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Do Employees Cheer for Private Equity? The Heterogeneous Effects of Buyouts on Job Quality

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Abstract. We show that private equity leveraged buyouts reduce perceived job quality despite not impacting average base pay. This appears to reflect employees bearing more risk. Both job quality and employee incentive pay are more related to firm performance at private equity-owned companies than at public control firms with 1% higher deal rate of return associated with 0.7% more employee incentive pay. Deals with high leverage and employees with worse outside options fuel the declines in job satisfaction after a buyout. Our results highlight how job quality is tied to job security and how ownership affects employees through mechanisms beyond base pay.

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Keywords: private equity • leveraged buyouts • job quality • corporate culture • implicit contracts

1. Introduction

The relationship between workers and their employers involves far more than money. Company culture, commitment to work–life balance, and management are central to job quality but do not show up on a paycheck (see, e.g., Guiso et al. 2015, Graham et al. 2022, Gorton and Zentefis 2024). Although employees at larger firms may never meet the firms' owners, a growing literature links employee outcomes to ownership structure. In this paper, we explore the impact that private equity (PE) ownership has on job quality.

PE has a large footprint on the U.S. economy, accounting for more than one third of recent mergers and acquisitions (M&A) transactions.[1] This asset class delivers strong financial returns to both investors and fund managers, stemming from a combination of operational and capital structure changes (see, e.g., Jenkinson et al. 2023). Although PE deals generate value for owners, their effects on employees—among a firm's most important stakeholders—are far from clear. One hypothesis is that PE investors create value in part by extracting surplus from employees by cutting job amenities, reducing job security, and demanding more from workers. Indeed, media reports often highlight layoffs and oppressive working conditions following PE leveraged buyouts (LBOs).[2] Under such a model, returns to investors come at the expense of employee well-being. An alternative hypothesis is that the actions PE owners take to improve firm value might improve employee sentiment. This could arise if PE owners relax capital constraints and allow for needed investments (Boucly et al. 2011, Krishnan et al. 2015), improve struggling firms (Johnston-Ross et al. 2021, Cohn et al. 2022), or make active workforce investments that were forgone by myopic or capital-constrained prior owners (Edmans 2011, Welch and Yoon 2023).

This work tests and reconciles these contrasting predictions by asking how buyouts affect employee perceptions of job quality, including not just compensation, but also nonpecuniary amenities. To do so, we study employee reviews posted on the website Glassdoor and focus on hard-to-observe intangibles that are crucial to the employer–employee relationship: satisfaction with compensation, work–life balance, culture, and senior management along with reported base salary and variable

(i.e., performance) pay. We merge these employee reviews with data on changes in ownership, including PE deals from PitchBook, M&A transactions from Capital IQ, and deal-level returns from StepStone, a large fund of funds. The resulting data set includes more than three million reviews written by current employees at 271,000 unique companies between 2008 and 2019.

Employee review data offer several advantages over more standard data sets. First, understanding job quality requires eliciting the opinions of workers themselves, who, as Guiso et al. (2015) point out, offer a ground truth on firm culture that does not always align with how the firm advertises its own culture. Second, these data shed new light on earnings relative to administrative data sets such as those available from the U.S. Census Bureau. The Glassdoor measures cover all compensation, including equity, via both reported salary and performance pay. Third, our data allow us to measure not just pay, but also satisfaction with that pay, which is arguably most relevant for welfare. As we later discuss, existing research validates Glassdoor data, showing that it is informative about firm outcomes and representative of the U.S. wage distribution when disaggregated.

Our research design is a differences-in-differences model, using firm and industry–time fixed effects with never-PE-owned firms serving as counterfactuals for PE-owned firms. Although this design is common, it faces two identification challenges: the selection of targets by PE firms and the selection of employees into leaving reviews. We address the firm selection concern in four ways. First, dynamic differences-in-differences event studies describe whether target firms appear to be on track toward the changes we observe. These show no pretrends and a discontinuous change after the buyout. Second, we conduct a falsification test with canceled deals, which show no effects, suggesting that unobservable firm characteristics attracting PE investors do not drive our results. More generally, we show that the observables PE firms target do not drive our results. Third, most of our results are across deal or employee characteristics, which addresses many selection stories. Fourth, we show that our results survive a host of robustness checks, such as a matching estimator whereby PE-owned firms are paired based on observables with never-PE-owned firms.

