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 show that executive peer networks are important determinants of managerial decision-making and firm policies. Within a class, executive compensation and acquisitions strategy are significantly more similar among graduates from the same section than among graduates from different sections. Both executive 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. I further show that peer effects in compensation are not driven by similarities in underlying managerial productivity using a test of "pay for friend's luck": pay responds to lucky industry-level shocks to the compensation of peers in distant industries. Finally, I show that social interactions increased the between-section variance in outcomes by 20-40%, demonstrating that peer effects can significantly contribute to the large variation in outcomes across peer groups.

1 Introduction

What is the impact of social interactions on managerial decision-making and firm policies? Most traditional explanations of the determinants of firm policies, such as executive compensation and acquisitions behavior, emphasize the fundamentals of the firm, industry, and market. However, an abundance of empirical evidence shows that substantial heterogeneity in firm policies remains after conditioning for these fundamentals (Smith and Watts, 1992 and Andrade, Mitchell, and Stafford, 2001). More recent work focuses on the characteristics of executives and finds that executives "matter" for a wide range of firm policies (Bertrand and Schoar, 2003; Frank and Goyal, 2007; Bennedsen, Pérez-González, and Wolfenzon, 2008; and Graham, Li, and Qiu, 2009). However, if executives do matter for firm policies, what determines executive decision-making? Are executives guided only by their innate preferences and beliefs or are they also influenced by endogenous social interactions in their peer networks?

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 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. (2010) show in general contexts that individuals value relative earnings, while Goel and Thakor (2010) develop a model of envy-motivated mergers.[1]

Estimation of peer effects faces the twin identification challenges of selection and common shocks (Manski, 1993).[2] Selection occurs when executives with similar unobserved characteristics select

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[1]This paper also builds upon the recent literature on social networks in finance. 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 social network are predictive of compensation and firm performance. Fracassi (2008) and Leary and Roberts (2010) find that connected firms have similar corporate finance policies. 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 Duygan-Bump, 2009), stock market participation (Brown et al., 2008), and contracting in the mutual fund industry (Kuhnen, 2009).

[2]Recent work has called into question whether previous estimates of peer effects were biased due to selection and common shocks. In the general contexts of schooling and the workplace, Sacerdote (2001), Guryan, Kroft, and Notowidigdo (2009), Kling, Liebman, and Katz (2007), and Lyle (2007) find that peer effects may be weaker or more nuanced than expected. Meanwhile, Lerner and Malmendier (2007), Cohen, Frazzini, and Malloy (2008), Carrell, Fullerton, and West (2009), and Burchardi and Hassan (2010) show that peer effects can be large in the contexts of entrepreneurship entry decisions among randomly assigned MBA peer groups, insider trading by fund managers, academic performance among randomly assigned Air Force peer groups, and income per capita and entreprenurial activity following German reunification, respectively.

into peer groups. Common shocks occur when group members, by virtue of their association, experience 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 and firm policies using a natural experiment involving randomly assigned peer groups. Starting with the class of 1949, Harvard Business School (HBS) began randomly assigning all entering MBA students to sections.[3] 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.

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 educational 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 a broader set of executives, 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 (2008), which uses HBS sections to identify peer effects in the decision to become an entrepreneur. Aside from focusing on a different research question (peer effects in executive decision-making), other methodological differences between this paper and Lerner and Malmendier are discussed in detail in Section 3.

I explore the impact of peer interactions on two types of firm policies, executive compensation

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[3]HBS attempts to create sections that are balanced in terms of the variables observed by the Registrar, such as gender, marital status, undergraduate institution, and previous industry experience. Thus, 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. In the absence of peer effects, each individual is expected to be more similar to his class peers than his section peers. This intuition is formalized in Appendix A.

and acquisitions behavior, because compensation is likely to be of first-order importance to executives and acquisitions represent a large shift in firm strategy and allocation of capital (with mean and median values exceeding one billion US\$ and 100 million US\$, respectively).[4] Both firm policies also vary significantly over the sample period, allowing for tests identifying off the panel nature of the data. In addition, existing work on social networks in finance has linked executive networks to both policies (Haunschild, 1993; Butler and Gurun, 2009; Hwang and Kim, 2009; Barnea and Guedj, 2010; Fracassi, 2008; Ishii and Xuan, 2010; Fracassi and Tate, 2010; and Goel and Thakor, 2010).[5]

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.[6] I find that, relative to class peers, section peers receive significantly more similar executive compensation and are more likely to pursue similar acquisitions strategies. The amount of variation in compensation and levels of acquisitions 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[7] for the elasticity of the individual response to mean section peer characteristics of 10 to 20 percent. Peer effects also lead to the excess clustering of these activities by sections within each class year. 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 robustness of these baseline results (estimated using data from 1992-2008) is supported with an out-of-sample test using Forbes executive data covering an earlier period, spanning 1970-1991.

To investigate underlying mechanisms and rule out potential biases, I begin by differentiating

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[4]In other empirical tests, I explore executive peer influence in investment, dividend, leverage, and cash policy but generally find imprecisely estimated effects.

[5]The existing empirical literature on network effects in compensation and acquisitions has primarily focused on institutionalized links such as connections between the CEO and board members and overlap in terms of previous employment and 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 lack formal linkages. In addition, randomized HBS MBA social groups offer a convenient way to address the selection and common shocks biases which may confound empirical estimates of peer effects.

[6]Peer influence can also lead to a decrease in group similarity if individuals seek to become outliers. Empirically, this seems not to be the case – section peers are more similar than class peers. Nevertheless, the proposed model and empirical strategy will allow for both types of peer effects.

[7]It is difficult to disentangle peer effects among class peers from other selective forces. Therefore, I adopt the conservative approach of measuring whether section peers are significantly more similar than class peers. Insofar as peer effects are positive among class peers, my estimates provide a lower bound for the true magnitudes of peer influence.

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 (this intuition is formalized in Appendix C). 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 *causal* 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-group relations.[8] Because reunions cover the same time period as outcome measures, reunions should only affect estimates if contemporaneous interactions are important drivers of peer similarities. I find that peer similarities in compensation and acquisitions activity are more than twice as large in the year immediately following reunions relative to other years.

Evidence of contemporaneous interactions also 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 should generally only affect students during their time at business school and should not generate peer similarities that vary according to the reunion schedule.

Using an additional test of "pay for friend's luck," 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 (as defined using BEA input and output tables), highly-aggregated industries to reduce the likelihood that shocks to peers in different industries will have significant *direct* unobserved effects on executives. Results show that individual changes in compensation are significantly more responsive to section peers' lucky pay

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[8] The salience of reunions is verified using individual-level contributions data for reunion contribution campaigns in Section 6.2.

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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, evidence of pay for friend's luck again highlights the importance of contemporaneous interactions (so peer similarities are not driven by past interactions leading to selection into similar types of firms and industries). 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.

Overall, this paper documents the very persistent effects of peers on high-stakes decision-making among executives; peer groups formed in business school affect major firm policies at S&P 1500 companies several decades after graduation. Evidence of contemporaneous interactions supports the executive fixed effects literature (e.g., Bertrand and Schoar, 2003), which argues that executives "matter" because firm policies remain similar as executives transition across different firms. Specifically, this paper addresses the concern raised by Fee, Hadlock, and Pierce (2010) that executive hiring decisions are endogenous to unobserved firm fundamentals (such that executives don't really matter). I show that executives are able to change firm policies as responses to alumni reunions and lucky shocks to peers over time, neither of which are likely to be related to unobserved determinants in firm hiring. More importantly, this paper addresses *how* executives affect firm policies by showing that executive decision-making is tightly related to endogenous social interactions.

Evidence of pay for friend's luck shows that executives are paid for more than firm performance. While Bertrand and Mullainathan (2001) show that executives are paid for industry-level lucky shocks, I find that compensation also reflects lucky shocks to peers who are employed in other industries. Pay for friend's luck also offer insight into whether peer effects in compensation are driven by reactions to peer fundamentals or outcomes. Reactions to peer fundamentals occur when the skills, beliefs, and private information underlying managerial decisions are transferred through peer networks. Meanwhile, reactions to peer outcomes 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 relevant 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 types of policy interventions, only peer influence operating through reactions to peer outcomes will generate a social interactions multiplier (Glaeser, Sacerdote, and Scheinkman, 2003). Evidence of pay for friend's luck suggests that peer

effects in compensation are driven by direct reactions to peer outcomes (compensation) rather than peer similarities in underlying fundamentals such as skills or productivity: pay responds to lucky shocks to friends, even after controlling for own firm and industry performance.[9]

Finally, this paper sheds light on the potential aggregate implications of social interactions among executives. Peer influence implies that the aggregate impact of a change in the fundamental determinants of compensation or acquisitions activity will be larger than the direct effect because of contagion among connected agents. Peer influence, regardless of the exact channels, can also contribute to clustered financial activity. Empirical evidence has long documented the large variation in mean executive compensation and merger activity across time, industries, and regions (Murphy, 2005; Andrade, Mitchell, and Stafford, 2001). A variety of explanations emphasizing fundamental differences across groups have been offered for the clustering of pay and acquisitions.[10] This paper presents a complementary mechanism that could potentially contribute to clustered outcomes: differences in fundamentals across groups can be amplified through peer interactions. Along these lines, seminal work by Glaeser, Sacerdote, and Scheinkman (1996) uses peer influence among criminals to explain the high variation in crime across otherwise similar cities. Recent work by Demarzo, Kaniel, and Kremer (2007) and Goel and Thakor (2010) presents financial herding models in which envy among peers leads to financial bubbles and merger waves.[11] This paper explicitly shows that peer influence can lead to clustered activity among section peers in the same class year using the laboratory context of HBS sections.

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.

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[9]While the tests presented in this paper are informative of underlying mechanisms, further research is necessary to determine the exact channels through which peer effects operate. Peer reactions to *outcomes* in compensation could operate through several channels. Executives may value relative earnings and exert more effort in pay negotiations when their peers receive higher compensation. Alternatively, executive peers may hire similar firm intermediaries, e.g., compensation consultants, thereby linking pay practices, or peers' lucky shocks may alter the outside options of executives if job referrals operate through peer networks. It is similarly difficult to make strong statements regarding the mechanisms underlying peer effects in acquisitions. In supplementary tests, I do not find evidence of observably inefficient acquisitions behavior (e.g., lower ex-post acquirer returns or more diversified acquisitions) following reunions. Such evidence is consistent with the view that peer effects in acquisitions reflect the efficient sharing of information (e.g., merger expertise) across social networks.

