20] Assuming that group size $m=2$ offers a conservative estimate of the true peer elasticity that does not rely on assumptions for the sampling rate. The Linear-in-Means Model implies that the peer elasticity is increasing in the square root of group size, so the true magnitude of the peer elasticity may be larger if there are other HBS graduates in executive roles not covered by ExecuComp. However, recent work such as Carrell et al. (2009) and Carrell et al. (2011) have shown that the Linear-in-Means framework may not scale well to larger groups in which peer responses become non-linear and individuals begin to react to subgroups. Further, alternative models can allow for peer effects which need not vary with group size (e.g. neighborhood effects models as described in Blume et al., 2010 and Ioannides and Topa, 2011). Therefore, while the Linear-in-Means Model offers a reasonable approximation of behavior within the very small executive peer groups studied in this paper, the lower bounds estimated for γ are unlikely to substantially underestimate the true magnitude of peer influence within HBS executive networks.

follows, where m_{sct} is the number of observations in a section $\times$ fiscal firm year:

$$SS_{sct}^{W} \equiv \frac{1}{m_{sct}\left(m_{sct}-1\right)} \sum_{i=1}^{m_{sct}} \left(Y_{isct} - \overline{Y}_{sct}\right)^{2} \tag{12}$$

The between-section sum of squares is defined as:

$$SS_{sct}^{B} \equiv \left(\overline{Y}_{sct} - \overline{Y}_{ct}\right)^{2} \tag{13}$$

An informative reduced-form statistic is the excess variance ratio, defined as the ratio of the between-section sum of squares to the within-section sum of squares:

$$\text{Excess Variance Ratio } \delta^{EVM} \equiv \frac{E\left[SS_{sct}^{B}\right]}{E\left[SS_{sct}^{W}\right]} - 1 \tag{14}$$

To limit the bias from potential outliers, I impose the restriction that peer effects are equal across class and firm years, and estimate $E\left[SS_{sct}^{B}\right]$ and $E\left[SS_{sct}^{W}\right]$ using the full sample before forming the ratio. Under the null hypothesis that section divisions do not matter (random assignment and no peer effects), the expected excess variance ratio is equal to zero. Therefore, an excess variance ratio δ^{EVM} equal to 0.3 implies that the ratio of between- and within-section variances is 30 percent greater than expected under the null.

Adopting the additional assumptions of the Linear-in-Means Model, the excess variance ratio δ^{EVM} offers an estimate of the peer elasticity γ. As before, the random assignment of students to sections allows the assumption that fundamentals v are distributed iid with variance σ_v^2 within a class. Using the fact that $E\left[SS_{sct}^{B}\right] = Var\left(\overline{Y}_{sc}\right)$ and $E\left[SS_{sct}^{W}\right] = Var\left(Y_{isc}|s\right)/m_{isc}$, equations (5a) and (5b) imply:

$$\gamma = \left(1 + \delta^{EVM}\right)^{\frac{1}{2}} - 1. \tag{15}$$

There are several trade-offs relevant to the Pairs Distance and Excess Variance Metrics. The Excess Variance Metric relies on the familiar ANOVA model and results are easily comparable to previous work on peer effects (e.g., Graham, 2008 and Glaeser, Sacerdote and Scheinkman, 2003). Further, estimation of γ does not require the assumption of the normality of individual fundamentals v_{isc} as in the case of the Pairs Distance Metric. However, the Pairs Distance Metric is more robust to outliers bias because it relies on absolute distance rather than squared terms. It is also considerably more flexible.[21] Therefore, estimates using both metrics are presented in the baseline results and

[21]The Excess Variance Metric is less flexible because it uses the full distribution of outcomes by sections within

the Pairs Distance Metric is used for extensions that require additional flexibility.

4.4 Estimation of Standard Errors and Significance Levels

Estimation of standard errors and significance levels are complicated by the following issues. First, observations in the Pairs Distance Metric represent pairs of executives, so each executive can appear in multiple paired observations. Second, estimates from the Excess Variance Metric come from an ANOVA decomposition which does not generate an implied standard error. Finally, executive outcomes come from panel data which may exhibit serial correlation.

Estimation of standard errors and significance levels use a non-parametric permutation test in the style of Fisher (1922) and Rosenbaum (1996). Intuitively, the permutation test constructs a confidence interval of placebo estimates around the null hypothesis that section relationships don't matter and offers an estimate of how unlikely we are to observe the true point estimates by chance. I begin by estimating the vector for the parameters of interest $\hat{\beta}$ using the real data. Next, I conduct a Monte Carlo simulation of placebo effects. In each placebo test, students within each class year are randomly shuffled into placebo sections. The test is non-parametric in that the number of students assigned to each section follows the distribution of sections in the real data (e.g., if there are four students in section A and three students in section B in a given class year in the real data, this structure is maintained in each placebo test). Further, student assignment to sections remains the same across all panel observations in each placebo simulation; this accounts for serial correlation. Using the simulated section assignment in each placebo test, I re-estimate the vector of parameters $\hat{\beta}^{placebo}$. 10,000 placebo estimates are simulated, generating a standard error around the null hypothesis and $G(\cdot)$ as the empirical cumulative distribution function of the placebo effects. Applying $G(\cdot)$ to the original point estimates offers a p-value estimate of significance.

In supplementary tests using the Pairs Distance Metric, I also estimate equations (7b) and (7c) allowing for clustering of the error term separately by i and j following the double-clustering algorithm outlined in Cameron, Gelbach and Miller (2010) and Peterson (2008). Significance levels estimated using double clustering are very similar to those from the permutation placebo tests.[22]

each class year. In contrast, the Pairs Distance Metric uses observations at the pairs level so pairs of observations for executives in the same industry can be dropped from the sample for tests of "excess" peer influence. The Pairs Distance Metric also allows measurement of similarity between actual outcomes and peers' predicted outcomes (described in Section 5.4 with regard to tests for pay for friend's luck).

[22] I focus on permutation test estimates of significance because double-clustering standard errors cannot fully account for correlated error structures across paired observations. For example, consider three pairs of individuals: (A B), (A C), and (B C). Double clustering will allow for correlations between the first and second pairs and the second and third pairs, but will not allow for correlations between the first and third pairs. I thank Mitchell Peterson for this observation.

5 Results

5.1 Verifying Balanced Conditionally-Random Section Assignment

The empirical model assumes that section assignment is a random lottery. In practice, HBS follows balanced, conditionally-random, section assignment. As shown in Appendix A, balanced section assignment leads to a small bias against findings of positive peer effects relative to the case of random, lotteried, section assignment. In this section, I present empirical evidence that HBS practices balanced section assignment. Under balanced section assignment, section peers should not be more similar in terms of ex-ante characteristics than class peers (same class year, different section). Panel (A.1) of Table 4 tests this assumption for the sample of all HBS MBA graduates by comparing section and class peer commonalities in terms of citizenship, undergraduate institution, and gender – characteristics that are determined prior to matriculation at HBS and are unlikely to be altered by peer interactions. "Section commonalities" measure the fraction of section peers that share each individual's characteristics. "Class commonalities" measure the fraction of class peers that share each individual's characteristics. The commonalities ratio measures the ratio of section commonalities to class commonalities. I find that all commonalities ratios are slightly but significantly less than one, showing that section peers are not more similar than class peers. In fact, class peers are relatively more likely to share ex-ante characteristics. This empirically supports the theoretical argument presented in Appendix A that balanced section assignment presents a small bias against findings of positive peer effects. In unreported results (available upon request), I also compare section and class peer commonalities separately for each decade of HBS student cohorts and find evidence supporting the assumption that HBS has consistently balanced sections throughout its institutional history.

Panel (A.2) presents the same ex-ante commonalities measures for the executive subsample covering HBS graduates who appear as top earners in ExecuComp. The commonalities ratios are again very close to one, although a commonalities ratio greater than one within the executive subsample would not be evidence against balanced section assignment; it could represent true peer effects that led similar types of MBA graduates to select into the executive subsample (see Appendix B).

5.2 Baseline Measures of Peer Influence

Panel (B) of Table 4 presents estimates of peer commonalities in executive labor market outcomes that are determined *after* graduation. These ex-post commonalities tell a sharply different story relative to the ex-ante measures. Ex-post measures include director overlap, industry choice, and geographic location of firm headquarters. The sample includes all HBS top executives who appear in ExecuComp. Observations are at the individual level. Commonalities are measured over the known employment history of each executive and are not restricted to concurrent overlap.

Panel (B) shows that the overall base rates of commonalities are low, implying that HBS executives are spread across a variety of firms, industries, and regions. For example, a given executive is expected to share Fama French 49 Industry affiliation with only 6.5 percent of his section peers and 5.2 percent of his class peers. However, commonalities are sharply more likely to occur among section peers than among class peers. The commonalities ratios in Panel (B) show that section peers are 23 percent more likely to be employed in firms with overlapping directors (defined as a director employed by firms in the employment history of both peers).[23] Relative to class peers, section peers are about 25 percent more likely to overlap in SIC3 and Fama French 49 industry affiliation and 10 and 60 percent more likely to overlap in firm headquarter state and city locations, respectively.

The section and class commonalities in Panel (B) can further be compared to base rates of commonalities among all HBS executive graduates (irrespective of class or section divisions) and all ExecuComp executives, presented in the right-most two columns. Class commonalities are around 20 percent greater than the base rates presented in these last two columns, suggesting that substantial peer effects may also operate among class peers. However, the extent to which class commonalities exceed base rate commonalities may also be due to selection into each HBS class year and changes in curriculum over time. Therefore, this paper identifies a lower bound for peer effects using the marginal increase in commonalities among section peers relative to class peers.

In Panel (C), I test for peer influence in terms of selection into the ExecuComp subsample, e.g., if peers help each other attain high-level management positions. I find that section peers are not more likely to appear in the ExecuComp data relative to class peers. However, the outcome measure is the dummy variable for whether an HBS graduate appears in ExecuComp (covering S&P 1500 firms), which may be a very noisy approximation for what it means to be an "executive" in the more general sense. When outcomes are measured with error, estimates of peer effects using the distribution of outcomes within and across peer groups will biased toward zero (Graham, 2008). Therefore, data limitations prevent strong conclusions about the magnitude of peer effects in terms of selection into becoming executives. However, conditional on appearing in the ExecuComp subsample,

[23] Excluding observations corresponding to firms with overlapping board members does not change estimated peer effects in executive compensation or acquisitions behavior, implying that these peer similarities are not driven by peer effects leading to director overlaps.

firm outcomes (particularly executive compensation and acquisition outcomes) are relatively well measured and allow for the tests presented in later tables.

Altogether, Table 4 shows that section peers are not more similar than class peers prior to matriculation at HBS, but have sharply more similar ex-post outcomes, conditional on becoming executives. These results demonstrate that peers can significantly affect the career trajectories of executives and are complementary to survey evidence from Kaniel, Massey and Robinson (2010) showing that MBA peers are able to predict a graduate's career path. Note also that since firm, industry, and location choices typically occur before executives begin making management decisions in their firms, Table 4 illustrates the importance of "past" social interactions in determining executive career paths.

Next, I explore peer influence in executive decision-making with respect to the following firm policies: executive compensation, acquisitions strategy, investment, financial policy (leverage, dividends, interest coverage, cash holdings), and firm size. Table 5 presents baseline estimates of peer effects in executive compensation. In this and future tables, I restrict the sample to CEOs/CFOs, unless otherwise noted, because CEOs/CFOs have greater control over firm outcomes than other top executives. However, results for the full set of top earners in the ExecuComp database will be presented in later tables. Executive compensation can take many forms. For completeness, results are presented separately for the logs of direct compensation (sum of salary and bonus) and total compensation (sum of direct- and equity-linked compensation). Further, it is not obvious whether peers will influence compensation levels or compensation growth, so both are presented in the baseline results. Extensions of the baseline tests will focus on annual changes, because changes in compensation are more useful for identifying responses to shocks over time. Finally, all estimates use compensation outcomes that are measured in the same firm fiscal year. The estimates are also consistent with a story in which peers react to one another with a time lag. Lags are discussed in detail in Section 5.4.

Column (1) presents baseline evidence of peer influence in direct compensation. Panels (A.1) and (B.1) show results using the Pairs Distance Metric, δ^{PDM}, which measures the extent to which section peers are more similar on average than class peers. I find that section peers are 7.4 percent more similar than class peers for annual levels of direct compensation and 11 percent more similar for annual changes in direct compensation.

Panels (A.2) and (B.2) of Column (1) tell a similar story using the Excess Variance Metric, δ^{EVM}, which measures the extent to which the ratio of the variance of outcomes *across* sections to the variance of outcomes *within* sections is greater than expected under the null hypothesis of no peer effects. For both levels and changes in direct compensation, the ratio of between- to within-section variance is roughly 50 percent greater than expected under the null.

Under the additional assumptions of the Linear-in-Means Model, both metrics imply a sizable lower bound for γ, the peer elasticity, of around 20 percent. This means that the individual response to a one unit change in mean peer group fundamentals is 20 percent as strong as the individual response to a one unit change in one's own fundamentals. For example, we would expect an executive to receive an extra $200K in direct compensation if a change to the mean fundamentals of section peers lead to a $1 million increase in mean section peer direct compensation (equal to one standard deviation in levels of direct compensation).