Nonetheless, PE firms clearly do not pick targets at random. Our causality argument focuses on treatment effects for the treated rather than external validity to a random firm in the economy. We show that treated firms were not on track to the outcomes we observe and would have continued, at least in the medium term, on their preexisting path in the absence of the LBO. This interpretation is important for social welfare as PE expands its footprint in the economy; even to the degree that target firms need LBOs because they are mismanaged, we shed light on how employee well-being relates to value creation in PE.

Employee selection is the second challenge. People visit the Glassdoor website for a reason, namely, to read and write company reviews. Our key assumption is that whatever makes a person write a review at an LBO target is the same as what makes them write a review at a never-PE-owned firm. To mitigate selection, we consider only reviews by current employees, limiting bias from laid-off employees. We also aggregate reviews to company–quarter averages, which addresses potential buyout-induced changes in the number of reviewers. Beyond that, we show that changes in the number and composition of reviewers around the buyout are, in practice, small and insignificant. Fortunately, the structure of the Glassdoor site is designed to generate quality reviews both because reviews are anonymous, which reduces incentives to misrepresent, and because Glassdoor imposes a give-to-get policy that requires a user to contribute a review in order to access reviews on the platform written by others: a mechanism that reduces both selection bias and the polarization inherent in online reviews (Marinescu et al. 2018).

We show that, after an LBO, employee satisfaction with compensation declines by 0.083 points on a one-to-five scale with a slightly more negative effect on satisfaction with firm culture and somewhat smaller negative effects on satisfaction with work–life balance and senior management. This relationship is economically meaningful: this effect, for example, is 57% larger than the reported gap in satisfaction with compensation between reviewers in college degree–requiring jobs and those in high school–requiring jobs. We also find discontinuous increases in the use of phrases such as "cost-cutting" and "uncertainty" around the deal date, which provides support for causality and specific operational mechanisms in addition to corroborating the topic analysis of Lambert et al. (2021). Despite significant declines in reported satisfaction with compensation, we can rule out that they occur in tandem with decreases in average pay.

We posit that one important mechanism behind the decline in satisfaction with compensation is higher risk. LBOs increase the risk employees face through several channels, ranging from higher powered incentives to dramatically higher firm leverage. First, higher leverage deals exhibit far more negative effects on job quality along all dimensions with moderate-to-no effects for low-leverage deals. The strong relationship for leverage contrasts with weak relationships for other deal characteristics, such as size and deal type. Leverage is also more important than whether a deal was associated with large one-time layoffs, which we examine using a novel panel of LinkedIn profile information collected specifically for PE-targeted firms. Whereas there is evidence of significant one-time layoffs at the time of many buyouts, both the satisfaction and leverage results are not fully rationalized by the realized layoffs. Our effects are concentrated among longer tenure, lower skill workers in

high-unemployment industries, workers with perhaps the greatest cost of risk bearing. Although we cannot identify a causal effect of leverage, we document that, following public firm recapitalizations, a situation outside PE in which firms quickly take on large amounts of new debt, simultaneously buying back shares or issuing large dividends, we also see satisfaction with compensation falling with new leverage.

Second, we show direct evidence for more risk pass-through by documenting that LBOs lead to increased usage of managerial performance pay. This increase in performance-based compensation is consistent with research showing manager incentives are a key part of value creation (Gompers et al. 2016).

Third, we document an increase in the degree to which firm returns are passed through to employees. To our knowledge, this work is the first to link PE returns to employee welfare. We accomplish this by coupling deal-level cash flows from StepStone with Glassdoor reviews. The best performing PE deals are associated with improved employee sentiment, implying that returns in these deals are not earned at the expense of employees. Although base pay remains flat, a 1% higher deal internal rate of return (IRR) is associated with 0.7% more variable pay. This aligns with previous literature showing that performance pay is sensitive to firm performance (Sockin and Sockin 2021, Di Maggio et al. 2022, Efing et al. 2023) and firm performance is positively associated with employee satisfaction (Verwijmeren and Derwall 2010, Bae et al. 2011, Edmans 2011, Guiso et al. 2015, Green et al. 2019, Edmans et al. 2024). We show further that the relationship between performance and satisfaction is three times larger for PE targets than for public firms. This suggests that employees at LBO targets are drastically more exposed to the firm's fortunes. Although they are strongly correlated ex post, employee predeal satisfaction levels do not robustly predict investor returns.