[10]Explanations of the clustering of mergers include changes in macro fundamentals (Harford, 2005; Jovanovic and Rousseau, 2002), CEO overconfidence (Malmendier and Tate, 2008), and the strategic timing of equity misvaluations (Shleifer and Vishny, 2003).

[11]In comparison to these financial herding models which specify that peer interactions operate through envy, the model presented in this paper is more general, e.g., peer influence can also function through efficient information transmission.

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. The "49ers" were the first class of HBS MBA students to be randomly assigned to sections. Celebrated by *Fortune Magazine* as "the class the dollars fell on," the 49ers became extremely successful – more than a quarter were CEOs or heads of firms by their 25th reunion.[12] 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.

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 class peer social bonds do exist – Group members don't all belong to the same section – bonds within sections are stronger in expectation. In the empirical portion of the paper, 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

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

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."[13] 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 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.[14]

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 strengthen overall class bonds and particularly section peer bonds. For example, following the 49ers' 40th reunion gala, members of The Group retreated upstairs to a more exclusive cocktail party in a private suite.[15] 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.[16]

The Group provides just one example of how peer influence can powerfully affect executives.

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[13] David Callahan, *Kindred Spirits* (Hoboken, New Jersey: John Wiley & Sons, 2002), p. 133.
[14] Wellermeyer, p. 344.
[15] Stratford P. Sherman, "You're Invited to the CEO's Ball," *Fortune*, January 15, 1990, p. 140.
[16] Sherman, p. 140.

In the remainder of this paper, I investigate executive peer effects using data for the entire set of HBS S&P 1500 executives, from the 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 sections: A, B, C, D, E, F, and G; sections H, I, and J were added over time.[17] Sections foster extremely 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" to distinguish themselves from the first-year MBAs, who are known as "new A" or "new 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. Even after graduation, reunions are organized around section tents and section parties, and alumni contribution campaigns are similarly based upon inter-section competition.

Numerous studies of student life at HBS confirm the strong peer interactions within sections. In his study of section norms, Orth[18] 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
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> sits down at the lunch table ... it is almost always with a group of men from his section. When
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> ... he joins a study group, he finds that the members of the group are likely to be men in his
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> own section.

In an autobiographical memoir of her time at HBS, Henry[19] recalls that sections fostered both conformity and competition:

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[17]Section H was added with the class of 1970 while sections I and J were added with the class of 1971. Section J ceased to exist after the class of 1975 and was reinstituted in 1997.

[18]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.

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

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 determined by school administrators with the goal of creating "balanced" sections; the assignment process is random conditional on student characteristics observed by the HBS administration. School administrators attempt to balance sections on the following observables: race, ethnicity, nationality, industry background, undergraduate institution, geographical origin, and marital status. Because the sectioning process has occurred for over six decades, I verify the historical continuity of balanced section assignment using sources published in 1958, 1963, 1983, 1987, and 1990.[20]

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* my finding of positive peer effects. This intuition is formalized in Appendix A and demonstrated empirically in Table 3, discussed in Section 5.1.

The empirical context of HBS sections used in this paper is most similar to the context used in Lerner and Malmendier (2008), 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 (1949 to the present) while LM's sample spans 1997 to 2004. Second, LM identifies peer effects using the relationship between peer ex-ante entrepreneurship experience and an individual's ex-post decision to become an entrepreneur. Meanwhile, this paper 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 changes with shocks to the strength of peer bonds over time (e.g. reunions).[21] Third, LM focuses on en-

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[20]Sources in order of publication date:

Melvin T. Copeland, *And Mark an Era: The Story of the Harvard Business School*, Little Brown and Company, Boston, 1958.

Charles D. Orth, 3rd, *Social Structure and Learning Climate: The First Year at the Harvard Business School,* Graduate School of Business Administration, Harvard University, Boston, 1963.

Fran Worden Henry, *Toughing it Out at Harvard: The Making of a Woman MBA*, McGraw Hill, New York 1983.

Jeffrey L. Cruikshank, *A Delicate Experiment: The Harvard Business School, 1908-1945*, Harvard Business School Press, Boston, 1987.

David W. Ewing, *Inside the Harvard Business School: Strategies and Lessons of America's Leading School of Business*, Times Books, New York, 1990.

[21]As discussed in Blume et al. (2010), both (a) the relationship between ex-ante and ex-post outcomes and (b)

trepreneurial 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 2008.[22] 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 × 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 576 CEOs/CFOs and 1052 top executives (inclusive of CEOs and CFOs), resulting in 2895 CEO/CFO × year and 6379 top executive × year observations in the panel data. In the ExecuComp data covering the years 1992 to 2008, the median CEO/CFO has two section peers (same section and same class year) and 13 class peers (same class year, different sections), *excluding herself*, while the median top executive has three section peers and 25 class peers.[23] Note that while the data covers all HBS MBA graduates from 1949 to the present who also appear in ExecuComp, over 90% of the top-executive sample graduated between 1960 and 1990 (this is because ExecuComp covers executives from 1992-2008 with a median age of 58).

Table 3 summarizes compensation, demographics, performance, and acquisitions policy panel data 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

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the distribution of ex-post outcomes can be used to identify peer effects in the context of random assignment to peer groups. This paper uses the ex-post distributions of outcomes (e.g. firm policies) because those tend to be better measured for executives relative to ex-ante student characteristics.

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

[23]Because the ExecuComp data covers a limited time window, results do not imply that, in general, the median HBS CEO/CFO has 2 section peers and 13 class peers (excluding herself) during her tenure as a top executive. For example, executives who are very old or young during the time period covered by ExecuComp may have had a greater number of section and class peers during the peaks of their careers.

will be used in some specifications to control for general 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).[24] In general, compensation of HBS CEOs/CFOs is slightly higher than that for the overall ExecuComp sample, although both samples exhibit extremely high variance and right-skewness. Demographic characteristics are also fairly similar across the two samples, although HBS CEOs/CFOs are slightly more likely to be female and have median firm tenure of six years compared to nine 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).

Acquisitions 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 documented successful acquisitions in which acquiring firms successfully gained 50 percent or greater stakes in the acquired entities. Attempted acquisitions include any recorded acquisition in the SDC database and is inclusive of completed acquisitions. Attempted acquisitions are informative even if the acquisitions ultimately fail (e.g., due to takeover defences or regulatory restrictions) because they provide evidence of executives' underlying intentions to acquire.[25] Acquisitions represent a significant discrete change in firm policy with median values of \$138M and mean values of \$1022M.

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

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[24] A change in SEC compensation disclosure rules went into effect in 2006. Among other changes, the new rules require that the dollar value of equity-based compensation be reported as the grant date fair value according to FAS 123R standards. To enable continuity with compensation measures in previous years, 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.

[25] While SDC is one of the most comprehensive databases on M&A activity, data on transaction values and deal status are incomplete (see a summary of data issues in Netter, Stegemoller, and Wintoki, 2010). The attempted acquisitions measures also serve the purpose of including successful acquisitions that are not recorded as "completed" in the SDC database due to unknown deal status information.

react to the mean characteristics of the 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.[26] Since most forms of peer effects predict that section peers will have more similar outcomes than class peers,[27] the empirical results will offer both reduced-form comparisons of section versus class peer similarity as well as estimates of model parameters.

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

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

A sample utility function that generates optimal outcomes of this form is described in Appendix B. Student-level fundamentals (e.g., ex-ante skills or private information) are represented by v_{isc}. Following Graham (2008) and Manski (1993), I allow for two types of peer effects: responses θ to mean group outcomes $\overline{Y}_{sc}$ and responses ϕ to mean group fundamentals $\overline{v}_{sc}$. Responses $\theta \in (-1, 1)$ to mean group outcomes[28] 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 private information among peers. Both responses to peer outcomes and fundamentals are true peer effects. However, distinguishing between the two responses becomes important when evaluating 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 social interactions 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

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[26] HBS executive section peer groups tend to be small, with approximately three CEOs/CFOs per section in the data. Therefore, modeling individual reactions to the mean characteristics of a very small group of peers will be a reasonable approximation. In different contexts with substantially larger peer groups, the Linear-in-Means model may be less suitable because individuals tend to react to subgroups rather than the group mean (Carrell, Sacerdote, and West, 2010).

[27] Social interactions can also lead to dissimilarity among group members if, for example, individuals choose to be mavericks. Both the model and empirical estimates will allow for this type of peer effect.

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

the intuition developed in herding cascade models (e.g., Banerjee, 1992 and Hirshleifer and Teoh, 2003), $\rho \in (0,1]$ represents the extent to which peer interactions can lead individuals to under-weight their own fundamentals.[29] Note that, in the absence of both common shocks ($\alpha_{sc} = 0$) and peer interactions ($\rho = 1$, $\theta = \phi = 0$), individual outcomes are completely 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. Common shocks can occur if, for example, a particular professor indoctrinates a 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 balance across sections. Nevertheless, in later parts of the paper, 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 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

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[29]Existing Linear-in-Means Models (e.g. Graham, 2008) generally use a linear function of the form $Y_{isc} = \theta \overline{Y}_{sc} + \phi \overline{\varepsilon}_{sc} + \alpha_{sc} + \varepsilon_{isc}$, where $\varepsilon_{isc} = \rho v_{isc}$. Equation (1) in this paper is identical up to a rescaling of the fundamentals by ρ. The introduction of ρ makes it clear that social interactions can lead individuals to underweight their own fundamentals v_{isc}, allows for explicit 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.

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represent peer effects, I take an approach that is common in the literature[30] 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.

A useful and important 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.

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

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

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

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:

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

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, i.e., when the variance ratio is large. In the case of random assignment to peer groups used in this paper, positive peer influence increases the variance ratio from unity to $(1+\gamma)^2$. More generally, if groups are not randomly assigned and there are fundamental differences across groups, such that $\frac{m \cdot Var(\overline{v}_{sc})}{Var(v_{isc}|s)} > 1$, peer influence will amplify[31] existing differences across groups by the

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[30]Sacerdote (2001) contains a detailed discussion of the difficulties of disentangling responses to fundamentals and outcomes, initially referred to as endogenous and exogenous peer effects by Manski (1993).

[31]Amplification of differences in fundamentals across groups occurs regardless of whether peer influence acts through reactions to fundamentals ϕ or reactions to outcomes θ. However, amplification of group-level variation in shocks

factor $(1+\gamma)^2$. Thus, peer influence can be viewed as complementary to explanations of group differences in outcomes based upon group differences in fundamentals. Note that this paper focuses on HBS section peer groups, but the above result applies more broadly. In different contexts, 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.

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.

A second 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 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.

