The Long-Lasting Effects of Investment Apprenticeships*

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Abstract

I study how differences in apprenticeship experiences shape how professional asset managers make investments. Using hand-collected data on the careers of U.S. buyout investors, I document that private equity (PE) associate programs affect the subsequent investment returns and investing styles of PE partners, accounting for 20 to 40% of explained variation in buyout returns, industry selection, and key portfolio company decisions, such as CEO changes, acquisitiveness, and recapitalizations. A novel instrumental variable strategy indicates that associate programs matter because they provide effective training experiences, not just because of selection effects, although additional tests indicate both are important. Exposure to better mentors and to partners with more illiquid partnership shares predicts more effective associate training.

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Finance is a business that is like an apprentice business. It's almost like going back to the Middle Ages where they had guilds and you'd learn your business by apprenticing with someone who's already done it. - Stephen Schwarzman (2009), Blackstone Co-Founder.

Why do some professional investment managers perform differently than others? A natural explanation is that some managers are more skilled at investing than others. While much work has focused on identifying which investment managers have more investing skills than others, much less work has focused on where these investing skills come from.

In this paper, I use new data to provide evidence that an important source of investment skills is apprenticeship training. Practically, many investment firms have structures that can encourage the sharing of investing skills through "apprenticeship" training, where an expert works with an apprentice and in doing so passes down their knowledge. Buyout firms, hedge fund firms, and mutual fund firms, for example, all exhibit hierarchies where senior professionals (principals, partners, portfolio managers, or managing directors) make investments and are supported by junior professionals (associates or analysts). Human capital accumulation through training has been studied previously (e.g., Becker (1964) and Acemoglu and Pischke (1998)), but how such formative experience matters for investment professionals has not been despite its ubiquity. Importantly, examining how such apprenticeship experiences shape investment decisions delivers new insights for delegated asset management; namely, why some managers perform better than others even early in their careers, and why some investment managers stick to particular investing strategies but not every manager is able to adopt the same investing strategy successfully.

But studying the effects of apprenticeship experience faces two main challenges. The first is that it is not the only way to acquire investing skills.[1] The second is that the precise investments made by an individual, along with their early career experiences, are not observable through pre-existing datasets alone.

I address both challenges by focusing on the role of early-career experiences among professionals from an economically significant type of investment firm: buyout firms, which have grown to at least $4 trillion in Assets Under Management (AUM) (Bain 2025). This setting is important

[1] Although generally focused on different economic agents, other work suggests that professional investors could acquire skills or styles through their formal education (e.g., Chevalier and Ellison (1999)), formative early life experiences (e.g., Malmendier and Nagel (2011) and Schoar and Zuo (2017)), peers (e.g., Lerner and Malmendier (2013)), or through the process of making investments (e.g., Kempf, Manconi, and Spaly (2017)).

not only because of its increasing size, but also because partners of buyout firms have an outsized impact on real and financial outcomes (e.g., Kaplan and Strömberg (2009) and Davis, Haltiwanger, Handley, Jarmin, Lerner, and Miranda (2014)) and increasingly have Private Equity (PE) associate experience, reaching about 80% among buyout partners in 2024.

Focusing on buyout firms helps address the first challenge of "omitted sources of human capital accumulation" because the standard hierarchy of these firms separates apprenticeship experience from other ways of acquiring investing skills. Newcomers typically enter the industry through semi-structured associate programs, lasting around two years before promotion or separation. Associates do not lead deals but instead conduct due diligence or monitor investments under the partners and principals of their associate firm. Empirically, this creates variation in associates' formative early experiences *before* making buyout investments, exposing them to different types of investment committees, companies, deals, partners, and returns based on which firm they first worked at. Since roles shift from "apprentice" to investor as associates become vice presidents, principals, and partners, their associate firm serves as a proxy for their training. By matching associates to the deals they *subsequently* lead, I can then study how much these apprenticeship programs shape investment returns and styles – separate from later experiences – by estimating associate program fixed effects.

To address otherwise inhibitive data challenges, I assemble a comprehensive dataset of about 10,500 investment professionals who worked at 207 U.S. buyout firms from 2000 to 2022 based on archived webpages for each firm. I supplement this with hand-collected data on associate firm experience (for all buyout partners), commercial datasets (PitchBook, Preqin, and Revelio Labs), and deal-level returns (StepStone) to cross-validate characteristics and track outcomes.

These data indicate several new stylized facts about the career paths and backgrounds of buyout professionals. Almost half of the most senior professionals matriculated from Harvard, Stanford, or Wharton at the undergraduate or MBA level; most entered the industry after working in investment banking or management consulting; professionals usually work in the industry for five or more years before leading a deal or sitting on a portfolio company board; and career paths are fairly homogeneous, with separations typically occurring after a two-year associate program that precedes promotions to vice-president, principal, and finally partner over 15 years for about one-third of associates.

However, these stylized facts also highlight that junior PE professionals may accumulate investment skills through schooling, banking, or consulting aside from where they worked as a PE associate. To study the role of each, I estimate fixed effects for each experience and use Shapley value decompositions to attribute cross-sectional variation in deal characteristics and outcomes to schooling, banking/consulting, and PE associate programs.

The main finding is that where the professional worked as a PE associate is the most important of these three experiences, accounting for about 33% of explained variation in both gross deal returns and value-add, nearly double the individual contributions of schooling and triple that of where the professional worked as an investment banking analyst or junior consultant. Associate firm experiences also imply large differences in economic outcomes for investments: 75th percentile associate programs produce partners leading deals predicted to generate $1.2 billion more in value-add compared to associate programs at the 25th percentile, equivalent to 3.5-times the average value-add of deals in my sample. Two former associates of KKR, for example, working at different firms subsequently lead deals that perform similarly. And whether the partner worked as an associate at Blackstone versus KKR appears more important for deal returns than where they went to school and whether they were a banker at Goldman Sachs versus a consultant at McKinsey.

PE associate experience is important for not just returns, but also for investing "styles," in the spirit of Bertrand and Schoar (2003). Where the professional worked as an associate accounts for 20 to 40% of the explained variation in which industries and geographies they invest in, and whether their portfolio companies acquire other companies, change their CEOs, issue new debt, or are recapitalized. These styles persist, with associate programs being just as important in explaining variation for a professional's first few deals as for their later deals.

Interpreting this variance decomposition, however, is not so simple. One view suggests the results are driven by variation in selection: PE firms may differentially hire new associates that ultimately tend to a particular style once they rise in seniority, independent of the associate experience itself. Another view suggests the results are attributable to skills learned through the PE associate program: the associate experience at different PE firms varies in how it instills different practices in doing deals – ranging from how to bid for target companies to whether and when to recapitalize a portfolio company. While both views indicate that associate programs affect buyout investing, only the latter suggests that they do so directly.

To study the role of a hiring explanation, I start by examining how associate firms differ in their hiring practices and the subsequent career progressions of their associates. Supporting this view, I find that associate firms also play a large role in the characteristics of associates they hire and the subsequent career progressions of their associates. Some firms systematically see higher promotion rates and rely more heavily on lateral hires, "feeder firms," or post-MBA associates. However, a central takeaway – albeit somewhat limited by sample size – is that hiring practice fixed effects are seemingly unrelated to deal outcome fixed effects. The same associate firms that produce partners who tend towards particular investment performance or styles are not the same firms that load most heavily on particular hiring or career advancement features.

The next set of tests provides evidence supporting the latter interpretation of the variance decomposition: that one reason why associate firms systematically produce principals and partners that tend towards particular investment styles and outcomes is because they provide effective "buyout training." I think of such training as experience gained from working on buyout investments allowing a professional to do more or better deals. I study this by proposing a novel Instrumental Variables (IV) strategy to estimate the relationship between associate turnover and the deal-making of their firm. The key assumption mapping turnover to training is that turnover replaces more-experienced associates with less-experienced associates, which is consistent with the data: associates work at PE firms in programs lasting two or three years and typically leave their associate firm once their program finishes. If training accumulates over the program, a third-year associate with more experience presumably is better equipped to contribute to the deal-making process of their firm than a first-year associate with less experience. And if training matters for associates, replacing a third-year associate with a less-experienced associate should negatively affect how many buyout deals the firm completes.

Recognizing that associates may not randomly leave their firm, the econometric novelty of this test is to make use of the semi-structured length of associate programs. I instrument for the fraction of a firm's associates that turnover with the fraction of the firm's associates that are at the end of the predicted length of their associate program. The potential concerns with the exclusion restriction are reverse causality (that deal-making affects instrumented associate turnover rather than vice versa) and omitted variation (that an omitted factor simultaneously affects deal-making and associate turnover). Several additional tests provide confidence in the exclusion restriction;

namely, that the results hold across various subsets of firms and years, in particular both those that would be the most or least affected by these potential endogeneity concerns. For example, firms where deal-making either subsequently dries up or takes off both exhibit comparable results, which is inconsistent with reverse causality underpinning the estimation.

Estimating this IV regression provides evidence consistent with what training would predict. Instrumented associate turnover, which replaces more-seasoned associates with less-seasoned associates, is negatively correlated with the number of deals a PE firm does. For every three associates predicted to depart the firm, a firm makes one fewer platform buyout investment, a decline of about 33% relative to the mean.

Taken together, these results provide evidence of a rather novel explanation for sources of PE investment skills: the training and other formative experiences provided through associate firms. I argue that two main economic forces are important for determining how such training occurs: the abilities of partners to mentor their associates and the incentives for them to do so. Do better mentors mean that some professionals receive better training than others? And what economic frictions incentivize partners to invest in training their associates?

Subsequent cross-sectional tests indicate that both higher quality mentors and stronger mentoring incentives are important for more effective associate training. To study this, I examine the characteristics of associate firms that predict how similar the deals a professional leads are to the deals they observed (and were potentially staffed on) during their time as an associate. I measure degrees of similarity using the cosine similarity between the characteristics of each deal led by each professional and the average characteristics of all deals done by their associate firm in the five years before the professional first led a deal.

To help limit the role of potential selection, regressions use *within-associate firm* variation; including associate program fixed effects, which compare cosine similarities among professionals that were former associates of the same firm but at different times. The main evidence suggesting that mentoring abilities are important is that partners lead deals that are more similar to those of their associate firm if they had early exposure to successful buyout investors. Importantly, because initial partner composition may also influence sorting of associates to firms, these tests focus on senior professional departures after the associate had already joined the firm. Former associates of a firm where active high-performing partners separated during their associate tenure subsequently

lead deals that are relatively less similar to those observed over their associate programs. The relationship is concentrated in active partner departures. Inactive low-performing partners separations, in contrast, correlate with higher similarity scores. These results are also robust to controlling for professionals' tenure, which has a negative relationship with deal similarities that is statistically significant, but not economically significant.

The tests highlighting the role of mentoring incentives focus on the relationship between the illiquidity of associate firms' "partnership shares" and subsequent deal similarities. Morrison and Wilhelm (2004) present a model where partnerships facilitate intergenerational transfers of tacit human capital due to the extreme illiquidity of partnership shares. Since there is no external market for these shares, retiring partners must sell them to sufficiently skilled junior partners, creating an incentive to mentor associates to ensure a return on their claims.

A key prediction from this model is that increasing the liquidity of partnership shares reduces partners' incentives to mentor. I test this by comparing deal similarities among partners trained around three events that increased the liquidity of PE partnership shares: the first U.S. PE firm IPO, the rise of institutional General Partner (GP) stake sales, and when a partner's associate firm first sold a GP stake or went public. The first two events represent broad increases in PE firms' ability to liquidate shares, allowing partners to sell to outside investors rather than junior colleagues. The third event is firm-specific and helps control for broader market changes.

Importantly, the findings across all three events are similar: partners who worked as associates when their associate firm's partnership shares were less liquid tend to do deals with higher cosine similarities compared to partners that worked as associates while their associate firm's partnership shares were relatively more liquid. This evidence points to the illiquidity of partnership shares as one economically important force that incentivizes mentoring in PE partnerships.

1 Related Literature

Most broadly, this paper is not the first to document the effects of apprenticeship training, which has been covered and discussed extensively across many theoretical and empirical settings in the labor literature (e.g., Becker (1964), Acemoglu (1997), Acemoglu and Pischke (1998), Acemoglu and Pischke (1999), and Garicano and Rayo (2017)). This paper, however, is the first, to the best of

my knowledge, to document the effects of training on returns and styles for professional investors.

In doing so, my findings contribute to the literature studying the determinants of PE returns, which is often focused on measuring whether the returns to PE investments exceed public markets and whether performance persists at the fund-level (e.g., Kaplan and Schoar (2005), Phalippou and Gottschalg (2009), Harris, Jenkinson, and Kaplan (2014), Korteweg and Sorensen (2017), and Harris, Jenkinson, Kaplan, and Stucke (2022)), deal-level (e.g., Braun, Jenkinson, and Stoff (2017)), and senior professional-level (e.g., Ewens and Rhodes-Kropf (2015) and Braun, Dorau, Jenkinson, and Urban (2023)). Most closely related to my paper, Ewens and Rhodes-Kropf (2015) estimate venture capital (VC) partner fixed effects to show that variation in venture investment exits is attributable more so to the individual partner that served on the board of the portfolio company than to VC firm at which they are a partner. My contribution is to provide evidence identifying the source of PE partner fixed effects based on where they first worked as a PE associate. These fixed effects almost perfectly decompose the partner fixed effects when combined with fixed effects for where professionals worked and went to school before entering the buyout industry.

The findings in this paper also provide evidence on the determinants of the sources of investment professionals' styles. Survey results from Gompers, Kaplan, and Mukharlyamov (2016) document several ways buyout investors expect to select and add value to their portfolio companies. Further empirical results show that buyout managers exhibit substantial variation in how they do so, which industries they invest in, and how it matters for their portfolio companies (e.g., Acharya, Gottschalg, Hahn, and Kehoe (2013), Davis et al. (2014), Bernstein and Sheen (2016), Eaton, Howell, and Yannelis (2020), Davis, Haltiwanger, Handley, Lipsius, Lerner, and Miranda (2021), Ewens, Gupta, and Howell (2022), and Bhardwaj, Gupta, and Howell (2025)). My contribution is to show that PE investment strategies, ranging from changing the CEO to pursuing add-on acquisitions or engaging in a dividend recapitalization, have partner-specific specific components that are attributable to the training the partner received. More generally, my findings add to the literature documenting "styles" among other types of economically important professionals such as CEOs and CFOs (e.g., Bertrand and Schoar (2003)).

The results in this paper also contribute to the literature studying how cohort effects shape long-term career outcomes; finding, for example; the timing of when MBAs enter the labor market affects long-term career and earnings outcomes (e.g., Oyer (2008)); whether CEOs began their

careers during a recession affects their career paths and management styles (e.g., Schoar and Zuo (2017)); and, generally, personal experiences of economic fluctuations shape individuals' willingness to take risk (e.g., Malmendier and Nagel (2011)). The novel aspect of this paper is its focus on cross-sectional, rather than time-series, variation in cohort effects. In the context of buyout training, my findings show that, conditional on entering the buyout industry, *where* a partner first worked as an associate appears to be more important than *when* they first worked as an associate.

Lastly, my findings contribute to the literature examining the role of the PE firm. Ewens and Rhodes-Kropf (2015) conclude that variation in VC firm-level organizational capital is dwarfed by the role of partner-specific human capital in explaining outcomes of VC investments, suggesting the boundaries of a VC firm are likely driven by fundraising purposes. Recent work shows that an important role for investment firms is to use their internal capital markets to efficiently match the capital it raises from outside investors with the skill of their investment managers (e.g., Berk, Van Binsbergen, and Liu (2017) and Braun et al. (2023)). Theoretical work emphasizes that partnerships (the predominant organizing form among buyout firms) have emerged due to the presence of knowledge hierarchies (e.g., Garicano (2000)), intergenerational knowledge transfers (e.g., Morrison and Wilhelm (2004) and Morrison and Wilhelm (2008)), difficulties in assessing partners' skills (e.g., Levin and Tadelis (2005)), moral hazard and dynamic segregation in joining the industry (e.g., Axelson and Bond (2015)), and partners' incentives to extract rents from junior professionals (e.g., Ferreira and Nikolowa (2024)). My results highlight, empirically, that transferring knowledge intergenerationally has historically been an important role for buyout firms.

2 Data Sources and Uses

As background, institutional PE investing involves three main economic layers: PE firms, the funds sponsored by these firms, and the investments held in funds sponsored by the firms. The firms are infinitely lived and typically organized as limited partnerships or limited liability corporations. The funds are typically closed-end, limited life, limited partnerships, lasting eight or more years. Funds raise and manage capital on behalf of large institutional investors (e.g., pension funds, endowments, and sovereign wealth funds) and high net worth individuals who compensate the sponsoring PE firm with fees (predominantly management fees and carried interest profit sharing, e.g., Metrick

and Yasuda (2010), Robinson and Sensoy (2013), and Gahng and Jackson (2023)). This paper focuses on why professionals that work at these firms – the partners, principals, associates, and other investment professionals working on PE deal teams – differ in the investment styles they adopt and returns they achieve.

A central challenge in doing so is data availability. The track records of PE investment professionals, their career structures and histories (if any), and the full teams of PE firms are not readily available. One contribution of this paper, therefore, is to construct the first comprehensive dataset of U.S. buyout professionals, their careers, and their investments. I combine five main data sources to study U.S. buyout professionals and their investments: a novel matched employer-employee dataset assembled via archived websites, PitchBook, Preqin, StepStone, and Revelio Labs. This section discusses each and its usage. Where possible, I validate data across multiple sources and hand-collect missing data points.

2.1 Buyout Investment Professionals

The main sample includes matched employer-employee panel data on 10,546 investment professionals employed at 207 U.S. buyout firms from 2000 to 2022. These data, which are novel to this study, come from archived webpages for each firm hosted on the internet archive and accessed via the Wayback machine. The archived website data provide historical snapshots of a firm's investment professionals over time, displaying the team of professionals who work for a given PE firm each year as of that year. Figure IA1 displays an example of archived webpages for Hellman & Friedman (a large PE firm) in 2003. I retrieve these data by collecting and parsing the archived team and personal HTML pages that list the professionals working at each firm (alongside titles and biographical excerpts) every year the firms' websites were archived. I discuss the sample selection, website collection, parsing, and subsequent validation processes in detail throughout Section IA.1. Alongside other statistics about the sample of firms, such as the number of active U.S. buyout funds it manages each year (from Preqin) and the number of platform or stand-alone buyouts it completes (from PitchBook), Panel A of Table 1 indicates that the average firm has about 13.5 years of data with website collections.[2]

[2]The AUM (Assets Under Management) and PME (Public Market Equivalent) measures I report follow from Gahng and Jackson (2023), both of which are estimated using all U.S. buyout funds managed by firm j in year t.

Using the biographical data from these archived webpages, I also collect several characteristics for each investment professional and how they change over time; namely, their title, education, prior work experience, and tenure with their firm. Employment spells and seniority are not reliably available in other datasets, especially for earlier years in the sample period. Nonetheless, to limit the role of plausible deficiencies in parsing and collection of website data, I also attempt to retrieve the same characteristics from professionals' "resume" data via Revelio and reconcile differences where they appear. To do so, I merge each buyout professional collected from websites to a LinkedIn profile in Revelio – attempting a sequence of matches based on names and affiliations and then manually reviewing unmatched professionals. In total, 8,604 investment professionals employed by 193 of the sample firms are mapped across websites and the Revelio dataset. In the final dataset, about 20% of overlapping observations on the timing of employment spells, education, and previous work experience differed and were reviewed and reconciled.

The main sample of 10,546 buyout professionals results from attempts to include only investment professionals based on their reported titles – individuals working on PE deal teams and responsible for investing the capital raised by the funds sponsored by their firm. This excludes firm operations professionals (involved in internal functions such as legal, finance, and human resources), portfolio operations professionals (often operating partners), and retired investment professionals (such as emeritus partners). I also require that the equivalent person-year observation for the professional in Revelio agree on the investment team classification, limiting false positives at the expense of potentially more false negatives.

Also using the title stems, I assign investment professional to one of three layers in each year they are observed reflecting the hierarchy observed in practice: a bottom-level (associates), a middle-level (vice presidents and principals), and a top-level (partners). Doing so allows investment professionals' career progressions to be reasonably comparable across firms and over time. Constructing this full hierarchy is an advantage of the website data over others; standard commercial datasets often list only the most senior position obtained with a company, thereby omitting experience at less senior positions. Section IA.1.4.2 discusses the full sorting algorithm that accounts for differences in title meaning across firms and changes within firms over time.

2.1.1 Buyout Team Composition

Panel B of Table 1 displays the average composition of PE firm teams across firm-year cells over the sample period. Firms employ 18 total investment professionals on average, split evenly across the three seniority groups. Few professionals are female, consistent with Ewens (2024). The observations apparent from the remaining rows in Panel B of Table 1 fall into three main groups: education, prior work experience, and turnover.

Education: On average, 20%, 40%, and 46.5% of bottom-level, middle-level, and top-level investment professionals respectively employed by a given firm have received a degree from Harvard, Stanford, or the University of Pennsylvania at either the undergraduate or MBA level. The increase in these degrees comes partially from high rates of MBA program attendance, with about 2/3 of partners of a given U.S. buyout firm having obtained an MBA degree.

Work Experience: 80% of bottom-level investment professionals at a given firm have prior investment banking experience, and 15% have prior consulting experience. These patterns persist but tilt more towards consulting as seniority increases, with 60% of partners having investment banking experience and 30% having consulting experience.

Turnover: I consider professional i to be promoted by firm j in year t if their position in year $t+1$ places them in a higher seniority with firm j than they are in during year t. Analogously, professional i is said to separate from firm j in year t if they are no longer listed in an investing role on the firm's website in year $t+1$. At the bottom-level, about 50% of the team has an adjustment to their position within the firm in a given year (26% join, 9% are promoted to the middle-level, and 15% separate). By contrast, this number reaches only 22% among mid-level professionals and 5.6% among top-level professionals. About 45% of middle-level teams include former associates of the firm. The remaining 55% are external hires. Turning to partners, on average, 30% are founders of the firm, 15% were hired at the middle-level and promoted up and 11% were hired at the associate level and promoted up, meaning about 44% joined the firm as a partner.

2.2 Buyouts Deals and Outcomes

For each of the professionals in the sample, I also track which, if any, stand-alone or platform buyouts of U.S. companies they led according to PitchBook data. I improve upon these data in

four main ways.

1. Sample Extension: The first is to include not just lead partners who have been employed by one of the 207 sample firms, but also any professional who represented a U.S. buyout firm that has raised at least $50 million for a U.S. focused buyout fund (according to both PitchBook and Preqin) in leading a platform or stand-alone buyout of a U.S. company from 1990 to 2024 according to PitchBook. Doing so is not necessary to produce the main results but increases the statistical power of subsequent tests estimating fixed effect coefficients. This dataset includes 4,066 lead professionals that represented 732 U.S. buyout firms in 11,209 buyout deals, which are further described in Table IA1.

2. PE Associate Experience: The second is to identify which, if any, PE firm these 4,066 lead professionals previously worked at as an associate. Where these data are not available in the main sample of employment at 207 buyout firms from 2000 to 2022, I hand-collect PE associate work experience primarily by reading titles and biographies from archived webpages stored on the Internet Archive. Education and prior investment banking or consulting experience come from the biographical and educational data listed on PitchBook for professionals outside the main sample of 10,546. I find that 1,948 of these professionals worked as an associate at one of 552 PE firms before representing their firm in a buyout investment.

Figure IA2 reports that the fraction of lead professionals with PE associate experience has increased over time, rising from about 20% in 2000 to 80% in 2024. Those without PE associate experience typically led deals after previously working as a managing director at an investment bank, a partner at a management consulting firm, or as a C-suite executive at an operating company. The five most common prior PE associate experiences among professionals are Bain Capital (59 professionals), GTCR (36), Summit Partners (34), Carlyle (33), and Blackstone (31).

3. Deal Returns: Third, for a subset of the buyout deals in the sample, I obtain the gross performance of the investment and the exact dollar amount a GP invested in a company via StepStone, a private markets investor, consultant, and sponsor. For U.S. buyout funds, StepStone data are similar to Burgiss data, one of the most reliable performance datasets used in PE research (Ercan, Kaplan, and Strebulaev 2024). These data are important for this study because they are de-anonymized, allowing for matches between partners, deals, and companies across datasets. Gross performance is measured as a Total Value Multiple ("TVM"), equaling the dollar amount of capital

returned to the GP from its investment divided by the amount of capital the GP invested. After standardizing StepStone deal returns and investment amounts to account for duplicated entries (arising primarily when the GP acquires a portfolio company using multiple funds or through multiple tranches within the same fund), I attempt to merge each PitchBook deal with a StepStone deal – attempting a sequence of matches based on portfolio company names, GPs, and a window of no more than three years around the entry years. I exclude a handful of deals where the GP invested less than $1 million (inflation adjusted to 2022) and censor deals where the TVM was negative (due to relative changes in currency valuations). TVM data are available for about 50% of deals with lead partners and the average deal has a gross TVM of 2.7x. The final dataset of deals uses entry and exit dates from StepStone rather than PitchBook where possible.

4. Portfolio Company Activity: The fourth way I improve upon the standard PitchBook deals data is by tracking changes to the acquired company around the deal. I track: (i) changes in CEOs of the portfolio company; (ii) any add-on acquisitions completed by the portfolio company; and (iii) any capital issuances completed by the portfolio company (e.g., debt issuances or recapitalizations). I obtain these data from PitchBook. These data are useful for measuring any investing "styles" to which partners tend towards.

2.3 Buyout Firm Associate Programs

The focus of this paper is on the initial apprenticeship that PE professionals who become partners or otherwise lead deals for their firms receive as associates at PE firms. Table 2 summarizes PE associate jobs. The PE associates described in this table include those that are observed as an investing associate on the archived websites of one of the 207 sample firms between 2000 and 2022. They also must be matched to Revelio to track career progressions outside of the sample firms, which reduces the sample.