The peer elasticity directly translates into a peer multiplier for the levels and variance of aggregate outcomes (described in Section 4.1). A peer elasticity of 20 percent implies that the aggregate response to a change in the fundamental determinants of direct compensation will be 20 percent larger than the direct response of any individual firm to the change in fundamentals, due to contagion across peers. Similarly, the scaled variance of mean outcomes across different peer groups will be up to 50 percent larger than what we'd expect given fundamental differences across groups, i.e., outcomes will exhibit excess clustering across peer groups.

Column (3) of Table 5 repeats the baseline analysis for peer influence in total compensation (direct plus equity-linked compensation). In general, peer effects for total compensation are not significant. Point estimates can be economically large – the peer elasticity is 10 percent for levels of total compensation – but tend to be smaller than those estimated for direct compensation. Total compensation is discussed in detail in Section 5.5, where I find significant and large peer effects in total compensation using the Forbes compensation data which covers an earlier time period. Section 5.5 also describes institutional structures relating to equity-linked compensation (such as multi-year cycles and lumpiness) which confound analysis of total compensation in the more recent sample period. For now, I focus on estimates using direct compensation.

The baseline compensation results in Columns (1) and (3) of Table 5 purposely use the minimum set of controls in the first stage: demographics controls and year fixed effects. Results represent the overall effect of peers on compensation. Specifically, compensation could be relatively more similar among section peers because past social interactions led section peers to differentially select into the ExecuComp subsample and to enter similar firms, industries, and geographical regions as documented in Table 4. Selection into similar firms is a true peer effect but operates through past interactions (see Appendix B).

Columns (2) and (4) test for "excess" peer influence in direct compensation, i.e., peer similarities in compensation beyond what can be explained by observable similarities in firm and industry trends. In these tests, I include controls for firm and industry SIC3 current and lagged fiscal year returns, firm size as measured by the log of fiscal year sales, and Fama French 49 industry fixed effects interacted with firm fiscal year fixed effects. In order to estimate the coefficients for the controls as efficiently as possible, the full ExecuComp sample is used in the first stage, although second stage results only use compensation residuals from the first stage that pertain to the HBS sample. Estimates in Panel (B) of Columns (2) and (4) also exclude observations representing executive transitions to different firms. All estimates using the Pairs Distance Metric also exclude pairs of executives belonging to the same Fama French 49 industry and pairs in which at least one executive is employed in the financial sector. Excluding these pairs of executives further reduces the likelihood that compensation similarities are driven by industry interdependencies among section peers. Overall, the distance ratios, excess variance ratios, and implied peer elasticities γ remain stable and significant as more controls and sample restrictions are introduced. The peer elasticities for direct compensation consistently exceed 18 percent and estimates for total compensation remain stable or increase in magnitude.[24]

Table 6 turns to evidence showing that peer effects can lead to similar acquisitions strategies. Two measures of acquisitions are used: the attempted acquisitions dummy and completed acquisitions dummy. As discussed in Section 3.2, attempted acquisitions are informative of the intentions of executives even if the acquisitions ultimately fail. Results are similar using alternative measures of acquisition value (see Table 10). I find that executives are more likely to acquire when their section peers acquire than when their class peers acquire. Distance ratios in Panel (A) Columns (1) and (3) show that section peers are 11 percent more similar in terms of acquisition levels, as measured by both acquisition attempts and completed acquisitions. Excess variance ratios in Panel (B) Columns (1) and (3) show that the ratio of between- to within-section variance is more than 35 percent greater than expected under the null hypothesis of no peer effects.[25] The baseline distance

[24]The addition of controls in the first stage may over-control for shared firm characteristics that are chosen by executives as the result of contemporaneous peer interactions. For completeness, I present results with and without additional controls. In some specifications, peer effects are insignificantly stronger with the inclusion of controls for firm and industry trends. This could reflect either improvements in the precision of outcome measures or the case in which executives respond more to the industry-adjusted outcomes of their peers rather than the raw outcomes.

[25]Fracassi and Tate (2011) find related evidence that firms with more CEO-director connections make more frequent acquisitions, which destroy shareholder value on average. In contrast, I focus on social connections outside the firm (rather than close CEO-director connections) and show that executives are more likely to acquire when peers in their education networks acquire, even when these peers work in distant industries.

ratios and excess variance ratios in Columns (1) and (3) imply significant peer elasticities in the range of 13 to 25 percent. In other words, the expected change in the probability of attempting or completing an acquisition in response to a one unit change in mean section peer fundamentals is at least 13 percent as large as a response to a one unit change in own fundamentals.

As with compensation, the peer elasticity translates into a peer multiplier for the levels and variance of aggregate acquisitions outcomes. If there is a shock to the fundamental determinants of acquisition activity, the aggregate response to the shock will be 13 to 25 percent larger than the direct response of any individual firm due to contagion among peers. Similarly, acquisition activity across peer groups boundaries will appear 35 more clustered than what we'd expect given fundamental differences in the determinants of acquisition activity across peer groups.

In Columns (2) and (4), additional controls and sample restrictions are introduced to explore excess peer influence beyond what can be explained by past interactions which may have led to selection into observably similar firms. For example, the industry x year fixed effects control for industry-level waves in acquisition activity. Relative to the baseline results, the estimates drop slightly and imply peer elasticities ranging from eight to 15 percent. The drop in estimated magnitudes suggests that a portion of the peer similarities in firm acquisition policy could be due to past peer interactions leading to selection into similar firms and industries. However, peer effect magnitudes remain sizable and significant even with controls and later tests using reunions will offer more concrete evidence of contemporaneous peer influence.

Finally, Table 7 estimates peer influence in other firm policies including investment, firm size, and the set of proxies for financial policy used in Bertrand and Schoar (2003): leverage, dividends, interest coverage, and cash holdings. Columns (1), (3), and (5) use the minimum set of controls in the first stage to capture overall peer influence, including similarities in firm policies resulting from past peer interactions leading to selection into similar types of firms and industries. I find large and significant peer elasticities ranging from 15 to 25 percent for investment and firm size, which is consistent with the findings of large peer effects in acquisitions – acquisitions represent a large discrete form of investment and contribute substantially to firm size. I also find economically large but marginally significant peer elasticities of 10 to 20 percent for leverage and interest coverage. On the other hand, peer elasticities for dividend policy and cash holdings are zero or slightly negative. However, the associated standard errors are large, so these are not precisely estimated zero effects.

Columns (2), (4), and (6) estimate "excess" peer influence, i.e., peer similarities beyond what can be explained by observable peer similarities in firm and industry trends. While the magnitude of the peer elasticities for investment, interest coverage, and firm size remain economically meaningful at 10 to 20 percent, point estimates generally decline and become less significant. This suggests that a portion of the peer similarities in these other firm policies was driven by past peer interactions leading to selection into similar types of firms and industries.

It is important to note that, even though these estimates are less significant than those for compensation and acquisitions, we should not *definitively* conclude that excess peer effects are weaker for these other firm policies. As shown in Graham (2008), estimates of peer effects that use the distribution of outcomes within and across groups will be biased toward zero if outcomes are measured with error. Direct compensation and acquisitions activity tend to be more precisely measured and also vary significantly over the sample period, allowing for more power in estimation with panel data. In comparison, other firm policies such as dividend distributions tend to be slow-moving and may be noisy proxies for the true policy targeted by the manager (e.g., executives may vary in whether they target the dividend level, the dividend-to-price ratio, or the dividend-to-earnings ratio). Therefore, it remains possible that there are large excess peer effects in these other firm policies. Unfortunately, the limited size of the HBS random assignment sample precludes strong conclusions, and I leave more in-depth analysis of peer influence in these other firm policies to future work. In the remainder of the analysis, I focus on exploring the mechanisms behind the strong and significant estimates of "excess" peer effects in compensation and acquisitions.

Tests of "excess" peer influence in Tables 5 and 6 show that peer effects in compensation and acquisitions are not driven only by past peer interactions leading to selection into *observably* similar firms, but rather are suggestive of the role of contemporaneous interactions. However, the addition of firm controls cannot rule out entry into *unobservably* similar firms. In the next set of results, I explicitly test for the underlying mechanisms behind peer influence in compensation and acquisitions. Further analysis of compensation and acquisitions also serves the purpose of showing that it is not by chance that compensation and acquisitions generate large estimates relative to the other firm policies tested in Table 7. Only true peer effects in compensation and acquisitions are likely to lead to reunion-year effects or reactions to lucky shocks to peers as shown in the next sections.

5.3 Alumni Reunions: The Timing of Social Interactions

I begin by exploring the timing of social interactions. Past interactions describe peers who leave business school holding similar management philosophies or select into similar types of firms (this includes selection into the ExecuComp subsample). Meanwhile, contemporaneous interactions describe interactions that occur while executives manage firms and are more informative about how peers directly affect firm policies.

To determine the timing of social interactions, I use the natural experiment of alumni reunions, which occur every five years after each executive's specific graduation year. Because reunions occur in the same time period as firm policy measures, reunions act as exogenously-timed shocks to the strength of contemporaneous peer bonds. Reunions at HBS are extravagant four day celebrations consisting of formal galas and group discussions, as well as section-based tents and parties. Reunions are well attended – for example, more than 40 percent of the class of 1985 registered to attend the Fall 2010 reunion celebrations. However, reunion year effects do not only operate through direct attendance. Reunions also intensify accompanying activities and communication. For example, during reunion contribution campaigns, each graduate is contacted by volunteers from her section with requests for donations, with wealthy executives receiving extra attention. HBS administrators collect information on the financial well-being its graduates through direct research and through surveys of its alumni. This information is then disseminated to section-based reunion contribution campaign volunteers and provides another avenue through which graduates can become more aware of their peers' activities. Individual donation amounts and section-based giving records are then published in a brochure that is mailed to all graduates. In addition, graduates are encouraged to update their personal information and accomplishments in the "Class Notes," a directory for alumni news. These formal updates may be supplemented by informal activities which coordinate around the reunion schedule. For example, as described in Section 2, Group members in the Class of 1949 hosted informal gatherings following formal reunion activities.

Importantly, some of the information that is disseminated in the reunion year is likely to be publicly accessible due to disclosure requirements for executive compensation and M&A. However, the reunion year can still affect behavior if reunions increase the salience of section peer outcomes. For example, an executive can easily look up the compensation of all executives in large public firms, but he may care more about the compensation of his particular section peers following a reunion. In addition, section peers may increase contact with one-another following reunions and jointly plan actions that are not public information (e.g., to bargain harder for raises next year or to take advantage of financing conditions by increasing acquisitions activity).

Reunion cycle variation in peer effects also offers a check on potential bias from section-specific common shocks, such as a professor who teaches students in a particular section to be aggressive in compensation negotiations and acquisitions. Section-specific common shocks are unlikely to generate peer similarities that vary with exogenously-timed reunion shocks.

For the analysis, I do not use measures of reunion attendance because data on attendance is unavailable going back to the 1980s. Moreover, reunion attendance is a endogenous choice that may be correlated with unobserved individual characteristics, so estimates that compare peer similarities among those who do and do not attend reunions may be biased. Instead, I compare average peer similarities for all executive graduates in the year following reunions with peer similarities in other years. In other words, I measure the intent-to-treat effect of the reunion year, under the assumption that the reunion year schedule is exogenously set. To control for possible direct effects of reunions on levels of executive outcomes, I focus only on the extent to which section peers are more similar than class peers following reunions.[26]

Figures 1 and 2 show that the ratio of within- to across- section similarities in executive compensation and acquisitions sharply increase following alumni reunions. Figure 1 plots the distance ratio for annual changes in log direct compensation for each year in the five year reunion cycle. Figure 2 repeats the exercise for acquisition policy, as measured by the acquisition attempt dummy. Both figures show that reunions lead to a sharp increase in section peer similarity (relative to class peer similarity) in the year immediately following reunions, i.e., reunions affect measured peer effects with a one-year lag. This is not surprising given that reunions occur in the summer or fall of each year, so effects may not manifest in terms of firm outcomes until the following fiscal year. Section peers are 25 and 13 percent more similar than class peers in the year immediately following reunions for compensation and acquisitions, respectively. Peer effects in the year following reunions are more than twice as large as peer effects in the other four years of the reunion cycle.

Table 8 presents more detailed evidence that peer influence becomes stronger following reunions. Panel (A) examines annual changes in compensation. For direct compensation, the distance ratio and peer elasticity in the year following reunions is two to four times larger in magnitude than in the other four years of the reunion cycle. For total compensation, peer effects following reunions are also larger than in other years, although the estimates are noisier. The results are robust to the addition of firm and industry controls in Columns (2) and (4).

The above analysis focuses on annual changes in compensation. In unreported results (omitted for brevity) for annual *levels* of compensation, I find that peer similarities are again higher after reunion years, with distance ratios of around 9 percent following reunions relative to 6 percent

[26] In theory, reunions may also increase similarities among class peers or across class years (e.g., the classes of 1960 and 1965 have their reunions at the same time). In unreported results, I do not find significant increases in similarities across sections or across class years. The reunion year appears to primarily strengthen bonds among section peers.

in other years. However, the difference in effect sizes is not significant. This is likely due to the fact that levels of direct compensation are more strongly serially correlated than changes in direct compensation, i.e., compensation levels cannot become more similar in reunion year + 1 and then sharply less similar in reunion year + 2.