Finally, 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 causal effect of executive networks on firm policies). Extensions of the model to account

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to outcomes $\overline{Y}_{sc}$ does critically depend on the relative values of ϕ and θ. Consider shocks to outcomes that leave fundamentals unchanged, such as regulations that limit takeovers in certain industries or locations. If $\theta = 0$, shocks to peer outcomes will not affect individual outcomes and peer influence will not amplify the variance of mean shocks across groups. If $\phi = 0$, shocks to peer outcomes will be amplified by the full $(1+\gamma)^2$. For intermediate cases in which ϕ and θ are both strictly positive, estimates of $(1+\gamma)^2$ provide an upper bound of the peer effects amplifier with respect to shocks to outcomes that leave fundamentals unchanged.

for selection into the ExecuComp subsample are presented in Appendix C and additional tests, presented in Sections 5.3 and 5.4, will isolate peer effects due to contemporaneous interactions.

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).[32]

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

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

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

Here, i indexes individuals and t indexes firm fiscal years. 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} (e.g., compensation or acquisition policy) 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 by observable selection into similar firms and industries (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. A class year with n executives generates $n(n-1)/2$ pairs. Note that executives graduating from different class years 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

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[32]Fracassi (2008) uses a similar measure of pairs distance but does not use a context with random assignment to peer groups. Rather, Fracassi 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. Fracassi's analysis also does not adopt a Linear-in-Means framework and therefore does not estimate an implied peer elasticity γ.

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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. A β_1 that is significantly negative indicates that section peers are significantly more similar than class peers, as measured by the Pairs Distance Metric, and is evidence of positive peer effects.

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:

$$
\begin{aligned}
\text{Distance Ratio} \quad \delta^{PDM} &\equiv 1 - \frac{E\left[|Y_{isc} - Y_{jsc}|\right]}{E\left[|Y_{isc} - Y_{js'c}|\right]} &&(8) \\
\widehat{\delta}^{PDM} &\equiv -\frac{\beta_1}{\beta_0} &&(9)
\end{aligned}
$$

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:[33]

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

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

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.[34]

4.3 The Excess Variance Metric

Peer influence will tend to reduce variance 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

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

[34]Assuming that $m = 2$ offers a conservative estimate of the true peer elasticity that does not rely on any assumptions of the sampling rate. Given that the peer elasticity is increasing in the square root of group size m, one may wonder if the Linear-in-Means Model implies that the true magnitude of the peer elasticity may be much larger if there are other HBS graduates in executive roles not covered by ExecuComp. However, recent work such as Carrell, Fullerton, and West (2009) and Carrell, Sacerdote, and West (2010) have shown that the Linear-in-Means framework may not apply to larger peer groups in which peer responses become non-linear and individuals begin to react to subgroups. 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 extent 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:

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

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 et al., 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.[35] Therefore, estimates using both metrics are presented in the baseline results and the

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[35] The Excess Variance Metric is less flexible because measurement uses the full distribution of outcomes by sections within each class year – all outcomes must be used for estimation. In contrast, the Pairs Distance Metric uses observations at the pairs level. Pairs of observations for executives in the same industry can be dropped from the sample for tests of "excess" peer influence: peer similarities beyond what can be explained by selection into similar

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.

To assess significance levels for estimates from the Pairs Distance Metric, I 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 (2006) and Peterson (2008). This accounts for both serial correlation and the fact that each executive $\times$ year appears multiple times in the formation of paired observations.

Estimation of standard errors and significance levels for the Excess Variance Metric uses a non-parametric permutation test in the style of Fisher (1922) and Rosenbaum (1996). This test is also applied to estimates from the Pairs Distance Metric as a robustness check. 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 $\widehat{\boldsymbol{\beta}}$ 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 lotteried into 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; this accounts for serial correlation. Using the simulated section assignment in each placebo test, I re-estimate the vector of parameters $\widehat{\boldsymbol{\beta}}^{placebo}$. 10,000 placebo estimates are simulated, generating a standard error around the null hypothesis and $G\left(\cdot\right)$ as the empirical cumulative distribution function of the placebo effects. Applying $G\left(\cdot\right)$ to the original point estimates offers a p-value estimate of significance.

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industries. The Pairs Distance Metric also allows measurement of the degree of similarity between actual outcomes and peers' predicted outcomes (described in detail in Section 5.4 with regard to tests for pay for friend's luck).

5   Results

5.1   Verifying Balanced Conditionally-Random Section Assignment

I begin with empirical support for the assumption of balanced conditionally-random section assignment (which, as shown in Appendix A, results in a small bias against findings of positive peer effects relative to the case of completely random section assignment). Under this type of 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 from 1949 to 2008 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" are defined as the fraction of section peers that share each individual's characteristics (e.g., undergraduate institution). "Class commonalities" are defined similarly as the fraction of class peers that share each individual's characteristics. The commonalities ratio measures the ratio of section commonalities to class commonalities. For citizenship, undergraduate institution, and gender, the commonalities ratios are all slightly less than one, showing that section peers are not more similar than class peers with respect to these ex-ante characteristics. Moreover, a paired t-test of equality can confidently reject equality of section and class commonalities for all three characteristics with p-values under 0.0001. In other words, class peers are slightly but significantly more similar than section peers in terms of ex-ante characteristics. This empirically supports the theoretical argument presented in Appendix A that, relative to a completely random lottery, balanced section assignment presents a small bias against findings of positive peer effects.

While Panel (A.1) presents results using data for all HBS MBA graduates from 1949 to the present, results are also similar for subsamples. In unreported results (available upon request), I 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 ex-ante commonalities measures for the executive subsample covering HBS graduates who appear as top earners in the ExecuComp database. Results are very similar despite the reduced sample size.

5.2 Baseline Measures of Peer Influence

In Panel (B) of Table 4, peer commonalities in terms of executive labor market outcomes that are determined *after* graduation tell a sharply different story relative to the ex-ante measures presented in Panel (A). Ex-post measures include firm employment, director overlap, industry choice, and geographic location of firm headquarters. Observations are at the individual level, although each individual can be associated with more than one outcome; e.g., an individual can have more than one industry affiliation due to employment changes over time. The sample includes all HBS top executives who appear in ExecuComp. Commonalities are measured over the entire known employment history of each executive and are not restricted to concurrent overlap; for example, two executives will overlap in affiliation with the auto industry if both executives were employed in the auto industry at any time in their careers.

The commonalities ratios in Panel (B) show that section peers are 25 percent more likely to overlap in firm employment (this includes all known past and present direct employment and board affiliations) than class peers. Similarly, 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).[36] Relative to class peers, section peers are about 25 percent more likely to overlap in SIC3 and Fama French 49 industry affiliation. Finally, section peers are 10 and 50 percent more likely to overlap in firm headquarter state and city locations, respectively.[37] P-values in the second-to-last column indicate that section commonalities are generally significantly larger than class commonalities – all p-values are well below the ten percent level except for the p-value for firm employment, which is marginally significant.

In unreported results (available upon request), I also test whether students are more likely to become executives if their section peers becomes executives, e.g., if peers help each other attain high-level management positions. In general, I find very noisy estimates – section peers are not more likely to appear in the ExecuComp data relative to class peers.

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

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[36] Relative to class peers, section peers are significantly more likely to manage firms with overlapping directors. However, the probability of director overlap is low in general, as shown in Table 4. 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 effects are not driven only by director overlaps.

[37] These refer to the industry and location of each executive's primary employment and are not driven by board overlaps or other secondary roles.

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.

Altogether, the results in Table 4 show that section peers are not more similar than class peers prior to matriculation at HBS, but have sharply more similar outcomes after graduation. These results demonstrate that peers can significantly affect the career trajectories of executives and are consistent with survey evidence from Kaniel, Massey, and Robinson (2010) showing that MBA peers are able to predict a graduate's job market success. Note also that since firm, industry, and location choices typically occur before executives begin making management decisions in their firms, Table 4 also illustrates the importance of "past" social interactions (defined as peer interactions occurring prior to executive's managerial roles at S&P 1500 firms) in determining executive career outcomes.

Table 5 presents evidence of baseline peer effects in executive compensation. In this and future tables, I restrict the sample to CEOs/CFOs, unless otherwise noted, because CEOs/CFOs arguably have the greatest control over firm outcomes. 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. However, extensions of the baseline tests will focus on annual changes, because changes in compensation are more useful for identifying responses to shocks over time.

Column (1) presents evidence of peer influence in direct compensation. Panels (A.1) and (B.1) show results using the Pairs Distance Metric, described in Section 4.2. The relevant reduced-form statistic is the distance ratio δ^{PDM}, which measures the extent to which section peers are more similar on average than class peers (equal to the fractional difference in absolute distance in compensation between section pairs and class pairs). The distance ratio in Panel (A.1) shows that section peers are 9 percent more similar than class peers in annual levels of direct compensation. Similarly, the distance ratio in Panel (B.1) shows that section peers are 12 percent more similar than class peers in annual changes in direct compensation. Both distance ratios are significant at the five percent level or lower.

The results in Panels (A.2) and (B.2) tell a similar story using the Excess Variance Metric, described in Section 4.3. The relevant reduced-form statistic here is the excess variance ratio 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. Both variance ratios are also significant at the five percent level. These variance ratios offer a direct measure of how peer influence can lead to clustered financial outcomes. In the context of HBS sections, the excess variance ratio measures the extent of excess clustering of compensation within sections in each class year. While this paper focuses on HBS sections in order to use the natural experiment of randomly assigned peer groups, a similar peer effects mechanism could lead to clustering of outcomes along other dimensions if peer group boundaries were defined at the geographical or industry level.

Under the additional assumptions of the Linear-in-Means Model, which assumes that individuals react to the mean of peer characteristics, both the Pairs Distance and Excess Variance Metrics imply a lower bound[38] for γ, the peer elasticity. For both levels and changes in direct compensation, both metrics imply a significant peer elasticity of around 20 percent. This implies 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 in the mean fundamentals of section peers lead to a \$1 million increase in mean section peer direct compensation. Scaled in this manner, estimates for γ show that peer effects are large compared to the direct responses to changes in own characteristics.

Column (2) of Table 5 repeats the analysis for peer influence in total compensation (direct plus equity-linked compensation). In general, estimates of peer effects for total compensation are not significantly different from zero. Point estimates can be economically large – the peer elasticity can be as large as 10 percent for levels of total compensation – but tend to be smaller than those estimated for direct compensation in Column (1). In Section 5.5, I find significant and large peer effects in total compensation using the Forbes compensation data which covers an earlier time period. I also discuss institutional structures relating to equity-linked compensation (such as

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[38]The estimate of γ is a lower bound for two reasons. First, γ is strictly increasing in peer group size (as shown in Section 4) and estimates assume the minimum group size of two. Second, γ is calculated from the marginal increase in peer influence among section peers relative to class peers, and assumes zero peer influence among class peers. If peer influence is positive among class peers, the true γ will exceed the estimates presented in this paper.

multi-year cycles and lumpiness) which confound analysis of total compensation in the more recent sample period. For now, I focus on results using direct compensation.