The average cohort includes about 2.5 associates (=3,287 associates / 1,310 cohorts). Recent reporting highlights that PE associates are often interviewed and hired during the first few months of their banking experience, between 16 and 25 months before they start as a PE associate (e.g., Levine (2018) and Cumming (2023)). Panel A indicates that 56% of associates were hired from a "feeder" firm or school of the PE firm (= 34.9% hired from a feeder firm + 32.4% from a

feeder school – 11.1% from both a feeder firm and school). Feeder firms or schools are the three most popular schools or banking/consulting firms among other professionals at the firm the year the associate joined.

Panel A shows the average associate is about 25 years when they join the firm, estimated by assuming they were 22 years old when they graduated from college. Most associates work at their firm in an associate role for two years (which spans three calendar years) and about 50% have an associate tenure equal to the modal tenure of other associates that worked at their firm.

The left-most panel of Figure 1 shows that separations and promotions both cluster during the third calendar year after an associate joined the firm. This pattern reflects the "up-or-out" promotion structure of a PE associate job, with many firms having selected two years to be when the up-or-out decision is realized. For comparison, the middle panel shows promotions and separations for professionals hired at the middle-level cluster somewhat near the eighth calendar year, but this clustering is not as pronounced. Carried interest for middle-level professionals often fully vests around this time. The right-most panel of Figure 1 shows professionals who joined their firm at the highest seniority or founded their firm tend to remain at the firm.

Panel B of Table 2 shows 20% of associates join their firm after receiving an MBA. About 50 to 60% of associates eventually receive an MBA, implying 30 to 40% of PE associates attend business school after working as an associate. Of those that obtained an MBA, about half did so at one of Harvard Business School, Stanford Graduate School of Business, or Wharton.

Associate Promotions: Panel C of Table 2 summarizes the rates at which associates progress to subsequent seniority levels. Focusing on the associates who entered the industry in 2009 or earlier, and therefore had at least 15 years to progress to a partner position (the right-most column), about 40% progress to the next seniority at their initial firm. About 22% are observed as a partner at their initial firm and 15% were listed in PitchBook as having led a platform or stand-alone buyout of a U.S. company for their initial firm. Most promotions occur within the initial firm, but about 20% more of the observed associates (totaling 60%) progress to the middle-level at any U.S. buyout firm. 11% more reach the top-level (totaling 33%) and 6% more lead a deal (totaling 22%) at any U.S. buyout firm. Most of a firm's partners were not previously associates at the firm (Table 1) but most associates that became partners did so at their initial firm.

Associate Separations: The remaining 40% of associates that did not progress to the

middle-level left the PE industry after working as a PE associate. Panel D shows nearly three-quarters of leavers with at least one year of subsequent work experience at a non-U.S. PE firm obtain experience at an "other operating company" – a Revelio company that I fail to classify as a PE firm, investment bank, VC firm, hedge fund firm, law firm, other finance firm, or consulting firm (i.e., a company in an industry at which PE associates tend not to have worked at before entering the industry).

Role Progressions: Figure 2 shows the fraction of associates in Table 2 that have led at least one buyout deal or sat on the board of at least one company acquired in a buyout deal increases as the professional gains experience and seniority. Engaging in these investing activities constitutes a clear shift in roles, from associate (who is staffed on deals) to a principal investor. About 1/3 of associates that work as a principal buyout investor spend about five years in the industry (and/or obtaining an MBA) before leading a deal or sitting on the board of a portfolio company. Such role changes often coincide with seniority changes (Table IA2).

3 Buyout Deal Attribution Analysis

This section attributes variation in buyout deal selection and outcomes to the PE firm where the lead professional worked as an associate. Partner i, representing firm j, leads buyout investments in year t with characteristics and outcomes (y) according to the following equation:

$$y_{d,i(j,t)} = \lambda_j + \delta_i + \omega_t + \epsilon_{d,i(j,t)}. \tag{1}$$

This specification follows from Ewens and Rhodes-Kropf (2015), who show that variation in venture investment exits is attributable more so to the partner that served on the board of the portfolio company than to the VC firm that they represented in the investment after accounting for year fixed effects and other covariates.

Equation (1) assumes that deal selection and outcomes have person-specific (δ_i), firm-specific (λ_j), and time-specific (ω_t) features that are additively separable. These notions follow from the seminal study of Abowd, Kramarz, and Margolis (1999) ("AKM"), which focuses on estimating the importance of person-specific versus firm-specific pay premia as part of a large lit-

erature in labor economics. In my context, firm-specific features represent fixed investment criteria and return premia specific to PE firm j regardless of which partner leads the deal. These features can arise from investment processes, deal-flow, or other features of organizational capital that are less portable across firms. And person-specific features represent a combination of skills and other factors specific to partner i affecting deals that are equally present across PE firms.

One key innovation of my study is to document how important apprenticeship-specific work experience is for the deals and outcomes led by partner i. Doing so provides specificity about where person-specific skills and styles captured in the partner fixed effects come from or are selected upon. I augment Equation (1) by decomposing the partner fixed effects into partner-invariant experiences that occurred before they led any buyout investments. I substitute the partner fixed effect with separate fixed effects for the associate firm where partner i worked as a PE associate ($i(a)$), the investment bank or consulting firm where the partner worked before entering the PE industry ($i(f)$), and the highest-ranking U.S. school the partner attended ($i(s)$). Accordingly, the main estimating regression becomes:

$$y_{d,i(j,t)} = \lambda_j + \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \omega_t + \varepsilon_{d,i(j,t)}. \tag{2}$$

The associate firm fixed effects, $\delta_{i(a)}$, are the main coefficients of interest. To study their role in buyout investment outcomes and selection, I attribute R^2 obtained from estimating Equation (2) to each of the five estimated fixed effects $\chi_b \in \{\hat{\lambda}_j, \hat{\delta}_{i(a)}, \hat{\delta}_{i(f)}, \hat{\delta}_{i(s)}, \hat{\omega}_t\}$.

Equation (2) assumes that PE firm and PE professional match-components reside in the error term, similar to AKM and subsequent work (e.g., Ewens and Rhodes-Kropf (2015) in my setting and Card, Heining, and Kline (2013) more generally). This assumption matters for the interpretation of $\delta_{i(a)}$, discussed further in Section 3.2.

Connectedness Sample: There are two important challenges with the specified R^2 decompositions. The first is defining the set of observations on which the partner firm and associate firm fixed effect coefficients can be estimated, requiring that associates do not perfectly sort into partner firms, in the spirit of the classic AKM mover design. As shown by AKM, separating worker and firm fixed effects is possible for the largest "connected set" of workers across firms. Similarly, I restrict regression samples to deals completed by professionals representing a partner firm in the

largest connected set of partner firms based on associate experience. Two partner firms are connected if they hire from the same associate firm and any string of partner firms is connected if they are connected to a partner firm that is connected to another partner firm. Deals where the lead partner's banking/consulting or education data are missing are further excluded given these data are required to estimate Equation (2). Table IA1 reports that about 75% of deals led by professionals with PE associate experience are in the largest connected set, typically omitting smaller deals.

Decomposition Method: Another challenge is distributing R^2 among correlated variables. The fraction of R^2 attributed to fixed effect χ_b includes all terms reflecting its contribution to R^2, equal to not just its own variance, but also potentially its covariance with other included regressors. The approach typically followed in the literature is to report the variance and covariance contributions to R^2 separately. This is preferable if estimating regressions analogous to Equation (1) when there is only one covariance term, but it does not take a stand on how the explanatory power of covariance terms relate to R^2 shares for coefficients of interest. An approach often followed in the literature for attributing covariance terms estimates the contribution of χ_b to R^2 as $\frac{\text{Cov}(\chi_b, y)}{\text{Var}(\hat{y})}$, which implicitly assumes covariance terms are split evenly among each of the included regressors. This need not be the case, potentially misattributing explanatory power.

To address this potential attribution problem, I attribute R^2 to each fixed effect based on their Shapley values (Shapley 1953). In this setting, the Shapley value of fixed effect χ_b, denoted φ_b, equals its average marginal contribution to R^2 across all combinations of fixed effect groupings, equivalent to the average increase in R^2 when fixed effect χ_b is added to a model already containing a subset T of all possible sets of fixed effects, K.[3] The Shapley values sum to the R^2 of the regression ($\sum_b \varphi_b = R^2$), implying the fraction of R^2 attributed to a given fixed effect equals φ_b / R^2 and the overall variation in y explained by χ_b equals φ_b. The Shapley value decomposition is equivalent to the $\frac{\text{Cov}(\chi_b, y)}{\text{Var}(\hat{y})}$ decomposition if there are only two regressors or if any two regressors are perfectly correlated: including one but not the other increases R^2 by the same amount, so the Shapley values of the two variables are the same. But more generally, if the variable has

[3] Formally, the Shapley value attributing R^2 (total surplus) among fixed effects (coalitions) equals:

$$\varphi_b = \sum_{T \subseteq K \setminus \{\chi_b\}} \frac{|T|!(|K| - |T| - 1)!}{|K|!} \left(R^2(T \cup \{\chi_b\}) - R^2(T) \right),$$

which weights the marginal increase in R^2 from adding χ_b to each possible subset T by the proportion of possible orderings in which χ_b would be added at that stage.

incremental explanatory power relative to the others, even those that it is highly correlated with, it will have a higher Shapley value and therefore be allocated a larger share of the R^2. However, to ensure that this method of allocating covariance terms does not drive my results, in unreported tests, I confirm that the variation in y explained by associate program fixed effects is similar using ANOVA (Type-III) and the decomposition based on $\frac{\text{Cov}(\chi_b, y)}{\text{Var}(\hat{y})}$.

3.1 Associate Program Attribution

Table 3 presents the main results, focusing on three different groups of deal outcomes, and attributing R^2 to each set of fixed effects in Equation (2) based on their Shapley value. Regression samples are restricted to only the connectedness sample of deals.

Panel A of Table 3 displays the R^2 decomposition for deal performance using deal-level gross return data from StepStone. The key finding is that a sizeable fraction of explained variance in deal returns is attributable to the firm at which the lead professional worked as a PE associate. 40% of the R^2 in value-add regressions (estimated as \$ Invested inflation adjusted to 2022 $\times$ (Gross TVM - 1)) is attributable to the associate program. Meanwhile, 47% is attributable to the firm at which the professional works when they lead the deal, with the remaining 13% of R^2 evenly split among the year the deal took place, the banking or consulting firm at which they previously worked, and the school at which they matriculated.

Panels B and C of Table 3 focus on investing "styles." Panel B shows that 36% of the explained variation in a partner's positioning within a deal as a lead investor or propensity to complete deals in a consortium with other PE firms is attributable to where they worked as a PE associate. Panel B further documents that variation in the type of buyouts a partner leads is attributable to where they worked as a PE associate, measured using any of the indicators for deal type (e.g., secondary buyouts versus management buyouts), portfolio company industry (e.g., software versus healthcare), or portfolio company location (e.g., California versus Texas).

Panel C documents that variation in subsequent changes to the portfolio company is also attributable to where the lead partner worked as an associate. Survey results from Gompers et al. (2016) document several ways that PE investors expect to add value to their portfolio companies, ranging from changing the management to pursuing add-on acquisitions. And in the data, for

example, about one quarter of variation in whether the target company changes its CEO in the first year is attributable to associate firms. Meanwhile, about 20 to 40% of the explained variation in whether the portfolio company makes an add-on acquisition, issues new debt, or completes a dividend recapitalization is attributable to a partner's associate program. Additionally, the p-values obtained from the F-tests reported in Table 3 reject the null of equal coefficients across both performance and style outcomes, with every specification except for the Take-Private indicator in Table 3 achieving statistical significance at the 1% level.

Figure 3 indicates that the economic magnitude of differences in associate firm fixed effect coefficients is sizeable. For example, associate programs at the 75$^{\text{th}}$ percentile (of estimated associate firm fixed effect coefficients) produce partners leading deals predicted to generate $1.2 billion more in value-add compared to associate programs at the 25$^{\text{th}}$ percentile. This difference corresponds to about 1.5 standard deviations in value-add (or about 3.5-times the average value-add of a deal in the regression sample). Such increases in estimated associate firm coefficients imply 10 to 50 percentage point increases in style differences measured via geography, industry, entry methods, and financial or operational changes, each of which is economically meaningful.

3.1.1 Comparison with Lead Professional Fixed Effects

Figure 4 presents the R^2 decomposition visually for several outcomes across each Panel of Table 3. To further benchmark these results, Figure 4 additionally reports the Shapley value R^2 decomposition obtained from a regression of deal outcomes and styles on lead professional and partner firm fixed effects (Equation (1)). Regressions require that the professional led more than one deal, restricting the sample slightly compared to prior regression samples underlying Table 3.

Two further observations are apparent from Figure 4. The first is a result similar to that documented by Ewens and Rhodes-Kropf (2015) in VC. Variation in investment outcomes is more attributable to who is leading the deal than where the person leading the deal works. This finding is apparent despite different sample periods, industries (buyout versus venture capital), mappings of partners to deals (lead partners versus board members), outcomes (gross returns versus imputed multiples), and decomposition approaches (Shapley values versus $\frac{\text{Cov}(\chi_b, y)}{\text{Var}(\hat{y})}$). Focusing on TVM, about three-quarters of explained variation goes to the partner.

The second observation is that estimating Equation (2) obtains an R^2 close to that resulting

from estimating Equation (1). The R^2 of TVM regressions equals about 21% when estimated using Equation (1) or about 17% when estimated following Equation (2). These R^2 measures for return multiples are also similar to those reported in Ewens and Rhodes-Kropf (2015) who instead construct several proxies for the return multiple. As a back of the envelope calculation, dividing the R^2 attributed to the associate program fixed effects by the R^2 attributed to the partner fixed effects implies, roughly, that 37% of the partner fixed effects that explain cross-sectional variation in gross deal returns comes from where they worked as an associate ($\approx 17\% \times 33\%$ divided by $21\% \times 71\%$). Similar patterns are present across other outcome variables.

3.1.2 Sub-Sample Analysis and Additional Specifications

Table 4 presents the variance decomposition separately for the first two buyout deals a professional leads ("Early Deals") and any subsequent deals the professional leads ("Later Deals"). There is some decline in attribution, but the main finding is that a similar fraction of explained variance in deal returns, value-add, and investing styles is attributed to the associate program for both early and later deals, similarly hovering between 20 and 40%. This stability indicates that associate program effects are persistent throughout the course of a PE professional's career.

Related work in other settings has found strong evidence of persistence in outcomes due to cohort effects (e.g., Oyer (2008), Malmendier and Nagel (2011), and Schoar and Zuo (2017)). To examine the role of analogous time-series cohort effects, I augment Equation (2) with an additional set of fixed effects for the year the professional first led a buyout deal at any firm, denoted by $\delta_{i(t)}$. With the new term underlined, the regression becomes:

$$y_{d,i(j,t)} = \lambda_j + \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \underline{\delta_{i(t)}} + \omega_t + \epsilon_{d,i(j,t)}. \tag{3}$$

The results presented in Table IA3 after including measures of cohort fixed effects show that a similar fraction of explained variance in deal returns, value-add, and styles is attributed to the associate firm. The cohort fixed effects explain little of the variation in deal outcomes, hovering around an attribution of 2 to 10% across most specifications. One exception is the TVM measure, where about 20% is attributable to the cohort fixed effects, gained mostly by increasing the overall model R^2 and absorbing a small part of the variance attributed to the other fixed effects. These

findings, overall, indicate that variation in time-series cohort effects do not explain much of the variation in buyout outcomes relative to cross-sectional cohort effects.

Table IA4 focuses on deals where the lead professional is a partner or principal at a firm other than their associate firm, documenting that the fraction of R^2 attributed to associate programs is robust to using this set of deals, nearing 20 to 40% across all outcomes. Table IA5 replaces partner firm fixed effects with fund fixed effects, effectively comparing investments led by different professionals within the same fund. The fraction of R^2 attributed to fund fixed effects is higher than the fraction attributed to firm fixed effects. But the fraction attributed to associate programs is also robust to this tighter specification, hovering just below 30% for most outcomes.

3.2 Interpreting the Variance Decomposition

The main takeaway from the results presented in Section 3 is that cross-sectional variation in buyout investment returns and styles is attributable to which PE firm the professional leading the deal worked at as an associate. Compared to others that worked as an associate at the same PE firm, PE professionals generate similar investment returns, invest in similar companies in similar ways, and oversee investments that exhibit similar financial and operational changes.

A natural question is: why? What makes some associate firms different than others? There are two main interpretations. One view suggests the results are driven by selection: PE firms may differentially hire new associates that ultimately tend to a particular style once they rise in seniority, independent of the associate experience itself. Another view suggests the results are attributable to skills learned through apprenticeship training as a PE associate: the associate experience at different PE firms varies in how it instills different practices in doing deals – ranging from how to bid for target companies to whether and when to recapitalize a portfolio company. While both views indicate that associate programs affect buyout investing, only the latter suggests that they do so directly.

3.2.1 Associate Firm Hiring and Career Path Decompositions

To study the role of a hiring explanation, I start by examining how associate firms differ in their hiring practices and the subsequent career progressions of their associates. I regress indicators

describing the pre-joining characteristics of associate i (such as whether they joined after completing an MBA) or post-joining career outcomes of associate i (such as whether they were promoted to the middle-level) on fixed effects for the associate firm, the school of the associate ($i(s)$), the bank/consulting firm of the associate ($i(f)$), and the year they joined their associate firm (ω_t). The estimating regression is:

$$y_i = \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \omega_t + \varepsilon_i. \tag{4}$$

The associate firm fixed effects, $\delta_{i(a)}$, are the main coefficients of interest, capturing how important across-associate firm variation is for explaining the characteristics of associates that join and subsequent career progressions of such associates. I then attribute the R^2 obtained from estimating Equation (2) to each of the four estimated fixed effects $\chi_b \in \{\hat{\delta}_{i(a)}, \hat{\delta}_{i(f)}, \hat{\delta}_{i(s)}, \hat{\omega}_t\}$ based on their Shapley values. Regression samples include the same associates as in Column (2) of Table 2, where data are available. This restriction allows me to view all associates of a given firm. Regressions of school characteristics omit $\delta_{i(s)}$ and regressions of prior experience omit $\delta_{i(f)}$. I do not further restrict to a connectedness sample (as in Section 3.1).

Figure 5 shows a large fraction of variation in hiring variables is attributable to associate firms. Equation (4) explains about 30% of the variation in whether an associate was hired from a feeder school, of which 35% is attributable to which associate firm the professional worked for. Prior firm and school fixed effects are respectively important for whether an associate is categorized as joining from a feeder firm or feeder school, indicating that some banks and U.S. schools are more likely to be feeders to the PE industry than others. Almost all associates have a degree from an AAU (Association of American Universities) school or prior investment banking experience, so the R^2 of these regressions is low, but similarly attributable to the associate firm. Associate firms explain at least 2/3 of the R^2 for whether associates are hired with an MBA, have previous PE experience (lateral hires), or have prior consulting experience, suggesting that firms vary systematically in whether they hire associates with such characteristics. Similar results are present for all characteristics, indicating that associate firms vary in whether they hire associates with given characteristics.

Figure 5 also shows that associate firms explain subsequent career outcomes of their as-

sociates. Between 40 and 50% of R^2 from predictive regressions for whether associates progress to a subsequent seniority, join an "other operating company" after leaving the PE industry, obtain an MBA, or lead a buyout deal (those studied in Section 3.1) is attributable to associate firms. Fixed effects for the year when the professional joined the firm have little explanatory power for the characteristics of hired associates, with attributions ranging from 3 to 14%. Such cohort fixed effects are more important for explaining subsequent career progressions, but still comparatively less important than associate firm fixed effects, with attributions ranging from about 10 to 20%. Table IA6 summarizes the regression results underlying Figure 5 alongside additional diagnostics.

To relate the hiring and outcome results, Figure 6 reports the coefficients obtained from regressions of associate firm fixed effects estimated for several deal outcomes (Equation (2)) on associate firm fixed effects for hiring and career outcomes (Equation (4)). Because the coefficients are estimated with noise, I standardize each coefficient to have a mean of zero and a standard deviation of one, shrink the variables towards the mean using an empirical Bayes procedure, and weight regressions by the t-statistic of the outcome variable. This test suffers from a small sample size (containing between 35 and 42 associate firms for which both coefficients are estimated and the correlations can be calculated). Still, the results indicate that outcome effects are only weakly related to hiring and career progression effects.

This finding – although only correlational and likely quite noisy – suggests that variation in associate firms *unrelated* to the characteristics of associates they hire plays an important role in why there are persistent differences in associate firm effects. The same associate firms that produce partners who tend towards particular performance or styles are not the same firms that load most heavily on particular hiring or career advancement features. To more directly examine these unexplained differences, subsequent tests quantify variation in training and mentorship using two distinct approaches. Section 4 presents the first approach, introducing a novel IV strategy to estimate the magnitude of training effects. Section 5 presents the second approach, measuring the cross-sectional variation in investing styles and outcomes of partners from the same associate firm where it is more challenging for the results to be explained by variation in hiring practices across firms. Results attained using both approaches support a training interpretation.

4 Associate Program Effects: Instrumental Variables

The goal of this section is to estimate a "treatment effect" of associate firms on associates to provide evidence as to why associate firms systematically produce principals and partners that tend towards particular investment styles and outcomes. An ideal experiment would randomly assign newcomers to associate firms and then track how their productivity, styles, and performance changes after working at their associate firm. The two main challenges with such an experiment are that (i) associates are not perfectly randomly assigned to firms and (ii) associates do not lead investments before joining their associate firm, leaving no "pre-treatment" period (e.g., Figure 2).

To overcome these challenges while still making progress on estimating associate firm treatment effects, this section proposes a novel IV strategy to test for skill changes; i.e., whether associate programs make associates better equipped at doing buyout deals on average. These tests are designed to provide evidence of one channel through which associate firms influence their associate employees: "buyout training." I think of "buyout training" as experience gained from working on buyout investments allowing a professional to do more or better deals. I test for evidence of such training through associate programs by estimating the relationship between associate turnover and the deal-making of their firm. The key assumption mapping associate turnover to training provided over the associate program is that associate turnover involves replacing more-experienced associates with less-experienced associates. Statistics presented in Figure 1 and Table 2 support this assumption as associates are significantly more likely to turnover (separate, leave for an MBA, or be promoted) at the end of their program than at the beginning.

If training matters and if its benefits are accumulated over the program, a third-year associate with more experience is presumably better equipped to contribute to the deal-making process of their firm than a first-year associate with less experience. A limitation of this test is that it can only provide evidence of training insofar as it relates to the investments of a PE firm rather than the investments of a given PE associate. The benefits of this test, however, are two-fold: (1) it circumvents the problem of endogenous associate and firm matching by focusing on firm-level effects and (2) it offers direct evidence of how partners acquire deal-making skills through an apprenticeship: associates gain experience working on deals on behalf of partners and principals but do not themselves lead the deals, which distinguishes "apprenticeship" learning from other channels of

acquiring investing skills such as learning-by-doing.

The relationship of interest is:

$$y_{jt} = \beta_1 \times (\%\ \text{Associates Turnover})_{jt} + \lambda_j + \gamma_{j(g,t)} \times \omega_t + \gamma_{j(p,t)} \times \omega_t + \epsilon_{jt}, \tag{5}$$

where y_{jt} denotes the number of buyout deals that PE firm j completes in year t, λ_j denotes a PE firm fixed effect, $\gamma_{j(g,t)} \times \omega_t$ denotes fixed effects for the state $\times$ year fixed effects and $\gamma_{j(p,t)} \times \omega_t$ denotes industry $\times$ year fixed effects. The interacted state and industry fixed effects are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial Services $\times$ 2015). Modal industries and geographies refer to the most common industry or state (by PitchBook classifications) of a target company that the firm has acquired via a stand-alone or platform buyout over a rolling window of no more than 10 years, spanning, for each firm, $[t-10, t-1]$. The firm and interacted year fixed effects partial out any firm-invariant, geography-time-invariant, or industry-time-invariant factors that may also influence deal activity. Throughout, regressions are weighted by the number of associates employed by firm j in year t and standard errors are independently double clustered at the firm and year levels. Table IA7 reports that subsequent 2SLS results are robust to a wide array of other reasonable specifications of the focal regression.

A negative β_1 estimate is consistent with training. The key challenge my empirical strategy seeks to overcome is that associates may not randomly turnover. OLS estimates of Equation (5) can be biased if turnover is correlated with unobserved determinants of buyout activity. Associates may, for example, separate from a firm when their outside options are comparatively higher, which could correlate with the investment opportunities available to the associate firm. Instead, to better capture the relationship between associate turnover and PE firm deal-making, I propose instrumenting for the fraction of a firm's associates that turnover in a given year with the fraction of its associates in their third calendar year or longer with the firm.

4.1 Identifying Assumptions

Identifying variation comes from comparing the number of deals a PE firm does in years where a higher fraction of its associates are predicted to turnover (based on their tenure) to the number

of deals a PE firm does in years where this fraction is lower. The key identifying assumptions are that (1) the fraction of associates in their third (calendar) year or higher is associated with associate turnover, and (2) the fraction of associates in their third (calendar) year or higher only affects buyout deal quantities through the fraction of associates that turnover in a given year.