Panel (B) repeats the analysis for annual levels of acquisitions. For both acquisition attempts and acquisition completions, the distance ratio is at least twice as large in the year following reunions as in other years. Magnitudes are again large – section peers are around 10 to 20 percent more similar than class peers in the year following reunions and the implied peer elasticity γ exceeds 20 percent. P-values at the bottom of each panel test whether the distance ratio in the year following reunions is equal to the distance ratio in other years. Equality can be rejected at around the ten percent level or lower for all cases except total compensation, which may be noisier for reasons described in Section 5.5.

Note also that excess peer elasticities in *non-reunion* years ranges from 15 for direct compensation to less than 5 percent for acquisitions. This does not necessarily imply that contemporaneous peer effects in acquisitions are near zero in non-reunion years. As shown in Section 6.2, peer effects in acquisitions can be very large even in non-reunion years if we focus on the subset of graduates who donate to HBS, i.e., those who are more likely to have close bonds with their alumni peers.

Under the assumption that only the marginal increase in peer similarities following reunions reflects contemporaneous interactions, contemporaneous interactions can lead to substantial peer elasticities of over 20 percent. These numbers may be overly conservative in the likely event that a steady level of contemporaneous interactions occur in all years, which are then magnified during reunion years. These calculations are described in Appendix B.

5.3.1 Implications and External Validity of Reunions Analysis

The external validity of the reunion analysis does not hinge upon the existence of randomized section assignment and alumni reunions. Reunions are merely a more powerful way to capture contemporaneous interactions among HBS executives. Every executive, including non-HBS graduates, is likely to have a close and influential set of executive peers. This set of close peers may include other executives in the same industry, trading partners, or friends from past educational or employment experiences. The difficulty lies in identifying who, among the numerous people associated with each executive, belongs in this set of very close peers. The benefit of HBS sections is that it allows the econometrician to identify a subset of individuals (randomly-assigned section peers) that are likely to belong to each HBS executive's peer group and when that subset of peers is likely to be the most influential (following alumni reunions). Thus, the magnitude of the peer effects among HBS section peers following reunions offers a guide to how strong contemporaneous peer effects can be within other similarly close executive peer groups. The same reasoning implies that *total* contemporaneous peer effects for a given HBS executive are not necessarily lower in non-reunion years: contemporaneous interactions with section peers could just be replaced by contemporaneous interactions with other non-HBS peers.

The marginal increase in peer similarity following reunions offers a lower bound for true peer effects because most common shocks, such as influential teachers, should affect behavior in ways that are orthogonal to the reunion schedule. However, it is conceivable, although unlikely, that section members receive section-specific common shocks *at* reunions. A large section-specific reunion shock is unlikely to occur because reunion seminars with influential outside speakers usually address class-wide audiences and should not affect the degree of similarity of section peers relative to class peers. Nevertheless, the next section provides an additional check against bias from section-specific common shocks using industry shocks to peers.

5.4 Pay for Friend's Luck: Reactions to Peer Outcomes or Fundamentals?

This section explores whether peer influence reflects reactions to peer fundamentals or peer outcomes. As described in Section 4.1, a reaction ϕ to fundamentals can represent the social transmission of any fundamental determinant of outcomes (e.g., managerial skill/insight or compensation negotiation skills that can be shared through a social network). A reaction θ to peer *outcomes* can represent the effects of relative earnings on compensation (e.g., through "catching up with the Joneses" preferences as described in Luttmer (2005) or a change in the executive's outside options). While both ϕ and θ are peer effects, only θ will generate a social multiplier effect with respect to policies or shocks that affect peer outcomes while leaving peer fundamentals unchanged (e.g., takeover regulations or compensation caps).

It is possible to isolate reactions θ to peer outcomes using shocks to peer outcomes that leave peer fundamentals unchanged, so reactions to peer shocks necessarily occur through the θ channel. The "pay for friend's luck" tests in Table 9 explore one such shock: lucky industry returns as shocks to the compensation of peers in different industries. I adopt a modified form of the second stage of the Pairs Distance Metric:

$$\left|\left(\widetilde{Y}_{it}-\widetilde{Y}_{it-1}\right)-\left(\widehat{Y}_{jt}-\widehat{Y}_{jt-1}\right)\right|=\beta_0+\beta_1 I_{ijt}^{\text{section peer}}+\varepsilon_{ijt}$$

Here, $\widetilde{Y}$ is the residual from the first stage regression of log compensation levels on the controls for firm and industry trends listed in the bottom panel. $\widehat{Y}$ is the peer's predicted "lucky" compensation from a regression, estimated using the full ExecuComp sample, of log compensation levels on the peer's current and lagged fiscal-year industry returns (calculated excluding the peer's firm returns). The distance ratio δ^{PDM} is again defined as $-\beta_1/\beta_0$. A δ^{PDM} significantly greater than zero implies executives react more to changes in section peer's lucky pay than to changes in class peer's lucky pay.

The specification described above extends research on “pay for luck” in Bertrand and Mullainathan (2001), which examines how an executive's compensation responds to lucky shocks in the executive's own industry. However, the analysis in this paper does not require the assumption in Bertrand and Mullainathan that pay for luck necessarily reflects governance or agency problems. Pay that responds to own industry-level returns can be efficient, as argued in Gopalan, Milbourn and Song (2010). It is sufficient to assume that changes in industry returns do not alter the underlying managerial skills and other fundamentals of peer j and to test whether i's compensation responds to j's predicted changes in pay.

A potential concern is that j's lucky industry returns may have a direct impact on i's compensation if i and j work in related firms. This is not a problem for the analysis *per se*, because the distance ratio measures the *relative* similarity of section peers to class peers. However, if section peers belong to *more* related firms than class peers, the direct impact of peers' lucky industry shocks would lead to positive estimates of peer effects even in the absence of true peer influence. This concern is mitigated through the use of i's residual compensation after controlling for i's own firm and industry SIC3 current and lagged fiscal year returns. I also limit bias by excluding all pairs of executives belonging to the same broad Fama French 49 industry classification. It is further reassuring that all estimated magnitudes remain stable if I exclude all executives working in the financial sector (SIC codes 6000-6999) and all pairs of executives in linked industries. Using the BEA input-output tables and following Ahern and Harford (2010), industries are considered linked if a customer industry buys at least two percent of a supplier industry's total output or if a supplying industry supplies at least two percent of the total inputs of a customer industry. Results, not reported, remain similar if the analysis further excludes all pairs of executives in the same Fama

French 5 industry, one of the broadest industry classifications systems available.

Columns (1) and (2) in Panel (A) of Table 9 present the pay for friend's luck tests for direct compensation. Section peers are seven to 12 percent more similar than class peers, even when peers' compensation is due to lucky shocks and detailed controls are included for own firm and industry performance. Results for total compensation, shown in Columns (3) and (4), estimate that section peers are four to eight percent more similar than class peers, although the distance ratio is only significant in Column (4). Significance levels are estimated using the permutation test described in Section 4.4 in which executives are shuffled into placebo sections to show that the point estimates for peer similarities are unlikely to be generated by chance. In these tables, I present distance ratios rather than peer elasticities γ because estimates of γ rely on assumptions about the distribution of outcomes across peer groups, while the pay for friend's luck tests compare the relationship between actual outcomes and peers' predicted outcomes.

5.4.1 Implications of Pay for Friend's Luck

Evidence of pay for friend's luck shows that peer influence can lead to movements in compensation that do not reflect changes to real firm productivity or other fundamental determinants of compensation. Executives (after controlling for own firm performance) are paid more when their friends receive lucky shocks to their compensation. The results are supportive of evidence in Bertrand and Mullainathan (2001) showing that executives are rewarded for more than their effort or skill.

Pay for friend's luck is consistent with two models in which relative compensation directly affects individual compensation. In the first model, relative earnings and status enter directly into an executive's utility function (e.g., Luttmer, 2005).[27] Consider a simple Nash bargaining game between the executive and the board in which the executive's utility from working in the firm is a function of wages and amenities (e.g., job satisfaction). If peer wages increase, job satisfaction decreases (as shown in Card et al., 2011) for any given level of own wages. Holding bargaining power fixed, Nash bargaining implies that an increase in peer wages will lead to an increase in own wages. [28] In the second model, an exogenous increase in peer compensation improves an executive's

[27] "Keeping up with the Joneses" preferences could also lead executives to increase their managerial productivity in order to match or exceed the pay of peers who receive lucky shocks (e.g., Bandiera, Barankay and Rasul, 2010 find evidence of social incentives in worker productivity). However, all specifications include a large set of controls for own firm and industry performance. Pay for friends' luck effects do not appear to operate through increases in observed managerial productivity.

[28] In this model, the executive never systematically leaves money on the table. Suppose that the executive's utility of working in the firm is the sum of wages and amenities: $U = w + a$. The executive has a threat point of $\underline{U}$. Define $\underline{w} \equiv \underline{U} - a$ as the minimum wage such that the executive is willing to work. Suppose the executive generates $X > \underline{w}$ in surplus for the firm. With fixed bargaining power $b \in [0, 1]$, he will earn $w^* = b(X - \underline{w}) + \underline{w}$. It's straightforward to

outside options if the executive can credibly leave to work in his friend's firm or industry. Given that executive transitions are costly, a change in outside options will again lead to higher executive pay. In general, it is difficult to clearly distinguish between these two models, although I present some results in the next few sections supporting the relative earnings utility model. However, regardless of the exact model, the results imply that an increase in the strength of peer effects can lead to changes in compensation that do not correspond to changes in firm or managerial productivity.

Evidence of pay for friend's luck has two other important implications similar to those from the analysis of reunions. First, the measured peer effects are unlikely to be driven by section-specific common shocks such an influential professor, because common shocks should not affect executive behavior that varies over time with industry level shocks to their peers in different industries. Second, pay for friend's luck is evidence of contemporaneous social interactions – past interactions leading into selection into similar types of firms should not lead to compensation that varies over time with lucky shocks to peers.

5.4.2 Lagged Responses

In general, peer similarities could be due to contemporaneous or lagged influences (Abel, 1990). So far, I have presented estimates of contemporaneous peer similarities without taking a stand on whether the true effects are contemporaneous or lagged. Panel (B) of Table 9 modifies the specification to test the relationship between one's change in residual compensation and one's peer's *lagged* change in predicted compensation:

$$\left|\left(\widetilde{Y}_{it}-\widetilde{Y}_{it-1}\right)-\left(\widehat{Y}_{j,t-1}-\widehat{Y}_{j,t-2}\right)\right|=\beta_0+\beta_1 I_{ijt}^{\text{section peer}}+e_{ijt}$$

This test explores whether pay for friend's luck holds with a one year lag between leaders (represented by the person with the predicted lucky compensation) and followers. The estimates of lagged peer effects are very similar to those of contemporaneous peer effects and have slightly higher point estimates – the evidence is strongly consistent with a theory of leaders and followers.

However, lagged effects should only be viewed as exploratory for two reasons. First, peer influence involves symmetric and reflective feedback among peers. To explicitly test for leaders and followers, one must make assumptions identifying the set of leaders. Second, estimates may not represent true *lagged* responses because both outcomes and industry shocks tend to be serially cor-

see that if amenities decrease because of lower job satisfaction, then the threat point increases. Wages must increase, even while holding bargaining power fixed.

related. In Panel (B), I alternatively allow each member of a pair to act as a possible "leader," i.e., I treat peer j as the leader and observe i's reaction to $j's$ lagged predicted lucky compensation and vice versa. However, lucky industry shocks are serially correlated, so a significant distance ratio in the lagged specification does not necessarily imply that i reacts to j's lucky shock with a true time lag. In unreported results that are available upon request, I estimate the baseline results allowing for a one-year lag in peer responses using the Pairs Distance Metric. The results yield significant peer elasticities of up to 20 percent and are consistent with the presence of leaders and followers. However, serial correlation in the outcome measures implies that we cannot reject the alternative theory that peers talk and jointly plan future actions without a time lag.

5.5 Robustness

Table 10 supports the robustness of the baseline results of peer effects in compensation and acquisitions. For brevity, only distance ratios are presented.

Rows (1) addresses the issues that peer effect estimates are significant and large for direct compensation but are less robust for total compensation, defined as the sum of direct and equity-linked compensation. Equity-linked compensation has grown sharply in recent decades (Frydman and Saks, 2010) and accounts for roughly 50 percent of total compensation in the sample. However, institutional features of equity-linked compensation suggest that finding measurable peer similarities is unlikely. Hall (1999) shows that stock options (which comprise the bulk of equity-linked compensation) are distributed according to multi-year plans. In these multi-year plans, the value or number of options granted in any particular year can be set several years in advance at the start of the multi-year cycle. Since firms differ in the timing of their cycles, it may be harder for executives to negotiate for stock options to match that of their peers in any year and the amount that peers earn in any particular year may be less meaningful. In unreported results (available upon request), I test for peer effects in moving averages of total compensation. While I find estimates of peer elasticities of more than ten percent, p-values remain at 20 percent or larger, suggesting a lack of power given the averaging and limited sample size.