This paper joins other studies documenting how PE ownership affects workers, including Boucly et al. (2011) and Davis et al. (2014) on the number of employees, Agrawal and Tambe (2016) on career paths, Cohn et al. (2021) on workplace safety, Olsson and Tåg (2017) on unemployment incidence at LBO targets in Sweden, Fang et al. (2021) on wage gaps at LBO targets in France, and Antoni et al. (2019) on employment and wages at LBO targets in Germany. Our work builds upon this literature in four ways. First, we shed new light on the importance of nonpecuniary amenities, such as firm culture, which are important but largely unstudied (e.g., Lins et al. 2017, Mas and Pallais 2017, Lamadon et al. 2022). Second, our performance pay measure includes stock option grants and other types of variable pay that are typically difficult to observe. Third, we study a large and representative sample. As Morris and Phalippou (2020) note, the literature on PE ownership generally uses small and selected samples restricted to a single industry or European country with markedly different labor laws from the United States. Finally, we shed light on the important role of risk sharing between stakeholders in PE transactions.

This work shows how changes in ownership can affect workers' nonpecuniary amenities and perceptions of firm culture. In recent years, job quality has received more attention as a key outcome of interest. Its rising importance may reflect the advent of new information sources, such as "Best Places to Work" lists and information-sharing platforms such as Glassdoor, which can influence how workers sort throughout the labor market (Turban and Cable 2003, Benson et al. 2020, Sockin and Sojourner 2023). It may also, though, reflect the growing body of evidence that firms have a vested interest in investing in job quality. For instance, workers' separation decisions often reflect nonpecuniary aspects of work (Akerlof et al. 1988), more satisfied workers are more productive (Bellet et al. 2023), and workers may accept lower wages to enjoy improved workplace amenities (e.g., Stern 2004, Folke and Rickne 2022). Indeed, firms with more satisfied employees have better stock performance (Edmans 2011) and appear better able to weather large negative shocks (Li et al. 2021, Shan and Tang 2022). Whereas the literature separately studies how corporate ownership structures affect firm outcomes (e.g., Maksimovic and Phillips 2008, Cremers et al. 2009, Bena and Li 2014) and the importance of culture, management, and other nonwage amenities for workers (e.g., Maestas et al. 2018, Sockin 2022, Pacelli et al. 2024), rarely are the two examined together.[3] Given that employee well-being is one of its components, this work also speaks to the emphasis institutional investors place on environmental, social, and governance (ESG) (Lins et al. 2017, Barber et al. 2021).

In contemporaneous work, Lambert et al. (2021) also study PE and employee satisfaction using Glassdoor reviews though their focus is largely on the role of predeal ownership, sponsor firm fixed effects, and the relation between satisfaction scores and the free-response text submitted within a review of an employer. Whereas they similarly document an average decrease in employee satisfaction after an LBO, our study is distinct in exploring the economic mechanisms of the effect, that is, the deal-induced increase in uncertainty and greater risk sharing. In addition to augmenting the satisfaction ratings with the pay survey data at the individual respondent level, our paper uses more comprehensive and granular data on the deal characteristics from PitchBook (as compared with Capital IQ) that is matched to the deal-level investor returns.

2. Data Sources and Summary Statistics

We take data from several sources. Employee-level job quality variables are from Glassdoor reviews. The companies in Glassdoor are matched to PE deals from

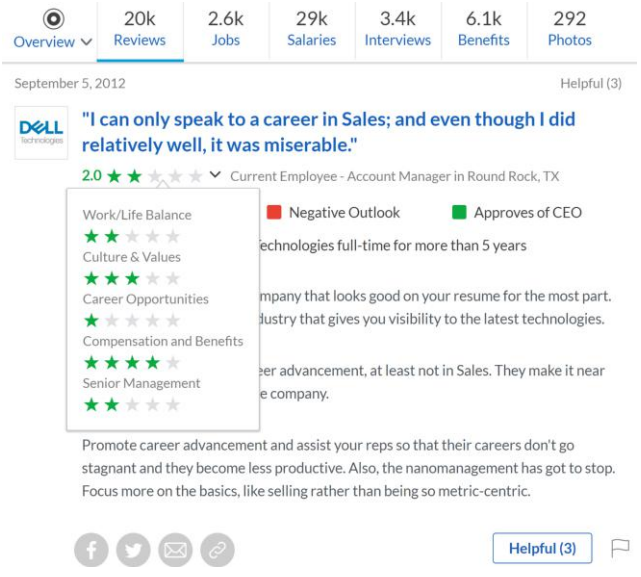
PitchBook, employment histories from LinkedIn, and investor returns from StepStone. We use Center for Research in Security Prices (CRSP) return data, CapitalIQ M&A deals, Compustat accounting data, and Preqin ESG data for supplemental analyses.