4.1.1 Relevance Condition

The first assumption, the relevance condition, is testable. The first stage regression is:

$$\begin{aligned} \%\text{ Associates Turnover}_{jt} = \beta_{\text{First-Stage}} \times (\%\text{ Associates in Year 3+})_{jt} \\ + \lambda_j + \gamma_{j(g,t)} \times \omega_t + \gamma_{j(p,t)} \times \omega_t + \epsilon_{jt}, \end{aligned} \tag{6}$$

where the regression benefits from the same fixed effects and weighting as in Equation (5). Table 5 presents the results of estimating this first-stage regression. Column (1) indicates that a one standard deviation increase in the fraction of associates that are in their third year or higher (28%) is associated with a 12% increase in the fraction of associates that turnover (= 28% ×0.436 estimated regression coefficient). Panel A of Figure 7 presents this relationship visually, showing an upwards-sloping linear relationship between the two variables. Figure IA3 displays the results using the alternative *binsreg* command introduced by Cattaneo, Crump, Farrell, and Feng (2024), which adjusts the residualization and the number of bins.

The remaining columns in Table 5 present the results of re-estimating Equation (6) after including increasingly stringent controls for characteristics of the associates, the firm, and financial market conditions that may otherwise not be absorbed by the included fixed effects. The positive relationship between associate tenure and turnover retains statistical and economic significance across all specifications. Aside from associate tenure, the only other robust predictor of associate turnover is how many associates a firm hired three calendar years prior. I discuss this further in Section 4.1.2. Stability in the estimated $\beta_{\text{First-Stage}}$ coefficient (changing mostly due to restrictions that covariates impose on regression samples) and R^2 support the idea that associates often go "up or out" because their associate program is completed, rather than due to unobserved confounders, following the logic of Oster (2019). Moreover, the first-stage F-statistic obtained in subsequent 2SLS regressions of nearly 200 indicates a strong instrument.

4.1.2 Exclusion Restriction

The second stage regression relates the number of buyout deals that firm j invests in during year t to instrumented associate turnover:

$$y_{jt} = \beta_{IV} \times (\%\ \widehat{\text{Associates Turnover}})_{jt} + \lambda_j + \gamma_{j(g,t)} \times \omega_t + \gamma_{j(s,t)} \times \omega_t + \varepsilon_{jt}. \tag{7}$$

Associate turnover is instrumented following Equation (6). The regression benefits from the same fixed effects and weighting as in Equations (5) and (6).

One potential concern with the exclusion restriction is reverse causality; that differences in deal flow determine how long associates are predicted to remain at their firm. The main prediction of reverse causality, therefore, is that results should be driven by firms with different deal-making prospects. For example, associates at firms that have fewer investment opportunities may be more likely to "jump ship" and results would concentrate among PE firms that are going out of business.

As one way to examine this possibility, I split the sample to isolate GPs that are doing fewer deals or otherwise lack the capital to make more investments and estimate the 2SLS specification separately in each sub-sample of firms. Specifically, I split firm-year cells based on (a) whether the GPs hired new associates (and therefore had success in recruiting new talent, which should be less likely if their "ship was sinking"); (b) whether the GP has raised a new fund in the prior three calendar years (over the associate's predicted tenure); or (c) whether their deal volume was on an upwards or downwards trend around when associates are predicted to turnover. These tests are designed to remove observations where reverse causality would play a major role in explaining the results if it is a driving force.

Table IA8 presents the results, finding identically signed and economically similar estimates of β_{IV} among each of these sub-samples of firms. Whether the firm hired new junior talent, raised new capital to deploy or, made relatively more investments, does little to attenuate the relationship between buyout activity and instrumented associate turnover, suggesting that the reverse explanation does not have sufficiently powerful traction in the data.

As another way of examining the role of potential reverse causality, Table IA9 presents evidence of how associate turnover in year t relates to deal quantities observed with leads and lags spanning several years. A distinguishing prediction of reverse causality is that associate turnover

should forecast future deal activity because it reflects PE firm employees' perceptions about how many deals their firm will invest in. If so, higher turnover in year t should strongly predict a directional change in future buyout investing. The evidence presented in Table IA9, however, also fails to favor this reverse interpretation. Although instrumented associate turnover in year t has a negative relationship with deal-making in years $t+1$ through $t+3$, the relationship is weak, not always robust to alternate specifications, and by year $t+4$, the relationship reverses sign and is weakly positive, failing to predict a permanent directional change in future buyout investing.

In addition, the results in Table IA9 provide evidence of a more plausible accounting of the identifying variation used to estimate Equation (6): instrumented associate turnover in year t is positively correlated with the number of deals a firm completed in year $t-3$. One interpretation of this pattern is that spikes in deal activity in year $t-3$ lead to increases in associate hiring in year $t-3$. And then because associates work under structured programs, three calendar years later in year t, the firms that experienced abnormally high deal volume will have a higher fraction of associates that turnover. This interpretation is distinct from the reverse causality interpretation of the IV because it indicates that deal flow affects hiring but it does not affect separations. Axelson and Bond (2015) provide support for this assumption with a model of finance jobs that predicts financial sector firms respond to bad times by hiring less rather than increasing firing.

The training interpretation is valid given this pattern so long as firms, on average, replace marginally more-experienced associates with marginally less-experienced associates (or none at all). The caveat is that they are in the position to do so because they had different deal volume in prior years. This potential caveat is not problematic for the exclusion restriction insofar as the abnormal increases in lagged deal volume are idiosyncratic to the firm. Table IA7 provides evidence supporting this assumption as the results remain robust to including lagged controls of the number of deals a firm does and associates it hires, which partials out any predictable variation in in the number of deals done in year t based on deal volume and associate hiring in year $t-3$. But if firms do not randomly experience abnormal deal flow and concurrent associate hiring in year $t-3$, there is scope for omitted factors to potentially confound the results, offering a second potential concern with the exclusion restriction.

To examine the scope of potentially omitted variation in explaining subsequent results, I split the sample to isolate GPs that are differentially exposed to other omitted factors and estimate

the 2SLS specification separately in each sub-sample of firms. Specifically, I split firm-year cells based on (a) measures of equity and credit market conditions; (b) whether any partners or principals of the firm separated in year t; (c) whether the associates worked at their firm in an analyst or senior associate position which may otherwise result in endogenous deviations from a "standard" associate program; or (d) to focus only on associates that separated or that were promoted. These tests are designed to remove observations where potentially omitted factors related to lagged spikes in deal volume would play a more pronounced role in explaining the results if they are a driving force. Table IA8 presents the results, finding identically signed and economically similar estimates of β_{IV} among each of these sub-samples of firms, suggesting that these potentially omitted sources of variation also do not have sufficiently powerful traction in the data.

4.2 Instrumental Variable Regressions

Table 6 presents the results of estimating the 2SLS regression presented in Equation (7). Column (1) indicates that there is a negative relationship between instrumented associate turnover and the number of buyout deals that a PE firm does in a given year. The estimated coefficient (β_{IV}) has a value of -2.23, indicating that a one standard deviation increase in instrumented associate turnover results in a decline of about 0.5 buyouts per year (= 2.23 regression coefficient $\times$ 0.25 standard deviation). This estimated effect is economically meaningful, equaling about 17% of the average number of deals completed each year per standard deviation of associate turnover. Panel B of Figure 7 presents this relationship visually, showing a downwards-sloping relationship between deal quantities and instrumented associate turnover.

The remaining columns of Table 6 re-estimate the 2SLS regressions after splitting the deals by whether they conform to the modal style of the PE firm, estimated by either the industry or geography in which it most frequently invested. These tests are meant to focus on investments in companies where the partners of the firm have less experience (those that the firm has historically not focused on) and any contribution of the associates may be magnified. Consistent with this hypothesis, the reported coefficient estimates are negative across all specifications but only attain statistical significance for deals outside of the previously observed styles.

The results in this section are consistent with the hypothesis that associate firms play a role

in shaping associates, beyond merely selecting them. These findings indicate that instrumented associate turnover, which involves replacing a more experienced associate with a less experienced associate, is negatively related to the number of buyout investments a PE firm makes. This evidence points to experience gained in PE associate programs as increasing the extent to which the average PE associate contributes to the buyout deal-making process.

5 Associate Program Effects: Cross-Sectional Evidence

This section measures how similar the deals led by a buyout professional are to the deals completed by their associate firm while they worked as an associate at that firm. Doing so helps to gauge the economic determinants of a training mechanism.

I measure the cosine similarity between the characteristics of each deal led by each professional (d_i) and the average characteristics of all deals done by their associate firm in the five years before the professional first led a deal (a_i). For example, if the professional first led a deal for GTCR in 2011 and was an associate at Blackstone, I calculate the cosine similarity between each deal the professional led in 2011 and later to the average deal completed by Blackstone from 2006 to 2010. The null hypothesis is that the deals are dissimilar.

Deal characteristic vectors include 116 indicators for: the industry and state of the portfolio company, whether there were any specific changes to the portfolio company (e.g., CEO changes or recapitalizations), and how the professional entered the deal (e.g., through a management buyout or through a secondary buyout). Regression samples, therefore, exclude deals led by professionals where PitchBook does not have data on the associate firm's deals for this lookback period. I further restrict the sample to only deals led by professionals with associate experience at U.S. buyout firms. Doing so omits deals led by professionals with PE associate experience at non-U.S. buyout firms but allows me to remain realistic about interpreting higher cosine similarity scores as representing more similar investments. These restrictions reduce regression samples relative to the connected sample discussed in Section 3, but result in an observably similar set of deals.

Figure 8 plots the histograms of cosine similarities between professional's deals and those completed by their associate firms over the five years before they first led a deal. Cosine similarities range from 0 to 1, where 0 indicates that all characteristics of the deal are orthogonal and 1 indicates

that all characteristics are the same. All distributions exhibit a mass at zero, indicating a fraction of deals have characteristics that were not present in deals completed by the partner's associate firm in the five years before they first led a deal. Nonetheless, the cosine similarity for vectors of all considered characteristics is fairly high, averaging about 0.45. This indicates that professionals' investment decisions bear resemblance to the deals they likely observed at their associate firm while working as an associate.

To study the economic determinants of these similarities, I estimate regressions of the form:

$$\text{Similarity}(d_i, a_i) = \beta' X + \lambda_j + \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \omega_t + \epsilon_{d_i}, \tag{8}$$

where $\text{Similarity}(d_i, a_i)$ denotes the cosine similarity score between buyout investment d, with lead partner i, of firm j undertaken in year t and the average buyout investment their associate firm completed in the five years *before* partner i led any deals, denoted a_i. The coefficients of interest are in the β vector which includes several characteristics that attempt to explain economic determinants of similarities. Similar to Equation (2), the regression benefits from fixed effects for; the investment bank or consulting firm that the lead professional worked at before entering the PE industry ($\delta_{i(f)}$), the school from which the professional matriculated ($\delta_{i(s)}$), the PE firm the professional represents in the deal (λ_j), and the year in which the professional made the investment (ω_t). To help limit persistence in similarities that could be somewhat more mechanical, I also include an indicator for whether the partners' associate firm invested in the deal (either as part of the investing consortium or because it employed the former associate as a partner) as a control.

Most crucially, to help limit the role of assortative matching between PE firms and associates in explaining the estimated coefficients, Equation (8) includes fixed effects for the associate firm of professional i, denoted $\delta_{i(a)}$. Accordingly, coefficients result from comparisons within lead principals and partners that previously worked as an associate at the same PE firm.

Table 7 presents the first set of results examining the relationship between similarities and a lead professional's experience, estimated based on how many years have elapsed between the focal deal and the year of a professional's first deal. The reported coefficients indicate that among two former associates of the same PE firm, those that are further along in their careers lead investments that are less similar to deals their associate firm completed. Although these coefficients

are statistically significant, the key finding is that the decline is not economically very large. The coefficient reported in Column (1) of Table 7, indicates that cosine similarities decline linearly by only about 0.012 per year, implying a long "half-life" of about 20 years ($\approx \frac{.046 \text{ first deal}}{.012 \times 2}$). Figure 9 reports that this finding visually. These findings are consistent with the results reported in Table 4 showing that associate program fixed effects have similar, but declining, explanatory power for a professional's earlier and later deals.

5.1 Training Mechanism: Mentorship

Table 8 focuses on the relationship between deal similarities and whether the associate was plausibly exposed to top-performing partners at their associate firm. Because the initial partner and principal composition may also influence sorting of associates to firms, I focus on senior professional departures after the associate had already joined the firm.

For lead professional i that first led a deal in year t and worked at associate firm $i(a)$, the set of senior professionals that they may have worked for includes all middle- and top-level professionals employed at firm $i(a)$ from years $[t-1, t-5]$. The five year window is selected roughly to match empirical patterns between when professionals enter the industry and when they begin leading deals seen in Figure 2, but subsequent results are robust to other windows as well. I define active senior professionals to be those that led a deal representing associate firm $i(a)$ over this time period. "Non-active" professionals did not. "Top-performing" professionals include those that have led a top-quartile platform or stand-alone buyout investment in a U.S. company, based on the gross TVM of the exited investment within any year $\tau \leq t-1$. "Bottom-performing" professionals include those that have led a bottom-quartile deal, based on the gross TVM of the exited investment within any year $\tau \leq t-1$. These performance quartiles are not mutually exclusive. A professional that led a top-quartile buyout deal in 2010 and a bottom-quartile deal in 2008, for example, would be flagged as a bottom-performing partner from 2008 and later and both a top- and bottom-performing partner from 2010 and later. For each senior professional, I consider their full history of deals, not just those led while representing firm $i(a)$.

These tests are potentially informative about how exposure to different types of partners shapes future partner's investing styles and performance. But they come at the cost of imposing

tight restrictions on the regression samples where the relationship is estimable, requiring the full employment history for a given PE firm each year and the investing history for at least some of the firm's senior professionals. They are therefore restricted to only include (i) deals led by former associates of any of the 207 firms with archived website data where employment data is observable (spanning 2000 to 2022) and (ii) deals lead by former associates of firms where the gross returns of senior professional's deals is available in the StepStone dataset. These data limitations reduce the regression sample to about 1,000 deals led by 360 investment professionals, covering about 20% of the connected sample of deals. Compared to the connected sample, these deals are observably similar in terms of average cosine similarity, size, performance, and other deal characteristics, but generally cover more recent years.

For this set of deals, Column (1) of Table 8 shows that among two former associates of the same PE firm, those where an active top-performing partner left during the five years before they were a lead investor, make investments that are less similar to those of their common associate firm. This relationship is economically significant, equal to the decline in similarity that an average associate would experience over about 7.5 years (= .089 estimated regression coefficient divided by .012 cosine similarity decline per year estimated in Table 7). Column (1) shows that this relationship works in reverse also. Professionals for whom an active bottom-quartile partner left their associate firm during the five years before they were a lead investor, make investments that are more similar to those of their common associate firm.

Column (2) additionally reports the relationship based on the turnover of any top- and bottom-performing professionals, not just those that actively led deals for the firm. The negative relationship between top-performing partner turnover and subsequent deal similarity is weaker, equal to the decline in similarity that an average associate would experience over about 3 years. In contrast, the positive relationship between bottom-performing partner turnover and subsequent similarity has a similar magnitude but attains statistical significance.

The results in this section further support the role of training by offering evidence of one way in which buyout investing skills are instilled upon future partners: through exposure to higher quality potential mentors. Senior professionals that have better track records may either directly staff the associates on deals and mentor them or espouse their views during investment committee meetings, and thereby indirectly mentor them. In contrast, that the departure of inactive senior

professionals with worse track records leads to subsequently more similar deals suggests these partners may otherwise impede the mentoring process.

5.2 Economic Mechanism: Partnership Share Liquidity

I posit that one potentially relevant economic force underlying this training equilibrium is the illiquidity of partners' and principals economic interests in their partnership, their "partnership shares." This section examines this hypothesis by relating measures of the training received by PE associates to the liquidity of their PE associate firms' partnership claims.

Morrison and Wilhelm (2004) present a model wherein organizing as a partnership facilitates intergenerational transfers of tacit human capital. A key element of this model is extreme illiquidity of partnership shares. In the model, there will only be a market for partnership shares if there are sufficiently skilled ascending junior partners willing to buy the shares from a retiring partner. This illiquidity, therefore, incentivizes partners to mentor and train new associates to assure themselves of an adequate return on their claim in the partnership.

A central prediction from this model is that increases in the liquidity of partnership shares decrease the incentives of the incumbent partners to invest in mentoring the next generation. When it is easier to cash out from a partnership, the incentive to ensure the next generation is sufficiently skilled to buy their shares weakens.

To test the role of such an illiquidity mechanism in explaining how PE firms train their associates, Table 9 regresses the cosine similarity of a focal deal (observed at the lead professional $\times$ deal level) on indicators measuring whether the lead professional on the deal worked as an associate for partners and principals that owned partnership shares of different liquidity. Measures of exposure to differential partnership share liquidity include the fully saturated interaction of two components: (1) whether the professional worked at their associate firm around key changes in the liquidity of PE partnership shares and (2) whether their associate firm was sufficiently exposed to the liquidity changes. I identify three main events that sharply increased the liquidity of PE partnership shares.

1. Blackstone IPO. The first is the Blackstone IPO, completed on June 22, 2007. This was the first major public listing of a U.S. PE firm and represented a sharp increase in the possibility

that PE firms could go public.[4] One motivating factor often cited for why Blackstone chose to complete an IPO was to provide liquidity for one of the two co-founders. But not every PE firm is equally exposed to the increase in the probability of completing a public listing; going public is an option for larger PE firms but IPOs come with costs, discouraging smaller firms from going public (e.g., Ritter and Welch (2002) and Gahng, Ritter, and Zhang (2023)). To focus results on firms that are more plausibly exposed to these concurrent liquidity changes, the second component of the interaction term includes the average AUM of the associate firm in the five years before the professional began leading deals. This restricts regression samples to deals led by professionals where their historical associate firm AUM is available.

The results presented in Table 9 benefit from the same fixed effects as in Equation (8), effectively comparing cosine similarities among professionals that were former associates of the same firm but at different times. Column (1) indicates that cosine similarities are, on average, higher after the Blackstone IPO and among former associates of larger firms. The main takeaway from Column (1), however, concerns the interaction term: former associates of larger firms that worked as PE associates after the Blackstone IPO lead deals that have lower cosine similarity scores. The coefficient estimate of -0.141 per \$10 billion of AUM is economically sizeable, equivalent to the decline in similarity seen over about 15 years working as a PE professional.

2. GP Stake Sales. The second liquidity event is the start of institutional General Partner (GP) stake sales. GP stake sales are deals where non-exchange listed PE firms sell a minority interest in their future income streams (management fees, carried interest, and GP returns on their own investments) to other financial institutions without going public. Gahng and Jackson (2023) study these deals and find that the institutional market for the deals started in 2010 – at which point it became characterized by a competitive bidding process with multiple potential buyers. The advent of these deals represented a strong increase in the possibility that PE firms could sell their partnership claims to outside investors, not just public investors, thereby increasing the liquidity of their partnership shares. This second liquidity event is useful because it occurs at a different time than the Blackstone IPO and also because GP stake sales are more common the IPOs, plausibly

[4]Fortress Investment Group also went public in 2007 (delisted in 2017 after being acquired by SoftBank) but did not predominantly sponsor buyout funds. American Capital (delisted in 2017 after being acquired by Ares) was a U.S. PE firm that completed in an IPO in 1997 after changing its structure to a Business Development Company (BDC), but it, and other earlier non-U.S. PE firms that were publicly listed, such as 3i, are widely perceived to be outliers (e.g., Ivashina and Lerner (2019)).

being relevant for more PE firms.

Column (2) of Table 9 reports lower cosine similarities among former associates of larger firms following the advent of GP stake sales. The estimated coefficient is about −.098. This coefficient is less than the difference following the 2007 Blackstone IPO, but still economically sizeable, equivalent to the decline in similarity seen over about 10 years working as a PE professional.

3. Firm-Specific Sales. Despite the fairly stringent use of fixed effects, one concern with interpreting evidence from the first two events is that other aggregate factors may also coincide with the posited changes in liquidity. The Blackstone IPO, for example, was shortly followed by the Great Financial Crisis. Accordingly, in the third set of liquidity events, rather than compare lead professionals' deals based on whether their associate experience occurred before or after *market-wide* changes in the liquidity of PE partnership shares, I make use of *firm-specific* changes in the liquidity of PE partnership shares. This third set of liquidity events is useful because it occurs at a different time for each firm, if it occurs, helping to limit the role of other confounding aggregate factors. Professionals are grouped by whether they were associates before or after the year their associate firm first sold a GP stake or completed an IPO (if at all). The timing of GP stake sales and IPOs is from Gahng and Jackson (2023) and about 1/3 of deals in the regression samples are led by a professional whose associate firm either sold a GP stake or completed an IPO.

Column (3) of Table 9 presents these results, finding that cosine similarities are lower among cohorts of associates that worked at a larger PE firm after it sold a GP stake or completed an IPO compared to those that worked at the same firm before. The coefficient estimate of about −.040 is equivalent to the decline in similarity seen over about four years working as a PE professional.

The main takeaway from the results presented in this section is that partners who worked as associates before their associate firm's partnership shares became more liquid tend to do deals that are subsequently more similar to those completed by their associate firm compared to partners who worked as associates their associate firms' partnership shares were relatively less liquid. This evidence points to the illiquidity of partnership shares as one economically important force that incentivizes mentoring in PE partnerships.

6 Conclusion

The central conclusion of this paper is that PE partners from different PE associate programs systematically invest differently from one another. The reason for these differences appears to be due, in part, to the differential training they received.

Using a newly-created dataset of buyout professionals and their careers combined with several other PE datasets, I estimate that a sizeable fraction of the explained cross-sectional variation in deal returns and deal characteristics is attributable to where PE partners first worked as PE associates. Decomposing the variation in deal returns, for example, shows that about 33% of the R^2 in deal-level performance regressions can be explained by which firm the lead partner worked at as a PE associate. Similar results attain for investment styles: associate programs explain 20 to 40% of the explained variation in various deal characteristics, ranging from which industries the partner invests in to whether the portfolio company changes its CEO.

Moreover, using a novel instrumental variables strategy based on variation in when associates are predicted to turnover (based on the predicted expiration of their associate programs), I find evidence indicating that associate programs train associates. Replacing more experienced associates with less experienced associates results in a decrease in how many deals the associate firm completes.

Cross-sectional evidence suggests that two factors are important for determining the quality of training an associate receives: the presence of higher quality mentors and partners with more illiquid shares. Comparisons between two former associates of the same firm indicate that those that worked as an associate when better-performing partners left the firm or when the partners' shares became more liquid later tend to make investments that are less similar to what they observed while working as an associate.

Overall, my results highlight that one important reason why some PE investors perform differently and adopt different styles compared to one another is because of the differential training they received.

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Figure 1: Up-or-out Career Paths

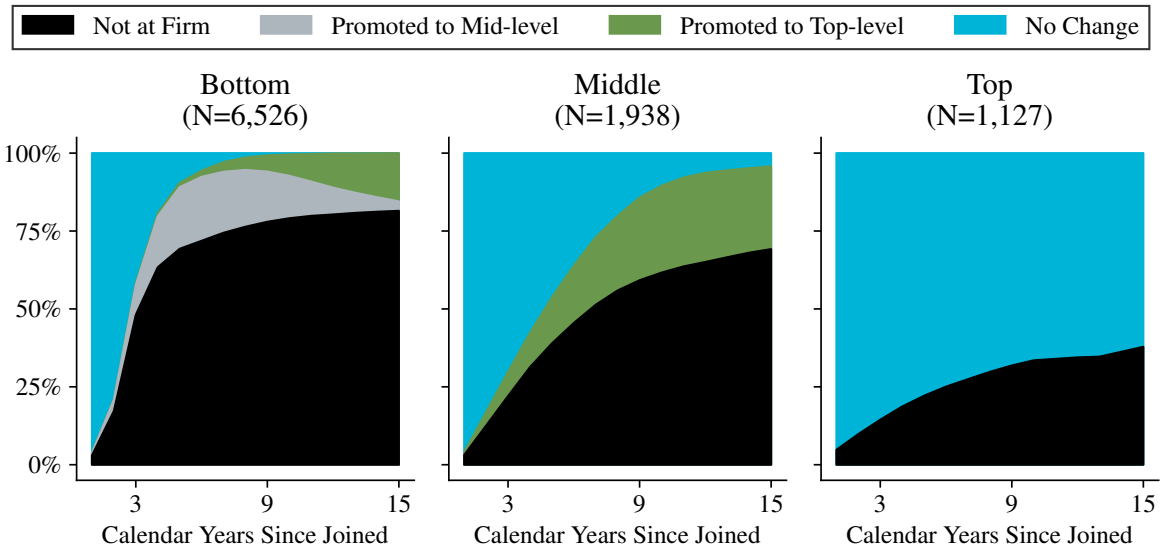


Figure 1: Plots the fraction of private equity professionals that separated from their firm or were promoted to a subsequent hierarchal level within their firm conditioned on (1) how many years have elapsed since the year they first joined their firm and (2) their seniority upon joining the firm. Only within-firm promotions are considered. Years since joining equals the count of unique calendar years elapsed since professional *i* joined firm *j*. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. Samples require that the year the person joined the firm and their seniority upon joining are available. Panels are unbalanced; individuals joining in, for example, 2014, have at most nine years of data (2014 to 2022, inclusive). Each plot is smoothed by a lowess function with a smoothing weight of 0.30. "N" displays the total number of investment professionals included in each subsample.