The baseline executive compensation results in Table 5 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 enter similar firms, industries, and geographical regions as documented in Table 4. Selection into similar firms is a true peer effect but may be less informative of how peers directly affect firm policies (see Appendix C). In Table 6, I 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. These tests help to pin down the underlying mechanisms.

Columns (1) through (4) of Table 6 progressively add more controls to the first stage estimation and sample restrictions to the second stage estimation. For brevity, only results using the Pairs Distance Metric are reported. Column (1) replicates baseline results from Column (1) of Table 5 for direct compensation. Column (2) adds controls for firm and industry SIC3 current and lagged fiscal year returns, Fama French 49 industry fixed effects, as well as controls for firm size as measured by the log of fiscal year sales. 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.[39] Estimates in Panel (B) of Column (2) also exclude observations representing executive transitions to different firms. Column (3) adds a full set of Fama French industry fixed effects interacted with year fixed effects to control for industry trends over time. Column (4) restricts the second stage sample to pairs of executives belonging to different Fama French 49 industries. This further reduces the likelihood that compensation similarities are driven by industry similarities among section peers. Overall, the distance ratios and implied peer elasticities γ remain remarkably stable and significant as more controls and sample restrictions are introduced. The distance ratios consistently exceed 0.08 and 0.09 for levels and changes in direct compensation, respectively, and imply peer elasticities exceeding 16 percent.[40]

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[39] Results, not reported, remain similar in Column (2) if the first stage is restricted to the HBS executive sample.

[40] The addition of controls in the first stage may offer a conservative measure of excess peer influence because they may over-control for firm and industry characteristics that are endogenously chosen by executives. For example, two executives that are close friends may choose to enter the same industry and then continue to actively discuss firm policy. Their firm policies would display similarities beyond what could be expected by entry into the same industry. The addition of firm and industry controls or the restriction of the sample to peers in different industries would not capture this additional effect. For completeness, this paper presents results with and without additional industry and

Table 7 turns to evidence showing that section peers are relatively more likely to pursue similar acquisitions strategies.[41] 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 acquisitions (see discussion of 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. These differences are significant at the five percent level or lower. Excess variance ratios in Panel (B) Columns (1) and (3) show that the ratio of between- to within-section variance is more than 25 percent greater than expected under the null hypothesis of no peer effects. In a similar vein to the earlier results for compensation, the variance ratios for acquisitions show that peer influence leads to the excess clustering of acquisitions activity within sections.[42]

The baseline distance ratios and excess variance ratios in Columns (1) and (3) imply peer elasticities in the range of 13 to 25 percent. These peer elasticities are significant at the one percent level and economically large. The variance ratios imply that the expected 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.

In Columns (2) and (4), additional controls and sample restrictions, listed in the bottom panel, are introduced in the first stage in order to narrow the set of possible mechanisms driving observed peer effects. Relative to the baseline results, the estimates drop slightly and imply peer elasticities ranging from 10 to 13 percent. The drop in estimated magnitudes suggests that a portion of the similarities in firm acquisition policy among section peers could be due to past peer interactions leading to selection into similar firms and industries. However, peer effect magnitudes remain

firm controls.

[41] Peer influence can potentially affect other firm policies. In unreported results (available upon request), I explore peer effects in investment policy, dividend policy, leverage ratios, cash reserves, and overall firm returns. The results generally exhibit large standard errors. There is significant evidence of peer influence in investment activity. This result is consistent with the high estimates of peer effects in acquisitions (because acquisitions represent a large discrete form of investment) and is also supportive of Fracassi (2008), which finds that investment activity is correlated among connected peers. However, measures of peer effects for investment policy become small and insignificant after the addition of controls for firm and industry trends in the first stage estimation. Overall, evidence of peer effects in other management styles is weak. Peer effects may exist, but the estimates are too noisy to offer definitive conclusions.

[42] Fracassi and Tate (2010) find related evidence of peer effects in acquisitions strategy by showing that firms with more CEO-director connections make more frequent acquisitions, which destroy shareholder value on average. In contrast, this paper focuses on social connections outside the firm (rather than close CEO-director connections) and shows that executives are more likely to acquire when peers in their education networks acquire, even when those peers manage firms in distant industries.

sizeable and significantly different from zero even with controls.

Tests of "excess" peer influence in Tables (5) and (6) show that the distance ratio remains significant and sizeable after controlling for a comprehensive set of controls for firm and industry performance and other trends. These tables show that peer effects in compensation and acquisitions are not driven by past interactions leading to selection into *observably* similar firms, but rather are suggestive of the role of contemporaneous interactions as the mechanism driving peer similarities. However, the addition of firm controls cannot rule out selection into *unobservably* similar firms. In the next set of results, I explicitly test for the underlying mechanisms behind peer influence in compensation and acquisitions.[43]

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 and executive roles (this includes selection into the ExecuComp subsample). Past interactions are informative about peer interdependencies in labor market trajectories. 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 contemporaneous peer-bonds. Reunions at HBS are extravagant four day celebrations consisting of formal galas and panel discussions, as well as section-based tents and parties. Reunions are well attended[44] – for example, more than 40 percent of the class of 1985 pre-registered to attend the Fall 2010 reunion celebrations. Aside from direct reunion attendance and interaction, reunions also affect social interactions through the escalation of accompanying HBS 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. Individual donation amounts and section-based giving records are then published in a brochure

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[43]While higher levels of acquisitions activity significantly predict higher levels of compensation, peer similarities in compensation do not appear to be driven by peer similarities in acquisitions. Analysis of peer effects in compensation controlling for a flexible set of controls for acquisitions (including total value, value to assets ratio, and number of acquisitions) in the first stage estimation yields very similar estimates for the peer elasticity for compensation. For brevity, these results are omitted and available upon request.

[44]While historical reunion attendance records are not available, supplementary results in Section 6.2 proxy for reunion attendance and salience using individual data from reunion contribution campaigns.

that is mailed to all graduates. In addition, all graduates are encouraged to update their personal information and accomplishments in the official "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 exclusive private gatherings following formal reunion activities.

Aside from providing evidence of contemporaneous peer interactions, reunion cycle variation in peer effects offers a check on potential bias from section-specific common shocks. An example of a common shock is a professor who teaches students in a particular section to be aggressive in compensation negotiations and acquisitions. This shock would lead to ex-post similarities among section peers that are not the result of peer influence. However, section-specific common shocks are much more likely to occur in the past – they are not likely to lead to peer effects that vary by the reunion cycle.[45]

Figures 1 and 2 show that within-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 (reunion year zero represents the year of the reunion). Figure 2 repeats the exercise for distance ratios in acquisitions policy, as measured by the completed acquisition 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. The plotted distance ratios show that section peers are 20 and 15 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. It is also noteworthy that the distance ratios for annual changes in direct compensation sequentially fall in magnitude in the years following reunions. This likely reflects the strong serial correlation in compensation changes and documents the declining effects of reunions on peer relations over time.

Table 8 presents more detailed evidence that peer influence becomes significantly stronger fol-

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[45]It is conceivable that section members receive section-specific common shocks *at* reunions, which have activities organized by section tents and section parties. However, external common shocks are unlikely to have large effects on executive behavior because section activities are generally attended only by section members. Reunion seminars with influential outside speakers usually address class-wide audiences and should not affect the degree of similarity among section peers relative to class peers. In addition, evidence of pay for friend's luck (see Section 5.4) provides an additional check against bias from section-specific common shocks.

lowing alumni reunions. Panel (A) examines annual changes in compensation.[46] For both direct and total compensation, the distance ratio and γ in the year following reunions is two to three times larger in magnitude than in the other four years of the reunion cycle. Magnitudes are also large – section peers are 20 and 10 percent more similar then class peers (with implied peer elasticities of up to 40 percent), for direct and total compensation, respectively, in the year following reunions. Results are robust to the addition of firm and industry controls in Columns (2) and (4). This is to be expected since the exogenously timed reunions should not be strongly correlated with firm and industry controls. Panel (B) repeats the analysis for annual levels of acquisitions. For both acquisition attempts and acquisition completions, the distance ratio is at least three times larger in the year following reunions than in other years. Magnitudes are again large – section peers are around 15 percent more similar than class peers in the year following reunions and the implied peer elasticity γ exceeds 0.30.

P-values at the bottom of each panel test whether the distance ratio measure of peer effects in the year following reunions is equal to the distance ratio in other years. Equality can be rejected at the ten percent level or lower for all cases except total compensation, which may be noisier for reasons described in Section 5.5. Calculations described in Appendix C show that, under the extreme assumption that only the marginal increase in peer similarities following reunions reflects contemporaneous interactions, contemporaneous interactions following reunions can lead to substantial peer elasticities of over 20 percent. These numbers may be over-conservative in the likely event that a steady level of contemporaneous interactions occur in all years, which are then magnified during reunion years.

Altogether, tests using alumni reunions show that peer effects in compensation and acquisitions are driven by contemporaneous interactions rather than past interactions that lead to selection into the executive subsample or into similar types of firms.[47] Results further offer a check against

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[46]Unreported results for annual *levels* of compensation do not vary significantly by reunions. This is likely due to the fact that levels of compensation are even more strongly serially correlated than changes in compensation, i.e., compensation levels cannot become more similar in reunion year + 1 and then sharply less similar in reunion year + 2.

[47]In theory, reunions may also strengthen social bonds among class peers as well as section peers and/or affect the overall levels of compensation and acquisitions in addition to similarity measures among peers. Empirical tests show that reunions do not significantly affect class peer similarities with respect to compensation or acquisitions policy. This may be because section peers are more likely to interact at reunions and because some reunion events are organized around section tents. Empirical tests show that the raw levels of direct compensation increases by three to four percent in the year following reunions, even with a detailed set of controls for firm current and lagged performance and industry × year fixed effects. This suggests that reunions cause executives to negotiate pay raises that cannot be explained by observable changes in firm performance. In contrast, acquisitions levels do not rise significantly following reunions, even though reunions do lead to increased within-section peer similarity in acquisitions policy. This result is consistent with the view that peer effects in acquisitions are driven by the sharing of information among peers rather

bias from section-specific common shocks. Additional reunion-related results are presented in Section 6.2, which explores proxies for reunion attendance and salience using data on individual-level reunion contribution campaign participation.