However, peer effects in total compensation can be large during the period prior to the rise of equity-linked compensation. To examine this earlier time period, I match HBS alumni records to the Forbes database which covers executive compensation from the years 1970 to 1991 for approximately 800 companies each year. Total compensation during this earlier time period is dominated by cash payments; the mean and median direct compensation as a fraction of total compensation is 84 and

93 percent, respectively.

Importantly, the Forbes database also offers an out-of-sample test of the baseline estimates of peer effects in compensation. The Forbes and ExecuComp samples cover non-overlapping time periods from 1970-1991 and 1992-2009, respectively.

Using the Forbes sample, I find that annual changes in total compensation are 19 percent more similar among section peers than among class peers and the distance ratio is significant at the ten percent level (higher standard errors likely reflect the smaller sample size). This suggests that peer effects for total compensation were large in the period prior to the rise of option grants. However, I cannot conclusively test for peer effects in total compensation or the composition of pay (e.g., percentage of total pay in the form of options) using the more recent ExecuComp sample. Investigation of this question is a promising direction for future research, but is outside the scope of this paper due to the limited sample size and the noisiness of measures of option grants.

The remaining rows in Table 10 support the general robustness of the baseline results for peer effects in annual changes in direct compensation and annual levels of acquisitions. In all results presented thus far, significance levels and standard errors are calculated using the permutation test. Rows (2), (5), and (6) present alternative estimates of significance levels using the double clustering procedure described in Section 4.4. The double clustering approach yields standard errors that are very similar (+/- .003) to those in the baseline estimates. Row (4) uses direct compensation winsorized at the top and bottom one percent levels in the first stage estimation. Row (5) excludes observations after fiscal year 2006 to ensure that results are not driven by a change in SEC compensation disclosure rules.

Rows (7) through (9) of Panel (B) test the robustness of the baseline results for acquisitions using alternative measures of acquisition activity. Row (7) uses the ratio of the total value of acquisitions scaled by lagged firm assets. Data for total acquisitions value comes from the CompuStat database and offers a check on the acquisitions data from the SDC Platinum database used in the baseline results. Row (8) uses the number of acquisitions completed. Row (9) restricts the measure of completed acquisitions to those with known transaction values greater than one million. Note that because of missing deal values for some transactions, this measure will miss some acquisitions with high but unreported transaction values. For all three alternative measures of acquisition activity, the distance ratios exceed ten percent and are highly significant.

6 Extensions

6.1 Peer Influence Among Other Top Earners

With the exception of Table 4, all aforementioned results utilize data on HBS CEOs and CFOs in the ExecuComp database. I restrict attention to CEOs and CFOs because they tend to exercise more control over firm outcomes than other top executives in the firm; manifestation of peer effects in firm outcomes requires both that executives react to peers *and* have the ability to affect changes in firm policies. Table 11 extends the analysis to all top earners in the ExecuComp data who graduated from HBS. Using a modified version of the Pairs Distance Metric, I form all possible pair combinations of executives graduating from the same class year and working in the same firm fiscal year. A pair is classified as a "CEO/CFO pair" if both members of the pair are CEOs or CFOs. Similarly, a pair is consider an "other exec pair" or "mixed pair" if the pair consists of two non-CEO/CFO executives or one CEO/CFO and one non-CEO/CFO, respectively. I estimate a modified form of the Pairs Distance Metric second stage regression in which the absolute distance in pair residuals is regressed on a set of dummies for each type of pair and the interaction between the pair type dummies and the same section dummy. This specification allows for the estimation of separate distance ratios and peer elasticities γ for each of the three types of executive pairs.

I find that distance ratios and associated peer elasticities γ for annual changes in direct compensation and levels of acquisition activity are significant only for the CEO/CFO pairs. Further, the point estimates for the CEO/CFO pairs are at least 2.5 times larger than those for the other two types of executive pairs. P-values at the bottom of the table can marginally reject equality for the distance ratios across the three types of executive pairs at the 0.03 to 0.16 levels. Only in the case of total compensation are the measured peer effects comparable across the three types of executive pairs, although all estimates for total compensation are close to zero with large standard errors.

These results support the hypothesis that peer influence in firm policies will be stronger when the executive has more discretionary power over firm policies, because CEOs and CFOs are likely to have more control over firm policies than lower level executives. Evidence in Table 11 also cuts against a pure human-capital transfer theory in which management skills and insights travel across executive networks. Under the human-capital transfer view, compensation and acquisitions should be correlated among all executive peers, rather than only among CEOs and CFOs. Similarly, these results cut against a pure outside opportunities theory of peer effects in compensation. If peers' compensation (even in distant industries) represent the outside opportunities of executives, then top executives other than the CEO and CFO should also experience similar compensation. However, it is important to note that the evidence is only supportive of the above discussion. The results are also consistent with the alternative hypothesis that CEOs and CFOs share stronger friendship bonds relative to other top earners.

6.2 Participation in Reunion Contribution Campaigns

Peer effect magnitudes should be larger among HBS graduates who have closer ties to their alma mater – these graduates are also likely to have stronger bonds with their section peers. A natural proxy for closeness to HBS would be reunion attendance, but unfortunately historical individual-level data on reunion attendance is unavailable. However, individual-level data on participation in reunion contribution campaigns offers an alternative proxy for the connectedness of graduates to HBS. Reunion contribution campaigns occur every five years after each executive's graduation year and coincide with reunion events. These contribution campaigns are organized around sections; sections that achieve contribution records are awarded special recognition in the annual contributors report mailed to all participating alumni.

Reunion contribution campaigns are intensified versions of annual campaigns. Data on alumni contributions in the years 1990 to 2008 covering the class years 1952 to 2008 shows that an average of 47 percent of alumni participate in each reunion campaign compared to only 26 percent in each non-reunion year campaign. Among HBS alumni who also appear in ExecuComp as CEOs or CFOs, 83 percent contributed to at least one reunion campaign held between 1990 and 2008. In 66 percent of the observations at the executive $\times$ year level, the executive contributed to the preceding reunion campaign. The median amount donated among participating executives falls between $1000 and $2500 (data on donation amounts are binned).

Reunion campaign participation offers a convenient but imperfect proxy for the connectedness of HBS executives, especially during reunion years.[29] This is because executives can attend reunion events without participating in reunion contribution campaigns and vice versa. In addition, executives who give one-time large contributions may choose not to contribute during other reunion campaigns despite being closely involved with reunion activities. To mitigate these concerns, I adopt a smoothed measure of reunion campaign participation. Pairs of executives are considered "donors" if they both contributed at least $1000-$2500 (the median donation category) in at least

[29] Meer (2010) and Meer and Rosen (2010) document peer effects in alumni contributions at an anonymous research university: individuals are more likely to donate when personally solicited, particularly by those in their social networks. In unreported results, I also find that participation in contribution campaigns are more similar among section peers than among class peers.

one reunion campaign held between 1990 and 2008. Donor pairs account for approximately half of all executive pair observations. The results are very similar if donors are instead defined as those who participated (donated any amount) in the reunion campaign immediately preceding the fiscal year of the observation.

Table 11 examines how peer effects in compensation and acquisitions vary with the reunion cycle and participation in reunion campaigns. Four distance ratios are estimated and vary depending on whether the observation occurs in the year following reunions and whether the pair of executives are both donors. I focus on distance ratios because they control for the endogenous choice to donate. In particular, more successful executives may choose to donate, but this should not affect the extent to which donors from the same section are more similar to each other than donors from different sections in the same class year.

For both reunion and non-reunion years, the distance ratios for donor pairs are two to three times larger than those for non-donors. Donor pairs in the year following reunions consistently have the largest distance ratios. While p-values at the bottom of each panel cannot always reject equality of the distance ratios for donor and non-donor pairs, the results are nonetheless very reassuring. Alumni who contribute to reunion campaigns are likely to have the strongest social bonds with their section peers, so their distance ratios should be relatively larger in all years, and particularly large in the year following reunions because they are more likely to be affected by the reunion-year shock to peer bonds.

6.3 Does Peer Influence Lead to More Efficient Acquisitions?

Is peer influence in acquisitions driven by information sharing about valuable investment opportunities? If so, peer interactions can lead to more efficient acquisitions. An ideal empirical test of this question would compare peer-motivated acquisitions with non-peer motivated acquisitions. In practice, even the gold standard of randomly assigned peer groups (e.g., HBS sections) only provides exogenous variation in the composition of a subset of an executive's peer group (his section peers) and exogenous variation in when that subset of peers is likely to be the most salient (after reunions). Most natural experiments cannot provide variation in whether an executive has peers. Executives never live in a social vacuum, so the counterfactual of a pure, non-peer motivated acquisition does not exist.

With these empirical limitations in mind, this section presents exploratory evidence concerning the efficiency of peer motivated mergers. First, I test whether acquisitions following HBS alumni reunions are more likely to exhibit characteristics associated with inefficient acquisitions. The identifying assumption is that acquisitions following reunions are more likely to be peer motivated than acquisitions in other years. To the extent that acquisitions in non-reunion years can also be motivated by peers (including non-HBS peers in an executive's industry or geographic region), this test may be underpowered.

If the market predicts that an acquisition will destroy acquirer value, acquirer returns around the acquisition announcement should be negative. In addition, both diversifying and equity-financed acquisitions are associated with inefficient outcomes in the M&A literature. A large literature going back to Jensen (1986) argues that diversifying acquisitions are linked with poor firm performance because they are symptomatic of managerial agency problems such as empire building or entrenchment. Similarly, Shleifer and Vishny (2003) and others argue that equity financed acquisitions are likely to be driven by the desire to off-load over-valued equity rather than by real merger synergies. Column (1) of Table 13 shows that, relative to acquisitions in other years, acquisitions following reunions are 5.4 percentage points more likely to be diversified. Acquisitions are considered diversifying if the acquirer and target belong to different Fama French 49 industries (results are very similar using SIC2 industry codes). Relative to the base rate (43 percent of acquisitions are diversifying in general), the reunion year leads to a substantial and significant 13 percent increase in the diversification rate. Column (2) shows that acquisitions following reunions are six percentage points more likely to be financed by equity rather than by cash. Relative to the base rate (25 percent of acquisitions are financed with equity), the reunion year again leads to a substantial and significant 25 percent increase in the equity-financing rate. However, in Column (3), I also test whether acquisitions following reunions exhibit lower ex-post acquirer abnormal returns, but find noisy zero estimates.

Next, I test whether executives react asymmetrically to positive and negative (or absent) peer acquisition activity. Consider two peers A and B. Suppose B first chooses whether to conduct an acquisition based upon his private information. If A and B share information about how to conduct "optimal" acquisitions strategy, we would generally expect A to be less likely to acquire if B did not acquire in the previous period and more likely to acquire if B acquired in the previous period. This prediction holds regardless of whether we define "optimal" from the point of view of the social planner (e.g., it's optimal to share information about the availability of targets with high merger synergies) or firm shareholders (e.g., it's optimal to share information about how to time equity markets). In Columns (4) and (5) of Table 13, I find sharp asymmetries in the individual's response to lagged peer acquisition activity. Following reunions, distance ratios are close to 20 percent if a section peer acquired in the previous year, but are close to zero if the section peer did not acquire in the previous year. The asymmetry in reactions is significant at the five percent level. In other words, relative to class peers, an individual is more likely to acquire if his section peer acquired in the previous period, but he is not less likely to acquire if his section peer did not acquire in the previous period. This suggests that peer information transmission is at best only partially efficient. Any information contained in the absence of acquisition activity is ignored.

Overall, the results suggest that peer effects do not lead to more efficient acquisitions. Peers tend to ignore information that could lead to more efficient acquisitions. Further, acquisitions following reunions exhibit characteristics commonly associated with inefficient acquisitions. These results are consistent with an alternative hypothesis that the peer effects in acquisitions are driven by the pursuit of relative social status. However, I caution that these results are exploratory due to the empirical caveats described earlier in this section.[30]

7 Conclusion

I explore how executive social interactions can affect managerial decision-making and firm policies using the historical random assignment of MBA students to sections at Harvard Business School. Under the identifying assumption that social bonds are stronger within randomly assigned sections than across sections in the same class year, I test whether executive and firm outcomes are more similar among section peers than among class peers. I find evidence of significant peer effects in firm investment, leverage, interest coverage, and firm size, with the strongest effects in executive compensation and acquisition activity. Section peers are ten percent more similar than class peers in terms of compensation and acquisitions. Under the additional structural assumptions of the Linear-in-Means Model, I estimate a substantial lower bound for the elasticity of individual outcomes to mean section peer characteristics of 10 to 20 percent.