2.1. Employee Review and Pay Data

We begin with comprehensive employee review data from Glassdoor.com between the platform's January 2008 inception and 2019, the year before the COVID-19 pandemic. These data cover almost all major companies and contain measures of employee satisfaction and reported pay and benefits as well as the reviewer's job title, tenure, employment status, and location. Figure 1 offers an example of one of the reviews that composes the underlying data. We hover the mouse over the overall rating (a two out of a maximum of five) to view the dimension ratings. This reviewer gave Dell one point on career opportunities but four points on compensation and benefits.

Why do people review on Glassdoor? They are likely usually searching for jobs though they may also simply be interested in knowing more about their own company or another one. Glassdoor employs a give-to-get model, in which a review must be entered in order to view the reviews written by others on the website. This reduces the selection bias and polarization that are inherent in online reviews (Marinescu et al. 2018). Evidence of this is the fact that the distributions of ratings for the four categories exhibit central tendency, shown in Online Figure A.1. For example, the mean senior management rating is 3.4 with a standard deviation of 1.4 (recall that the range is one to five).

Figure 1. (Color online) Example Glassdoor Review



Note. This figure provides an example of a Glassdoor review retrieved on March 12, 2021, from glassdoor.com.

Nonetheless, reviewers are not randomly drawn from employees, a selection bias that poses a challenge to all survey-based papers. In our analysis, we make the key assumption that whatever makes a person write a review at an LBO target is the same as what makes the person write a review at a control firm. We take several steps to test this assumption and ensure our results are robust to violations of it. First, we address potential variation in the number of reviews at a company over time by performing analyses at the company–quarter level or the company–quarter–cohort level (for example, long-tenured employees at a firm in a quarter). Second, we drop former employees, the most obvious source of bias, although our results persist in that cohort. Third, most of our analysis focuses on heterogeneity in treatment, which mitigates concerns about selection because we are, for example, comparing longer to shorter tenured workers at a PE target relative to that same gap at a nontarget. Fourth, we replicate our main analysis with controls for key observable employee characteristics. Finally, in practice, we do not see significant changes in the number or composition of workers leaving reviews around buyouts (discussed more below).

Much of the existing research on how PE affects employees uses administrative sources, in particular, the employee–employer panels available at the U.S. Census Bureau and similar institutions in other countries. The major advantage of these sources is that they do not face the problem of selection into reviewing. However, employee reviews and Glassdoor data specifically offer insights that are not available from traditional administrative data. First, they shed light on corporate culture and other nonpecuniary amenities, which are completely absent from census data. Second, they contain information about performance pay, including stock compensation and benefits, whereas the census contains only base salaries. Third, observations are identified by the day, whereas the census wage data are annual. Even though annual frequency is consistent with that of the salary negotiations under normal circumstances, the real-time observability adds power and identification in the LBO event study context, also allowing us to disentangle immediate responses from enduring effects. The potential for greater power and sharper identification may not be realized, however, because of higher measurement errors endemic to such surveys (when compared with administrative data).[4] Mindful of the measurement error issue, we present a number of robustness tests that scrutinize our assumptions. Fourth, we observe granular occupation data, whereas the census data lack occupation information. Finally, Glassdoor data may be linked to proprietary information, such as our deal-level returns data, whereas census data are tightly sequestered. In using the Glassdoor data, we consider only U.S.-based current employees of U.S. companies, yielding a final sample of 3.3 million reviews, 1.1 million of which have

pay data in addition to satisfaction ratings, that covers 271,000 companies.[5] For these reviews, we focus primarily on seven dimensions. The first four are the employee satisfaction ratings with compensation and benefits, culture and values, senior management, and work–life balance, abbreviating the first two to "compensation" and "culture" for parsimony. These measures are quantified in numeric ratings, which range from one being the worst to five being the best.