Figure 2: Task Transitions Over Private Equity Careers

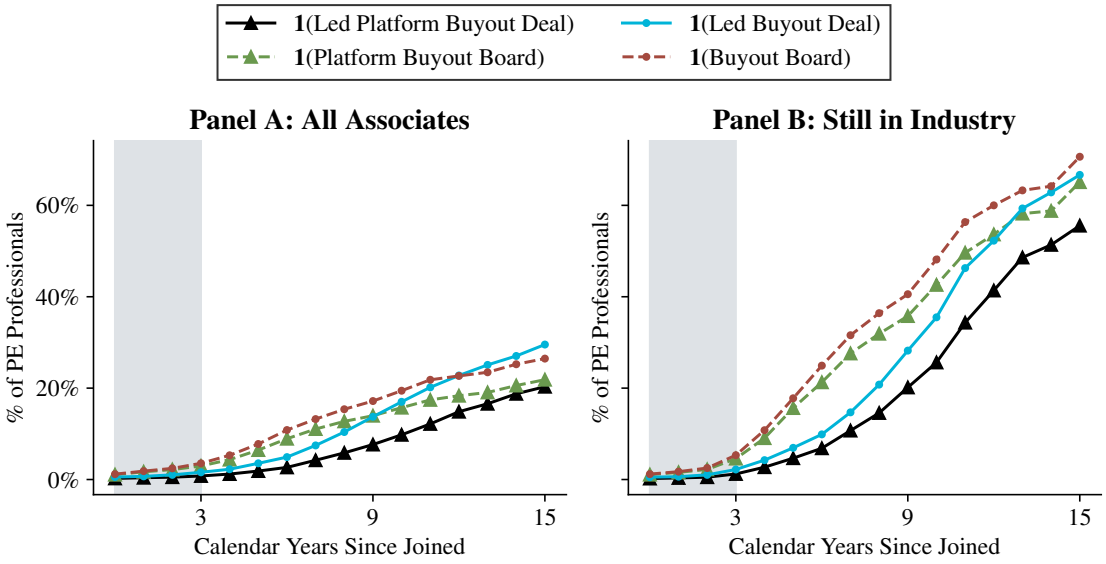


Figure 2: Plots the fraction of private equity professionals that have led at least one buyout deal or sat on the board of at least one company acquired in a buyout deal conditioned on how many years have elapsed since the year they first joined their firm as an associate. The sample includes all associates from Table 2 whose careers, both within and across firms, are tracked each year on and after they first worked as an associate at one the sample firms collected via archived websites. The shaded gray box spans the anticipated calendar years spent working at the bottom-level. Panel A includes all associates and Panel B restricts the sample to only include professionals that still work at a U.S. PE firm in a considered year. For example, an associate of GTCR that joined in 2000, left to get an MBA in 2003, returned to GTCR in 2005, and then left in 2010 to work at Apple, would be included in in Panel A from 2000 to 2015 and in Panel B from 2000 to 2010. Panels are unbalanced; individuals joining in, for example, 2014, have at most nine years of data (2014 to 2022, inclusive). Platform buyouts include stand-alone or platform buyouts of U.S. companies completed by U.S. buyout firms. The more general "buyouts" category includes any completed buyout investment. Dashed lines are for board experience. Solid lines are for deal experience. Triangle markers are for platform buyouts. Circle markers are for more general buyouts. Deals and affiliations are from PitchBook. Career progression data are from archived websites cross-validated with Revelio Labs.

Figure 3: Implied Economic Magnitudes of Associate Firm Fixed Effect Coefficients

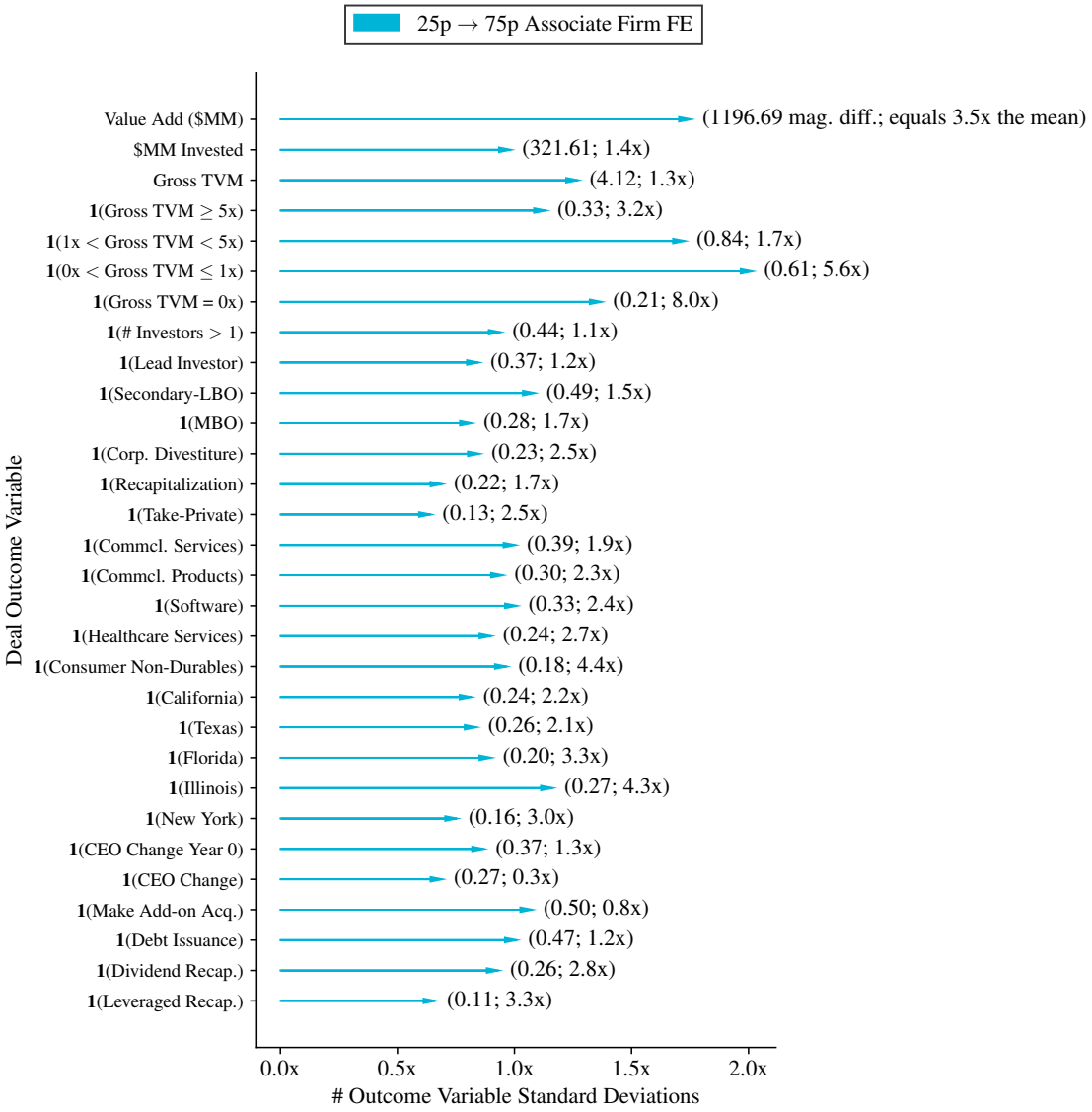


Figure 3: Plots the predicted differences in economic magnitudes of outcome variables given changes in the percentile ranking of the associate firm fixed effect coefficient. The y-axis reports outcomes including deal returns, deal characteristics, and operating and financial changes at the portfolio company following the LBO. The x-axis reports the number of standard deviations the outcome variable is predicted to increase by if the associate firm fixed effect coefficient is at the 75th percentile of associate firm fixed effects for that outcome variable rather than the 25th percentile. Numbers in parentheses report the level change in outcome variable alongside the level change in the outcome variable scaled by the average of the outcome variable in the regression sample. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. The PE firm associate program attended by the lead professional must be available. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted.

Figure 4: Variance in Partners' Explained Deal Outcomes Attributable to their Associate Firms, Education, and Prior Work Experience

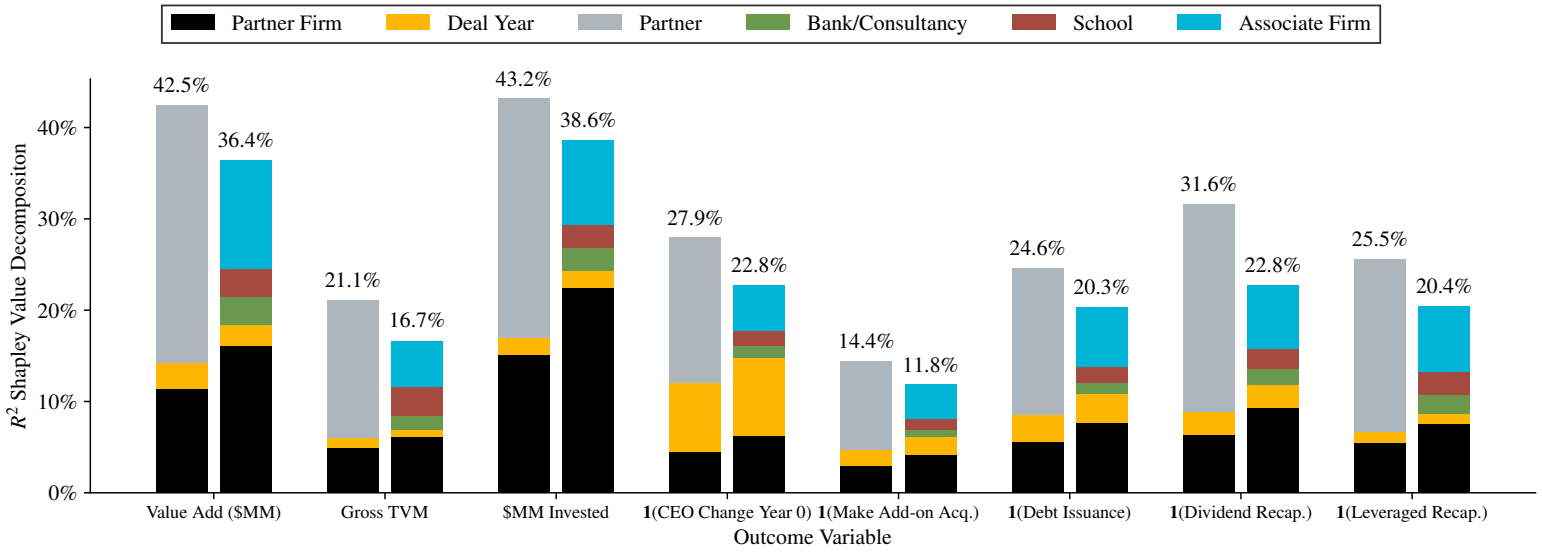


Figure 4: Plots Shapley-value decompositions of several fixed effects in explaining variation in deal outcomes. Decompositions are calculated separately after estimating two different regressions for each outcome variable. The regressions used to construct the first bar for each outcome variable include fixed effects for the partner, their firm, and the year of the deal: $y_{d,i(j,t)} = \lambda_j + \delta_i + \omega_t + \varepsilon_{d,i(j,t)}$, where $y_{d,i(j,t)}$ denotes the outcome of deal d, with lead partner i, of firm j as of year t. λ_j, δ_i, and ω_t respectively denote firm, partner, and year fixed effects. The second estimates the regression: $y_{d,i(j,t)} = \lambda_j + \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \omega_t + \epsilon_{d,i(j,t)}$, which includes firm and year fixed effects alongside $\delta_{i(a)}, \delta_{i(f)}$, and $\delta_{i(s)}$ which respectively denote fixed effects for the associate program of partner i, the investment bank or consulting firm that previously employed partner i, and the school from which partner i matriculated. Values at the top of each bar report the full model R^2. Bands on each bar report the estimated contribution to the full model R^2 resulting from each set of fixed effects based on a Shapley value decomposition. Regression samples only include platform or stand-alone buyouts of U.S. companies. Regressions estimated with partner fixed effects additionally require that the professional led at least two investments. Deals are from PitchBook and deal returns are from StepStone. Total Value Multiples (TVM; the gross cash flows to the GP divided by their gross investment amount) and value-add regressions additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the buyout unless otherwise noted.

Figure 5: Variance in Associate Characteristics and Careers Attributable to Associate Firms

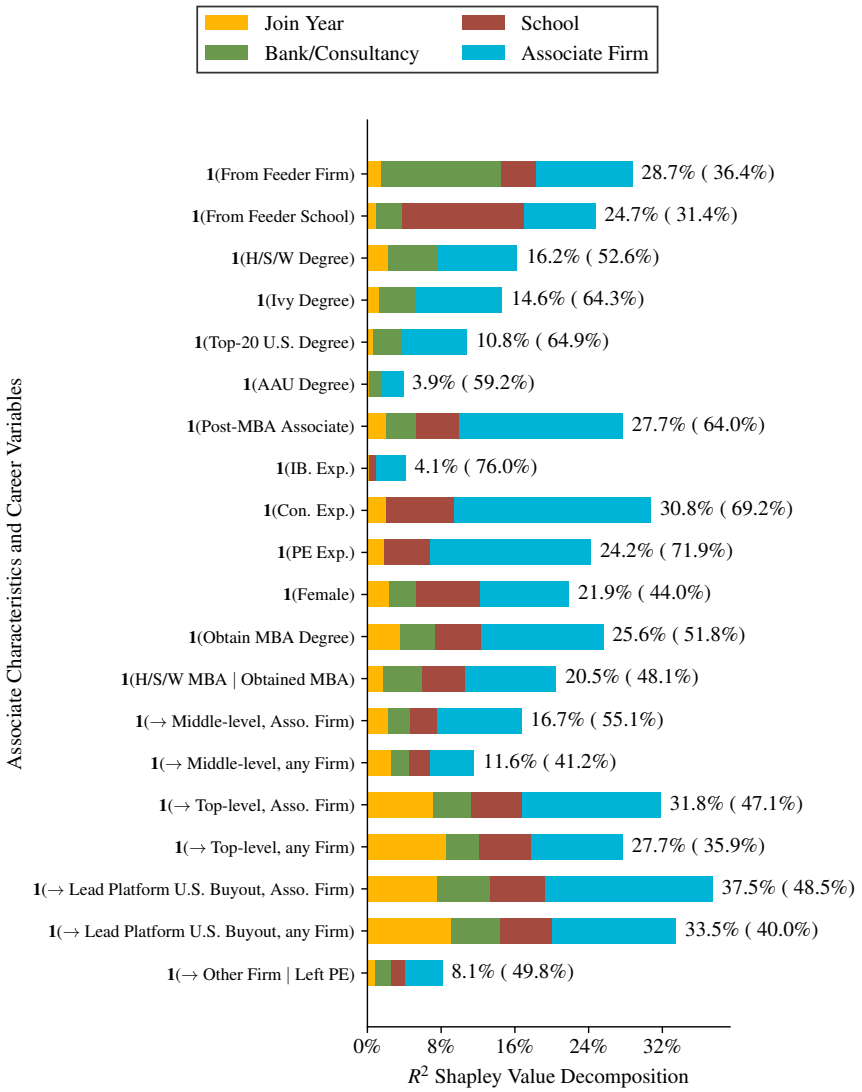


Figure 5: Plots Shapley-value decompositions of several fixed effects in explaining variation in the hiring practices of associate firms and subsequent career progressions of PE associates. The unit of observation is the associate (i). The regressions used to construct the bars for each outcome variable include fixed effects for the associate firm, the school of the associate, the bank/consulting firm of the associate, and the year they joined their associate firm: $y_i = \delta_{i(a)} + \delta_{i(f)} + \delta_{i(s)} + \omega_t + \varepsilon_i$ where y_i denotes characteristics or career outcomes of associate i, of associate firm a that joined the associate firm in year t. $\delta_{i(f)}$ and $\delta_{i(s)}$ respectively denote fixed effects for the investment bank or consulting firm that previously employed associate i, and the school from which associate i matriculated. Regressions of school characteristics omit $\delta_{i(s)}$ and regressions of prior experience omit $\delta_{i(f)}$. Values at the top of each bar report the full model R^2. Bands on each bar report the estimated contribution to the full model R^2 resulting from each set of fixed effects based on a Shapley-value decomposition. Values in parentheses report the associate firm contribution. Top-level and lead partner regressions only include associates that joined in 2014 or earlier. MBA regressions require that the associate joined without an MBA. Regression samples otherwise include the same associates as in Table 2, Column (2) where data are available.

Figure 6: Associate Firm Hiring and Outcome Fixed Effect Correlations

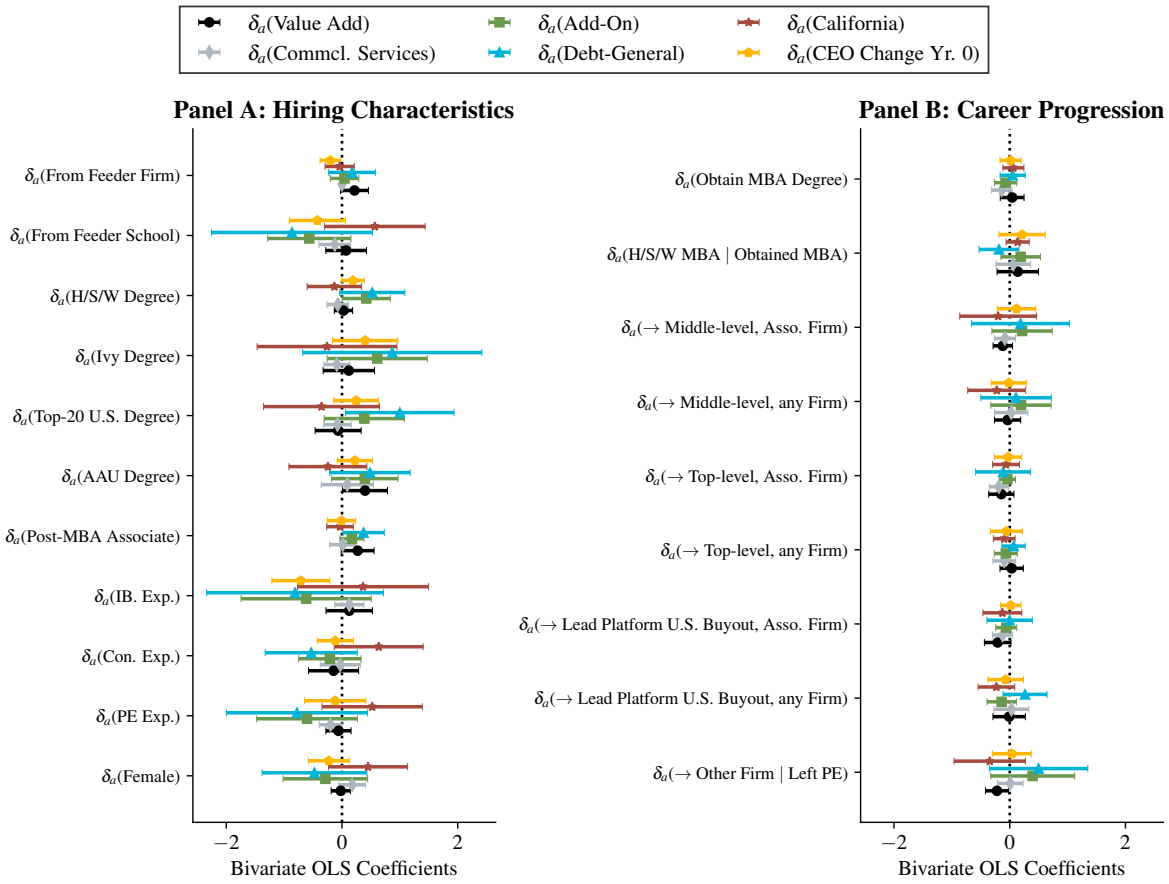


Figure 6: Plots coefficients estimated from bivariate OLS regressions of associate firm fixed effects for deal outcomes on associate firm fixed effects for hiring and career progressions. The unit of observation is the associate firm (a). Each coefficient in the plot is the output from a separate regression. Dependent variables are reported in the figure legend and independent variables are reported along the y-axis of the subplots. For example, the black series with circle markers reports the results of regressing the estimated value-add coefficient for associate firm a on its estimated fixed effects for the characteristics of associates it hired. Regression samples require that the associate firm fixed effect was estimated for the outcome variable (using the deals sample; Equation (2)) and the hiring variables (using the website sample; Equation (4)). Each regression in Panel A has between 40 and 42 observations. Each regression in Panel B has between 35 and 41 observations. All associate firm fixed effect coefficients are standardized to have a mean of zero and standard deviation of one. Because coefficients are estimated with noise, I employ the empirical Bayes method to shrink associate firm coefficient estimates (δ_a) towards the mean as: $\hat{\delta}_{a,\text{shrink}} = (\frac{\text{Var}(\delta_a)}{\text{Var}(\delta_a)+(1/t^2_{\delta_a})})\delta_a + (1 - \frac{\text{Var}(\delta_a)}{\text{Var}(\delta_a)+(1/t^2_{\delta_a})})\mu_{\delta_a}$. Regressions are further weighted by the t-stat of the outcome variable. Markers report the coefficient estimate and bars report the estimated 95% confidence intervals.

Figure 7: Instrumented Associate Turnover and Buyout Deals

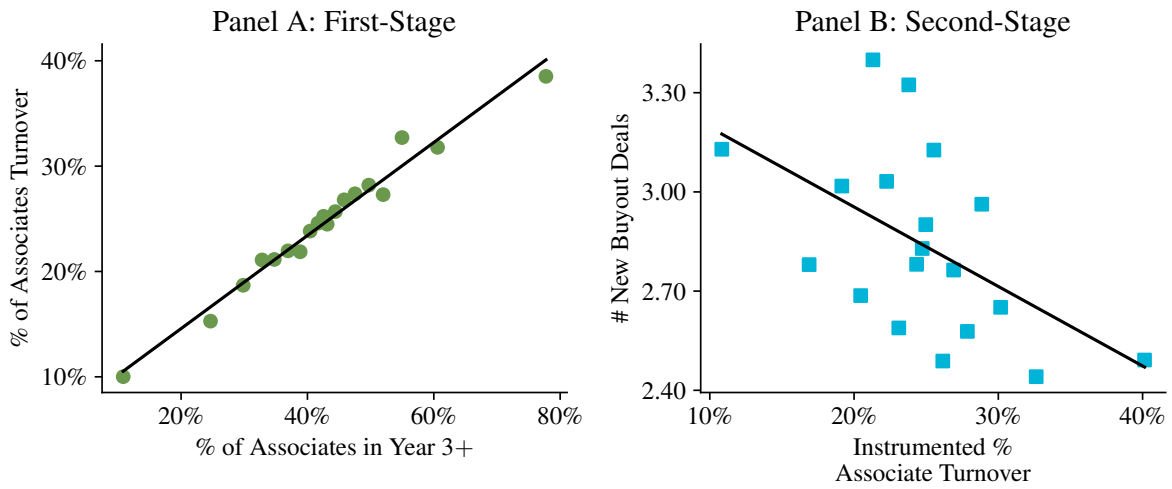


Figure 7: Displays binscatter regressions of the count of PE firm's buyout investments on the instrumented fraction of its associates that turnover each year. The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been observed with the firm for three or more *calendar* years as of year t, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. All regressions are weighted by the number of associates employed by firm j in year t. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with a firm. Deals only include stand-alone or platform buyouts of U.S. companies. Panel A displays the first stage of the IV regression: the relationship between associate tenure and associate turnover. Panel B displays the second stage: the relationship between the number of stand-alone or platform buyouts of U.S. companies a PE firm does and predicted associate turnover. Regressions are weighted by the number of associates employed by firm j in year t and include firm fixed effects, industry $\times$ year fixed effects, and state $\times$ year fixed effects. The State $\times$ Year FE and Industry $\times$ Year FE are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial Services $\times$ 2015). Modal industries and geographies refer to the most common industry or state (by PitchBook classifications) of a target company that the firm has acquired via a stand-alone or platform buyout over a rolling 10-year window, spanning, for each firm, $[t-10, t-1]$. Table 5 presents first-stage coefficient estimates. Table 6 presents second-stage coefficient estimates. Figure IA3 displays the results using the alternative *binsreg* method introduced by Cattaneo, Crump, Farrell, and Feng (2024) which adjusts the residualization and the number of bins.

Figure 8: Distributions of Cosine Similarities Between Partners' Deals and Associate Firm Deals

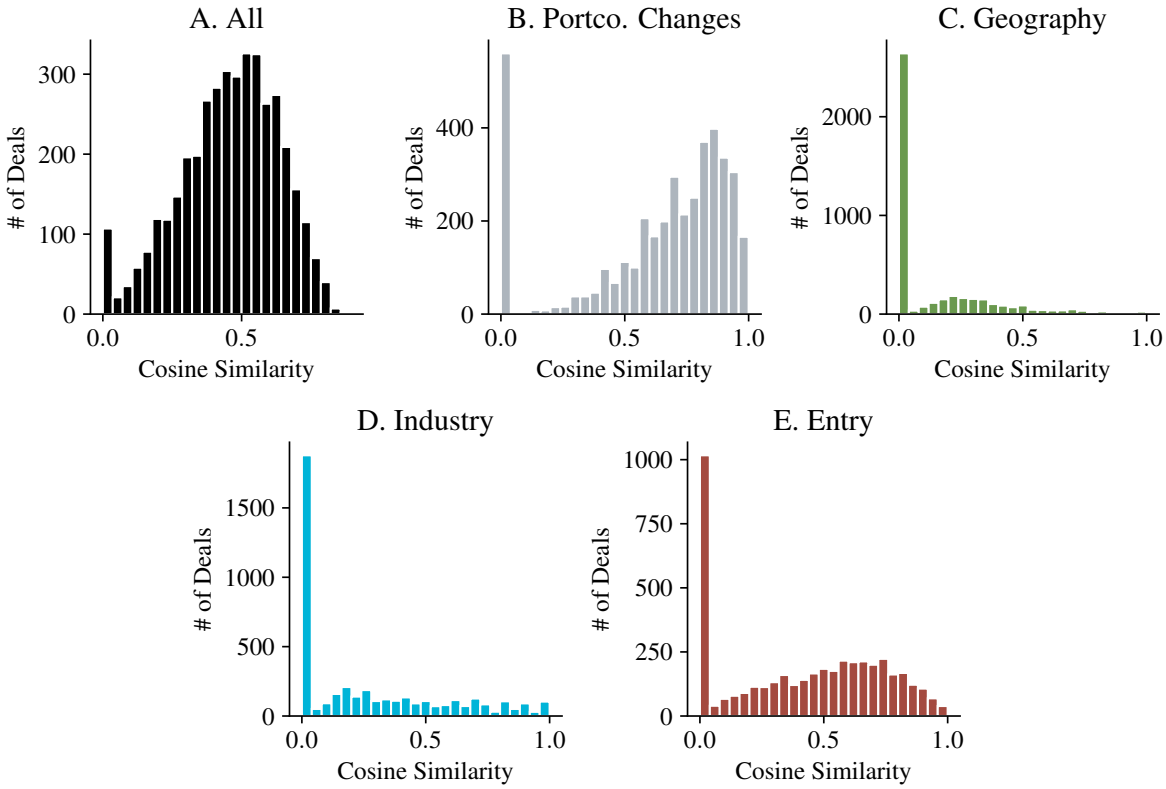


Figure 8: Displays histograms of cosine similarities between deals led by professional i and the deals completed by the firm professional i worked as an associate. The unit of observation is the lead partner × deal. Subplot titles indicate the characteristics used to calculate cosine similarities. "Portco. Changes" includes separate indicators for whether a CEO change, leveraged recapitalization, dividend recapitalization, general debt issuance, or add-on acquisition was observed in the five years following the deal. "Geography" and "Industry" each include indicators for each states/industries. "Entry" includes separate indicators for the type of deal (e.g., management buyout versus secondary buyout) and whether the professional's firm was the lead investor or also invested alongside other PE firms. "All" includes a vector of all indicators. Associate firm deal characteristics are averaged over the five years before the first deal led by professional i. For example, if the professional first led a deal for GTCR in 2011 and was an associate at Blackstone, the plot calculates the cosine similarity for each deal the professional led in 2011 and later to the average deal completed by Blackstone from 2006 to 2010. Deals are restricted to completed platform/stand-alone leveraged buyouts of U.S. companies from 1990 or later.