I show that peers are also important determinants of executive career outcomes such as choice of industry, firm, and geographical locale. However, past peer interactions leading to selection into executive roles and into similar types of firms do not drive peer effects in executive compensation and acquisitions. Rather, the underlying mechanism is due to contemporaneous social interactions: peer similarities in compensation and acquisitions are more than twice as large in the year following

[30] In unreported results (available upon request), I also test whether section peers are relatively more likely to employ a common investment bank advisor. I find no evidence that this is the case. However, even if section peers do hire common intermediaries, this would merely represent the channel through which peer effects operate.

staggered alumni reunions, which act as shocks to contemporaneous interactions. Tests of "pay for friend's luck" further narrow the mechanism driving peer effects in compensation. I find that compensation responds to lucky industry-level shocks to peers in distant industries after controlling for own firm and industry performance. Since these industry shocks alter peer outcomes while leaving peer fundamentals unchanged, peer effects in compensation are not driven only by the sharing of managerial skills within peer networks. Rather, relative compensation among peers directly affects individual compensation. This can occur if compensation negotiations depend on the relative earnings preferences of executives or if lucky shocks to peers change the outside options of executives in different industries.

Executive peer effects imply that executives matter for firm policies in a systematic way that can drive aggregate patterns. Multipliers arising from social interactions imply that the aggregate effect of a change in the fundamental determinants of compensation or acquisition activity will be 20 percent larger than the direct effect because of contagion among connected agents. Positive peer effects also lead to correlated behavior among group members which can amplify fundamental differences across peer group boundaries. Specifically, executive peer effects lead to reduced within-group variation and increased across-group variation, i.e., clustered financial outcomes. In the context of HBS sections, I find that the ratio of between- to within-section variance is 20 to 40 percent greater than expected under the null hypothesis of no peer effects. A similar peer interaction mechanism can potentially amplify fundamental differences across other types of executive peer groups operating at the industry or geographic level.

While executive peer effects have clear implications for our understanding of managerial decision-making, consequences for social welfare are less obvious. Peer effects in acquisitions can be efficient if private information about how to conduct value-enhancing mergers is transmitted through social networks. However, I find evidence that peer influence in acquisitions does not operate through fully efficient information sharing and may lead to weakly less efficient acquisitions. Similarly, peer influence in compensation can lead to movements in compensation that do not reflect changes in firm or managerial productivity. Empirical investigation of the efficiency implications of peer influence among executives is a very promising direction for future research.

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Appendices

A Balanced Section Assignment

Harvard Business School assigns the entering class of MBAs to equally sized sections. Assignment is random *conditional* on ex-ante student characteristics such as gender, ethnicity, and previous industry experience that the Registrar observes. As the econometrician, I do not observe all conditioning variables. However, under the conservative assumption that the Registrar seeks to create mean-balanced sections, the following proof shows that balanced sectioning generates a bias *against* findings of positive peer effects.

The intuition is straightforward. Suppose there are no true peer effects. As described in Section 4, peer effects are measured using the ratio of the between- to within-section sum of squares of the outcome of interest Y. Under lotteried sectioning (completely random), the expected ratio of the sum of squares of the ex-ante student characteristics X is equal to one. Under balanced sectioning, the ratio of the sum of squares of X should be weakly less than one, because the Registrar attempts to equalize the means of X across sections. Assuming that the measured outcome Y is a monotonic differentiable function of X, the expected ratio of the sum of squares of Y will also be weakly less than one, implying weakly negative measures of peer effects in expectation.

Formally, consider a single class c of HBS MBAs. The total number of students equals n, and the Registrar assigns students to k sections each of size m such that $k \times m = n$. Sections are indexed by $s = 1, ..., k$. X_{is} is the ex-ante student characteristic for student i in section s. For brevity, I omit the time subscript although panel observations will only be compared to others in the same firm fiscal year t. Let $\overline{X}$ be the class mean of X_{is}, and $\overline{X}_s$ be the mean of X_{is} in section s. Let Y_{is} be the ex-post student outcome that is used to measure peer effects. Let the between sum of squares, BSS, equal the sum of squared deviations of the section mean from the class mean: $\sum_{s=1}^{k} \left(\overline{X}_s - \overline{X}\right)^2$.

Assumption 1: *The between sum of squares under balanced sectioning does not exceed the expected between sum of squares under lotteried sectioning:*

$$BSS^{balanced} \leq E\left[BSS^{lottery}\right].$$

Assumption 2: *Absent peer effects, $Y_{is} = f\left(X_{is}\right) + \varepsilon_{is}$ where $f\left(\cdot\right)$ is monotonic differentiable, and ε_{is} is an iid error term with variance σ_{ε}^{2}.*

Assumption 1 is a "do no harm" assumption – in actively trying to balance the mean of ex-ante student characteristics across sections, the Registrar does not do worse than they would have if they had randomly lotteried students to sections. Assumption 2 guarantees that, in the absence of peer effects, the ex-post outcomes are not backward-bending or otherwise perverse functions of ex-ante student characteristics.

Proposition 1. *In the absence of peer effects, balanced sectioning implies that the expected excess* *variance ratio δ^{EVM} and peer elasticity γ (defined in Section 4.3) are weakly negative:*

$$\delta^{EVM\_balanced} = \frac{m \cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]} - 1 \leq 0$$

$$\gamma^{balanced} = \left(\frac{m \cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]}\right)^{\frac{1}{2}} - 1 \leq 0$$

Proof. For any section assignment scheme, standard ANOVA results show that the total sum of squares can be exactly decomposed into the between and within sum of squares:

$$TSS = BSS + WSS$$

$$\sum_{s=1}^{k}\sum_{i=1}^{m}\left(X_{is} - \overline{X}\right)^2 = m\sum_{s=1}^{k}\left(\overline{X}_s - \overline{X}\right)^2 + \sum_{s=1}^{k}\sum_{i=1}^{m}\left(X_{is} - \overline{X}_s\right)^2$$

Note that $E\left[\frac{1}{n}BSS\right] = E\left[Var\left(\overline{X}_s\right)\right]$ and $E\left[\frac{1}{n}WSS\right] = E\left[Var\left(X_{is}|s\right)\right]$, so $E\left[Var\left(\overline{X}_s\right)\right]$ and $E\left[Var\left(X_{is}|s\right)\right]$ are inversely related under any assignment section assignment scheme because TSS remains fixed. First consider lotteried sectioning. Independence of X_{is} implies that the variance ratio of ex-ante student characteristics equals unity:

$$\frac{m \cdot E\left[Var\left(\overline{X}_s^{lottery}\right)\right]}{E\left[Var\left(X_{is}^{lottery}|s\right)\right]} = 1.$$

Letting $\mu \equiv E\left[X_{is}\right]$, Assumption 2 and the delta method imply that variance ratio for outcomes Y also equals unity:

$$\frac{m \cdot E\left[Var\left(\overline{Y}_s^{lottery}\right)\right]}{E\left[Var\left(Y_{is}^{lottery}|s\right)\right]} = \frac{f'\left(\mu\right)^2 m \cdot E\left[Var\left(\overline{X}_s^{lottery}\right)\right] + \sigma_\varepsilon^2}{f'\left(\mu\right)^2 E\left[Var\left(X_{is}^{lottery}|s\right)\right] + \sigma_\varepsilon^2} = 1.$$

Therefore, measures of peer effects under lotteried sectioning equal zero in expectation: $E\left[\delta^{EVM\_lottery}\right] = E\left[\gamma^{lottery}\right] = 0$. Now consider balanced sectioning. Assumption 1 implies that $E\left[\frac{1}{n}BSS^{balanced}\right] \leq E\left[\frac{1}{n}BSS^{lottery}\right]$, i.e., $E\left[Var\left(\overline{X}_s^{balanced}\right)\right] \leq E\left[Var\left(\overline{X}_s^{lottery}\right)\right]$. The sum of squares decomposition implies that $E\left[Var\left(\overline{X}_s^{balanced}\right)\right]$ and $E\left[Var\left(X_{is}^{balanced}|s\right)\right]$ are inversely related. Therefore,

$$\frac{m \cdot E\left[Var\left(\overline{X}_s^{balanced}\right)\right]}{E\left[Var\left(X_{is}^{balanced}|s\right)\right]} \leq 1.$$

Let $\Sigma_s^{balanced}$ be the $m \times m$ covariance matrix for X_{is} within section s under balanced sectioning. The above result, along with Assumption 2 and the delta method, imply that the variance ratio for ex-post outcomes under balanced sectioning is weakly less than unity:

$$\frac{m \cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]} = \frac{f'(\mu)^2 \frac{1}{m} E\left[\iota^{\intercal}\Sigma_s^{balanced}\iota\right] + \sigma_\varepsilon^2}{f'(\mu)^2 \cdot E\left[Var\left(X_{is}^{balanced}|s\right)\right] + \sigma_\varepsilon^2}$$
$$= \frac{f'(\mu)^2 \cdot mE\left[Var\left(\overline{X}_s^{balanced}\right)\right] + \sigma_\varepsilon^2}{f'(\mu)^2 \cdot E\left[Var\left(X_{is}^{balanced}|s\right)\right] + \sigma_\varepsilon^2} \leq 1.$$

Therefore, under balanced sectioning, the expected measures of peer effects are weakly negative in the absence of true peer effects: $E\left[\delta^{EVM\_balanced}\right] \leq 0$ and $E\left[\gamma^{balanced}\right] \leq 0$.

□

B Selection Into the Executive Subsample

Only a small subset (4 - 5 percent) of HBS MBA graduates become top executives who appear in the sample of S&P 1500 firms covered in the ExecuComp data. Given that HBS students are randomly assigned to sections, selection of students into the executive subsample can be a true peer effect, operating through "past" social interactions.

The following simple three stage model illustrates that the baseline results presented in Section 5.2 cannot separately identify peer similarities that are the result of:

1. Contemporaneous interactions, i.e., interactions occurring while executives manage firms
2. Past interactions, i.e., similar people within each section select into the executive subsample, and/or section peers are more likely to enter into similar types of firms, industries, etc.
3. Section-specific common shocks, e.g., professor shock.

Both contemporaneous and past interactions represent true peer effects, but only contemporaneous interactions represent the causal impact of executives on firm policies. While baseline tests cannot separately identify these three effects, tests involving reunions and lucky shocks to peers presented in Sections 5.3 - 5.4 use variation in peer similarities *over time* along with exogenous shocks to isolate peer effects due to contemporaneous interactions.

Stage 1 – Random Assignment to HBS Sections

Consider the full HBS class of MBA students. Let Y_{isc} be the ultimate outcome of interest for person i in section s in class year c. For brevity, I omit the time subscript although panel observations will only be compared to others in the same firm fiscal year t. Let v_{isc} be the latent fundamental determinant of Y_{isc} (e.g., if Y_{isc} is executive compensation, then v_{isc} is the exogenous managerial skill of person i that would determine compensation if person i becomes an executive). Random sectioning guarantees:

$$v_{isc} \sim iid \text{ (within each class year } c\text{)}.$$

Stage 2 - Selection into Executive Roles at Firms

A subset of HBS graduates become top executives in S&P 1500 firms covered by the ExecuComp database according to the following selection rule:

$$f\left(v_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{v} \quad iff \text{ student } i \text{ becomes an executive.}$$

Let ω_{isc} represent the intermediate fundamental determinants of outcomes Y_{isc} as of Stage 2. ω_{isc} is a function $g(\cdot)$ of individual fundamentals v_{isc} (conditional on i becoming an executive) as well as firm fundamentals ε_{isc}, e.g., the firm's q and industry characteristics:

$$\omega_{isc} = g\left(v_{isc} | f\left(v_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{v}, \varepsilon_{isc}\right).$$

Unlike the base fundamentals v_{isc}, intermediate fundamentals ω_{isc} need not be distributed iid within each class year. To the extent that ω_{isc} are not distributed iid, this could be due to past peer interactions or common shocks leading (1) students with similar fundamentals v_{isc} within each section to select into becoming executives and/or (2) students within each section to enter similar types of firms, such that ε_{isc} is more similar within sections than across sections.

Stage 3 - Executives Choose Firm Policies

Now consider only the ExecuComp subsample, consisting of all students i for which $f\left(v_{isc}, \overrightarrow{\mathbf{v}}_{-i,sc}\right) \geq \underline{v}$. As in Section 4.1, I approximate optimal outcomes Y_{isc} as a linear function of mean section outcomes $\overline{Y}_{sc}$, mean section intermediate fundamentals $\overline{\omega}_{sc}$, and own intermediate fundamentals ω_{isc}.

$$Y_{isc} = \theta \overline{Y}_{sc} + \phi \overline{\omega}_{sc} + \rho \omega_{isc}.$$

It is possible to solve for Y_{isc} as a function only of $\overline{\omega}_{sc}$ and ω_{isc}:

$$Y_{isc} = \tau \overline{\omega}_{sc} + \rho \omega_{isc} \quad , \quad \tau \equiv \frac{\phi + \theta \rho}{1 - \theta}.$$

Applying variance restrictions as described in Section 4.3 for the Excess Variance Metric implies that the estimated peer elasticity γ represents the following:

$$\text{Peer Elasticity } \gamma = \left(1 + \gamma^{contemp}\right)\left(1 + \gamma^{past}\right) - 1 \quad ,$$
$$\gamma^{contemp} \equiv \frac{\tau}{\rho} \quad , \quad \gamma^{past} \equiv \left(\frac{m \cdot Var\left(\overline{\omega}_{sc}\right)}{Var\left(\omega_{isc}|s\right)}\right)^{1/2} - 1.$$

Baseline measures of the peer elasticity γ will capture the joint effects of $\gamma^{contemp}$ and γ^{past}. $\gamma^{contemp}$ captures contemporaneous interactions and includes both responses ϕ to peer intermediate fundamentals and responses θ to peer outcomes. γ^{past} captures the extent to which intermediate fundamentals ω_{isc} are more similar within sections than across sections ($\gamma^{past} = 0$ if ω_{isc} is distributed iid within each class year and $\gamma^{past} > 0$ if ω_{isc} is more similar within sections than across sections). γ^{past} can represent both past interactions and common shocks.