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

Having established that contemporaneous interactions can account for a majority of observed peer effects in compensation and acquisitions following reunions, the next section explores whether peer influence reflects reactions to peer fundamentals or outcomes. As described in Section 4.1, a reaction ϕ to fundamentals can represent learning of private information or management skills from peers. A reaction θ to peer *outcomes* can represent the effects of relative earnings on compensation (e.g., through "keeping up with the Joneses" preferences 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.

It is possible to estimate 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. For the specifications in Table 9, 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).[48] The distance ratio δ^{PDM} is again defined as $-\beta_1/\beta_0$ and represents the fractional difference in similarity between a pair of section peers and a pair of class peers. However, I do not estimate a peer elasticity 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

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than by the universal desire to increase acquisition activity following social interactions. For brevity, these empirical tests are not reported in table form and are available upon request.

[48] Note that, for each pair of executives i and j, there are two observations per fiscal year. The first observation has the absolute difference between i's change in residual compensation and j's change in predicted compensation as the dependent variable. The second observation has i's and j's roles reversed.

actual outcomes and peers' predicted outcomes.

A potential concern with pay for friend's luck tests is that, for an executive pair i and j in different industries, 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 the magnitudes of the distance ratios remain stable (or grow larger) in Columns (2) and (4) which take the more conservative approach of excluding 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 1% of a supplier industry's total output or if a supplying industry supplies at least 1% 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 six to ten 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 seven percent more similar than class peers, although distance ratios lose statistical significance. All standard errors are allowed to be double clustering observations by each member in an executive pair. Standard errors remain similar (and are generally smaller) using a placebo permutation test (described in Section 4.4) in which executives are assigned to placebo sections within each classyear to show that the point estimates for peer similarities are unlikely to be generated by chance.

Evidence of pay for friend's luck contributes to our understanding of the determinants of executive compensation. The results are supportive of evidence in Bertrand and Mullainathan (2003) showing that executives are rewarded for more than their effort or skill. Bertrand and Mullainathan find that executives are paid for lucky industry shocks in their own industry. This paper shows

that executives (after controlling for own firm performance) are paid more when their friends, in different industries, receive lucky shocks to their compensation. These results show that peer similarities in compensation are not driven by similarities in underlying managerial skills or other fundamentals. Rather, relative earnings within executive social networks directly affect compensation. These results are consistent both with relative earnings directly entering into each executive's utility function (e.g., executives bargain harder when their friends receive lucky pay shocks[49]) and with outside options changing with the industry performance of peers in one's social network.

Evidence of pay for friend's luck has two other important implications similar to those from the analysis of reunions. First, evidence of pay for friend's luck shows that 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 peer interactions leading into selection into similar types of firms should not lead to compensation that varies over time with lucky shocks to peers.

Panel (B) of Table 9 modifies the pay for luck 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 the pay for friend's luck results hold with a one year lag between leaders (represented by the person with the predicted lucky compensation) and followers. Estimates of peer effects in Panel (B) are very similar to those in Panel (A) and have even higher significance levels. Evidence of lagged responses to peer's lucky shocks are strongly consistent with a theory of leaders and followers in peer compensation.

However, lagged effects must be interpreted with caution and should be viewed as exploratory. In general, it is difficult to conclusively test for leaders and followers 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 outcomes and industry shocks tend to be strongly serially

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[49]"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 related evidence of social incentives in worker productivity). However the results in Table 9 include a large set of controls for own firm and industry performance. Therefore, the results suggest that pay for friends' luck effects do not occurr through the increased managerial productivity channel.

correlated. In the specifications 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 $j's$ predicted lucky compensation. However, since lucky industry shocks are serially correlated, a significant distance ratio 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 in Tables 5 and 7 allowing for a one-year lag in peer responses using the Pairs Distance Metric. Results yield significant peer elasticities of up to 20 percent. These results are again very consistent with the presence of peer leaders and followers. However, the results 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) through (3) address an important caveat to the measures of peer effects in compensation presented thus far. In general, estimates are significant and large for direct compensation but not 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 this paper's baseline 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 that are set several years in advance of the annual option grants. Option grants generally fall into one of three categories: fixed value plans (a fixed value of options are distributed each year), fixed number plans (a fixed number of shares are distributed each year), and mega grants (a large lumpy distribution of shares once every few years). Under all three plans, the value of options granted in any particular year is fixed as of the start of the multi-year cycle. Since firms differ in the timing of their multi-year stock option cycles, which are adjusted and renegotiated only once every several years, we are unlikely to observe significant levels of annual-level peer similarities in total compensation.

However, peer effects in total compensation can be large and significant in certain contexts. One important context is the period prior to the rise of equity-linked compensation. To examine this earlier time period, I match HBS alumni to records contained in the Forbes data which covers

executive compensation from the years 1970 to 1991 for approximately 800 companies each year.[50] Total compensation during this earlier time period is dominated by cash payments; the mean and median direct compensation as a fraction of total compensation among Forbes HBS executives is 84 and 93 percent, respectively. Row (1) in Table 10 presents estimates of peer effects using the Forbes data as represented by the distance ratio. Annual changes in total compensation are 17 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 estimate exceeds the distance ratio of 12 percent in Table 5 for annual changes in direct compensation using the more recent ExecuComp data and suggests that peer effects for total compensation were large in the period prior to the rise of option grants.

The Forbes data is also informative because it offers a supplementary estimate of strong peer effects in compensation using an independent sample; the Forbes and ExecuComp samples cover non-overlapping time periods from 1970-1991 and 1992-2008 respectively. Thus the Forbes data offers an out-of-sample check on the baseline results presented in Table 5.

Rows (2) and (3) of Table 10 revisit the baseline ExecuComp HBS data, but restrict the sample to pairs of executives for which direct compensation accounts for more than half or two-thirds of total compensation, respectively, for each executive. The estimated distance ratios are highly significant and larger than those in the baseline results in Table 5. Overall, these results show that peer effects in total compensation can be strong and significant in samples for which cash payment accounts for a large proportion of total compensation. In other words, peers can significantly affect total pay, rather than just the composition of total pay (i.e., the division of total compensation into direct and equity-linked pay). However, there may also be important compositional effects in total compensation among the subset of executives receiving a significant proportion of their pay in equity-linked compensation. 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 annual 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. Rows (4), (7), and (8) present alternative estimates of significance levels and standard errors using the permutation

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[50]The Forbes data covers executive compensation at the 500 largest companies by revenues, income, total assets, or market capitalization. Coverage includes approximately 800 companies each year. Matching between the HBS alumni data and the Forbes data follows the same procedure as the main matching procedure between the HBS alumni data and the ExecuComp data: a fuzzy name match is verified using BoardEx records, executive biographies, alumni records, political donations, and annual reports.

test described in Section 4.4. The permutation test is essentially a Monte Carlo simulation of a large number (10,000) of falsification tests. Reported significance levels represent how unlikely it is to arrive at the point estimate for the distance ratio if executives in the data are randomly shuffled into sections within each class year. Results using the permutation test estimate standard errors that are slightly smaller than those in baseline estimates and suggest that the double-clustering method of estimating standard errors for the distance ratio used in all other tables offers a conservative estimate of significance. Row (5) uses direct compensation winsorized at the top and bottom one percent levels (with cutoffs determined by the full ExecuComp CEO/CFO sample) in the first stage estimation. Row (6) excludes observations after fiscal year 2006 to ensure that results are not driven by a change in SEC compensation disclosure rules.

Rows (9) through (11) of Panel (B) test the robustness of the baseline results for acquisitions using alternative measures of acquisitions activity. Row (9) 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 (10) uses the number of acquisitions completed. Row (11) 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 (see Netter, Stegemoller, and Wintoki, 2010, for a summary), this measure will miss some acquisitions with high but unreported transaction value. For all three alternative measures of acquisitions activity, the distance ratio is greater than 0.09 and is highly significant at the one percent level.

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 outcomes. 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 described in Section 4.2, I form all possible pair combinations of executives graduating from the same class year. In this extended sample, executive pairs are classified into one of three categories. An executive pair is

considered 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 separate estimation of distance ratios and peer elasticities γ for each of the three types of executive pairs.

Table 11 presents estimates of peer effects in compensation and acquisitions for the expanded sample of all top earners in ExecuComp who graduated from HBS. In Column (1) for annual changes in direct compensation, the distance ratio and associated peer elasticity γ are significant only for the CEO/CFO pairs, and the point estimates for the CEO/CFO pairs are twice as large as those for the other two pair types. The estimates for total compensation tell a similar story, although all point estimates are insignificant. Overall, results show that peer influence in compensation is strongest for CEOs and CFOs, although p-values at the bottom of the table indicate that the differences relative to other top executives are not significant given the sample size.

Tests of peer influence in acquisitions in Columns (3) and (4) suggest a larger divide between CEOs/CFOs and other top executives. Only the distance ratio and associated peer elasticity γ for CEO/CFO pairs are large and significant. Estimates of peer effects among the two other types of executive pairs are close to zero with small standard errors. The p-values at the bottom of the table can further reject that the distance ratios are jointly equal across the three types of executive pairs for attempted acquisitions at the one percent level. Thus, the acquisitions-related results show that, in the sample, only CEO/CFO peers influence each other in terms of acquisitions activity.

Overall, I find that CEOs and CFOs exercise greater authority over compensation and acquisitions policy relative to other top earners. These results cut 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, I find suggestive evidence against an outside opportunities theory of peer effects in compensation. If peers' compensation (even in distant industries) represent the outside opportunities of executives, this would lead to correlated compensation within executive networks. However, if outside opportunities represent the sole mechanism driving peer effects, top executives other than the CEO and CFO should also experience correlated compensation.

6.2 Participation in Reunion Contribution Campaigns

In light of earlier results relating to alumni reunions, a natural question is whether executives in the sample attend reunions or are affected by reunion year activities, i.e., whether the assumption that reunions strengthen peer bonds is valid. While historical individual-level data on reunion attendance is unavailable, individual-level data on participation in reunion contribution campaigns offers a proxy for the salience of reunions. Reunion contribution campaigns occur every five years after each executive's graduation year and coincide with reunion events. These reunion campaigns are organized around section competition; 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 sample observations at the executive $\times$ year level, the executive contributed to the most recent previous reunion campaign. The mean 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 connectness of HBS executives during reunion years.[51] This is because executives can attend reunion events without participating in reunion contribution campaigns and vice versa. In particular, 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 present two measures of reunion campaign participation. Pairs of executives in a given fiscal year are considered "recent donors" if they both participated (donated any amount) in the most recent reunion campaign. Meanwhile, pairs of executives are considered "high donors" if they both contributed at least \$1000-\$2500 in at least one reunion campaign held between 1990 and 2008.