Figure 9: Cosine Similarity Over Careers

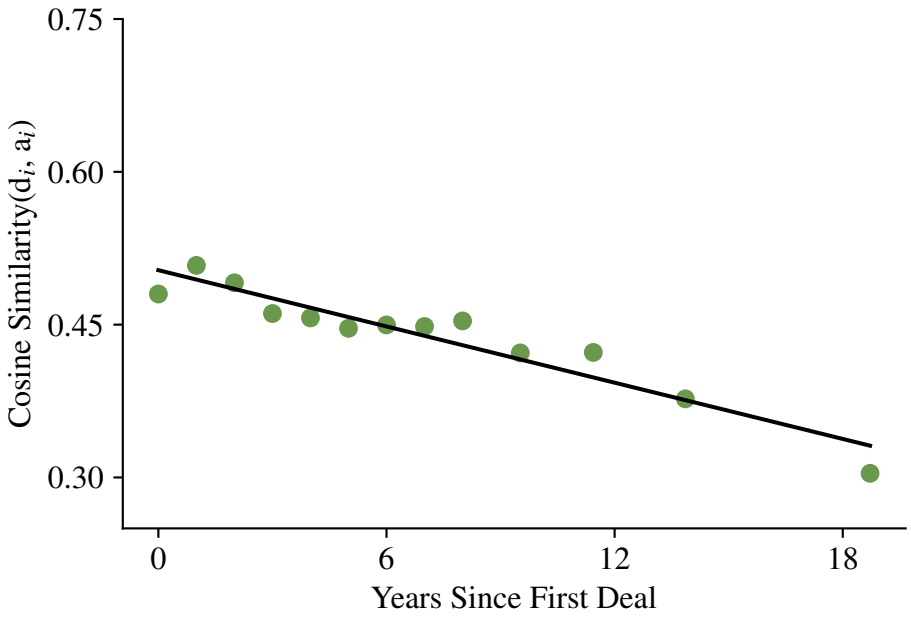


Figure 9: Plots the average cosine similarity scores between deals led by professional (d_i) and the deals completed by the firm professional (a_i) worked as an associate conditioned on how many years have elapsed since professional i led their first deal. The unit of observation is the lead professional $\times$ deal. Associate firm ($a(i)$) deal characteristics are averaged over the five years before the first deal led by professional i. For example, if the professional first led a deal for GTCR in 2011 and was an associate at Blackstone, the plot calculates the cosine similarity for each deal the professional led in 2011 and later to the average deal completed by Blackstone from 2006 to 2010. Deals are restricted to completed platform/stand-alone leveraged buyouts of U.S. companies from 1990 or later. Table 7 presents coefficient estimates that include additional fixed effects.

Table 1: Summary Statistics

	Nobs.	Avg.	Std.	25p	50p	75p
Panel A: PE Firms 2000 to 2022 (207 firms)						
# Years Per Firm	207	13.4	5.8	9	14	18
US Buyout AUM ($B)	2,667	$2.9	$6.4	$0.3	$0.9	$2.7
# US Buyout Funds < 15 yrs	2,779	3.5	3.1	2	3	4
# US Buyout Funds < 5 yrs	2,779	1.5	1.6	1	1	2
Avg. to-date PME (active funds)	1,853	1.4	0.57	1.1	1.31	1.61
# New Buyout Deals	2,779	2	1.9	1	2	3
# Active Portcos.	2,779	13	10.1	7	11	17
# Exits	2,740	1.4	1.8	0	1	2
Panel B: PE Firm Investment Teams 2000 to 2022 (10,546 professionals)						
# Bottom-Level Professionals	2,711	6.5	9	1	4	8
# Middle-Level Professionals	2,711	5.4	9	1	3	6
# Top-Level Professionals	2,711	6.3	5.3	3	5	8
# Investment Professionals	2,711	18.2	19.9	7	12	22
AUM per Inv. Pro. ($B)	2,606	$0.12	$0.19	$0.04	$0.08	$0.15
AUM per Partner ($B)	2,586	$0.38	$0.63	$0.09	$0.21	$0.45
B.1 Bottom-Level Teams (6,693 professionals)						
% Female	2,093	10.9%	17.8%	0.0%	0.0%	18.2%
% First-Year	2,244	26.2%	25.8%	0.0%	25.0%	39.0%
% Promotion-Year	2,244	9.2%	18.9%	0.0%	0.0%	11.1%
% Separation-Year	2,244	14.9%	19.7%	0.0%	7.7%	25.0%
% Up-or-Out	2,244	24.1%	25.6%	0.0%	20.0%	37.5%
% w/ H/S/W Degree	2,244	17.6%	22.4%	0.0%	11.1%	29.2%
% w/ IB. Exp.	2,206	81.3%	26.9%	71.4%	94.4%	100.0%
% w/ Con. Exp.	2,206	14.8%	23.8%	0.0%	0.0%	22.9%
B.2 Middle-Level Teams (3,950 professionals)						
% Female	2,098	7.7%	15.3%	0.0%	0.0%	10.4%
% Promoted From Associate	2,205	42.4%	36.5%	0.0%	41.7%	66.7%
% First-Year	2,283	5.8%	15.0%	0.0%	0.0%	0.0%
% Promotion-Year	2,283	8.4%	20.4%	0.0%	0.0%	0.0%
% Separation Year	2,283	7.2%	16.1%	0.0%	0.0%	7.7%
% Up-or-Out	2,283	15.6%	25.2%	0.0%	0.0%	25.0%
% w/ H/S/W Degree	2,283	39.2%	33.6%	0.0%	34.8%	63.6%
% w/ MBA Degree	2,268	55.7%	34.4%	33.3%	57.1%	85.7%
% w/ IB. Exp.	2,258	72.6%	30.1%	50.0%	80.0%	100.0%
% w/ Con. Exp.	2,258	22.0%	26.7%	0.0%	14.3%	34.4%
B.3 Top-Level Teams (2,601 professionals)						
% Female	2,381	4.1%	13.3%	0.0%	0.0%	0.0%
% Founders	2,685	31.2%	30.3%	0.0%	25.0%	50.0%
% Promoted From Middle-Level	2,624	15.4%	22.9%	0.0%	0.0%	28.6%
% Promoted From Associate	2,624	11.6%	21.6%	0.0%	0.0%	20.0%
% First-Year	2,685	1.6%	7.6%	0.0%	0.0%	0.0%
% Separation-Year	2,685	4.0%	10.4%	0.0%	0.0%	0.0%
% w/ H/S/W Degree	2,685	46.5%	30.9%	22.2%	50.0%	66.7%
% w/ MBA Degree	2,680	63.2%	28.2%	50.0%	66.7%	83.3%
% w/ IB. Exp.	2,631	61.5%	29.3%	47.6%	66.7%	83.3%
% w/ Con. Exp.	2,631	29.1%	27.9%	0.0%	25.0%	50.0%

Table 1: Panel A tabulates the distribution of firm size, performance, and investing activity across firm-year cells. Panel B tabulates characteristics of PE firm investment team compositions (by seniority) across firm-year cells. Promotion statistics refer only to within-firm promotions.

Table 2: Associate Program Length and Outcomes

	All	Left or Promoted	≥ 10 Yrs.	≥ 15 Yrs.
# Firms	177	161	142	102
# Associate Cohorts	1,310	1,037	643	326
# Associates	3,287	2,003	1,216	557
Panel A: Recruitment and Length				
𝟙(From Feeder School)	34.9%	36.6%	38.4%	42.8%
𝟙(From Feeder Firm)	32.4%	33.6%	34.4%	39.3%
𝟙(From Feeder Firm & School)	11.1%	11.7%	11.8%	17.0%
Avg. Age	25.1	25.1	25.1	25.4
Avg. Start Year	2014.7	2012.6	2009.4	2006
Avg. Time as Associate (Yrs.)	1.7	1.9	2.1	2.2
> Modal Length	24.8%	30.2%	34.7%	38.0%
= Modal Length	48.3%	50.4%	50.0%	47.2%
< Modal Length	27.0%	19.4%	15.3%	14.8%
Panel B: MBA Degrees				
𝟙(Join as Post-MBA Associate)	15.9%	18.4%	22.1%	23.5%
𝟙(Obtain MBA Degree)	39.7%	46.4%	55.7%	60.4%
𝟙(H/S/W MBA | Obtained MBA)	53.5%	56.1%	58.8%	53.7%
Panel C: PE Career Progression				
Same Firm:				
𝟙(→ Middle-level)	23.9%	39.2%	37.1%	40.5%
𝟙(→ Top-level)	5.8%	9.4%	15.1%	22.6%
𝟙(→ Lead Platform U.S. Buyout)	3.4%	5.6%	9.1%	15.2%
Any U.S. Buyout Firm:				
𝟙(→ Middle-level)	36.4%	59.6%	58.5%	58.1%
𝟙(→ Top-level)	8.6%	14.1%	22.5%	33.0%
𝟙(→ Lead Platform U.S. Buyout)	5.0%	8.2%	13.4%	21.7%
Panel D: Subsequent Non-PE Experience				
𝟙(Subsequent Non-PE Exp.)	31.6%	51.7%	55.7%	57.6%
𝟙(→ Law | Left PE)	-	0.3%	0.3%	0.6%
𝟙(→ Consulting | Left PE)	-	5.6%	6.3%	6.8%
𝟙(→ VC | Left PE)	-	7.0%	6.0%	6.2%
𝟙(→ Hedge Fund | Left PE)	-	7.3%	7.6%	6.5%
𝟙(→ Banking | Left PE)	-	9.8%	10.4%	14.2%
𝟙(→ Finance Firm | Left PE)	-	12.3%	13.7%	13.6%
𝟙(→ Other Firm | Left PE)	-	73.5%	75.9%	76.2%

Table 2: Tabulates characteristics of buyout associates and subsequent career progressions conditioned on (a) nothing additional; (b) whether the associate had finished their associate program ("Left or Promoted"), (c) joined their associate firm in 2014 or earlier ("≥ 10 Yrs."), or (d) joined their associate firm in 2009 or earlier ("≥ 15 Yrs."). "Feeder" firms and schools are the three most popular prior schools or banking/consulting firms among other professionals the year the associate joined. "Modal Length" refers to the modal time that professionals of PE firm j spent as associates at a PE firm j. Samples are restricted to professionals with the exact title of "Associate" according to archived websites that spent no more than six years as a bottom-level professional with their firms (attempting to exclude analysts and senior associates). The sample also requires the professional is matched to a Revelio profile. Outcomes are calculated as of data recorded in August 2024.

Table 3: Associate Program Effects on Partners' Subsequent Value Add, Returns, and Investing Style

			% of R^2 Attributable to Fixed Effects							
	Y-Var.	R^2	Associate Firm	p-value	Bank/Consultancy	School	Deal Year	Partner Firm	Y-mean	N.
	Panel A: Gross Deal Returns									
(1)	Value Add ($MM)	36.4%	32.7%	<.01	8.6%	8.2%	6.3%	44.2%	340.73	1,175
(2)	$MM Invested	38.6%	24.0%	<.01	6.5%	6.5%	4.9%	58.1%	222.98	2,163
(3)	Gross TVM	16.7%	30.1%	<.01	8.6%	19.6%	5.3%	36.5%	3.15	1,175
(4)	𝟙(Gross TVM ≥ 5x)	29.6%	33.0%	<.01	9.1%	12.8%	6.4%	38.8%	0.14	1,175
(5)	𝟙(1x < Gross TVM < 5x)	11.6%	33.0%	<.01	8.3%	11.1%	8.0%	39.6%	0.67	1,175
(6)	𝟙(0x < Gross TVM ≤ 1x)	31.4%	31.4%	<.01	9.7%	13.4%	10.7%	35.0%	0.15	1,175
(7)	𝟙(Gross TVM = 0x)	34.2%	31.2%	<.01	7.2%	19.6%	4.9%	37.0%	0.03	1,175
	Panel B: Deal Characteristics									
	Entry:									
(8)	𝟙(# Investors >1)	16.2%	32.4%	<.01	7.9%	8.0%	5.5%	46.2%	0.4	3,706
(9)	𝟙(Lead Investor)	23.0%	22.2%	<.01	5.6%	5.8%	42.3%	24.1%	0.31	3,706
(10)	𝟙(Secondary-LBO)	21.0%	34.3%	<.01	8.1%	9.2%	7.7%	40.7%	0.33	3,706
(11)	𝟙(MBO)	26.1%	32.1%	<.01	8.1%	7.6%	6.2%	45.9%	0.16	3,706
(12)	𝟙(Corp. Divestiture)	19.6%	35.5%	<.01	6.4%	8.9%	4.3%	44.9%	0.09	3,706
(13)	𝟙(Recapitalization)	29.3%	30.4%	<.01	6.2%	7.5%	1.7%	54.2%	0.13	3,706
(14)	𝟙(Take-Private)	26.3%	25.1%	> .10	7.8%	12.0%	7.1%	48.0%	0.05	3,706
	Industry (Top-5):									
(15)	𝟙(Commcl. Services)	23.1%	33.8%	<.01	7.5%	13.2%	4.0%	41.5%	0.21	3,706
(16)	𝟙(Commcl. Products)	31.6%	35.4%	<.01	8.6%	10.3%	2.5%	43.2%	0.13	3,706
(17)	𝟙(Software)	40.2%	33.1%	<.01	8.1%	7.1%	1.9%	49.8%	0.14	3,706
(18)	𝟙(Healthcare Services)	37.3%	31.6%	<.01	9.3%	8.7%	2.2%	48.2%	0.09	3,706
(19)	𝟙(Consumer Non-Durables)	35.5%	29.6%	<.01	8.6%	10.8%	2.5%	48.4%	0.04	3,706
	State (Top-5):									
(20)	𝟙(California)	20.2%	36.5%	<.01	6.7%	11.5%	3.6%	41.8%	0.11	3,706
(21)	𝟙(Texas)	21.0%	34.7%	<.01	8.4%	10.0%	2.8%	44.1%	0.12	3,706
(22)	𝟙(Florida)	20.8%	35.1%	<.01	7.0%	14.9%	3.1%	39.9%	0.06	3,706
(23)	𝟙(Illinois)	21.2%	36.0%	<.01	7.5%	10.2%	2.5%	43.9%	0.06	3,706
(24)	𝟙(New York)	21.9%	41.9%	<.01	8.2%	11.4%	2.8%	35.7%	0.05	3,706
	Panel C: Portfolio Company Changes									
(25)	𝟙(CEO Change Year 0)	22.8%	22.1%	<.01	5.8%	7.2%	37.3%	27.6%	0.29	3,442
(26)	𝟙(CEO Change)	9.6%	20.0%	<.01	4.7%	6.4%	46.0%	22.9%	0.78	3,542
(27)	𝟙(Make Add-on Acq.)	11.8%	31.6%	<.01	6.8%	9.3%	16.7%	35.5%	0.62	3,617
(28)	𝟙(Debt Issuance)	20.3%	31.9%	<.01	5.5%	9.1%	15.8%	37.7%	0.38	3,617
(29)	𝟙(Dividend Recap.)	22.8%	30.8%	<.01	7.6%	9.5%	11.0%	41.0%	0.09	3,617
(30)	𝟙(Leveraged Recap.)	20.4%	34.9%	<.01	10.0%	12.7%	5.4%	37.0%	0.03	3,617

Table 3: Tabulates the fraction of explained variation in buyout deal outcomes attributable to a set of fixed effects based on Shapley value decompositions. Outcomes include deal returns (Panel A), deal characteristics (Panel B), and operating and financial changes at the portfolio company following the LBO (Panel C). The p-value column reports the p-value obtained from F-tests for the joint significance of associate program fixed effects on each outcome. Each row represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. The PE firm associate program attended by the lead professional must be available. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted. Standard errors are clustered at the deal level.

Table 4: Associate Program Effects in Earlier Versus Later Deals

		I. Early Deals (First and Second)				II. Later Deals (Third+)			
	Y-Var.	R^2	% Associate Firm	Y-mean	N.	R^2	% Associate Firm	Y-mean	N.
	Panel A: Gross Deal Returns								
(1)	Value Add ($MM)	45.5%	29.2%	309.57	719	49.6%	30.2%	420.26	435
(2)	$MM Invested	45.0%	33.7%	198.56	1,228	47.8%	28.2%	257.26	936
(3)	Gross TVM	25.8%	35.8%	3.19	719	25.1%	26.4%	3.04	435
(4)	𝟙(Gross TVM ≥ 5x)	45.9%	38.5%	0.14	719	46.1%	36.7%	0.13	435
(5)	𝟙(1x < Gross TVM < 5x)	18.2%	40.3%	0.67	719	14.3%	36.0%	0.69	435
(6)	𝟙(0x < Gross TVM ≤ 1x)	49.8%	39.5%	0.15	719	40.4%	32.4%	0.14	435
(7)	𝟙(Gross TVM = 0x)	53.9%	39.3%	0.04	719	43.5%	32.4%	0.03	435
	Panel B: Deal Characteristics								
	Entry:								
(8)	𝟙(# Investors >1)	26.0%	38.0%	0.41	2,117	23.3%	34.9%	0.38	1,607
(9)	𝟙(Lead Investor)	31.8%	30.9%	0.33	2,117	28.1%	26.2%	0.29	1,607
(10)	𝟙(Secondary-LBO)	31.8%	40.5%	0.31	2,117	26.6%	34.7%	0.35	1,607
(11)	𝟙(MBO)	40.8%	36.9%	0.16	2,117	32.0%	32.4%	0.16	1,607
(12)	𝟙(Corp. Divestiture)	34.3%	38.7%	0.09	2,117	26.1%	35.1%	0.09	1,607
(13)	𝟙(Recapitalization)	42.7%	39.5%	0.13	2,117	36.5%	30.2%	0.14	1,607
(14)	𝟙(Take-Private)	36.1%	31.7%	0.05	2,117	37.9%	24.1%	0.05	1,607
	Industry (Top-5):								
(15)	𝟙(Commcl. Services)	37.2%	39.7%	0.2	2,117	31.3%	35.3%	0.21	1,607
(16)	𝟙(Commcl. Products)	43.9%	37.3%	0.13	2,117	41.3%	33.3%	0.13	1,607
(17)	𝟙(Software)	46.4%	39.4%	0.12	2,117	51.7%	34.3%	0.16	1,607
(18)	𝟙(Healthcare Services)	51.6%	34.4%	0.09	2,117	43.2%	37.0%	0.09	1,607
(19)	𝟙(Consumer Non-Durables)	48.7%	36.5%	0.05	2,117	46.2%	32.7%	0.03	1,607
	State (Top-5):								
(20)	𝟙(California)	35.5%	43.5%	0.11	2,117	30.4%	35.9%	0.11	1,607
(21)	𝟙(Texas)	34.0%	37.1%	0.13	2,117	27.3%	34.0%	0.11	1,607
(22)	𝟙(Florida)	37.0%	35.0%	0.06	2,117	27.5%	38.5%	0.06	1,607
(23)	𝟙(Illinois)	38.2%	44.0%	0.06	2,117	30.2%	34.2%	0.07	1,607
(24)	𝟙(New York)	38.7%	46.4%	0.06	2,117	31.8%	36.4%	0.05	1,607
	Panel C: Portfolio Company Changes								
(25)	𝟙(CEO Change Year 0)	32.5%	33.7%	0.29	1,943	31.0%	26.0%	0.29	1,512
(26)	𝟙(CEO Change)	13.0%	29.5%	0.75	2,016	10.0%	22.2%	0.81	1,540
(27)	𝟙(Make Add-on Acq.)	18.7%	37.4%	0.61	2,069	14.8%	33.2%	0.63	1,561
(28)	𝟙(Debt Issuance)	29.9%	37.4%	0.36	2,069	25.4%	35.5%	0.39	1,561
(29)	𝟙(Dividend Recap.)	34.7%	36.7%	0.09	2,069	33.2%	32.9%	0.1	1,561
(30)	𝟙(Leveraged Recap.)	38.2%	43.1%	0.03	2,069	23.3%	33.2%	0.03	1,561

Table 4: Tabulates the fraction of explained variation in buyout deal outcomes attributable to a set of fixed effects based on Shapley value decompositions after splitting the sample to only include a professionals early deals or their later deals. The estimating regression is Equation (2). Only the R^2 attributed to the associate program fixed effects is displayed but all fixed effects in Equation (2) are included in estimation. Outcomes include deal returns (Panel A), deal characteristics (Panel B), and operating and financial changes at the portfolio company following the LBO (Panel C). Each row in each subset of deals (Early or Later) represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted.