Isolating Contemporaneous Peer Effects using Reunions

I isolate $\gamma^{contemp}$ under the assumption that γ^{past} does not vary with the reunion schedule. Assume that reunions increase the base level of contemporaneous interactions $\gamma^{contemp}_{base}$ by the amount $\gamma^{reunion\_shock}$:

$$\gamma^{contemp}_{postreunion} = \gamma^{contemp}_{base} + \gamma^{reunion\_shock}.$$

Comparisons of measured peer elasticities in the year immediately following reunions with the peer elasticities in other years establish a lower bound for contemporaneous peer effects assuming that contemporaneous peer effects in non-reunion years are non-negative:

$$\begin{aligned}
\frac{1+\gamma^{reunion}}{1+\gamma^{nonreunion}} &= \frac{\left(1+\gamma^{contemp}_{postreunion}\right)\left(1+\gamma^{past}\right)}{\left(1+\gamma^{contemp}_{base}\right)\left(1+\gamma^{past}\right)} \\
&= \frac{\left(1+\gamma^{contemp}_{base}+\gamma^{reunion_shock}\right)}{\left(1+\gamma^{contemp}_{base}\right)} \\
&\leq 1+\gamma^{contemp}_{postreunion}.
\end{aligned}$$

The above ratio offers a lower bound for contemporaneous peer effects in the year following reunions because it assumes that $\gamma^{contemp}_{base} = 0$ even though positive contemporaneous interactions may likely occur in non-reunion years.

Table 1: Members of "The Group" and "Operation Snowflake"

Name	Section	Job Title
Robert Baldwin	C	Owner Bird Imported Motors
Jim Burke	C	CEO Johnson & Johnson
Jack Lanahan	C	President Greenbrier Resorts
Winslow Martin	C	Director Arthur D. Little
Peter McColough	C	CEO Xerox
John Muller Jr.	C	Chairman General Housewares
Tom Murphy	C	CEO Cap Cities/ABC
Will Hanley Jr.	A	President Elizabeth Arden
Robert Landrum	A	Professor Eastern Kentucky University
Frank Mayers	A	Chairman Bristol-Myers
Jack Davis	D	President Resorts International

"The Group" was a social clique within the HBS class of 1949. Members of "The Group" launched "Operation Snowflake" in the mid 1970s, an exclusive annual ski retreat that continued into the early 1990s.

Table 2: Summary Statistics - HBS Executives

	CEOs/CFOs			All Top Earners		
	Mean	Median	S.D.	Mean	Median	S.D.
Number of executives	596			1051		
Observations (executive x year)	3071			6413		
Number section peers per executive (excl. self)	1.879	2	1.376	3.153	3	1.877
Number class peers per executive (excl. self)	13.755	14	5.530	23.810	25	8.699

The HBS executive sample covers MBA alumni who graduated from 1949 to 2008 and also appear ExecuComp, which covers executives in S&P 1500 firms from 1992 to 2008. An executive is included in the CEOs/CFOs subsample for a given year if (1) the ExecuComp *ceoann* or *cfoann* markers are flagged, (2) her title indicates that she is a CEO or CFO and her annual total compensation rank is greater than or equal to 5, or (3) her total compensation rank is equal to one and there are no other identified CEOs for her firm that year. The All Top Earners sample includes all HBS MBA alumni in ExecuComp and includes the CEOs/CFOs sample. All results in later tables refer to the CEOs/CFOs sample unless otherwise noted. All counts of the number of section and class peers exclude the individual herself.

Table 3: Summary Statistics - Executive Characteristics and Firm Policies

	HBS CEOs/CFOs			All CEOs/CFOs		
	Mean	Median	S.D.	Mean	Median	S.D.
Direct compensation $K	1168	806	1104	906	619	901
Total compensation $K	3690	1930	4796	2628	1259	3882
Annual percent change in direct compensation	14.1%	5.8%	60.4%	12.9%	5.9%	63.3%
Annual percent change in total compensation	44.8%	5.4%	275.9%	42.6%	5.8%	245.4%
Percent female	1.9%			0.1%		
Age	57.5	58.0	9.2	58.4	58.0	9.4
Firm tenure (years)	9.6	6.0	9.9	12.3	8.0	11.4
Fiscal year firm return	0.198	0.064	3.503	0.609	0.062	1086.985
Investment (capx in $M)	435	63	1796	287	46	14257
Debt to Equity Ratio (lt/ceq)	3.03	1.31	5.21	2.75	1.29	4.51
Dividend to Price Ratio	0.02	0.01	0.05	0.01	0.00	0.04
Interest Coverage (oibdp/xint)	31.3	8.0	105.8	38.3	8.0	125.7
Cash Res. to Assets Ratio ((ib+dpc+che)/at)	0.21	0.16	0.20	0.21	0.16	0.24
Plant Property and Equipment ($M)	2208	302	6820	1716	242	5619
Sales ($M)	6513	1462	18142	4588	1107	14257
Number of attempted acquisitions	1.56	1	3.01	1.28	0	2.59
Number of completed acquisitions	1.24	0	2.38	1.06	0	2.21
Fraction attempted at least one acquisition	0.55			0.48		
Fraction completed at least one acquisition	0.50			0.43		
Value of completed acquisitions $Mil	1005	140	3359	897	121	3910
Observations (executive x year)	3071			55630		

Direct compensation is the sum of *salary* and *bonus*. *Total compensation* is the sum of *direct compensation*, value of restricted stock grants, and the Black Scholes value of options granted and long term incentive plans. Summary statistics for *direct compensation* and *total compensation* consist of winsorized means and winsorized standard deviations at the 1% level of both tails. *Firm returns* are matched to firm fiscal year month end dates and come from CRSP. Completed acquisitions are documented successful acquisitions in which acquiring firms gained 50% or greater stakes in the acquired entities. *Attempted acquisitions* include any recorded acquisition in the SDC database and is inclusive of completed acquisitions. Acquisitions that are not noted as complete may represent failed acquisition attempts or incomplete reporting in the SDC data. Calculations of other variables using Compustat data items are noted in parentheses with winsoring as described in Table 7.

Table 4: Peer Group Commonalities

Commonality Rates:	Section	Class	Ratio: Section/ Class	P-value T-test	Obs	All HBS Executives	All ExecuComp
(A.1) Pre-HBS Student Characteristics: All HBS Graduates							
Citizenship	0.7181	0.7192	0.998	0.0000	30385		
Undergraduate institution	0.0197	0.0205	0.959	0.0000	35155		
Gender	0.7380	0.7396	0.998	0.0000	42975		
(A.2) Pre-HBS Student Characteristics: ExecuComp Top Earners							
Citizenship	0.9520	0.9555	0.996	0.4032	750		
Undergraduate institution	0.0211	0.0220	0.959	0.8022	829		
Gender	0.9224	0.9190	1.004	0.3878	964		
(B) Post-HBS Executive Outcomes: ExecuComp Top Earners							
Director overlap	0.0586	0.0472	1.233	0.0228	960	0.0447	
Industry SIC3	0.0377	0.0296	1.273	0.0545	960	0.0274	0.0196
Industry Fama French 49	0.0652	0.0522	1.250	0.0134	960	0.0484	0.0405
State of headquarters	0.0926	0.0843	1.098	0.1563	960	0.0790	0.0653
City of headquarters	0.0269	0.0165	1.627	0.0024	960	0.0151	0.0092
(C) Selection into ExecuComp Top Earners							
Execucomp Dummy	0.9221	0.9220	1.000	0.9705	22066		

Panels (A.1) and (A.2) test for peer similarities in student characteristics that are determined prior to matriculation at HBS. Panel (A.1) includes all HBS MBA graduates from 1949 to 2008 while Panel (A.2) includes all HBS ExecuComp top earners as described in Table 2. *Section commonalities* and *class commonalities* measure the fraction of section peers (same section and same class year) and class peers (same class year, different sections), respectively, that have at least one characteristic (e.g., undergraduate institution) in common with the student. The *commonalities ratio* represents the ratio of *section commonalities* to *class commonalities* -- ratios significantly less than unity show that section peers are not more similar than class peers in terms of ex-ante characteristics, and support the argument in Appendix A that balanced section assignment leads to a small bias against findings of positive peer influence. The next column reports the p-value from a paired t-test of the hypothesis that *section commonalities* is equal to *class commonalities*. Panel (B) performs similar tests for peer similarities in categorical executive labor market outcomes that occur after graduation from HBS. The sample includes all HBS ExecuComp top earners and observations are at the executive level. Each executive is allowed to have multiple values for each characteristic (e.g. multiple firm affiliations) to allow for changes in employment over time. Individuals without at least one section peer and one class peer are not included in the estimation. *Commonalities ratios* greater than unity imply that section peers have more similar ex-post outcomes than class peers. The two right-most columns present commonalities among all HBS ExecuComp top earners (regardless of class year and section boundaries) and all ExecuComp top executives (firm and director overlap are not measured because employment history is only matched for HBS executives), respectively. While these latter two columns are useful as a reference for the expected base rates of commonalities, the extent to which *class commonalities* exceeds the commonalities in the two right-most columns can reflect both peer influence and selection into each HBS class year -- for that reason, the analysis focuses on the difference between *section commonalities* and *class commonalities*. Panel (C) examines *section* and *class commonalities* in terms of selection into the ExecuComp subsample. The sample is restricted to HBS graduates from class years in which at least 20 alumni per class year appear in ExecuComp. Results are similar using the full sample of HBS graduates.

Table 5: Peer Influence in Compensation

Log Compensation Type:	Direct Comp (Salary + Bonus)		Total Comp	
	(1)	(2)	(3)	(4)
Levels				
(A.1) Pairs Distance Metric				
Distance ratio	0.074 *	0.089 **	0.029	0.059
	(0.040)	(0.044)	(0.037)	(0.040)
γ	0.154 *	0.187 **	0.060	0.123
	(0.080)	(0.089)	(0.075)	(0.082)
Obs (pair x year)	10003	6963	10003	6963
(A.2) Excess Variance Metric				
Excess variance ratio	0.423 **	0.673 *	0.207	0.256
	(0.199)	(0.350)	(0.150)	(0.350)
γ	0.193 **	0.294 *	0.099	0.121
	(0.089)	(0.153)	(0.067)	(0.153)
Obs (executive x year)	3042	3042	3042	3042
Changes (1st Differences)				
(B.1) Pairs Distance Metric				
Distance ratio	0.110 **	0.113 **	0.008	0.118 **
	(0.046)	(0.051)	(0.041)	(0.048)
γ	0.234 **	0.241 **	0.016	0.254 **
	(0.093)	(0.104)	(0.085)	(0.100)
Obs (pair x year)	6658	4524	6658	4524
(B.2) Excess Variance Metric				
Excess variance ratio	0.584 ***	0.450 **	0.117	0.104
	(0.189)	(0.196)	(0.155)	(0.072)
γ	0.259 ***	0.204 **	0.057	0.051
	(0.083)	(0.085)	(0.068)	(0.032)
Obs (executive x year)	2320	2320	2320	2320
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, excl. firm transitions	N	Y	N	Y
First stage uses full ExecuComp sample	N	Y	N	Y
Firm and SIC3 industry returns, size	N	Y	N	Y
Industry FF49 x year fixed effects	N	Y	N	Y
Exclude pairs with industry overlap†	N	Y	N	Y

This table uses the Pairs Distance and Excess Variance Metrics, described in Section 4. All specifications use compensation residuals which are estimated from a first stage regression of compensation levels using the controls and sample restrictions listed in the bottom panel. First stage results (available upon request) are not reported in this and future tables. Demographic controls include age and gender. Employment controls include dummies for executive type (CEO vs. CFO), and quadratics in firm tenure and CEO or CFO tenure. Exclusion of firm transitions excludes observations reflecting an executive transition to a different firm and only applies to specifications in Panel (B). In Columns (2) and (4), the first stage estimation uses all observations for CEOs and CFOs in ExecuComp to improve the fit of controls for industry trends over time. Firm and industry SIC3 returns include current and lagged annual returns matched to the fiscal year end month of each firm and are winsorized at the 1 percent level. Significance levels are estimated using the non-parametric permutation tests described in Section 4.4 (Monte Carlo simulations of 10,000 placebo draws), with * significant at 10%; ** significant at 5%; and *** significant at 1%. †Exclusion of executive pairs in the same Fama French 49 industry or financials (SIC codes 6000-6999) only applies to Panels (A.1) and (B.1).