The other three dimensions concern reported pay: base salary, variable pay, and an indicator for receiving any variable pay. This information is based on reports by workers who are prompted to report income earned through performance pay separately from income earned through base wages. We focus on pay reports from workers who also provided satisfaction ratings of their firm. For performance pay, workers can detail the amount and frequency of five types: cash bonuses, stock bonuses, profit sharing, sales commissions, and gratuities. We exclude gratuities and consider only the first four. Because we do not observe workers' hours, we restrict the pay analysis sample to full-time workers and consider annualized income, which we adjust to 2018 dollars using the U.S. consumer price index. To control for outliers, we restrict the sample to workers with reported incomes between $15,000 and $500,000. Further, to account for misreporting and avoid ambiguity in units, we exclude observations in which workers report earning less than 200 in any income category.[6] A worker's performance pay reflects how much they received for that year; we cannot discern to what extent their performance pay was guaranteed ex ante through contracting. In our regression analyses, we consider base and performance pay in log units.

Table 1, panel A, summarizes these seven outcomes. We present their correlations (plus that of separately provided benefits ratings) in Online Table A.1; the dimensions are correlated with one another in intuitive ways, but each contains independent information.[7]

Existing literature establishes that Glassdoor review data are both informative about firm outcomes and representative of the broader labor market, albeit somewhat skewed toward skilled occupations. Karabarbounis and Pinto (2018) show that the wage distribution of Glassdoor reviewers is consistent with external data from the U.S. Census Bureau. Sockin and Sockin (2019) show pay representativeness at the industry level, Gibson (2024) at the occupation level, and Martellini et al. (2024) at the university level. Sockin (2022) finds statistically significant correlations of about one half between Glassdoor and the National Longitudinal Survey of Youth 1997 in average job satisfaction across industries and occupations. In addition, Glassdoor reviews predict stock returns (Green et al. 2019, Sheng 2019), operating performance (Huang et al. 2020), and firm financing (Chemmanur et al. 2019), and they react to news of serious corporate events (see, for instance, Gadgil and Sockin 2020, Lee et al. 2021). In sum, we are confident that the ratings offer reasonably truthful information about the state of the company.

The Glassdoor data include each reviewer's tenure and job title, which we use to infer reviewer characteristics. These are summarized in Table 1, panel B. The data contain new hires and veteran employees with 25% of employees working no more than one year at the firm, 29% one to three years, 19% three to five years, and 26% five or more. We infer each worker's role using the worker's reported job title. Text matching reveals that one out of six workers reports a job title identifying as a manager. We merge job titles to Office of the Comptroller of the Currency codes using the mapping of Atalay et al. (2020).[8] About 14% of the matched raters are in jobs that typically require no more than a year of work experience, whereas at the other end of the spectrum, 23% are in jobs requiring more than five years of experience. The majority (76%) are in jobs that typically require a college degree, whereas 18% are in jobs that do not, and the remaining 6% are in jobs that require a masters or professional degree. We also observe to which of the 25 industries Glassdoor assigns the firm.[9] Finally, we use the free-response text respondents include in their reviews to observe whether the incidence of certain phrases changes following an LBO.

We construct company–quarter–level variables using the review-level information. Although users can leave multiple reviews, the vast majority do not, and so we abstract from reviewer identity. For each outcome of interest, we take the mean across reviews within each company quarter (the results are robust to using the median). For worker characteristics such as tenure or education, which we employ as right-hand side variables for heterogeneity analyses, we split the sample at logical breakpoints (e.g., jobs that require a college degree versus those that do not) and then calculate the mean company–quarter–level outcome separately for each group.

2.2. PE and Acquisition Deal Data

To obtain PE deal information, we manually match Glassdoor firms to PE deal targets in the PitchBook database. We focus on targets based in the United States with a transaction date between 2010 and 2016 that have Glassdoor reviews on both sides of the deal. PitchBook is widely regarded as one of the most comprehensive PE databases and is especially robust regarding U.S. data and the most recent decade. In turn, we exclude non-U.S. deals, secondary transactions (in which company ownership is transferred between two PE investors), private investment in public equity and other investments in companies that were not taken private, and debt-only deals. This gives us a total of 7,701 deals.