Table 5: Instrument First Stage: Determinants of Associate Turnover

	% of Associates Turnover				
	(1)	(2)	(3)	(4)	(5)
% of Associates in Year 3+	0.436***	0.461***	0.484***	0.519***	0.582***
	(12.75)	(6.74)	(6.89)	(6.11)	(5.19)
% of Associates in Year 3+ × S&P Return		-0.137	-0.156	-0.242*	-0.093
		(-1.43)	(-1.60)	(-2.05)	(-0.73)
% of Associates in Year 3+ × US HY Spread (bps)		-0.002	-0.007	-0.010	-0.020
		(-0.24)	(-0.69)	(-0.77)	(-1.17)
# LBOs Completed$_{t-3}$			0.003	0.002	0.002
			(1.55)	(0.88)	(0.64)
# LBOs Completed$_{t+1}$			0.001	0.001	0.001
			(0.44)	(0.29)	(0.21)
% Associates w/ Ivy Degree				0.023	0.017
				(0.49)	(0.38)
% Associates w/ H/S/W Degree				0.029	0.037
				(0.44)	(0.56)
% Associates w/ IB. Exp.				-0.008	-0.000
				(-0.14)	(-0.01)
% Associates w/ Con. Exp.				-0.030	-0.017
				(-0.54)	(-0.26)
# First-Year Associates				0.002	0.002
				(1.35)	(1.20)
# First-Year Associates$_{t-3}$				0.006**	0.006**
				(2.20)	(2.15)
# First-Year Associates$_{t-5}$				-0.001	-0.001
				(-0.57)	(-0.88)
log(AUM $B)					0.001
					(0.07)
Avg. to-date PME (active funds)					0.013
					(1.06)
Y-mean	0.241	0.247	0.248	0.246	0.247
Y-Std. Dev.	0.252	0.181	0.180	0.169	0.161
Firm FE	x	x	x	x	x
Industry × Year FE	x	x	x	x	x
State × Year FE	x	x	x	x	x
Adj R^2	0.223	0.261	0.257	0.273	0.291
N	1,923	1,923	1,852	1,380	1,059

Table 5: Tabulates coefficients estimated from the first stage of a 2SLS regression, where associate turnover is instrumented with the fraction of firm *j*'s associates that have been observed with the firm for three or more *calendar* years as of year *t*, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. Associates turnover in year *t* if they are separated from firm *j* by year *t* + 1 or are promoted to the Vice-President seniority or higher within firm *j* by the next year they are observed (often following a temporary separation to pursue an MBA). The unit of observation is the firm-year (*jt*). All regressions are weighted by the number of associates employed by firm *j* in year *t*. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with firm *j*. The State × Year FE and Industry × Year FE are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial services × 2015). Standard errors are independently double-clustered at the firm and year levels. *t*-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table 6: Instrumented Associate Turnover and Buyout Activity

	# LBOs Completed				
Buyout Type:	All	Modal Ind.	Not Modal Ind.	Modal Geo.	Not Modal Geo.
	(1)	(2)	(3)	(4)	(5)
% $\widehat{\text{Associate}}$ Turnover	-2.230***	-0.660	-1.570***	-0.304	-1.926***
	(-3.73)	(-1.57)	(-3.45)	(-0.89)	(-3.89)
Y-mean	2.909	0.953	1.956	0.437	2.472
Y-Std. Dev.	2.463	1.625	1.768	0.727	2.219
Firm FE	x	x	x	x	x
Industry × Year FE	x	x	x	x	x
State × Year FE	x	x	x	x	x
Kleibergen-Paap F-stat.			198.706		
N	1,923	1,923	1,923	1,923	1,923

Table 6: Tabulates 2SLS regressions of the count of PE firm's buyout investments on the instrumented fraction of its associates that turnover each year. The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been observed with the firm for three or more *calendar* years as of year t, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. All regressions are weighted by the number of associates employed by firm j in year t. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with a firm. Deals only include stand-alone or platform buyouts of U.S. companies. The State × Year FE and Industry × Year FE are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial Services × 2015). Modal industries and geographies refer to the most common industry or state (by PitchBook classifications) of a target company that the firm has acquired via a stand-alone or platform buyout over a rolling 10-year window, spanning, for each firm, $[t-10, t-1]$. Standard errors are independently double-clustered at the firm and year levels. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table 7: Cosine Similarity Between Professionals' Deals and Associate Firm Deals

	Cosine Similarity				
Deal Characteristics:	All	Portco. Changes	Geography	Industry	Entry
	(1)	(2)	(3)	(4)	(5)
Years Since First Deal	-0.012***	-0.009***	-0.003***	-0.006***	-0.005***
	(-11.22)	(-4.21)	(-2.92)	(-3.73)	(-2.60)
𝟙(Associate Firm Also Invested)	0.027**	0.028	0.038**	-0.010	0.054**
	(2.12)	(1.52)	(2.20)	(-0.37)	(2.46)
Y-mean	0.465	0.644	0.142	0.260	0.430
Y-Std. Dev.	0.176	0.296	0.218	0.300	0.305
Partner Firm FE	x	x	x	x	x
Deal Year FE	x	x	x	x	x
Associate Firm FE	x	x	x	x	x
IB/Con FE	x	x	x	x	x
School FE	x	x	x	x	x
Adj R^2	0.343	0.336	0.043	0.217	0.158
N	3,046	3,046	3,299	3,299	3,299

Table 7: Tabulates regressions of the cosine similarities between deals led by professional i and the deals completed by the firm professional i worked as an associate. The unit of observation is the lead partner $\times$ deal. Column titles indicate the characteristics used to calculate cosine similarities. "Portco. Changes" includes separate indicators for whether a CEO change, leveraged recapitalization, dividend recapitalization, general debt issuance, or add-on acquisition was observed in the five years following the deal. "Geography" and "Industry" each include indicators for each states/industries. "Entry" includes separate indicators for the type of deal (e.g., management buyout versus secondary buyout) and whether the professional's firm was the lead investor or also invested alongside other PE firms. "All" includes a vector of all indicators. Associate firm deal characteristics are averaged over the five years before the first deal led by professional i. For example, if the professional first led a deal for GTCR in 2011 and was an associate at Blackstone, the plot calculates the cosine similarity for each deal the professional led in 2011 and later to the average deal completed by Blackstone from 2006 to 2010. Deals are restricted to platform/stand-alone leveraged buyouts of U.S. companies completed by U.S. buyout firms in 1990 or later. "IB/Con FE" are fixed effects for the investment bank or consulting firm the associate previously worked at. "School FE" are fixed effects for the U.S. college where the associate matriculated. Standard errors are clustered at the associate firm level. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table 8: Subsequent Cosine Similarity and Associate Firm Partner Departures During Associate Experience

	Cosine Similarity (All)	
	(1)	(2)
𝟙(Active Q4 Partner Left)	-0.089***	
	(-3.08)	
𝟙(Active Q1 Partner Left)	0.052	
	(1.31)	
𝟙(Any Q4 Partner Left)		-0.041**
		(-2.19)
𝟙(Any Q1 Partner Left)		0.055***
		(2.83)
Years Since First Deal	-0.008***	-0.007***
	(-3.75)	(-3.09)
𝟙(Associate Firm Also Invested)	-0.021	-0.018
	(-0.83)	(-0.70)
Y-mean	0.504	0.504
Y-Std. Dev.	0.162	0.162
Partner Firm FE	x	x
Deal Year FE	x	x
Associate Firm FE	x	x
IB/Con FE	x	x
School FE	x	x
Adj R^2	0.262	0.261
N	1,016	1,016

Table 8: Tabulates regressions of the cosine similarity between a buyout professionals' deals and their associate firm's deals on whether top or bottom-performing partners separated or retired while they worked as an associate at their associate firm. The unit of observation is the lead partner × deal. "Q4" partners were *ever* listed as the lead partner on an exited platform/stand-alone buyout of a U.S. company that generated a top-quartile gross total value multiple compared to other deals undertaken in the same year. "Q1" *ever* led a deal that performed in the bottom quartile of all deals undertaken in the same year. Partners can be listed in more than one quartile. "Active" partners led a deal for professional *j*'s associate firm during the five years before the first deal led by partner *j* first led a deal. "Any" also includes partners that did not lead a deal for professional *j*'s associate firm during the five years before the first deal led by partner *j* first led a deal. Deals are restricted to completed platform/stand-alone leveraged buyouts of U.S. companies from 1990 or later. Regression samples require data on associate firm partner turnover during professional *j*'s estimated associate tenure. "IB/Con FE" are fixed effects for the investment bank or consulting firm the lead professional previously worked at. "School FE" are fixed effects for the U.S. college where the professional matriculated. Standard errors are clustered at the associate firm level. Standard errors are clustered at the associate firm level. *t*-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels. Standard errors are clustered at the associate firm level. *t*-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table 9: Subsequent Cosine Similarity and the Liquidity of PE Partnership Shares

	Cosine Similarity (All)		
	(1)	(2)	(3)
𝟙(Associate Cohort After Liq. Event) × Associate Firm AUM ($10B)	-0.141**	-0.098***	-0.040**
	(-2.00)	(-3.57)	(-2.06)
𝟙(Associate Cohort After Liq. Event)	0.164***	0.129***	0.026
	(6.86)	(7.85)	(0.74)
Associate Firm AUM ($10B)	0.163**	0.120***	0.064***
	(2.37)	(4.34)	(5.60)
𝟙(Associate Firm Also Invested)	0.006	0.014	0.006
	(0.40)	(0.92)	(0.40)
Person Deal Number	-0.007***	-0.006**	-0.012***
	(-2.68)	(-2.32)	(-4.24)
Partnership Liquidity Event	BX IPO	GP Stakes 2.0	Firm Sales
Y-mean	0.469	0.469	0.469
Y-Std. Dev.	0.173	0.173	0.173
Partner Firm FE	x	x	x
Deal Year FE	x	x	x
Associate Firm FE	x	x	x
IB/Con FE	x	x	x
School FE	x	x	x
Adj R^2	0.316	0.312	0.293
N	2,801	2,801	2,801

Table 9: Tabulates regressions of the cosine similarity between a buyout professionals' deals and their associate firm's deals on whether the buyout professional (a) began leading deals after the liquidity of PE partnership shares increased and (b) was an associate at larger PE firm that was more exposed to changes in the liquidity of PE partnership shares. The liquidity events in Column (1) and (2) are industry-specific; Column (1) sorts cohorts based on whether they were associates after The Blackstone Group's IPO (2007). Column (2) sorts cohorts based on whether they were associates after the first "institutional" GP Stake sale (2010). The liquidity event in Column (3) is specific to the associate firm, sorting professionals based on whether they were associates of their associate firm after the year when their associate firm first sold a GP stake or completed an IPO (if at all). Associate firm AUM is averaged over the five years before professional i first led a buyout deal. PE firm GP stake sale and IPO timing data are from Gahng and Jackson (2023). The unit of observation is the lead partner × deal. Deals are restricted to completed platform/stand-alone leveraged buyouts of U.S. companies from 1990 or later. Regression samples require data on associate firm partner turnover during professional j's estimated associate tenure. "IB/Con FE" are fixed effects for the investment bank or consulting firm the lead professional previously worked at. "School FE" are fixed effects for the U.S. college where the professional matriculated. Standard errors are clustered at the associate firm level. Standard errors are clustered at the associate firm level. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels. Standard errors are clustered at the associate firm level. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Internet Appendix for: The Long-Lasting Effects of Investment Apprenticeships

March 17, 2025

IA.1 PE Firm Investment Professional Database Construction

This section details the steps taken to compile a database of private equity firm investment professionals. The resulting sample includes approximately 10,500 unique PE investment professionals who worked for 207 PE firms between 2000 and 2022. I follow four main steps to construct this database:

1. **Section IA.1.1:** Construct a sample of private equity firms.
2. **Section IA.1.2:** For each firm, identify the historical webpages of the firm containing investment professionals for the years 2000 to 2022, where available.
3. **Section IA.1.3:** In each firm-year observation, parse the resulting HyperText Markup Language (HTML) files to (a) identify individual investment professionals and (b) extract pertinent information including their names, titles, education, work experience, employment start years, employment end years, and gender.
4. **Section IA.1.4:** Standardize and reconcile individuals' titles and other biographical information.

IA.1.1 Sample of Buyout Firms

The starting sample of institutional U.S. buyout firms from which I attempt to collect website data includes all investor firms covered in Preqin and PitchBook that satisfy any of the following criteria:

(A.) The firm sponsored at least two closed or liquidated North American buyout funds that raised at least $50 million in capital commitments from LPs in *either* Preqin or PitchBook.

(B.) The firm sponsored at least one closed or liquidated North American buyout fund that raised at least $50 million in capital commitments from LPs in *both* PitchBook and Preqin.

(C.) The firm sponsored at least one closed or liquidated North American buyout fund that raised at least $50 million in capital commitments from LPs in *either* PitchBook or Preqin while also having been the lead investor for at least one completed platform or primary buyout of a U.S. company in the PitchBook dataset.

From this resulting set of buyout firms, I further remove firms that are captive arms of a parent company, non-U.S. PE firms, a handful of firms that could not be matched across datasets, and non-PE firm types (firms listed in the commercial datasets as, for example, investment banks, fund of funds managers, Real Estate firms, or PE-backed companies). I retain venture capital firms that have been the lead investor for at least five completed platform or primary buyout of U.S. companies in the PitchBook dataset. I also retain Asset Managers that have sponsored at least five closed or liquidated Buyout funds focused in North America that raised at least $50 million in capital commitments from LPs in *both* Preqin and PitchBook. 792 U.S. buyout firms satisfy these criteria, each of which I manually match across datasets. These criteria result in the near universe of institutional U.S. buyout firms.

From this list of 792 total U.S. buyout firms, I select 219 firms to attempt to collect investment professionals using archived website data. Selection of these 219 firms is random among firms that sponsored funds *within* each fund vintage from 1995 to 2020, which favors larger firms that have raised funds across more years. I am successful for 207 of these 219 firms. The high success rate indicates that the presence of a webpage reporting investment professionals is unlikely to represent a material selection concern. Of the 12 firms in the list of 219 potential firms but not in the final sample of 207 firms, six are not in the final sample because they did not report investment team members on their websites throughout the entire sample period.

The remaining six firms in the list of 219 but not in the final sample are publicly traded U.S. firms (Apollo, Ares, Blackstone, Carlyle, KKR, and TPG). The publicly traded PE firms intermittently report senior investment professionals on their archived webpages. However, I exclude them from the sample because their team structures are significantly more complex compared to those of other PE firms and "pilot" scrapes for their websites exhibited substantial variation in completeness. Accordingly, an important caveat in the resulting sample of firms is that it omits some of the largest multi-asset class PE firms. Nonetheless, these firms' employment data are available in the Revelio dataset, which can produce similar results.

IA.1.1.1 Benchmarking the Sample of PE Firms

This subsection benchmarks reports the coverage of the 207 PE firms with collected website compared to the Preqin universe of U.S. buyout firms. For the benchmarking panel, I include all firms

in a given year t that have raised at least one U.S. buyout fund within a rolling 15-year window (a generous definition of "active") in a given year. Panel A of Figure IA.B1 shows that in a given year, firms in the sample represent about 30 to 40% of all active U.S. buyout firms and are managing between 50 and 60% of all active U.S. buyout funds based on Preqin data.

Figure IA.B1: Active Fund, Firm, AUM, and Fundraising Coverage

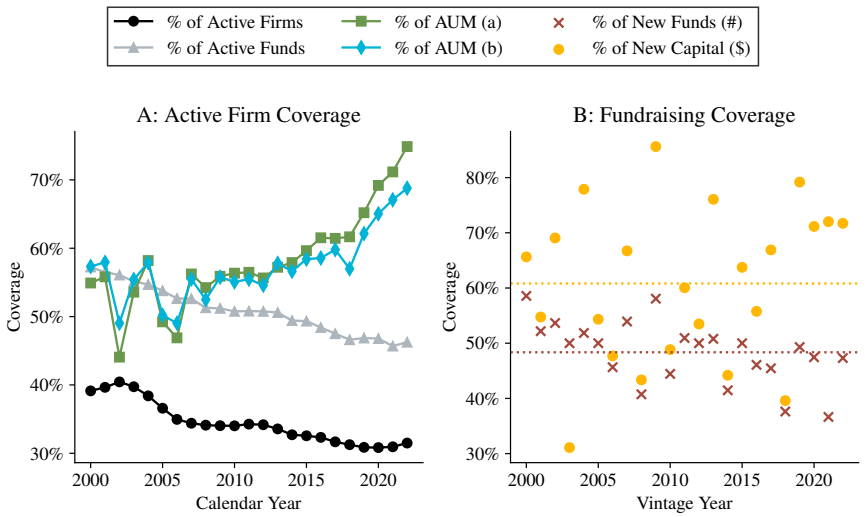


Figure IA.B1: Plots the coverage of the firms in the sample compared to U.S. buyout firms covered in Preqin data. In-sample firm-year cells are only those included in the sample of 207 firms with collected website data. AUM (a) is calculated using underlying performance data. AUM (b) is calculated using underlying performance data and imputations for funds with missing performance data. The panels are unbalanced. Dotted lines in Panel B report the time-series averages.

For the time-series presented in Panel A, I also use underlying fund-quarter level performance data to construct measures of a firm's Assets Under Management (AUM) in a given year. The AUM of firm j in year t equals the sum of year-end NAVs and uncalled capital (where uncalled capital equals $\max\{(100\% - \text{Percent Called}) \times \text{ Fund Size}, 0\}$) for all U.S. buyout funds that firm j manages in year t. This measure of AUM, however, requires performance data (NAVs), which may bias the estimation given positive selection in performance coverage. (For fund f in year t, $\text{NAV}_{ft} = \text{RVPI}_{ft} \times \text{ Fund Size}_f \times \text{Percent Called}_{ft}$.) To account for this potential concern, I also construct an "imputed AUM" for U.S. buyout funds that are observed by Preqin but are missing quarterly NAV reports. For the imputed AUM, I assume that all active funds with missing quarterly performance data have quarterly performance given by average values in the same: Vintage × Fund Age × Region Focus × Status × Fund Size decile cell and then deflated by 20% (to account for

positive selection). For example, in 2018, not-yet-liquidated buyout funds from the 2014 vintage, in the top size decile, focusing on investing in North America, reported a percent called value of 82.9% and an RVPI value of 1.04, on average. Both terms are necessary to estimate NAVs. The imputed AUM in 2018 for active, top-decile (by size), 2014 vintage buyout funds that are *missing* percent called and RVPI values would therefore be calculated assuming the funds were 66.32% called and held assets worth 83.2% of paid-in-capital, respectively after applying this deflation factor. If some cells are empty, I gradually broaden the funds used to impute values (using only the vintage year in the broadest case). This imputation procedure is similar to methods used by Preqin to estimate aggregate AUM where they are missing data.

Panel A of Figure IA.B1 shows that in a given year, the firms in the sample represent an increasing fraction of AUM that can be calculated based on Preqin data, equaling about 55% in 2000 and nearly 75% in 2022 by either AUM measure. Panel B of Figure IA.B1 reports the fraction of fundraising for U.S. buyout funds covered by the firms with collected webpage data. For a given vintage from 2000 to 2022, the sample of 207 firms represents about 1/3 to 4/5 of capital raised for U.S. buyout funds and between 40 and 60% of new U.S. buyout funds raised in Preqin.

Taken together, the firms selected for the resulting sample appear to represent a sizeable fraction of the U.S. buyout universe. Higher dollar-weighted coverage than equal-weighted coverage implies that the sample generally covers larger firms that sponsor larger funds, despite the omission of the largest publicly traded U.S. PE firms.

IA.1.2 Identifying Historical Team Pages

For the resulting 207 firms, I use the Internet Archive's Application Programming Interface (API) to collect the HTML files containing data on investment professionals. The Internet Archive is a nonprofit digital library that retains and provides free access to, among other digital media, historical websites. These data are available starting in 1996 but are generally not available for firms in the sample until 2000.

Website paths and the location of investment professional webpages vary across firms and within firms. Accordingly, I manually identify a new webpage to access for each firm-year cell. For the PE firm Kohlberg & Company, for example, team members are displayed on:

- "http://www.kohlberg.com/profess.html" from 2001 to 2005.
- "http://www.kohlberg.com/professionals.html" from 2006 to 2010.
- "http://www.kohlberg.com/Team/Default.aspx" from 2011 to 2019.
- "https://www.kohlberg.com/team/" from 2020 to 2022.

The preceding Kohlberg & Company example pages include one of three main ways that firms have displayed their investment professionals, what I refer to as a "team page." The team pages list the name of the investment professional, often their titles, and occasionally additional biographical information. In addition, firms often have unique "personal pages." Personal pages display more detailed biographical information for individual employees. For 2011 to 2019, for example, Kohlberg & Company includes individual pages at all links prepended by the phrase "http://www.kohlberg.com/team/." In earlier years, a handful of firms supplement their team pages by listing investment professionals by name, title, and email on contact pages (e.g., GTCR from 2006 to 2010.) Where available, I collect all three types of pages. Doing so results in 944 unique HTML pages for Kohlberg & Company for 2001 to 2022. I then repeat this process manually for each of the 207 firms, resulting in about 73,000 unique HTML pages containing information on investment professionals.

If there is more than one webpage for a person available in a given year, I retain the earliest combination of archived webpages with the most information. For example, suppose that John Smith works for a firm with a team page archived only in January 2017 (listing only names and titles) but he has a personal page (listing his name, title, and biography) that was archived in February 2017, June 2017, and December 2017. The 2017 data used for John Smith would therefore be drawn from his February 2017 personal page. Alternatively, if the personal pages instead displayed names and biographies (but not titles) while the team page displayed names and titles (but not biographical information), the 2017 information for John Smith would include the name and biography from the February 2017 personal page and the title data from the January 2017 team page.

Since webpages are manually identified in each firm-year cell, I account not only for cases where the location of team data within a website change over time, but also for cases where

firm website names change over time (e.g., TSG Consumer Partners from "www.shansby.com" to "www.tsgconsumer.com").

Table IA.B1 summarizes the resulting webpage coverage for firm year cells spanning 2000 to 2022. The table assumes that firms are active if they raised at least one U.S. buyout fund in the window $[t-15, t]$, where t denotes a given calendar year. For about 2/3 of possible firm-year cells, my procedure results in at least one investment professional. Missing data arise primarily if the internet archive did not archive a webpage for the firm in a given year, as about 98% of all firm-year cells with a collected webpage also have data on the firm's investment professionals. Some firms elect to only report their most senior investment professionals on their websites. Subsequent sections further discuss firm hierarchies.

Table IA.B1: Webpage Availability

	All (N=4,024)	Collected Webpage (N=2,750)	Active (N=3,417)	Slowing (N=607)
𝟙(Has Bottom-Level)	55.5%	81.2%	60.0%	30.1%
𝟙(Has Middle-Level)	56.4%	82.6%	60.7%	32.3%
𝟙(Has Top-Level)	66.1%	96.7%	69.7%	45.5%
𝟙(Has Any-Level)	66.7%	97.6%	70.5%	45.5%
𝟙(Has All-Levels)	53.4%	78.2%	57.9%	28.2%
𝟙(Personnel Changes)	83.0%	88.2%	85.9%	65.7%

Table IA.B1: Tabulates the fraction of firm-year cells that have collected investment professional data meeting specific characteristics. Active firms have raised at least one fund in the window $[t-5, t]$, where t denotes a given calendar year. Slowing firms have not. A five year window corresponds to the typical length of a fund's contracted investment period. "N" denotes the number of firm-year cells.

One concern with webpage data is that firms may not frequently update their websites, meaning that investment professionals' affiliations may be stale. As one way to assess this concern, the last row of Table IA.B1 also reports the fraction of firm-year cells exhibiting at least one personnel change relative to the previous year (hiring or separation), constituting one type of website update. At least one change was recorded for about 83% of firm-year cells, indicating that websites are likely to be updated at least once per year. In addition, firm-year cells exhibiting no updates based on website data do not appear to be false negatives: they generally have gone five or more years without raising a subsequent fund. These include some "zombie" firms (such as Fenway Partners, reported on here), which are typically managed by a small team of partners and exhibit little to no turnover as the firm begins a protracted wind-down period. As a result, concerns

regarding stale websites seem fairly muted at the firm-year level of observation.

IA.1.3 Parsing HTML Files

For each HTML page, I manually create a Cascading Style Sheet (CSS) selector that, where available, identifies (1) the name of the individual, (2) their title, and (3) biographical information including text that allows identification of characteristics such as education, work experience, employment start years, employment end years, and gender. For the 2011 to 2019 Kohlberg & Company team pages, names are stored in most HTML flags where the "`a`" element contains an internal link to another page with the phrase "bio." The corresponding CSS selector is `a[href*="bio"]`. For the 2011 to 2019 Kohlberg & Company personal pages, names are stored in the first "`h3`" flag, titles are stored in the first "`h5`" flag, and biographical information is stored in the first displayed "`BioContentHolder`" class. Extending to the full set of firms, I then manually construct at least three CSS selectors for each firm-year-webpage cell, which allows the information to vary across historical web addresses over time, and extract these data. Extracting data from other web pages often involves more complex operations and selectors specialized to how the firm's website is constructed. For example, I scrape the 2020 to 2022 webpages for Lindsay Goldberg by converting Javascript output to HTML and then applying CSS selectors. This procedure results in about 385,000 potential person-firm-year observations. Given the standardization of the procedure, many of these potential observations are duplicates or do not correspond to investment professionals. Subsequent steps address this issue.

IA.1.4 Standardizing and Reconciling Data

Within each firm's set of potential person-firm-year observations, I standardize the text data to allow for comparability within firms and people over time and across firms and people in the same year. Discussion of each data point follows.

IA.1.4.1 Names

Individuals' names change over time due to nicknames, the reporting of middle initials, inconsistencies with what is included in the CSS selection identifying a name (sometimes including text

aside from just a name itself), and sometimes misspellings. To account for these inconsistencies, I convert each "name" data point into a first name and a last name, which I then harmonize within firms and across firms over time. I use the `nicknames` package in Python to account for nicknames. For example, if a firm reports an employee named "Johnny Wilson" in 2018 but not in 2019 and "Johnathan Wilson" in 2019 but not in 2018, both would be recorded as "Johnathan Wilson." I then manually review every unique name, attempting to correct for misspellings and surname changes due to marriages, and to filter out erroneously collected pages that continue to this step but do not correspond to actual firm employees.

In addition to subsequent steps in the cleaning procedure that further account for potential inconsistencies, I attempt to cross-merge every person collected from websites across both PitchBook and Revelio datasets. About 80% of people subsequently classified as an investment professional are mapped from one dataset to another. Doing so standardizes names and identifiers within each professional across firms over time and also helps to cross-validate characteristics.

IA.1.4.2 Titles

Using titles and extracted biographical information, I determine whether individuals are investment professionals each year they are observed. Investment professionals have titles that contain one of 24 different word stems that typically refer to an investment professional (such as "partner" and "associate") and excludes all of 245 other stems indicating the professional does not serve in an investing role. Non-investment professionals include firm operations professionals (involved in internal functions such as legal, finance, compliance, human resources, and information technology), portfolio operations professionals (mostly operating partners), and retired investment professionals (such as emeritus partners or senior advisors). As a result, titles such as "Investor Relations Vice President" will fall into the firm operations group, rather than the investment professionals group. If the standardized title cannot be extracted directly from the "title" flag collected from HTML data, then it is parsed from either the name data (listing for example, "John Smith, Partner") or from biographical data (the first sentence of which may read "John Smith is a partner of . . . ").

The main analysis in this paper focuses only on investment professionals. Doing so has the benefit of removing spurious data but also omits employees for whom a standardized title could not be identified.

Seniorities: To allow for investment professionals to be reasonably comparable across firms and over time, I assign each to one of three layers reflecting the hierarchy typically used by PE firms: a bottom-level, a middle-level, and a top-level. Associates, senior associates, and analysts comprise the bottom-level. Vice presidents and associate directors always comprise the middle-level. Principals and directors are also included in the middle-level if the firm has managing directors or partners in the same year. Otherwise, principals and directors are included in the top-level as this would be the highest seniority of investment professional. Similarly, managing directors are assigned to the middle-level if the firm also has individuals with the title of partner in the same year, otherwise managing directors are assigned to the top-level. The top layer then includes the remaining investment professionals (partners, members of the investment committee, and senior management such as CEOs, managing partners, a president, or a chairman).

Upon manual review, I adjusted the hierarchy for a few firms that did not precisely meet the automated seniority described above (Altamont Capital Partners, JC Flowers & Co., Littlejohn & Co., Hellman & Friedman in 2009 and earlier and Silver Lake in 2009 and earlier). For each firm, I also manually collect its founder(s).

To ensure consistency of these seniorities within a firm over time, I do not allow for demotions. If an employee of firm j is observed in the middle-level in year t, they are assumed to be in the middle-level for all subsequent years until they separate from the investment team or are observed in the top-level. Similarly, if an employee of firm j is observed in the top-level in year t, they are assumed to be in the top-level for all subsequent years until they separate from the firm or retire. This correction accounts for the occasional edge case where firms adjust the seniority ranking associated with each title during year t and the extracted team page and person page for year t are not collected during the same side of the intra-year adjustment. For example, doing so corrects what would otherwise be spurious seniority changes where a personal page is collected in June 2006, a team page is collected in August 2006, and in July 2006 the firm updates its website to begin referring to its "principals" as "partners."