Table 6: Peer Influence in Acquisitions

Acquisition Type:	Acquisition Attempt Dummy		Completed Acquisition Dummy	
	(1)	(2)	(3)	(4)
(A) Pairs Distance Metric				
Distance ratio	0.112 ***	0.039 *	0.118 ***	0.042 *
	(0.032)	(0.024)	(0.031)	(0.025)
γ	0.239 ***	0.079 *	0.253 ***	0.086 *
	(0.066)	(0.049)	(0.062)	(0.049)
Obs (pair x year)	10155	9812	10155	9812
(B) Excess Variance Metric				
Excess variance ratio	0.362 ***	0.324 ***	0.351 ***	0.310 ***
	(0.073)	(0.073)	(0.072)	(0.072)
γ	0.167 ***	0.151 ***	0.134 ***	0.145 ***
	(0.033)	(0.033)	(0.032)	(0.032)
Obs (executive x year)	3071	3071	3071	3071
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, excl. firm transitions	N	Y	N	Y
First stage uses full ExecuComp sample	N	Y	N	Y
Firm and SIC3 industry returns, size	N	Y	N	Y
Industry FF49 x year fixed effects	N	Y	N	Y
Exclude pairs in same FF49†	N	Y	N	Y

This table tests for peer influence in annual levels of acquisitions using the Pairs Distance and Excess Variance Metrics, described in Section 4. All specifications and variable definitions are as described in Table 5, except the outcome is measured by acquisition activity rather than compensation. In Columns (1) and (2), acquisition activity is measured by the *acquisition attempt* dummy indicating whether the SDC database documents at least one acquisition attempt in the relevant year. In Columns (3) and (4), acquisition activity is measured by the *completed acquisition* dummy which indicates whether the SDC database documents at least one completed acquisition in which the firm gained a 50 percent or greater stake in another entity in the relevant year. †Exclusion of executive pairs in the same Fama French 49 industry only applies to the Pairs Distance Metric. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 7: Peer Influence in other Firm Policies

	(1)	(2)	(3)	(4)	(5)	(6)
	Investment		Leverage		Dividends	
(A.1) Pairs Distance Metric						
Distance ratio	0.105 *	0.038	0.125 *	0.046	-0.054	-0.008
	(0.051)	(0.045)	(0.073)	(0.070)	(0.077)	(0.071)
γ	0.223 **	0.078	0.270 *	0.094	-0.105	-0.016
	(0.104)	(0.092)	(0.150)	(0.144)	(0.160)	(0.149)
Obs (pair x year)	8757	8461	10070	9728	10020	9681
(A.2) Excess Variance Metric						
Excess variance ratio	0.321 **	0.227	0.188	0.012	-0.050	-0.020
	(0.161)	(0.153)	(0.219)	(0.229)	(0.968)	(1.016)
γ	0.149 **	0.108	0.090	0.006	-0.025	-0.010
	(0.073)	(0.068)	(0.094)	(0.100)	(0.323)	(0.336)
Obs (executive x year)	2827	2827	3057	3057	3049	3049
	Interest Coverage		Cash Holdings		Firm Size	
(B.1) Pairs Distance Metric						
Distance ratio	0.086	0.082	-0.053	-0.037	0.112 **	0.071
	(0.087)	(0.069)	(0.051)	(0.047)	(0.050)	(0.048)
γ	0.182	0.173	-0.104	-0.073	0.239 **	0.147
	(0.193)	(0.148)	(0.104)	(0.096)	(0.103)	(0.099)
Obs (pair x year)	7420	7167	9362	9035	9298	8992
(B.2) Excess Variance Metric						
Excess variance ratio	0.417	0.441	0.117	0.057	0.308 **	0.291
	(0.303)	(0.273)	(0.170)	(0.189)	(0.154)	(0.181)
γ	0.190	0.200 *	0.057	0.028	0.144 **	0.136 *
	(0.122)	(0.112)	(0.075)	(0.084)	(0.070)	(0.080)
Obs (executive x year)	2581	2581	2920	2920	2942	2942
Demographic controls & Year FE	Y	Y	Y	Y	Y	Y
Empl. controls, excl. firm transitions	N	Y	N	Y	N	Y
1st stage uses full ExecuComp sample	N	Y	N	Y	N	Y
Firm and SIC3 industry returns, size	N	Y	N	Y	N	Y
Industry FF49 x year fixed effects	N	Y	N	Y	N	Y
Exclude pairs with industry overlap†	N	Y	N	Y	N	Y

This table tests for peer influence in the annual levels of six other firm policies using the Pairs Distance and Excess Variance Metrics, described in Section 4. Firm policies in the first stage estimation are measured as follows: *Investment* = log(capx), *Leverage* = lt/ceq (debt to equity ratio, bottom capped at zero, top 1% winsorized), *Dividends* = dvpsx_f/prcc_f (dividend to price ratio, bottom and top capped between 0 and 1), *Interest Coverage* = oibdp/xint (ratio of operating income before depreciation and amortization to interest expenses, top and bottom 2% winsorized), *Cash Holdings* = (ib+dpc+che)/at (cash reserves to assets ratio, top and bottom 1% winsorized), *Firm Size* = log(ppent). All specifications and other variable definitions are as described in Table 5. †Exclusion of executive pairs in the same Fama French 49 industry only applies to the Pairs Distance Metric. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Figure 1: The Reunion Cycle and Peer Influence in Compensation

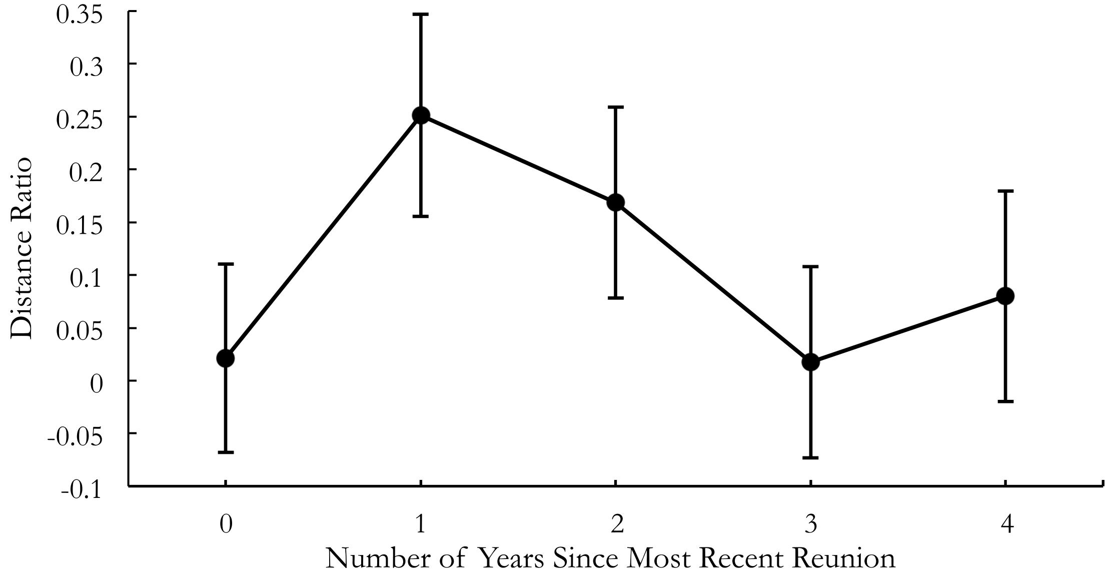

Figure 1 plots peer similarities in annual changes in log direct compensation for each year in the five year reunion cycle. Each point represents a distance ratio, which measures the extent to which section peers are more similar than class peers in terms of pairs absolute distance. Controls and sample restrictions are as described for Table 7 Column (2). Error bars represent standard errors, estimated using the permutation test described in Table 5.

Figure 2: The Reunion Cycle and Peer Influence in Acquisitions

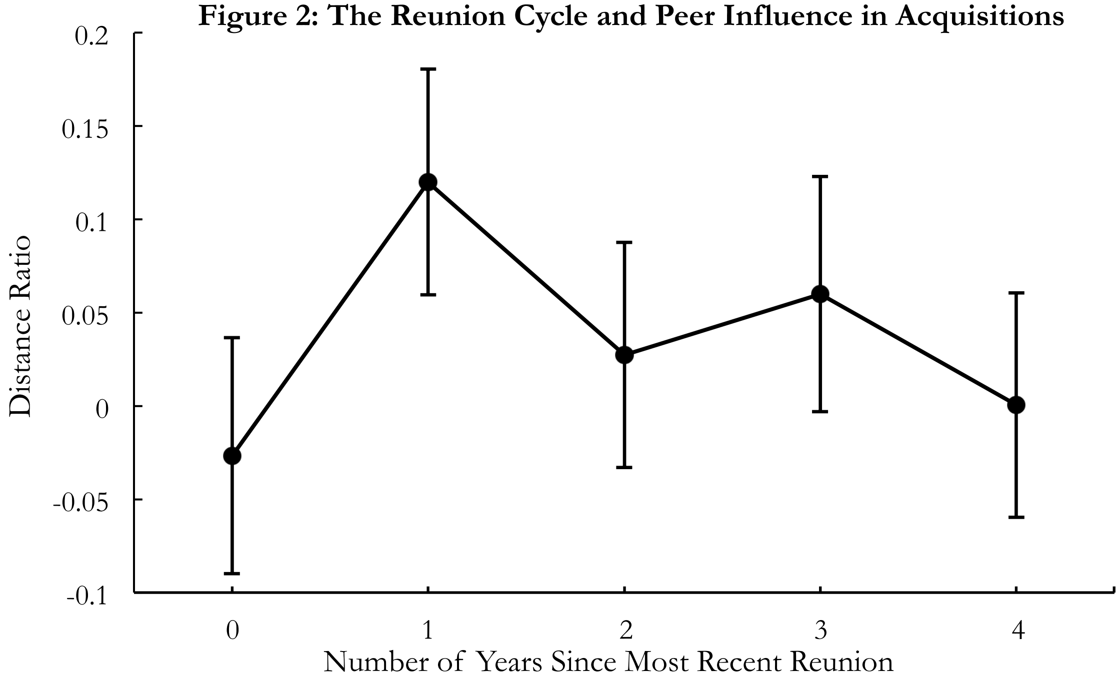

Figure 2 plots peer similarities, as measured by the distance ratio, in annual levels of the *acquisition attempt dummy* for each year in the five year reunion cycle. All figure properties are as described for Figure 1.

Table 8: Shocks to Peer Influence Following Alumni Reunions

	(1)	(2)	(3)	(4)
(A) Log Compensation Changes (First Dif)	Direct Compensation		Total Compensation	
Distance ratio: reunion year + 1	0.178 **	0.251 ***	0.047	0.140 *
	(0.084)	(0.096)	(0.070)	(0.085)
γ: reunion year + 1	0.399 **	0.603 ***	0.096	0.306 *
	(0.177)	(0.204)	(0.150)	(0.186)
Distance ratio: all other years	0.075 *	0.063	-0.002	0.113 **
	(0.048)	(0.052)	(0.044)	(0.052)
γ: all other years	0.192 **	0.155	-0.004	0.242 **
	(0.097)	(0.105)	(0.091)	(0.107)
P-value: distance ratio equality	0.028	0.005	0.297	0.675
Obs (pair x year)	6658	4524	6658	4524
(B) Acquisition Levels	Acquisition Attempt Dummy		Completed Acquisition Dummy	
Distance ratio: reunion year + 1	0.190 ***	0.114 **	0.219 ***	0.112 **
	(0.061)	(0.049)	(0.058)	(0.047)
γ: reunion year + 1	0.430 ***	0.244 **	0.511 ***	0.240 **
	(0.126)	(0.100)	(0.120)	(0.097)
Distance ratio: all other years	0.090 **	0.022	0.089 ***	0.018
	(0.035)	(0.028)	(0.033)	(0.027)
γ: all other years	0.189 ***	0.044	0.188 ***	0.037
	(0.071)	(0.056)	(0.067)	(0.054)
P-value: distance ratio equality	0.120	0.077	0.035	0.068
Obs (pair x year)	10155	9812	10155	9812
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, excl. firm transitions	N	Y	N	Y
First stage uses full ExecuComp sample	N	Y	N	Y
Firm and SIC3 industry returns, size	N	Y	N	Y
Industry FF49 x year fixed effects	N	Y	N	Y
Exclude pairs with industry overlap†	N	Y	N	Y

This table tests whether peer influence is stronger following alumni reunions, which occur every five years after each executive's specific graduation year. All variables are as described in Tables 5 and 6. In the second stage estimation, the absolute difference of pair residual outcomes are regressed on the same section dummy, a dummy for the year immediately following reunions, and the interaction of the former two dummies. *Distance ratio: reunion year + 1* is the fractional difference in mean distance between two section peers relative to the mean distance between two class peers in the year following reunions (measured by the negative ratio of the sum of the coefficients on *same section* and *same section x reunion year + 1* to the sum of the coefficients on *reunion year + 1* and the constant term). *Distance ratio: all other years* is the fractional difference in distance for section peers relative to class peers in all other years (measured as the negative ratio of the coefficient on *same section* to the constant term). *γ: reunion year + 1* and *γ: all other years* represent, under the assumptions of the Linear-in-Means Model, the elasticity of individual outcomes to mean group fundamentals (scaled by the elasticity of individual outcomes to own fundamentals) in the year following reunions and all other years, respectively. P-values are reported for the test of whether *distance ratio: reunion year + 1* is equal to *distance ratio: all other years*. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%. †Columns (2) and (4) excludes pairs of executives in the same Fama French 49 industry. Panel A also excludes financials (SIC codes 6000-6999).