Table 12 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

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[51]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.

are both donors, as measured by either the "recent donor" or "high donor" designation.[52] For annual changes in direct compensation, recent and high donor pairs have distance ratios that are two to three times larger than non-donors in the year following reunions. This result is reassuring because reunions should have relatively stronger effects for the subset of students for whom reunions are most salient. A similar, albeit noisier, trend occurs for total compensation, attempted acquisitions, and completed acquisitions: distance ratios are generally 25 to 50 percent larger for donors than non-donors in the year following reunions.[53] However, p-values at the bottom of each panel cannot reject equality of the two distance ratios except for the case of direct compensation in Column (2). Altogether, the point estimates in Table 12 are supportive of the assumption that reunions strengthen peer bonds because participating reunion donors experience larger changes in peer similarities following reunions than non-donors.

7 Conclusion

I demonstrate that executive social interactions are important determinants of 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 strong peer effects in executive compensation and acquisitions 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 staggered alumni reunions, which act as shocks to contemporaneous interactions. Tests of "pay

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[52]Note that the direct effect of campaign participation on compensation or acquisitions should not affect the distance ratios, which measure the relative degree of similarity among section peers as compared to class peers.

[53]The notable exception is that the distance ratio for non-donors is 20 percent greater than the distance ratio for donors following reunions in the case of completed acquisitions using the "recent donor" measure in Panel (B), Column (3). However, the difference in distance ratios is highly insignificant with a p-value of 0.77.

for friend's luck" further narrow the mechanism driving peer effects in compensation. Results show that compensation responds significantly 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, evidence of pay for friend's luck shows that peer effects in compensation are not driven by similarities in underlying managerial productivity. Rather, relative compensation within a peer network directly affects compensation. In other words, executives are paid for more than firm performance and even industry performance; they are paid for lucky shocks in their social networks. This can occur if compensation negotiations depend either on the relative earnings preferences of executives or if lucky shocks to peers change the outside options of executives in different industries.

Further, executive peer effects imply that executives matter for firm policies in a systematic way that can drive striking aggregate patterns. Multipliers arising from social interactions imply that the aggregate impact of a change in the fundamental determinants of compensation or acquisitions activity will be larger than the direct effect because of contagion among connected agents. Positive peer effects also lead to correlated behavior among group members which amplify fundamental differences across peer groups. 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 operating among peer groups at the industry or geographic level would amplify fundamental differences across these groups and thereby contribute to aggregate clustered financial activity.

While executive peer influence has clear implications for our understanding of the determinants of firm policies, consequences for efficiency and social welfare are less obvious. Social interactions leading to the pursuit of similar acquisitions strategies may be efficient if private information about the optimality of mergers is transmitted through social networks. Similarly, peer influence in compensation has ambiguous efficiency implications. Results show that compensation does not always reflect firm performance, i.e., executives receive pay for friend's lucky shocks after controlling for own firm performance. However, pay that depends on relative earnings is not necessarily inefficient if, for example, it increases long term effort or reduces the probability of costly job transitions. Empirical investigation of the efficiency implications of peer influence among executives is a very promising direction for future research.

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Appendix 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. Absent peer effects, mean-balanced sections should lead to weakly negative estimates of 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, e.g., compensation. 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. 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, $Y_{is} = f(X_{is}) + \varepsilon_{is}$ where $f(\cdot)$ 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:

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

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:

$$\frac{m \cdot E\left[Var\left(\overline{Y}_s^{balanced}\right)\right]}{E\left[Var\left(Y_{is}^{balanced}|s\right)\right]} = \frac{f'(\mu)^2 \frac{1}{m} E\left[\iota^{\mathsf{T}}\Sigma_s^{balanced}\iota\right] + \sigma_\varepsilon^2}{f'(\mu)^2 \cdot E\left[Var\left(X_{is}^{balanced}|s\right)\right] + \sigma_\varepsilon^2}$$

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

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 $E\left[\gamma^{balanced}\right] \leq 0$. ∎

Appendix B: Individual Optimization and the Linear-in-Means Model

This section presents a sample underlying utility function that generates the optimal individual actions assumed in the Linear-in-Means Model described in Section 4.1. Consider individual i in section s in class year c. Let section s represent each individual's relevant peer group. Individuals choose outcomes Y_{isc} to maximize the following indirect utility function, taking the mean peer group outcome $\overline{Y}_{sc}$ as given:

$$\max_{Y_{isc}} V(Y_{isc}) = (\alpha_{sc} + \rho v_{isc}) Y_{isc} - \frac{1-\beta}{2} Y_{isc}^2 - \frac{\beta}{2}\left[Y_{isc} - \left(\theta_* \overline{Y}_{sc} + \phi_* \overline{v}_{sc}\right)\right]^2. \quad (16)$$

The first two terms follow a standard quadratic utility form while the third term reflects peer influence, represented here by a preference for group conformity. $\beta \in (0,1)$ measures the relative importance of peer influence in the individual optimization problem.[54] $(\alpha_{sc} + \rho v_{isc})$ acts as a scale factor, with larger values leading to higher optimal values of Y_{isc}. α_{sc} represents section-specific

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[54]Although β also enters in the second term of the utility function, only the relative ratio of $(\alpha_{sc} + \rho v_{isc})$ and $\frac{1-\beta}{2}$ matters in the case of quadratic utility. As is clear in the optimal policy function in Equation 17, the relative weights on the three terms in the utility function are free up to a rescaling of α, v, θ, and ϕ.

common shocks such as a professor who affects the outcomes of all students in her section. Student-level fundamentals (e.g., ex-ante skill or private information) are represented by v_{isc}. $\rho \in (0,1]$ represents the extent to which peers lead individuals to underweight their fundamentals relative to the null hypothesis in which there are no social interactions. Note that, in the absence of both common shocks ($\alpha_{sc} = 0$) and peer influence ($\rho = 1$, $\beta = 0$), the only parameter governing the utility function is the individual fundamental v_{isc}.

The third term reflects a desire to conform to a linear function of mean group characteristics $\left(\theta_* \overline{Y}_{sc} + \phi_* \overline{v}_{sc}\right)$. As described in detail in Section 4.1, individuals can respond to mean group outcomes $\overline{Y}_{sc}$ and/or mean group fundamentals $\overline{v}_{sc}$. Responses θ_* to mean group outcomes occur when individuals react directly to $\overline{Y}_{sc}$. Responses ϕ_* to mean group fundamentals occur when individuals react to $\overline{v}_{sc}$.

Solving for the optimal action yields the following, with θ and ϕ rescaled for ease of exposition:

$$Y_{isc} = \alpha_{sc} + \theta \overline{Y}_{sc} + \phi \overline{v}_{sc} + \rho v_{isc} \quad , \quad \theta \equiv \beta \theta_* \quad \text{and} \quad \phi \equiv \beta \phi_* \ . \tag{17}$$

Optimal outcomes from the sample utility function in equation (16) take the form assumed by the Linear-in-Means Model.

Appendix C: 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. Let v_{isc} be the latent ex-ante (pre-business school) fundamental determinant of Y_{isc} (e.g., if Y_{isc} is executive compensation, then v_{isc} is the ex-ante skill of person i that would determine compensation if person i becomes an executive). Random sectioning guarantees:

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

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:

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

Let ω_{isc} represent the intermediate fundamental determinants of outcomes Y_{isc} as of Stage 2. ω_{isc} is a function $g\left(\cdot\right)$ 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:

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

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). Assuming no common shocks, γ^{past} represents peer effects operating through past peer interactions.

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 $\gamma^{contemp}_{base}$ in the year immediately following reunions:

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

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:

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

The above ratio offers a lower bound for contemporaneous peer effects in the year following reunions because it assumes that $\gamma_{base}^{contemp} = 0$ 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	576			1052		
Observations (executive x year)	2895			6379		
Number section peers per executive (excl. self)	1.750	2	1.284	3.135	3	1.862
Number class peers per executive (excl. self)	13.139	13	5.285	23.791	25	8.724

The HBS executive sample covers MBA alumni who graduated from 1949 to 2008 and are also listed in the ExecuComp dataset, 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 (a) 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 5 (to exclude regional or divisional CEOs and CFOs), or (3) her annual 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 listed 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

	Harvard CEOs/CFOs			All CEOs/CFOs		
	Mean	Median	S.D.	Mean	Median	S.D.
Salary $K	576	517	304	506	428	289
Bonus $K	816	428	1084	599	304	872
Direct compensation $K	1185	819	1122	933	641	922
Total compensation $K	3796	1993	4932	2716	1306	3983
Annual percent change in direct compensation	14.6%	5.9%	61.3%	12.6%	5.8%	62.8%
Annual percent change in total compensation	47.0%	5.6%	284.2%	43.6%	5.9%	247.7%
Percent female	1.7%			0.1%		
Age	57.7	58	9.3	58.6	58	9.4
Firm tenure (years)	9.8	6	10.0	12.6	9	11.5
Fiscal year firm return	1.198	1.064	3.503	1.609	1.062	29.942
Fiscal year SIC3 industry return	1.216	1.175	0.398	1.217	1.179	0.423
Sales ($M)	6581	1462	18539	4540	1095	14156
Number of attempted acquisitions	1.57	1	3.07	1.27	0	2.58
Number of completed acquisitions	1.25	0	2.42	1.05	0	2.20
Fraction attempted at least one acquisition	0.54			0.48		
Fraction completed at least one acquisition	0.50			0.43		
Value of completed acquisitions $Mil	1022	138	3430	892	121	3903
Observations (executive x year)	2895			52260		

Compensation data comes from ExecuComp. *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 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. *Percent female*, *age*, and *firm tenure* data come from ExecuComp and are supplemented, if missing, with data from BoardEx. *Firm* and *industry 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.