I also require that the equivalent person-year observation for the professional in Revelio (if available) agrees on the investment team classification, limiting false positives at the expense of potentially more false negatives. This procedure results in a sample of 10,546 investment professionals. Figure IA.B2 summarizes how many investment professionals are observed in each

seniority layer, accounting for how many are observed at multiple overlapping seniority layers at any in-sample firms.

Figure IA.B2: Investment Professional Counts by Seniority

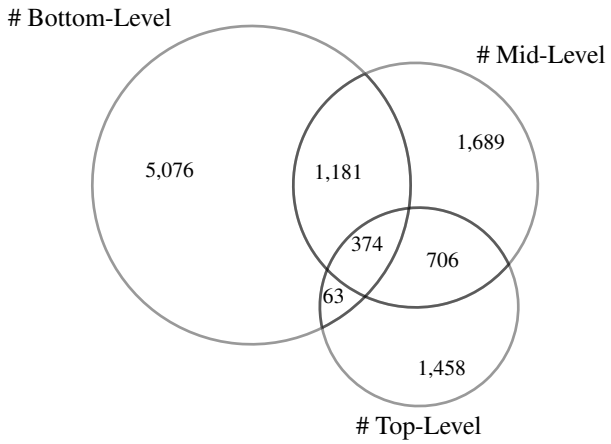


Figure IA.B2: Tabulates the count of investment professionals that appear in each seniority layer.

IA.1.4.3 Timing of Employment at the Firm

I calculate a person's first year at a firm based on regular expressions applied to biographical text excerpts. Doing so allows investment professionals who joined the firm outside of the sample to be assigned their correct join years. For example, if an individual's biographical data indicates that they joined firm j in 1998 but firm j does not have archived website data that starts until 2000, the investment professional's first year at firm j would be 1998. However, if the professional is first observed in year t, is without biographical data detailing when they joined the firm through the duration of their employment with the firm, and the firm has data in the previous year $(t-1)$, I allow the person's join year to be the first year the individual was observed at the firm (year t). Doing so ensures individuals are not spuriously said to be joining the firm because the previous year's website data was unavailable.

If the professional is not observed after year t and the firm has data in the subsequent year $(t+1, t+2$, etc.) then year t is the person's last year working for the firm. Analogously, an investment professional is said to have been promoted in year t if their hierarchal ranking within the firm increases in year $t+1$ compared to year t.

Interim Gaps in Coverage: The internet archive does not collect every web page every year, sometimes creating gaps in coverage. Where these gaps arise and surrounding data are sufficient, I impute positions across these gaps for individuals. For example, if an employee of firm j is listed as a vice president in 2007 and 2009, but (i) her firm does not have a team page in 2008, (ii) there are other employees present in 2008 based on their personal pages, and (iii) the employee did not leave and rejoin the firm (discussed further in subsequent paragraphs) according to her biographical information, then the panel would assume she was also a vice president of firm j in the missing 2008 year.

For longer gaps in coverage (e.g., from t to $t + 3$), I back-fill investment professional's positions. Because there may be legitimate breaks in employment within a given firm, I then adjust the back-filled affiliation with the firm to ensure that the investment professional did not leave the firm and rejoin over the gap (to, for example, obtain an MBA, work for a portfolio company, or take a planned career break). These cases are caught ordinarily but may otherwise be omitted without explicit accounting during the back-filling process. I flag these cases based on key phrases in biographical data, such as "rejoined the firm in 2014," and then do not back-fill data over missing years for these individuals. Doing so ensures that individuals with a firm from, for example, 2010 to 2011 and 2014 to 2020 but not from 2012 to 2013 are not incorrectly imputed to have been with the firm in 2012 and 2013. For associates, I also extend the panel back to their start year, where missing.

Importantly, I then reconcile all of these resulting data (start dates, end dates, and gaps in employment) with resume data obtained from Revelio where available, using the longest available window the professional worked at the firm after allowing for gaps in employment spells with the firm based on the timing of MBA degrees or other career experiences.

Non-PE Divisions: Where possible, I also attempt to remove investment professionals that work primarily in non-PE divisions. Specifically, I remove investment professionals from the public equities division of Berkshire Partners (Stockbridge), the public equities and permanent capital divisions of Vista Equity Partners (Vista Public Strategies and Vista Perennial Capital), the Real Estate, BioTech, Private Credit, and Business Development Companies of H.I.G. (respectively H.I.G. Realty, Bioventures/BioHealth, Bayside, and Whitehorse), the acquired and now divested credit division of Providence Equity (Benefit Street Partners), and the Business Development Com-

panies, Net Lease, and (now closed) public equity divisions of New Mountain Capital (NMFC, Net Lease Partners, and Vantage). For Audax and Bain, which also have material non-PE divisions, I use only their PE-specific websites, which bypasses the need to manually filter out these other divisions.

In total, the dataset contains about 55,000 person-firm-year observations, about 80% of which are strictly from the webpages, with the remaining data points having been derived from imputations between webpage observations.

IA.1.4.4 Education, Work Experience, and Gender

This subsection details characteristics of investment professionals that are extracted from their biographical data. With some exceptions to limit look-ahead bias, characteristics are generally constant at the person level. All characteristics are cross-validated against the professionals' resume data from Revelio, where available.

Education: I extract a person's education based on whether their biographical data in any year indicates that they attended one of the colleges or universities listed in the Integrated Postsecondary Education Data System (IPEDS) with a Carnegie basic code of 15, 16, 17, 18, or 19 for 2000, 2005, and 2015 (corresponding to most R1 and R2 U.S. schools). In flagging education, I also search for about 200 additional schools not included in this initial list that did not meet these original filters (such as Williams College) based on manual readings of biographical data. I additionally extract whether a person received an MBA, and, where possible, the year in which they did so. Tests that estimate fixed effects for the university at which the professional matriculated only focus on the most heavily-attended U.S. school from which the professional matriculated. These data rely on education reported on either the website or Revelio. Harvard, Stanford, and Wharton are the three most commonly attended schools.

I also include flags for (a) "H/S/W" (Harvard, Stanford, or Wharton); (b) "Ivy" (Brown, Columbia, Cornell, Dartmouth, Harvard, Princeton, University of Pennsylvania, and Yale); (c) "Top-20" based on the 2024 U.S. News Rankings (Brown, Caltech, Columbia, Cornell, Dartmouth, Duke, Harvard, Johns Hopkins, MIT, Northwestern, Notre Dame, Princeton, Rice, Stanford, University of Chicago, University of California-Berkeley, University of Pennsylvania, UCLA, Yale and Vanderbilt); and (d) "AAU" (the 69 members of the Association of American Universities as

of 2024).

Work Experience: I extract a person's work experience based on whether their biographical data in any year contains the name of a specific firm or key phrases detailing the type of firm they previously worked at. I primarily focus on work experience at investment banks and consulting firms, and other private equity firms, but also extract data on work experience at other private equity firms, hedge funds, venture capital firms, accounting firms, and law firms. These data rely on education reported on either the website or Revelio.

I consider a professional to have had prior investment banking experience if their biographical data include the phrase "investment bank" or one of 80 investment banks I identified based on manual readings of biographies combined with current and historical editions of the Vault 100 investment banking "prestige" rankings (selected each year based on the assessments of bankers at peer firms). All investment banks are also identified using common abbreviations, previous names, and Revelio listings. I consider employment at Private Equity groups within investment banks that often support PE investments (e.g., Wells Fargo Leveraged Finance) to be investment banking experience. I classify employment at PE affiliates of investment banks (e.g., JPMorgan Partners that eventually spun out to become CCMP Capital) as private equity experience rather than investment banking experience. Common investment banks include Goldman Sachs, JPMorgan, Morgan Stanley, and Bank of America Merrill Lynch.

Analogously, I consider a professional to have had prior consulting experience if their biographical data (or Revelio data) include telling phrases such as "was a consultant for" or mention any of the 41 firms I flag as consulting firms based on manual readings of the most prestigious consulting firms based on the assessments of consultants at peer firms, accessed in October 2024. "Big Three" management consulting experience (Bain & Company, Boston Consulting Group, or McKinsey), more general management consulting experience, and "Big Five" firm experience (Deloitte, EY, KPMG, PwC, or the now defunct Arthur Andersen) which also have more general accounting, consulting, and corporate finance advisory practices. The big-three management consulting firms are the most common, conditional on having consulting experience.

Gender: I assume a person to be of a female gender if their biographical data in any year includes pronouns associated with the female gender ("Mrs.", "Ms.", "Miss", "her", or "she") but not the male gender ("Mr.", "Mister", "he", or "him"). For remaining cases where the gender

could not be determined based on biographical data, I also use the `gender_guesser.detector` package in Python to subsequently infer the person's gender based on their first name. I then check these data against the Revelio data and manually adjust a handful of cases where the two report different genders for the same professional.

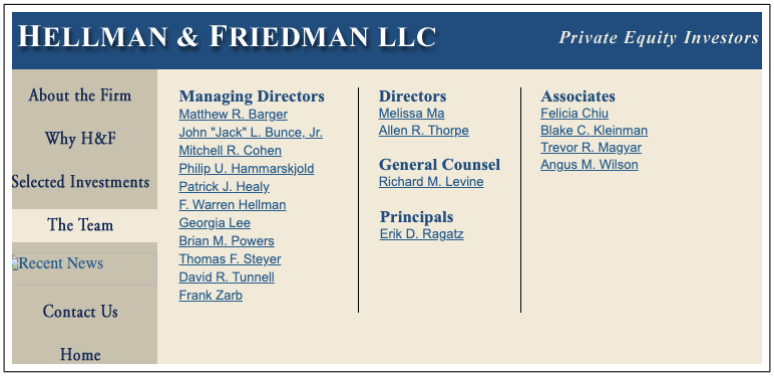
Internet Appendix References

Cattaneo, M. D., Crump, R. K., Farrell, M. H., & Feng, Y. (2024). On Binscatter. *American Economic Review*, *114*(5), 1488–1514.

IA.2 Additional Figures and Tables

Figure IA1: Example Archived Webpages

Panel A: Hellman & Friedman 2003 Team Page



Panel B: Hellman & Friedman 2003 Personal Page Example

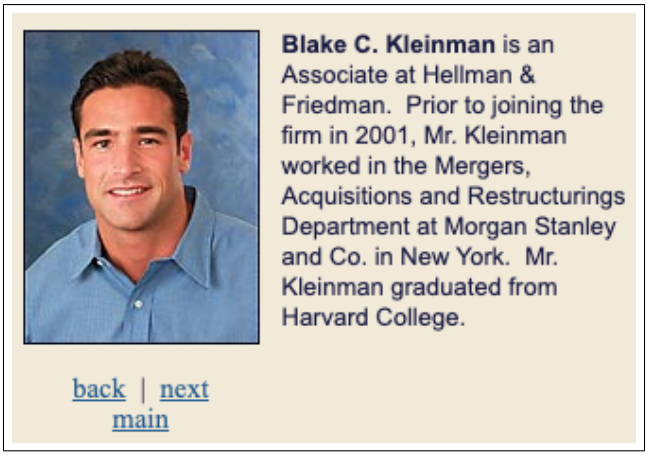


Figure IA1: Displays example webpages archived from the Wayback Machine used to construct the dataset of PE investment professionals. Panel A shows the 2003 team page for Hellman & Friedman (linked here). Panel B displays the 2003 personal page for the associate Blake C. Kleinman (linked here).

Figure IA2: PE Associate Program Trends Among Buyout Professionals

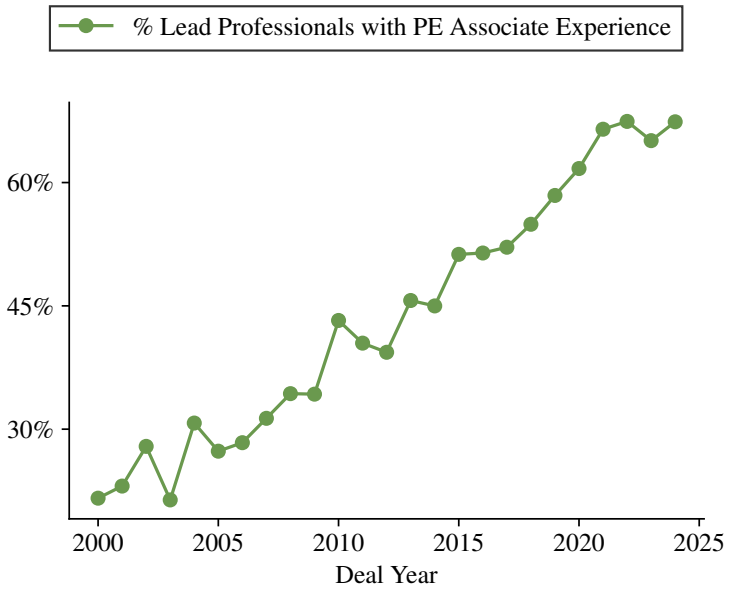


Figure IA2: Plots the fraction of lead professionals of stand-alone leveraged buyouts of U.S. companies that had previously worked as an associate at a PE firm. In 2015, for example, about 50% of all lead professionals had previously worked as an associate at a PE firm. Deal data are from PitchBook. PE associate program experience (if any) is hand-collected if not observed, usually using archived websites.

Figure IA3: Alternative Display: Instrumented Associate Turnover and Buyout Deals

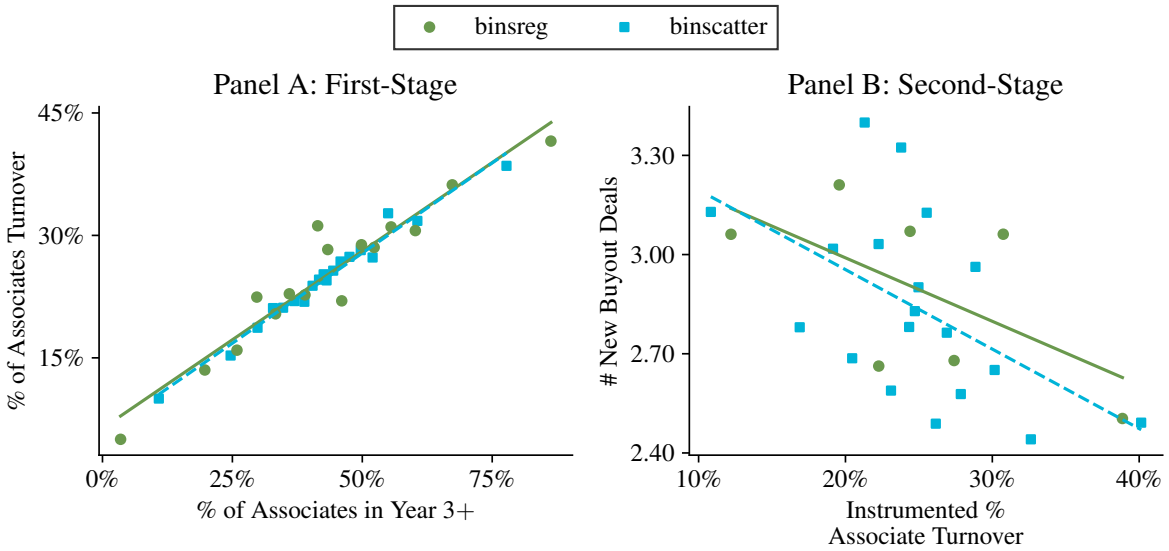


Figure IA3: Displays *binscatter* **and** *binsreg* regressions of the count of PE firm's buyout investments on the instrumented fraction of its associates that turnover each year. *binsreg* regressions are reported following the procedure in Cattaneo, Crump, Farrell, and Feng (2024), which performs the correct residualization and chooses the number of bins to balance variance and (squared) bias when estimating conditional means. *binscatter* uses 20 bins for both subplots. *binsreg* results in 16 bins for the first-stage plot and 7 bins for the second-stage plot. The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been observed with the firm for three or more *calendar* years as of year t, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. All regressions are weighted by the number of associates employed by firm j in year t. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with a firm. Deals only include stand-alone or platform buyouts of U.S. companies. Panel A displays the first stage of the IV regression: the relationship between associate tenure and associate turnover. Panel B displays the second stage: the relationship between the number of stand-alone or platform buyouts of U.S. companies a PE firm does and predicted associate turnover. Regressions are weighted by the number of associates employed by firm j in year t and include firm fixed effects, industry $\times$ year fixed effects, and state $\times$ year fixed effects. The State $\times$ Year FE and Industry $\times$ Year FE are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial Services $\times$ 2015). Modal industries and geographies refer to the most common industry or state (by PitchBook classifications) of a target company that the firm has acquired via a stand-alone or platform buyout over a rolling 10-year window, spanning, for each firm, $[t-10, t-1]$. Table 5 presents first-stage coefficient estimates. Table 6 presents second-stage coefficient estimates.

Table IA1: Deal Statistics

	Has Lead Professional (1)	PE Associate Exp. (2)	Non-Connected Sample (3)	Connected Sample (4)	Difference (4) - (3)
Panel A: Sample Size					
# Lead Professional × Deals	11,713	5,479	1,578	4,059	
# Deals	11,209	5,368	1,572	3,796	
# Companies	9,432	4,831	1,536	3,475	
# Lead Professionals	4,066	1,948	585	1,528	
# Partner Firms	732	565	361	435	
# Associate Firms	552	552	339	368	
Panel B: Gross Deal Returns					
1(Has Gross Return)	48.5%	52.8%	44.7%	56.0%	11.3%***
Value Add ($MM)	$214.0	$229.5	$177.6	$245.9	$68.4***
$MM Invested	$179.7	$205.3	$153.0	$221.7	$68.7***
Gross TVM	2.7x	2.5x	2.7x	2.5x	-0.1x
1(Gross TVM ≥ 5x)	5.6%	4.9%	4.8%	5.0%	0.2%
1(1x < Gross TVM < 5x)	31.5%	35.8%	29.3%	38.4%	9.1%***
1(0x < Gross TVM ≤ 1x)	9.8%	10.8%	9.1%	11.5%	2.4%***
1(Gross TVM = 0x)	1.7%	1.3%	1.6%	1.2%	-0.4%
Panel C: Deal Entry					
Deal Year	2013.1	2015.2	2014.5	2015.5	1.0***
1(# Investors >1)	45.6%	44.8%	41.2%	46.2%	5.1%***
1(Lead Investor)	31.1%	28.0%	27.5%	28.2%	0.7%
1(Secondary-LBO)	26.5%	30.2%	23.3%	32.9%	9.6%***
1(MBO)	17.2%	16.6%	17.1%	16.4%	-0.7%
1(Corp. Divestiture)	10.4%	9.0%	8.5%	9.2%	0.6%
1(Recapitalization)	13.1%	13.8%	15.5%	13.1%	-2.4%**
1(Take-Private)	5.3%	5.6%	3.7%	6.3%	2.5%***
1(Exited)	58.6%	49.4%	50.6%	48.9%	-1.7%
Panel D: Portfolio Company Industry (Top-5)					
1(Commcl. Services)	19.7%	20.9%	22.1%	20.5%	-1.6%
1(Commcl. Products)	15.1%	12.8%	14.0%	12.4%	-1.6%*
1(Software)	10.3%	13.0%	11.2%	13.7%	2.5%**
1(Healthcare Services)	7.6%	8.5%	7.1%	9.0%	1.9%**
1(Consumer Non-Durables)	4.7%	4.2%	4.5%	4.1%	-0.4%
Panel E: Portfolio Company State (Top-5)					
1(California)	11.1%	11.3%	12.2%	11.0%	-1.2%
1(Texas)	11.3%	11.3%	10.2%	11.8%	1.6%*
1(Florida)	5.9%	5.9%	5.5%	6.1%	0.6%
1(Illinois)	6.1%	6.3%	6.1%	6.4%	0.3%
1(New York)	5.9%	5.4%	4.9%	5.6%	0.7%
Panel F: Portfolio Company Changes					
1(CEO Change Year 0)	25.3%	28.5%	24.8%	30.0%	5.2%***
1(CEO Change)	67.6%	75.8%	70.6%	77.8%	7.2%***
1(Make Add-on Acq.)	54.8%	59.9%	52.9%	62.7%	9.8%***
1(Debt Issuance)	28.8%	34.4%	27.0%	37.3%	10.2%***
1(Dividend Recap.)	8.0%	8.3%	4.8%	9.7%	5.0%***
1(Leveraged Recap.)	3.2%	3.2%	2.5%	3.4%	1.0%*

Table IA1: Tabulates the average characteristics of deals with a lead professional (Column 1); with a lead professional that previously worked as a PE associate (Column 2); deals not retained in the largest connected group by associate programs (Column 3); and deals that are retained in the largest connected group of deals based on PE associate work experience (with known data on education and prior banking or consulting work experience as well; Column 4). The difference column reports the difference in means between columns (4) and (3). */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table IA2: Task Transitions Over Buyout Careers

	𝟙(Has Led a Deal)		𝟙(Has Been on a Board)	
	U.S. Platform (1)	Any Buyout (2)	U.S. Platform (3)	Any Buyout (4)
𝟙(Middle-Level)	0.086***	0.125***	0.221***	0.254***
	(8.72)	(11.06)	(15.10)	(16.72)
𝟙(Top-Level)	0.491***	0.596***	0.547***	0.603***
	(14.99)	(18.70)	(16.47)	(18.74)
Y-mean	0.053	0.073	0.104	0.117
Y-Std. Dev.	0.225	0.260	0.305	0.322
Associate Firm FE	x	x	x	x
Year FE	x	x	x	x
Adj R^2	0.301	0.328	0.303	0.324
N	16,322	16,322	16,322	16,322

Table IA2: Tabulates regressions of indicators for whether a professional has led a buyout investment or set on the board of a buyout investment conditioned on their seniority in a given year. The unit of observation is the professional-year. The sample includes all associates from Table 2 whose careers, both within and across firms are tracked each year on and after they first worked as an associate at one the sample firms collected via archived websites. Middle-level professionals are typically vice presidents or principals. Top-level professionals are typically partners or managing directors. The omitted category includes person-years where the professional was at the bottom level of the seniority hierarchy. A professional is considered to have led a deal or been on a board in all years on or after the first year they did either. Regression samples are restricted to only include professionals that still work at a U.S. PE firm in a considered year. For example, an associate of GTCR that joined in 2000, left to get an MBA in 2003, returned to GTCR in 2005, and then left in 2015 to work at Apple, would be included in the regressions from 2000 to 2015. Standard errors are clustered at the buyout professional level. *t*-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table IA3: Robustness: Associate Program Effects with Additional Cohort Fixed Effects

			% of R^2 Attributable to Fixed Effects							
	Y-Var.	R^2	Associate Firm	Cohort	Bank/Consultancy	School	Deal Year	Current Firm	Y-mean	N.
	Panel A: Gross Deal Returns									
(1)	Value Add ($MM)	39.8%	28.3%	5.9%	8.6%	6.7%	5.3%	45.3%	340.73	1,175
(2)	$MM Invested	39.5%	23.3%	4.3%	6.3%	6.4%	4.8%	55.0%	222.98	2,163
(3)	Gross TVM	19.4%	24.9%	18.3%	7.2%	14.7%	4.8%	30.0%	3.15	1,175
(4)	1(Gross TVM ≥ 5x)	32.8%	29.4%	9.7%	8.3%	11.6%	5.8%	35.2%	0.14	1,175
(5)	1(1x < Gross TVM < 5x)	12.3%	31.1%	5.7%	8.4%	11.0%	6.8%	37.0%	0.67	1,175
(6)	1(0x < Gross TVM ≤ 1x)	33.0%	29.2%	6.8%	9.6%	13.1%	8.8%	32.5%	0.15	1,175
(7)	1(Gross TVM = 0x)	36.6%	29.6%	5.3%	6.8%	18.6%	4.6%	35.1%	0.03	1,175
	Panel B: Deal Characteristics									
	Entry:									
(8)	1(# Investors >1)	16.8%	30.9%	4.2%	7.6%	8.0%	4.9%	44.4%	0.4	3,706
(9)	1(Lead Investor)	23.7%	20.8%	10.8%	5.4%	5.5%	34.6%	22.8%	0.31	3,706
(10)	1(Secondary-LBO)	21.3%	33.0%	3.2%	7.8%	8.9%	7.2%	40.0%	0.33	3,706
(11)	1(MBO)	26.9%	31.1%	3.4%	7.8%	7.4%	5.6%	44.7%	0.16	3,706
(12)	1(Corp. Divestiture)	20.2%	34.2%	3.7%	6.3%	8.6%	4.1%	43.2%	0.09	3,706
(13)	1(Recapitalization)	30.0%	29.6%	2.9%	6.1%	7.4%	1.6%	52.4%	0.13	3,706
(14)	1(Take-Private)	27.4%	23.9%	5.5%	7.4%	11.7%	5.9%	45.6%	0.05	3,706
	Industry (Top-5):									
(15)	1(Commcl. Services)	24.1%	32.8%	4.0%	7.2%	12.8%	3.6%	39.7%	0.21	3,706
(16)	1(Commcl. Products)	32.4%	34.4%	2.9%	8.4%	9.9%	2.4%	42.1%	0.13	3,706
(17)	1(Software)	41.6%	31.7%	3.9%	7.9%	7.1%	1.7%	47.8%	0.14	3,706
(18)	1(Healthcare Services)	38.7%	29.7%	5.9%	8.8%	8.2%	1.8%	45.6%	0.09	3,706
(19)	1(Consumer Non-Durables)	36.5%	28.4%	4.3%	8.2%	10.6%	2.3%	46.1%	0.04	3,706
	State (Top-5):									
(20)	1(California)	21.1%	35.7%	3.4%	6.2%	11.1%	3.5%	40.0%	0.11	3,706
(21)	1(Texas)	21.9%	33.4%	4.0%	8.1%	9.3%	2.9%	42.3%	0.12	3,706
(22)	1(Florida)	21.5%	34.2%	3.0%	7.1%	14.2%	2.8%	38.7%	0.06	3,706
(23)	1(Illinois)	22.2%	34.2%	5.1%	7.0%	9.5%	2.7%	41.5%	0.06	3,706
(24)	1(New York)	23.4%	40.4%	4.9%	7.7%	10.6%	2.7%	33.7%	0.05	3,706
	Panel C: Portfolio Company Changes									
(25)	1(CEO Change Year 0)	23.3%	21.4%	5.3%	5.6%	6.9%	33.9%	26.9%	0.29	3,441
(26)	1(CEO Change)	9.8%	18.4%	10.5%	4.2%	6.0%	39.8%	21.1%	0.78	3,542
(27)	1(Make Add-on Acq.)	12.1%	30.4%	4.8%	6.6%	8.9%	14.7%	34.6%	0.62	3,617
(28)	1(Debt Issuance)	20.7%	30.8%	4.9%	5.4%	8.7%	13.7%	36.6%	0.38	3,617
(29)	1(Dividend Recap.)	23.9%	29.1%	7.6%	6.9%	9.2%	9.5%	37.7%	0.09	3,617
(30)	1(Leveraged Recap.)	21.1%	34.0%	3.1%	9.7%	12.3%	5.1%	35.8%	0.03	3,617

Table IA3: Tabulates the fraction of explained variation in buyout deal outcomes attributable to a set of fixed effects based on Shapley value decompositions. The estimating equation is Equation (3), which augments the main specifcation by including fixed effects for the year a partner first led a buyout investment ("Cohort"). Outcomes include deal returns (Panel A), deal characteristics (Panel B), and operating and financial changes at the portfolio company following the LBO (Panel C). Each row represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. The PE firm associate program attended by the lead professional must be available. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted. Standard errors are clustered at the deal level.