Table 9: Pay For Friend's Luck

	Direct Compensation		Total Compensation	
	(1)	(2)	(3)	(4)
(A) Changes (First Differences)				
Distance ratio	0.073 *	0.124 **	0.041	0.081 *
	(0.037)	(0.048)	(0.034)	(0.047)
Obs (pair x year)	10082	6486	10082	6486
(B) Lagged Changes (First Differences)				
Distance ratio	0.099 **	0.150 ***	0.044	0.082
	(0.040)	(0.054)	(0.039)	(0.053)
Obs (pair x year)	7964	5076	7964	5076
Demographic controls	Y	Y	Y	Y
Employment controls, excl. firm transitions	Y	Y	Y	Y
First stage uses ExecuComp sample	Y	Y	Y	Y
Firm and SIC3 industry returns, size	Y	Y	Y	Y
Industry FF49 x year fixed effects	Y	Y	Y	Y
Exclude pairs in same FF49 industry	Y	Y	Y	Y
Excl. pairs in linked industries, financials	N	Y	N	Y

This table examines the relationship between an executive's change in compensation and her peer's "lucky" change in compensation. Peers' lucky changes in compensation are predicted using the peers' industry returns. Specifications follows the modified Pairs Distance Metric described in Section 4. In the top panel, the dependent variable is $(\tilde{Y}_{isct} - \tilde{Y}_{isc,t-1})-(\hat{Y}_{isct} - \hat{Y}_{isc,t-1})$. $\tilde{Y}$ is the residual from the standard first stage regression of compensation levels on the set of controls indicated in the bottom panel. $\hat{Y}$ is the predicted "lucky" compensation from a regression of log compensation levels on the firm's SIC3 industry current and lagged fiscal year returns (calculated excluding the relevant firm's own returns). Consider a pair of executives A and B in a given year. This pair will account for two observations. The dependent variable in the first observation is the absolute difference between A's change in residual compensation and B's change in predicted compensation. The dependent variable in the second observation is the absolute difference between A's change in predicted compensation and B's change in residual compensation. Specifications in Panel (B) are identical except for the use of lagged changes in peer predicted compensation as the dependent variable: $(\tilde{Y}_{isct} - \tilde{Y}_{isc,t-1})-(\hat{Y}_{isc,t-1} - \hat{Y}_{isc,t-2})$. All other variables are as described in Table 5. To address the concern that lucky shocks in a given industry may have a greater direct impact across industries linked by section peers than across industries linked by class peers, all tests exclude pairs of executives belonging to the same Fama French 49 industry. Columns (2) and (4) also exclude executives working in the financial sector (SIC codes 6000-6999) as well as pairs of peers in linked industries. (Following Ahern and Harford (2010) and using the BEA input-output tables, industries are considered linked if a customer industry buys at least 2% of a supplier industry's total output or if a supplying industry supplies at least 2% of the total inputs of a customer industry.) Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 10: Robustness of Baseline Results

	Distance Ratio		Obs
(A) Annual Changes in Log Compensation			
1. Forbes Sample: Years 1970 - 1991: total comp	0.188 *	(0.110)	927
2. Double cluster standard errors	0.110 **	(0.043)	6658
3. Winsorized compensation top and bottom 1%	0.095 **	(0.042)	6651
4. Exclude observations after 2006	0.088 *	(0.045)	5601
(B) Annual Levels in Acquisitions			
5. Double cluster standard errors: attempted acquisitions	0.112 ***	(0.032)	10155
6. Double cluster standard errors: completed acquisitions	0.118 ***	(0.032)	10155
7. Acquisition value to assets ratio	0.148 **	(0.071)	8090
8. Number of acquisitions completed	0.163 **	(0.071)	10155
9. Completed acquisitions, known value > $1M	0.102 ***	(0.032)	10155

This table supports the robustness of the baseline results for peer influence in log compensation and acquisitions as presented in Tables 5 and 6. Each row is a variation upon the baseline Pairs Distance Metric specification described in Section 4. Controls in the first stage are limited to year fixed effects and demographics controls as defined in Table 5 to capture the full measure of peer influence. Row (1) uses the measure of total compensation from the Forbes sample of HBS executives (927 panel observations covering 149 CEOs). Because the Forbes data covers an earlier period from 1970 to 1991, direct compensation represents 84% (mean) and 93% (median) of total compensation. All other rows in Panel (A) use the direct compensation measure in ExecuComp. Rows (2), (5), and (6) estimate standard errors and significance levels using the double clustering procedure described in Section 4.4. Row (3) uses log direct compensation winsorized at the top and bottom 1% levels as the dependent variable in the first stage estimation. Row (4) excludes observations with fiscal years ending after December 2006 to ensure that results are robust to a change in SEC compensation disclosure rules. Row (7) uses the ratio of acquisitions value to lagged assets (CompuStat data items aqc/at_{t-1}, limited to 0 at the lower bound and winsorized at the 1% upper tail of the CompuStat sample). Row (8) uses the number of known completed acquisitions in which the firm acquired a 50 percent or greater stake in the acquired entity. Row (9) uses a dummy for one or more completed mergers (with 50 percent or greater stakes in the acquired entities) in which the transaction value is known and exceeds $1M US. All standard errors, unless otherwise noted, are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 11: Peer Influence Among All Top Earners

	Direct Compensation	Total Compensation	Attempted Acquisitions	Completed Acquisitions
	(1)	(2)	(3)	(4)
Distance ratio: CEO/CFO pair	0.117 ***	0.029	0.107 ***	0.100 ***
	(0.045)	(0.040)	(0.033)	(0.031)
γ: CEO/CFO pair	0.251 ***	0.059	0.228 ***	0.211 ***
	(0.091)	(0.080)	(0.066)	(0.062)
Distance ratio: other exec pair	0.037	0.042	-0.011	0.012
	(0.051)	(0.043)	(0.031)	(0.030)
γ: other exec pair	0.076	0.085	-0.023	0.024
	(0.102)	(0.087)	(0.063)	(0.061)
Distance ratio: mixed pair	0.011	0.042	0.040	0.040
	(0.035)	(0.032)	(0.026)	(0.025)
γ: mixed pair	0.022	0.085	0.082	0.081
	(0.072)	(0.064)	(0.051)	(0.049)
P-value: distance ratios equal	0.163	0.961	0.031	0.112
P-value: distance ratios = 0	0.059	0.412	0.005	0.005
Obs (pair x year)	26912	26912	38153	38153
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y

This table extends the analysis for peer influence in compensation and acquisition activity to a sample containing all top earners (CEOs, CFOs, and other top five earners in ExecuComp). A executive pair is considered a "CEO/CFO pair" if both members of the pair are CEOs or CFOs. Similarly, a pair is considered an "other exec pair" or "mixed pair" if the pair consists of two non-CEO/CFO executives or one CEO/CFO and one non-CEO/CFO, respectively. Specifications follow the Pairs Distance Metric described in Section 4. In the second stage, the absolute value of the difference in pair residuals is regressed on dummies for the three types of pairs, a *same section* dummy, and interactions between the *same section* dummy and the pair type dummies. The distance ratios represent the fractional decrease in pair distance for a pair of section peers relative to a pair of class peers. Specifically, *distance ratio: CEO/CFO* is the negative ratio of the coefficient on *same section* x *CEO/CFO pair* to the constant term. *Distance ratio: other exec pair* is the negative ratio of the coefficient on *same section* x *other pair* to the sum of the constant term and the coefficient on *other pair*. *Distance ratio: mixed pair* is defined similarly. γ measures the elasticity of individual outcomes to mean section fundamentals (scaled by the elasticity of individual outcomes to own fundamentals) and is calculated separately for each pair type from the relevant distance ratios. P-values are reported for tests of whether the distance ratios are equal to one another or jointly equal to zero. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 12: Variation by Reunion Campaign Participation

	Direct Compensation	Total Compensation	Acquisition Attempt	Completed Acquisition
	(1)	(2)	(3)	(4)
Distance ratio 1: reunion yr+1, nondonor	0.072	-0.031	0.054	0.120
	(0.150)	(0.117)	(0.099)	(0.096)
Distance ratio 2: reunion yr+1, donor	0.251 **	0.099	0.295 ***	0.297 ***
	(0.105)	(0.091)	(0.077)	(0.074)
Distance ratio 3: all other yrs, nondonor	0.040	-0.061	0.047	0.034
	(0.060)	(0.075)	(0.055)	(0.052)
Distance ratio 4: all other yrs, donor	0.126 **	0.040	0.121 ***	0.131 ***
	(0.060)	(0.057)	(0.046)	(0.043)
P-value: distance ratios 1 & 2 are equal	0.117	0.238	0.055	0.147
Obs (pair x year)	6658	6658	10155	10155
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y

This table tests whether peer influence varies by the reunion cycle and participation in reunion contribution campaigns (which occur every five years after each executive's specific graduation year and coincides with reunion gatherings). Pairs of executives are considered donors if both executives contributed at least $1000-$2500 US (the median donation amount) in at least one reunion contribution campaign between 1990 and 2008. The results are similar under the alternative definition where donors are those who donate any amount in the reunion contribution campaign immediately preceding the year of the observation. All other variables are as defined in Table 8. The specifications are based upon the Pairs Distance Metric described in Section 4. In the second stage, the absolute difference of pair residuals (compensation changes in Panel A and acquisition levels in Panel B) are regressed on the *same section* dummy, the *donor* dummy, the *reunion year + 1* dummy, and all interaction terms. *Distance ratio* 1 through 4 describe the distance ratios for non-donor pairs during the year following reunions, donor pairs during the year following reunions, non-donor pairs during all other years, and donor pairs during all other years, respectively. P-values test for equality between the distance ratios for donors and non-donors in the year following reunions. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 13: Supplementary Analysis of Peer Effects in Acquisitions

	Diversify FF49	Equity Financed	Ex Post Acq Returns	Acquisition Attempt	Completed Acquisition
	(1)	(2)	(3)	(4)	(5)
Reunion yr+1	0.054 **	0.061 *	-0.006		
	(0.022)	(0.082)	(0.023)		
Distance ratio 1: reunion yr+1, $Acq_{i,t-1}$=0				-0.006	0.035
				(0.070)	(0.070)
Distance ratio 2: reunion yr+1, $Acq_{i,t-1}$=1				0.175 **	0.188 **
				(0.077)	(0.076)
Distance ratio 3: all other yrs, $Acq_{i,t-1}$=0				0.079	0.060
				(0.044)	(0.045)
Distance ratio 4: all other yrs, $Acq_{i,t-1}$=1				-0.011	-0.001
				(0.052)	(0.047)
P-value: distance ratios 1 & 2 are equal				0.065	0.391
Obs (executive x year)	26615	13727	23201		
Obs (executive x year - HBS only)	1674	826	1472		
Obs (pair x year)				13528	13528
Demographic controls	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y
Employment controls	Y	Y	Y	N	N
First stage uses full ExecuComp sample	Y	Y	Y	N	N
Firm and SIC3 industry returns, size	Y	Y	Y	N	N
Industry FF49 x year fixed effects	Y	Y	Y	N	N

Columns (1) and (2) regress a dummy for whether the acquisition is diversified (acquirer and target are in different Fama French 49 industries) or equity financed on a dummy for whether the observation corresponds to the year following each executive's specific reunion year and the set of controls in the bottom panel. Column (3) regresses the acquirer announcement day abnormal return [-1,+1] on the same dummy for the year following each executive's reunion year. The sample includes all acquisition attempts matched to ExecuComp CEOs and CFOs (non-HBS observations are included to control for industry trends over time). Columns (4) and (5) test whether an individual's current acquisition activity is more similar to her section peer's lagged activity than to her class peer's lagged activity and how that lagged relationship varies depending on the reunion cycle and whether the peer did or did not attempt an acquisition in the previous year. The specifications are modifications of the Pairs Distance Metric described in Section 4 and use only demographic and year controls in the first stage to capture the full effect of peer influence (results are similar controlling for firm and industry trends). In the second stage, the absolute difference between *i*'s current acquisition activity and *j*'s lagged acquisition activity, $|\tilde{Y}_{isct} - \tilde{Y}_{jsc,t-1}|$, is regressed on the *same section* dummy, the *reunion year + 1* dummy, the *positive lagged peer outcome* dummy (equal to one if $Y_{jsc,t-1}>0$), and all interaction terms. *Distance ratio 1* through *4* describe the distance ratios for the response to non-positive lagged peer outcomes during the year following reunions, the response to positive lagged peer outcomes during the year following reunions, the response to non-positive lagged peer outcomes during all other years, and the response to positive lagged peer outcomes during all other years, respectively. All other variables are as defined in Table 8. P-values test for equality between the distance ratios for responses to positive and non-positive lagged peer outcomes in the year following reunions. Standard errors in parentheses are estimated using the permutation test described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.