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							
Firm employment	0.0256	0.0203	1.259	0.1370	964	0.0161	
Director overlap	0.0579	0.0469	1.233	0.0280	964	0.0453	
Industry SIC3	0.0370	0.0294	1.259	0.0684	964	0.0272	0.0196
Industry Fama French 49	0.0646	0.0521	1.240	0.0176	964	0.0482	0.0405
State of headquarters	0.0958	0.0854	1.122	0.0850	964	0.0791	0.0653
City of headquarters	0.0266	0.0169	1.569	0.0048	964	0.0153	0.0092

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* measure the fraction of section peers (same section and same class year) that have at least one characteristic (e.g. undergraduate institution) in common with the student. *Class commonalities* measure the fraction of class peers (same class year, different sections) that have at least one characteristic in common with the student. The *commonalities ratio* represents the ratio of *section commonalities* to *class commonalities* -- ratios significantly less than one show that section peers are not more similar than class peers in terms of measurable ex-ante characteristics, and support the theoretical 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 as summarized in Table 2 and observations are at the executive level. Each executive is allowed to have multiple values for each characteristic (e.g. multiple firm affiliations) due to changes in employment over time. Individuals without at least one section peer and one class peer are not used in the estimation. *Firm overlap* describes individuals who have ever worked in the same firm. *Director overlap* describes individuals who have ever worked in firms that are connected by overlapping directors and is inclusive of firm overlap. All measures are as defined in Panel (A). *Commonalities ratios* greater than one 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 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*.

Log Compensation Type		Direct Comp (Salary + Bonus)		Total Comp	
		(1)		(2)	
Levels	*(A.1) Pairs Distance Metric*				
	Distance ratio	0.087 **	(0.042)	0.044	(0.038)
	γ	0.182 *	(0.094)	0.090	(0.080)
	Obs (pair x year)	8766		8766	
	(A.2) Excess Variance Metric				
	Excess variance ratio	0.499 **	(0.210)	0.220	(0.152)
	γ	0.224 **	(0.105)	0.105	(0.076)
	Obs (executive x year)	2866		2866	
Changes (1st Differences)	*(B.1) Pairs Distance Metric*				
	Distance ratio	0.115 ***	(0.043)	0.027	(0.042)
	γ	0.246 **	(0.100)	0.056	(0.086)
	Obs (pair x year)	5688		5688	
	(B.2) Excess Variance Metric				
	Excess variance ratio	0.485 **	(0.192)	0.024	(0.145)
	γ	0.219 **	(0.096)	0.012	(0.073)
	Obs (executive x year)	2168		2168	
Demographic controls		Y		Y	
Year fixed effects		Y		Y	

This table tests for peer influence in compensation using the Pairs Distance and Excess Variance Metrics, described in Section 4. Compensation type is indicated in column headings and all compensation measures in this and future tables are in log form. Panels (A) and (B) use compensation levels and changes (annual first differences), respectively. All specifications use compensation residuals which are estimated from a first stage regression of compensation levels on the controls listed in the bottom panel (demographic controls include age and gender). First stage results are not reported in this and future tables and are available upon request. *Pairs Distance Metric*: Observations are at the executive pair x year level and consist of all pairs of top executives (CEOs or CFOs) who graduated in the same class year. Second stage estimation uses a regression of the absolute difference in pair compensation residuals on a constant and the *same section* dummy (indicating whether a pair of executives are section peers). *Distance ratio* represents the fractional difference in the average distance between a pair of section peers relative to the average distance between a pair of class peers (equal to the negative ratio of the coefficient on *same section* to the constant term). Under the additional assumptions of the Linear-in-Means Model, the peer elasticity γ is calculated from the *distance ratio* and measures the elasticity of individual outcomes to mean section fundamentals (scaled by the elasticity of individual outcomes to own fundamentals). Standard errors are allowed to be double clustered by each member of an executive pair. *Excess Variance Metric*: Observations are at the executive x year level. The *excess variance ratio* measures the extent to which the ratio of the between to within section variance is large than unity (as expected under the null hypothesis of no peer effects). γ is as defined previously and is calculated from the *excess variance ratio*. Standard errors and significance levels are calculated using non-parametric permutation tests described in Section 4.4 (estimation via Monte Carlo simulations of 10,000 placebo draws). Standard errors for both models are in parentheses with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Log Direct Compensation	(1)	(2)	(3)	(4)
(A) Levels				
Distance ratio	0.087 **	0.080 *	0.084 **	0.084 **
	(0.042)	(0.041)	(0.040)	(0.039)
γ	0.182 *	0.168 *	0.177 **	0.176 **
	(0.094)	(0.091)	(0.090)	(0.087)
Obs (pair x year)	8766	8766	8766	8471
(B) Changes (First Differences)				
Distance ratio	0.115 ***	0.094 **	0.090 **	0.090 **
	(0.043)	(0.040)	(0.039)	(0.039)
γ	0.246 **	0.199 **	0.189 **	0.191 **
	(0.100)	(0.090)	(0.087)	(0.087)
Obs (pair x year)	5688	5547	5547	5372
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, exclude firm transitions	N	Y	Y	Y
First stage uses full ExecuComp sample	N	Y	Y	Y
Firm and SIC3 industry returns, size	N	Y	Y	Y
Industry FF49 fixed effects	N	Y	Y	Y
Industry FF49 x year fixed effects	N	N	Y	Y
Exclude pairs in same FF49 industry	N	N	N	Y

This table extends the results for *log direct compensation* (salary + bonus) from Column (1) Panels (A.1) and (B.1) in Table 5 to test for "excess" section peer similarities in compensation, i.e., similarities beyond what can be explained by selection into observably similar firms and industries. Specifications follow the Pairs Distance Metric described in Table 5. Additional controls in the first stage regression and additional sample restrictions in the second stage estimation are listed in the bottom panel. *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) - (4), the first stage estimation uses all observations for CEOs and CFOs in the ExecuComp sample (including non-HBS alumni) to improve the fit of controls for industry trends over time. Firm and industry SIC3 returns are matched to the fiscal year end month of each firm and winsorized at the 1 percent level. *FF49* refers to the Fama French 49 industry classification. All other controls and variables are as described in Table 5. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 7: Peer Influence in Acquisitions

Acquisition Type		Acquisition Attempt Dummy		Completed Acquisition Dummy	
		(1)	(2)	(3)	(4)
	(A) Pairs Distance Metric				
	Distance ratio	0.106 ***	0.049 *	0.116 ***	0.054 **
		(0.033)	(0.027)	(0.032)	(0.025)
	γ	0.225 ***	0.100 *	0.249 ***	0.111 **
		(0.076)	(0.056)	(0.074)	(0.053)
Levels	Obs (pair x year)	8904	8603	8904	8603
	(B) Excess Variance Metric				
	Excess variance ratio	0.285 ***	0.259 ***	0.293 ***	0.276 ***
		(0.010)	(0.009)	(0.010)	(0.010)
	γ	0.133 ***	0.122 ***	0.137 ***	0.130 ***
		(0.036)	(0.037)	(0.035)	(0.035)
	Obs (executive x year)	2895	2895	2895	2895
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 peers in same FF49 industry [†]		N	Y	N	Y

This table tests for peer influence in acquisitions using the Pairs Distance and Excess Variance Metrics, described in Section 4. All specifications and variable definitions are identical to those described in Tables 5 and 6, 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 Columns (3) and (4), acquisition policy 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. [†]Exclusion of executive pairs in the same Fama French 49 industry only applies to the Pairs Distance Metric. Estimation of standard errors and significance levels are as described in Table 5, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

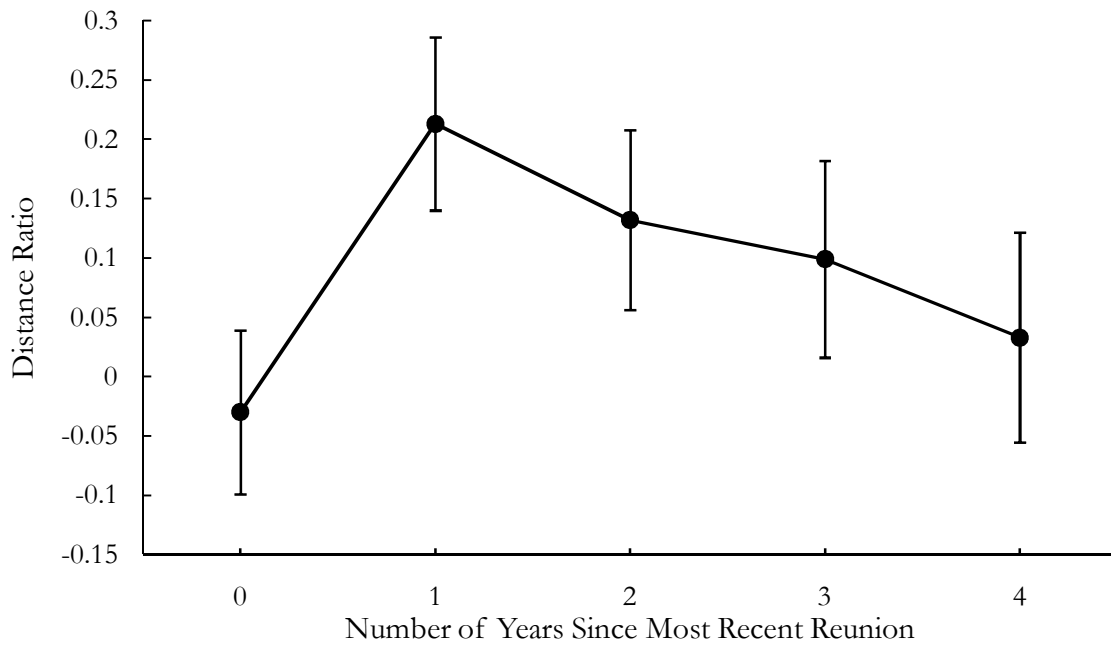## Figure 1: The Reunion Cycle and Peer Influence in Executive Compensation

Figure 1 shows how peer similarities (measured using the distance ratio described in Section 4) in annual changes in log direct compensation vary with the five year alumni reunion cycle. Vertical bars represent +/- one unit standard errors.