Table IA4: Robustness: Associate Program Effects with Only Switchers

			% of R^2 Attributable to Fixed Effects						
	Y-Var.	R^2	Associate Firm	Bank/Consultancy	School	Deal Year	Current Firm	Y-mean	N.
	Panel A: Gross Deal Returns								
(1)	Value Add ($MM)	46.1%	34.6%	7.7%	7.9%	7.6%	42.2%	278.36	651
(2)	$MM Invested	47.7%	31.4%	8.2%	7.1%	5.9%	47.4%	191.92	1,223
(3)	Gross TVM	23.0%	30.5%	6.4%	16.9%	6.5%	39.7%	3.18	651
(4)	𝟙(Gross TVM ≥ 5x)	38.2%	33.7%	9.4%	10.4%	7.2%	39.3%	0.16	651
(5)	𝟙(1x < Gross TVM < 5x)	16.1%	35.7%	7.2%	10.2%	8.8%	38.0%	0.65	651
(6)	𝟙(0x < Gross TVM ≤ 1x)	35.9%	35.7%	7.6%	10.3%	10.9%	35.4%	0.16	651
(7)	𝟙(Gross TVM = 0x)	49.0%	38.0%	5.8%	18.0%	7.5%	30.7%	0.04	651
	Panel B: Deal Characteristics								
	Entry:								
(8)	𝟙(# Investors >1)	22.4%	36.0%	8.9%	8.9%	4.8%	41.5%	0.4	2,249
(9)	𝟙(Lead Investor)	29.7%	27.7%	6.2%	7.3%	30.7%	28.2%	0.3	2,249
(10)	𝟙(Secondary-LBO)	28.4%	37.4%	8.0%	8.6%	4.5%	41.4%	0.29	2,249
(11)	𝟙(MBO)	32.8%	35.2%	7.8%	7.2%	6.9%	42.8%	0.16	2,249
(12)	𝟙(Corp. Divestiture)	28.3%	36.6%	6.6%	8.2%	6.2%	42.5%	0.1	2,249
(13)	𝟙(Recapitalization)	36.5%	31.4%	7.1%	9.9%	2.5%	49.1%	0.13	2,249
(14)	𝟙(Take-Private)	34.7%	29.4%	9.0%	7.8%	5.3%	48.5%	0.05	2,249
	Industry (Top-5):								
(15)	𝟙(Commcl. Services)	32.2%	38.3%	8.1%	9.2%	3.7%	40.7%	0.21	2,249
(16)	𝟙(Commcl. Products)	38.8%	34.2%	8.4%	12.5%	3.4%	41.5%	0.13	2,249
(17)	𝟙(Software)	50.1%	34.2%	8.2%	8.4%	1.9%	47.2%	0.14	2,249
(18)	𝟙(Healthcare Services)	50.9%	36.7%	8.9%	7.4%	2.5%	44.5%	0.1	2,249
(19)	𝟙(Consumer Non-Durables)	44.4%	33.1%	5.2%	11.2%	3.8%	46.7%	0.04	2,249
	State (Top-5):								
(20)	𝟙(California)	29.2%	39.3%	7.4%	11.2%	2.6%	39.6%	0.11	2,249
(21)	𝟙(Texas)	31.5%	40.2%	6.3%	7.4%	3.8%	42.3%	0.12	2,249
(22)	𝟙(Florida)	30.1%	41.7%	8.3%	10.9%	3.6%	35.5%	0.06	2,249
(23)	𝟙(Illinois)	29.5%	38.6%	6.3%	9.0%	3.6%	42.5%	0.07	2,249
(24)	𝟙(New York)	35.4%	41.4%	7.7%	11.3%	3.1%	36.5%	0.05	2,249
	Panel C: Portfolio Company Changes								
(25)	𝟙(CEO Change Year 0)	30.1%	28.2%	5.5%	7.9%	26.1%	32.3%	0.28	2,092
(26)	𝟙(CEO Change)	11.6%	26.0%	5.2%	5.7%	38.2%	25.0%	0.77	2,144
(27)	𝟙(Make Add-on Acq.)	15.9%	35.5%	7.0%	9.5%	11.2%	36.8%	0.59	2,192
(28)	𝟙(Debt Issuance)	26.8%	37.2%	6.0%	8.2%	9.4%	39.1%	0.34	2,192
(29)	𝟙(Dividend Recap.)	33.0%	36.7%	7.8%	8.6%	7.0%	39.9%	0.08	2,192
(30)	𝟙(Leveraged Recap.)	29.5%	43.2%	6.1%	9.2%	5.1%	36.4%	0.03	2,192

Table IA4: Tabulates the fraction of explained variation in buyout deal outcomes attributable to a set of fixed effects based on Shapley value decompositions after including only deals led by partners and principals representing a firm that is *not* where they worked as a PE associate. The estimating equation is Equation (2). Outcomes include deal returns (Panel A), deal characteristics (Panel B), and operating and financial changes at the portfolio company following the LBO (Panel C). Each row represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. The PE firm associate program attended by the lead professional must be available. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted. Standard errors are clustered at the deal level.

Table IA5: Robustness: Associate Program Effects with Fund Fixed Effects

			% of R^2 Attributable to Fixed Effects						
	Y-Var.	R^2	Associate Firm	Bank/Consultancy	School	Deal Year	Fund	Y-mean	N.
	Panel A: Gross Deal Returns								
(1)	Value Add ($MM)	58.4%	17.8%	5.7%	4.4%	3.0%	69.2%	361.38	963
(2)	$MM Invested	51.1%	16.0%	4.0%	4.6%	2.4%	73.0%	225.49	1,928
(3)	Gross TVM	26.0%	18.7%	4.9%	12.7%	4.2%	59.5%	3.14	963
(4)	𝟙(Gross TVM ≥ 5x)	43.6%	21.3%	6.6%	9.3%	5.0%	57.7%	0.14	963
(5)	𝟙(1x < Gross TVM < 5x)	16.5%	22.1%	6.7%	9.8%	4.6%	56.7%	0.67	963
(6)	𝟙(0x < Gross TVM ≤ 1x)	45.0%	20.3%	8.1%	10.1%	7.4%	54.1%	0.15	963
(7)	𝟙(Gross TVM = 0x)	49.3%	21.1%	6.0%	16.3%	4.0%	52.5%	0.04	963
	Panel B: Deal Characteristics								
	Entry:								
(8)	𝟙(# Investors >1)	26.6%	21.9%	6.1%	7.2%	3.8%	61.0%	0.4	1,928
(9)	𝟙(Lead Investor)	30.2%	17.8%	4.8%	6.3%	17.1%	54.1%	0.37	1,928
(10)	𝟙(Secondary-LBO)	30.7%	24.1%	8.4%	7.9%	4.0%	55.6%	0.38	1,928
(11)	𝟙(MBO)	43.4%	22.9%	8.9%	6.6%	3.1%	58.5%	0.17	1,928
(12)	𝟙(Corp. Divestiture)	38.7%	22.5%	6.6%	7.5%	3.5%	59.8%	0.09	1,928
(13)	𝟙(Recapitalization)	45.8%	23.7%	5.7%	6.2%	1.6%	62.8%	0.13	1,928
(14)	𝟙(Take-Private)	44.4%	18.4%	5.4%	9.0%	6.0%	61.2%	0.06	1,928
	Industry (Top-5):								
(15)	𝟙(Commcl. Services)	40.1%	25.8%	7.0%	9.7%	4.1%	53.4%	0.19	1,928
(16)	𝟙(Commcl. Products)	45.4%	25.0%	7.0%	11.4%	2.5%	54.2%	0.13	1,928
(17)	𝟙(Software)	51.9%	24.5%	10.0%	7.5%	2.1%	56.0%	0.17	1,928
(18)	𝟙(Healthcare Services)	49.0%	25.5%	9.7%	9.4%	2.5%	52.9%	0.09	1,928
(19)	𝟙(Consumer Non-Durables)	59.8%	23.7%	10.7%	10.6%	2.4%	52.6%	0.04	1,928
	State (Top-5):								
(20)	𝟙(California)	39.2%	20.1%	5.5%	11.4%	2.8%	60.2%	0.11	1,928
(21)	𝟙(Texas)	34.9%	21.8%	8.3%	7.7%	2.9%	59.3%	0.13	1,928
(22)	𝟙(Florida)	39.4%	21.0%	6.8%	13.2%	2.4%	56.6%	0.06	1,928
(23)	𝟙(Illinois)	35.6%	22.3%	7.7%	8.3%	2.6%	59.2%	0.07	1,928
(24)	𝟙(New York)	40.4%	21.5%	7.8%	7.1%	4.3%	59.2%	0.05	1,928
	Panel C: Portfolio Company Changes								
(25)	𝟙(CEO Change Year 0)	31.3%	20.4%	7.2%	7.3%	2.9%	62.1%	0.26	1,844
(26)	𝟙(CEO Change)	12.5%	17.7%	4.2%	6.3%	22.4%	49.4%	0.77	1,913
(27)	𝟙(Make Add-on Acq.)	14.9%	25.7%	6.0%	7.6%	4.7%	56.0%	0.7	1,928
(28)	𝟙(Debt Issuance)	27.1%	19.6%	5.8%	8.3%	6.3%	60.0%	0.45	1,928
(29)	𝟙(Dividend Recap.)	39.0%	21.0%	6.3%	8.0%	5.2%	59.5%	0.13	1,928
(30)	𝟙(Leveraged Recap.)	41.6%	20.5%	6.4%	10.2%	3.0%	59.9%	0.04	1,928

Table IA5: Tabulates the fraction of explained variation in buyout deal outcomes attributable to a set of fixed effects based on Shapley value decompositions after including only deals led by partners and principals of a firm that is *not* where they worked as a PE associate. The estimating equation is Equation (2) but replaces the firm fixed effects with fixed effects for the fund in which the investment was made. Samples require that this information is available through StepStone. Outcomes include deal returns (Panel A), deal characteristics (Panel B), and operating and financial changes at the portfolio company following the LBO (Panel C). Each row represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the "lead-partner" × deal level. Regression samples only include platform or stand-alone buyouts of U.S. companies. The PE firm associate program attended by the lead professional must be available. Deals are from PitchBook and deal returns are from StepStone. Value-add ((TVM-1) × $ Invested) and TVM additionally restrict the sample to only exited deals. Portfolio company outcomes are measured over the five years following the LBO unless otherwise noted. Standard errors are clustered at the deal level.

Table IA6: Associate Program Effects on Hiring and Careers

			% of R^2 Attributable to Fixed Effects						
	Y-Var.	R^2	Associate Firm	p-value	Join Year	Bank/Consultancy	School	Y-mean	N.
	Panel A: Hiring Characteristics								
(1)	𝟙(H/S/W Degree)	16.2%	52.6%	<.01	14.1%	33.4%		0.26	1,448
(2)	𝟙(Ivy Degree)	14.6%	64.3%	<.01	8.6%	27.1%		0.36	1,444
(3)	𝟙(Top-20 U.S. Degree)	10.8%	64.9%	<.01	5.3%	29.8%		0.58	1,444
(4)	𝟙(AAU Degree)	3.9%	59.2%	<.01	5.5%	35.3%		0.83	1,444
(5)	𝟙(Female)	21.9%	44.0%	<.01	10.8%	13.6%	31.7%	0.12	1,400
(6)	𝟙(IB. Exp.)	4.1%	76.0%	<.01	3.8%		20.2%	0.88	1,446
(7)	𝟙(Con. Exp.)	30.8%	69.2%	<.01	6.6%		24.2%	0.17	1,446
(8)	𝟙(PE Exp.)	24.2%	71.9%	<.01	7.7%		20.4%	0.07	1,448
(9)	𝟙(Post-MBA Associate)	27.7%	64.0%	<.01	7.4%	11.6%	17.0%	0.15	1,448
(10)	𝟙(From Feeder Firm)	28.7%	36.4%	<.01	5.2%	45.6%	12.8%	0.33	1,297
(11)	𝟙(From Feeder School)	24.7%	31.4%	<.01	3.7%	11.5%	53.3%	0.36	1,297
	Panel B: Career Progression Characteristics								
(12)	𝟙(Obtain MBA Degree)	25.6%	51.8%	<.01	13.9%	14.9%	19.4%	0.33	1,218
(13)	𝟙(H/S/W MBA | Obtained MBA)	20.5%	48.1%	<.01	8.5%	20.6%	22.8%	0.61	566
(14)	𝟙(→ Middle-level; Same)	16.7%	55.1%	<.01	13.7%	13.7%	17.4%	0.38	1,448
(15)	𝟙(→ Middle-level; Any)	11.6%	41.2%	<.01	22.5%	16.9%	19.5%	0.58	1,448
(16)	𝟙(→ Top-level; Same)	31.8%	47.1%	<.01	22.4%	12.9%	17.5%	0.15	829
(17)	𝟙(→ Top-level; Any)	27.7%	35.9%	<.01	30.9%	13.1%	20.1%	0.23	829
(18)	𝟙(→ Lead Buyout; Same)	37.5%	48.5%	<.01	20.3%	15.2%	15.9%	0.09	829
(19)	𝟙(→ Lead Buyout; Any)	33.5%	40.0%	<.01	27.3%	15.6%	17.1%	0.14	829
(20)	𝟙(→ Other Firm | Left PE)	8.1%	49.8%	<.01	10.7%	20.9%	18.5%	0.74	708

Table IA6: Tabulates the fraction of explained variation in hiring practices of associate firms and subsequent career progressions of PE associates attributable to a set of fixed effects based on Shapley value decompositions. The p-value column reports the p-value obtained from F-tests for the joint significance of associate program fixed effects on each outcome. Each row represents a decomposition for a separate regression estimated for the outcome variable in the "Y-Var." column. The unit of observation is the associate level. Regressions of school characteristics omit school fixed effects and regressions of prior experience omit prior experience fixed effects. Top-level and lead partner regressions only include associates that joined in 2014 or earlier. MBA regressions require that the associate joined without an MBA. Regression samples otherwise include the same associates as in Table 2, Column (2) where data are available. Lead buyout only refers to platform/stand-alone buyouts of U.S. companies completed by U.S. buyout firms. "Same" indicates that the associate represented their associate firm at a higher seniority. "Any" includes representation of other U.S. buyout firms. Standard errors are clustered by associate firms.

Table IA7: Robustness: Instrumented Associate Turnover and Buyout Activity With Alternate Specifications and Data Sources

	Specification Description	Coefficient for $\widehat{\%\ \text{Associate Turnover}}$	t-stat.	Y-mean	F-stat.	N
(1)	Baseline	-2.230***	(-3.73)	2.909	198.706	1,923
(2)	Announced Deals	-0.981**	(-2.79)	1.138	208.704	1,815
(3)	Include Add-on and Non-US Deals	-36.573**	(-2.55)	22.924	198.706	1,923
(4)	Stepstone Deals	-3.323***	(-2.85)	3.473	141.362	1,640
(5)	Unweighted	-1.453***	(-4.39)	2.137	162.569	1,923
(6)	At Least 5 Associates	-3.285***	(-3.06)	3.218	104.824	924
(7)	< 5 Associates	-1.514**	(-2.80)	1.592	47.958	699
(8)	All Bottom-Level	-2.208***	(-3.55)	2.909	206.200	1,931
(9)	Firm Cluster	-2.230***	(-3.20)	2.909	191.951	1,923
(10)	Year Cluster	-2.230***	(-4.05)	2.909	246.708	1,923
(11)	Only Firm and Year FE	-1.477***	(-2.91)	2.843	191.119	2,227
(12)	Add AUM Bin and PME Bin FE	-2.451***	(-3.49)	3.101	128.135	1,280
(13)	Add # Invest Fund AUM Bin and PME Bin FE	-2.417***	(-3.56)	3.088	132.323	1,278
(14)	Drop 2020	-2.081***	(-3.57)	2.907	197.572	1,809
(15)	Drop 2020-2022	-1.649***	(-3.73)	2.597	161.842	1,568
(16)	Drop 2008-2009	-2.163***	(-3.41)	3.029	171.782	1,759
(17)	Drop 2008-2009 and 2020-2022	-1.564***	(-3.11)	2.741	137.261	1,401
((18)	Senior Pro. Turnover Controls	-2.584***	(-3.33)	2.984	153.987	1,660
(19)	Associate Characteristic Controls	-2.118***	(-3.62)	2.930	223.930	1,753
(20)	Lagged Deal Flow Controls	-2.417***	(-3.63)	2.921	191.942	1,863
(21)	Lagged Associate Hiring Controls	-2.641***	(-3.00)	2.982	153.333	1,653
(22)	Lagged Associate Hiring and Deal Flow Controls	-2.764***	(-3.15)	2.982	154.959	1,653
(23)	Revelio Sample	-1.870***	(-3.96)	3.709	238.237	7,036

Table IA7: Tabulates regressions of the count of PE firm's buyout investments on the instrumented fraction of its associates that turnover each year. The "Specification Description" column indicates adjustments made to the main IV specification or regression sample before estimation (Equation (7)). The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been with the firm for three or more years as of year t, representing the increasing likelihood that their associate program has ended. The Revelio sample recreates the firm-year panel from scratch using data from Revelio Labs that are subsequently reconciled. The Revelio sample includes all U.S. buyout firms mapped to LinkedIn and removes about five firm-year observations where the number of estimated associates exceeds 200 due to the inclusion of non-PE associates that I could not otherwise filter out (e.g., associates of H.I.G.'s BDCs and Bain's venture funds). t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table IA8: Robustness: Instrumented Associate Turnover and Buyout Activity in Different Sub-Samples

	Specification Description	Coefficient for $\widehat{\%\ \text{Associate Turnover}}$	t-stat.	Y-mean	F-stat.	N
(1)	$\leq$ 1 Asso. Hire	-1.241*	(-1.91)	2.383	87.902	937
(2)	$\geq$ 2 Asso. Hire	-2.968**	(-2.73)	3.326	62.721	671
(3)	$\geq$ 3 Asso. Hire	-3.434**	(-2.80)	3.680	39.297	364
(4)	$\leq$ 3 Yrs. Since Last Fund	-2.444***	(-3.24)	3.104	161.905	1,462
(5)	$>$ 3 Yrs. Since Last Fund	-3.049**	(-2.90)	1.825	32.254	198
(6)	$\leq$ 3 Yrs. Since Last US LBO Fund	-2.421***	(-3.29)	3.138	158.974	1,374
(7)	$>$ 3 Yrs. Since Last US LBO Fund	-2.464**	(-2.77)	2.097	34.129	274
(8)	Deal Increase	-3.373***	(-3.35)	2.908	88.224	1,056
(9)	Deal Decrease	-2.532**	(-2.19)	3.143	77.340	542
(10)	No Partner Turnover	-2.100**	(-2.51)	2.919	137.759	1,413
(11)	No Middle-Level Turnover	-2.089***	(-2.87)	2.819	68.445	1,089
(12)	Exclude S&P Ret. Q1	-2.701***	(-3.63)	2.983	124.485	1,554
(13)	Exclude S&P Ret. Q2	-2.260***	(-3.34)	3.009	186.591	1,402
(14)	Exclude S&P Ret. Q3	-2.161**	(-2.79)	2.875	148.357	1,414
(15)	Exclude S&P Ret. Q4	-1.861**	(-2.32)	2.740	109.804	1,367
(16)	Exclude U.S. HY-Spread Q1	-2.505***	(-3.11)	2.591	110.567	1,394
(17)	Exclude U.S. HY-Spread Q2	-2.356***	(-3.05)	2.938	119.695	1,436
(18)	Exclude U.S. HY-Spread Q3	-1.985**	(-2.50)	3.006	194.265	1,329
(19)	Exclude U.S. HY-Spread Q4	-2.048***	(-3.18)	3.067	155.391	1,588
(20)	Exclude Analysts	-2.051***	(-3.25)	2.925	243.926	1,866
(21)	Exclude Senior Associates	-2.095***	(-3.23)	2.871	179.634	1,868
(22)	Exclude Analysts and Senior Associates	-1.877***	(-2.99)	2.885	230.283	1,799
(23)	Only Promotions	-6.050***	(-3.43)	2.909	56.600	1,923
(24)	Only Separations	-3.532***	(-3.65)	2.909	116.774	1,923

Table IA8: Tabulates regressions of the count of PE firm's buyout investments on the instrumented fraction of its associates that turnover each year. The "Specification Description" column indicates adjustments made to the main IV specification or regression sample before estimation (Equation (7)). "Asso Hire", for example, limits the regression sample to firm-year cells where the firm hired the given number of associates. "Deal Increase" focues on firm year cells where the number of deals done in year $t+1$ exceeds the number done in year $t-1$. S&P return and U.S. HY-Spread quartiles are calculated at the year level. Analysts and senior associates are identified based on their title. The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been observed with the firm for three or more *calendar* years as of year t, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. All regressions are weighted by the number of associates employed by firm j in year t. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with a firm. Deals only include stand-alone or platform buyouts of U.S. companies. Standard errors are independently double-clustered at the firm and year levels. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.

Table IA9: Instrumented Associate Turnover and Leads and Lags of Buyout Activity

	# LBOs Completed										
	(1) $t-5$	(2) $t-4$	(3) $t-3$	(4) $t-2$	(5) $t-1$	(6) t	(7) $t+1$	(8) $t+2$	(9) $t+3$	(10) $t+4$	(11) $t+5$
% $\widehat{\text{Associate Turnover}}$	0.198	0.755	1.617***	0.145	0.325	-2.230***	-1.088	-1.354*	-0.717	0.539	0.286
	(0.24)	(0.98)	(2.93)	(0.26)	(0.40)	(-3.73)	(-1.65)	(-1.90)	(-1.21)	(1.06)	(0.53)
Y-mean	2.493	2.561	2.672	2.685	2.899	2.909	2.789	2.716	2.663	2.687	2.707
Y-Std. Dev.	2.100	2.163	2.237	2.218	2.470	2.463	2.378	2.217	2.236	2.265	2.277
Firm FE	x	x	x	x	x	x	x	x	x	x	x
Industry × Year FE	x	x	x	x	x	x	x	x	x	x	x
State × Year FE	x	x	x	x	x	x	x	x	x	x	x
Kleibergen-Paap F-stat.	152.783	158.712	185.800	198.065	195.745	198.706	195.544	157.925	153.835	151.973	146.346
N	1,749	1,813	1,863	1,887	1,913	1,923	1,912	1,778	1,631	1,508	1,400

Table IA9: Tabulates 2SLS regressions of the count of PE firm's buyout investments, measured in years differing from year t, on the instrumented fraction of its associates that turnover each year. The unit of observation is the firm-year (jt). Associates turnover in year t if they are separated from firm j by year $t+1$ or are promoted to the Vice-President seniority or higher within firm j by the next year they are observed (often following a temporary separation to pursue an MBA). Associate turnover is instrumented with the fraction of firm j's associates that have been observed with the firm for three or more *calendar* years as of year t, representing the increasing likelihood that their associate program has ended. For example, an associate that joined in August 2018 would be in their third *calendar* year as of January 2020. The column denoted by "$t+3$," for example, reports the results of regressing the number of deals completed by firm j in year $t+3$ on the instrumented fraction of associates that turn over in year t. All regressions are weighted by the number of associates employed by firm j in year t. Regression samples cover 2000 to 2022 and only include associates within their first six years of employment with a firm. Deals only include stand-alone or platform buyouts of U.S. companies. The State × Year FE and Industry × Year FE are fixed effects for the modal state or industry of a PE firm's buyout acquisitions interacted with the observation year (e.g., Commercial Services × 2015). Modal industries and geographies refer to the most common industry or state (by PitchBook classifications) of a target company that the firm has acquired via a stand-alone or platform buyout over a rolling 10-year window, spanning, for each firm, $[t-10, t-1]$. Standard errors are independently double-clustered at the firm and year levels. t-stats are in parentheses and */**/*** respectively denote significance at the 10, 5, and 1 % levels.