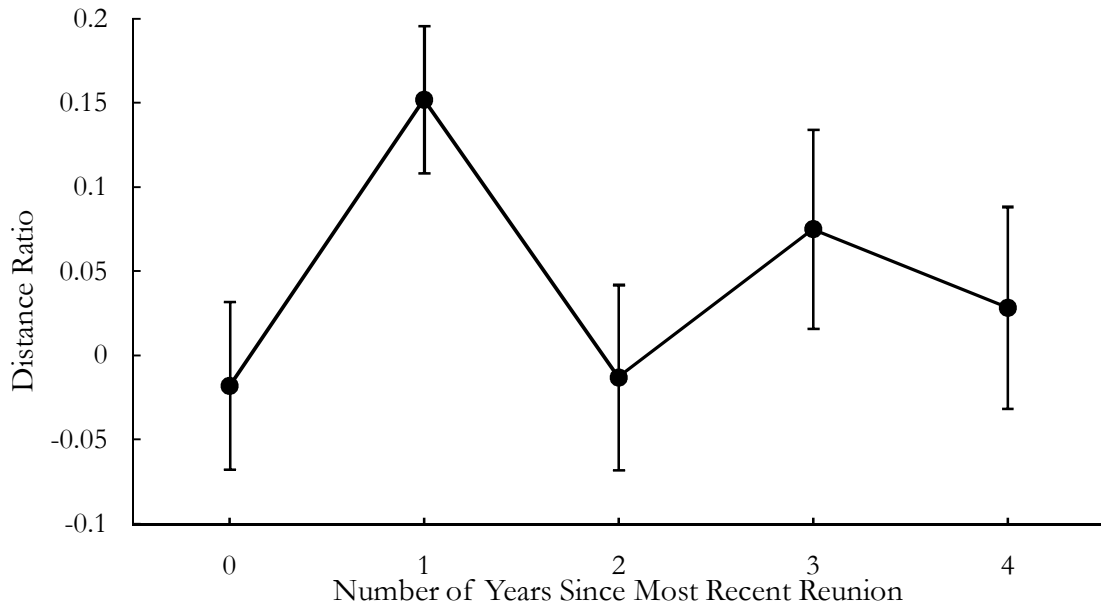## Figure 2: The Reunion Cycle and Peer Influence in Acquisitions

Figure 2 shows how peer similarities (measured using the distance ratio described in Section 4) in the completed acquisitions dummy vary with the five year alumni reunion cycle. Vertical bars represent +/- one unit standard errors.

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	0.207 ***	0.213 ***	0.076	0.127 *
	(0.071)	(0.073)	(0.084)	(0.078)
γ: reunion year + 1	0.459 **	0.493 **	0.158	0.275
	(0.188)	(0.201)	(0.185)	(0.184)
Distance ratio: all other years	0.090 *	0.062	0.015	0.008
	(0.047)	(0.042)	(0.044)	(0.044)
γ: all other years	0.191 *	0.127	0.031	0.016
	(0.105)	(0.090)	(0.089)	(0.088)
P-value: distance ratio equality	0.140	0.042	0.490	0.138
Obs (pair x year)	5688	5547	5688	5547
(B) Acquisition Levels	Acquisition Attempt Dummy		Completed Acquisition Dummy	
Distance ratio: reunion year + 1	0.202 ***	0.145 ***	0.232 ***	0.152 ***
	(0.062)	(0.047)	(0.058)	(0.044)
γ: reunion year + 1	0.463 ***	0.317 ***	0.548 ***	0.334 ***
	(0.166)	(0.114)	(0.165)	(0.107)
Distance ratio: all other years	0.080 **	0.013	0.085 **	0.018
	(0.037)	(0.030)	(0.035)	(0.028)
γ: all other years	0.167 **	0.027	0.178 **	0.037
	(0.082)	(0.061)	(0.079)	(0.057)
P-value: distance ratio equality	0.082	0.017	0.019	0.009
Obs (pair x year)	8904	8904	8904	8904
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

This table tests whether peer influence is stronger following alumni reunions, which occur every five years after each executive's graduation year. All variables are as described in Tables 5 through 7, and specifications are based upon the Pairs Distance Metric as described in Section 4. In the second stage estimation, the absolute difference of pair residuals (compensation changes in Panel A and acquisition levels in Panel B) 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 allowed to be double clustered by each member of an executive pair, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

Table 9: Pay For Friend's Luck

	Direct Compensation		Total Compensation	
	(1)	(2)	(3)	(4)
(A) Changes (First Differences)				
Distance ratio	0.062 *	0.099 **	0.040	0.068
	(0.035)	(0.041)	(0.037)	(0.051)
Obs (pair x year)	10738	5536	10738	5536
(B) Lagged Changes (First Differences)				
Distance ratio	0.091 **	0.119 ***	0.044	0.051
	(0.036)	(0.045)	(0.041)	(0.058)
Obs (pair x year)	8404	4290	8404	4290
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, where peers' lucky changes in compensation are predicted using the peers' industry returns. To address the concern that lucky shocks may have a greater direct impact a section peer's compensation than a class peer's compenation, all tests exclude pairs of executives belonging to the same broad 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 (using BEA input-output tables and following Ahern and Harford (2010), industries are considered linked if a customer industry buys at least 1% of a supplier industry's total output or if a supplying industry supplies at least 1% of the total inputs of a customer industry). Specification 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 (log direct or total compensation) 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 6. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair, 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	0.172 *	(0.090)	925
2. Exclude obs. if direct comp ≤ 50% of total comp	0.126 **	(0.065)	1328
3. Exclude obs. if direct comp ≤ 67% of total comp	0.290 ***	(0.085)	587
4. Permutation test confidence levels and std. errors	0.115 **	(0.051)	5688
5. Winsorized compensation top and bottom 1%	0.094 *	(0.043)	5688
6. Exclude observations after 2006	0.102 **	(0.046)	4687
(B) Annual Levels in Acquisitions			
7. Permutation test of significance: attempted acquisitions	0.106 ***	(0.035)	8904
8. Permutation test of significance: completed acquisitions	0.116 ***	(0.033)	8904
9. Acquisition value to assets ratio	0.145 ***	(0.038)	7112
10. Number of mergers completed	0.157 ***	(0.054)	8904
11. Completed acquisitions, known value > $1M	0.090 ***	(0.034)	8904

This table supports the robustness of the baseline results for peer influence in log compensation and acquisitions as presented in Tables 5 and 7. 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. Row (1) uses the measure of total compensation from the Forbes sample of HBS executives (920 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 in the sample. All other rows in Panel (A) use the direct compensation measure in ExecuComp. Row (2) restricts the sample to pairs of observations in which 50 percent or more of total compensation consists of direct compensation. Row (3) restricts the sample further to observations in which 66% or more of total compensation consists of direct compensation. Row (4) estimates standard errors and significance levels using the non-parametric simulated permutation test described in Section 4.4. Row (5) uses direct compensation winsorized at the top and bottom 1% levels as the dependent variable in the first stage estimation. Row (6) excludes observations with fiscal years ending after December 2006 to ensure that results are robust to a change in SEC compensation disclosure rules. Rows (7) and (8) estimate standard errors and significance levels using the non-parametric simulated permutation test described in Section 4.4 for attempted and completed acquisitions dummies, respectively. Row (9) 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 (10) uses the number of known completed mergers in which the firm acquired a 50 percent or greater stake in the acquired entity, as recorded in the SDC database. Row (11) 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 as recorded in the SDC database. All standard errors in parentheses are allowed to be double clustered by each member of an executive pair 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	0.104 **	0.055	0.105 ***	0.110 ***
	(0.042)	(0.039)	(0.033)	(0.032)
γ: CEO/CFO pair	0.221 **	0.113	0.223 ***	0.235 ***
	(0.096)	(0.083)	(0.076)	(0.074)
Distance ratio: other exec pair	0.044	0.050	-0.004	0.041
	(0.034)	(0.031)	(0.028)	(0.029)
γ: other exec pair	0.090	0.103	-0.008	0.083
	(0.071)	(0.065)	(0.056)	(0.060)
Distance ratio: mixed pair	0.042	0.025	0.040	0.047
	(0.034)	(0.032)	(0.028)	(0.026)
γ: mixed pair	0.087	0.051	0.082	0.095 *
	(0.070)	(0.066)	(0.057)	(0.054)
P-value: distance ratios equal	0.369	0.731	0.050	0.220
P-value: distance ratios = 0	0.034	0.234	0.008	0.001
Obs (pair x year)	36913	36913	37550	37550
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 all other top 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 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 allowed to be double clustered by each member of an executive pair, unless otherwise noted, with * significant at 10%; ** significant at 5%; and *** significant at 1%.

	(1)	(2)	(3)	(4)
	Recent Donor	High Donor	Recent Donor	High Donor
(A) Compensation Changes (First Dif)	Direct Compensation		Total Compensation	
Distance ratio 1: reunion yr+1, nondonor	0.152 *	0.084	0.118	0.069
	(0.085)	(0.111)	(0.091)	(0.097)
Distance ratio 2: reunion yr+1, donor	0.291 **	0.311 ***	0.149	0.167
	(0.119)	(0.086)	(0.131)	(0.107)
Distance ratio 3: all other yrs, nondonor	0.062	-0.012	-0.036	-0.070
	(0.053)	(0.057)	(0.058)	(0.070)
Distance ratio 4: all other yrs, donor	0.050	0.116 **	0.092	0.065
	(0.075)	(0.057)	(0.068)	(0.058)
P-value: distance ratio 1 & 2 are equal	0.295	0.090	0.841	0.481
Obs (pair x year)	5520	5547	5547	5547
(B) Acquisition Levels	Acquisition Attempt		Completed Acquisition	
Distance ratio 1: reunion yr+1, nondonor	0.129 **	0.106	0.165 ***	0.138 **
	(0.056)	(0.072)	(0.052)	(0.066)
Distance ratio 2: reunion yr+1, donor	0.169 *	0.175 ***	0.131	0.162 **
	(0.091)	(0.067)	(0.092)	(0.066)
Distance ratio 3: all other yrs, nondonor	0.006	-0.006	0.004	0.000
	(0.036)	(0.039)	(0.032)	(0.037)
Distance ratio 4: all other yrs, donor	0.005	0.027	0.010	0.031
	(0.060)	(0.040)	(0.052)	(0.037)
P-value: distance ratio 1 & 2 are equal	0.714	0.496	0.761	0.816
Obs (pair x year)	8762	8904	8762	8904
Demographic controls	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Employment controls, excl. firm transitions	Y	Y	Y	Y
First stage uses full ExecuComp sample	Y	Y	Y	Y
Firm and SIC3 industry returns, size	Y	Y	Y	Y
Industry FF49 fixed effects	Y	Y	Y	Y

This table tests whether peer influence varies by the reunion cycle and participation in reunion contribution campaigns. In Columns (1) and (3), executives pairs are considered "recent donors" in each year if both executives contributed any amount in the most recent reunion contribution campaign (which occurs every five years after graduation and coincides with reunion gatherings). In Columns (2) and (4), pairs are considered "high donors" if both executives contributed at least $1000-$2500 US in at least one reunion contribution campaign between 1990 and 2008. 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. Note that the sample sizes in Columns (1) and (3) are smaller than those in other columns because recent donor status cannot be calculated for some observations because alumni contributions data is only available starting in 1990. Standard errors in parentheses are allowed to be double clustered by each member of an executive pair, with * significant at 10%; ** significant at 5%; and *** significant at 1%.