Table 1. Summary Statistics

Panel A. Glassdoor employee review scores and earnings

	All			Ever-LBO sample			Control sample		
Variable	*N*	Mean	Standard deviation	*N*	Mean	Standard deviation	*N*	Mean	Standard deviation
Co-quarter level									
Number of reviews	865,723	4.19	18.76	23,261	5.74	13.16	842,462	4.15	18.89
Average compensation and benefits	859,501	3.42	1.15	23,184	3.18	1.03	836,317	3.43	1.15
Average work–life balance	861,251	3.64	1.17	23,212	3.45	1.07	838,039	3.64	1.17
Average culture and values	769,555	3.68	1.28	19,626	3.42	1.18	749,929	3.69	1.28
Average senior management	852,586	3.37	1.32	23,093	3.10	1.20	829,493	3.38	1.33
Average base earnings	334,668	61,148	34,128	8,846	60,767	33,304	325,822	61,159	34,150
Average variable earnings	334,668	4,551	14,408	8,846	5,949	16,384	325,822	4,513	14,349
Average any variable earnings	334,668	0.260	0.387	8,846	0.317	0.392	325,822	0.259	0.387
Review level									
Compensation and benefits	3,306,724	3.48	1.26	120,257	3.23	1.30	3,186,467	3.49	1.25
Work–life balance	3,313,771	3.59	1.32	120,361	3.47	1.36	3,193,410	3.60	1.31
Culture and values	3,042,656	3.71	1.39	110,110	3.49	1.45	2,932,546	3.72	1.39
Senior management	3,226,962	3.36	1.44	117,528	3.18	1.48	3,109,434	3.36	1.43
Base earnings	1,102,209	63,826	40,400	30,559	57,735	38,085	1,071,650	63,999	40,451
Variable earnings	1,102,209	5,804	19,129	30,559	5,977	20,680	1,071,650	5,799	19,083
Any variable earnings	1,102,209	0.303	0.460	30,559	0.306	0.461	1,071,650	0.303	0.460

Panel B. Employee characteristics

	All			Ever-LBO sample			Control sample		
Variable	*N*	Mean	Standard deviation	*N*	Mean	Standard deviation	*N*	Mean	Standard deviation
Reported tenure									
Tenure ≤1 years	2,395,634	0.25	0.43	82,972	0.28	0.45	2,312,662	0.25	0.43
Tenure 1–3 years	2,395,634	0.29	0.46	82,972	0.30	0.46	2,312,662	0.29	0.46
Tenure 3–5 years	2,395,634	0.19	0.39	82,972	0.19	0.39	2,312,662	0.19	0.39
Tenure >5 years	2,395,634	0.26	0.44	82,972	0.23	0.42	2,312,662	0.26	0.44
Reported job title									
Is managerial	2,319,534	0.16	0.36	79,809	0.17	0.38	2,239,725	0.16	0.36
Typically requires ≤1 years experience	1,529,258	0.14	0.35	52,509	0.14	0.35	1,476,749	0.14	0.35
Typically requires 1–3 years experience	1,529,258	0.27	0.44	52,509	0.26	0.44	1,476,749	0.27	0.44
Typically requires 3–5 years experience	1,529,258	0.37	0.48	52,509	0.34	0.47	1,476,749	0.37	0.48
Typically requires >5 years experience	1,529,258	0.23	0.42	52,509	0.26	0.44	1,476,749	0.23	0.42
Typically requires only high school	1,529,258	0.18	0.39	52,509	0.21	0.41	1,476,749	0.18	0.39
Typically requires college	1,529,258	0.76	0.43	52,509	0.75	0.43	1,476,749	0.76	0.43
Typically requires masters/professional	1,529,258	0.06	0.24	52,509	0.04	0.19	1,476,749	0.06	0.24
Reported experience									
Years of experience	1,102,209	6.31	6.82	30,559	5.96	6.57	1,071,650	6.33	6.83

Panel C. Deal statistics

Variable	*N*	Mean	Standard deviation
By deal type			
Vanilla LBO	1,371	0.73	0.44
Public to private	1,371	0.11	0.31
Corporate divestiture	1,371	0.16	0.36
By PitchBook industry sector			
Business products/services	1,371	0.32	0.47
Consumer products/services	1,371	0.23	0.42
Energy	1,371	0.02	0.13
Financial services	1,371	0.04	0.20
Healthcare	1,371	0.15	0.35
Information technology	1,371	0.23	0.42
Materials and resources	1,371	0.01	0.10

Table 1. (Continued)

Panel C. Deal statistics

Variable	*N*	Mean	Standard deviation
Deal characteristics			
Deal size (USD m)	547	723.09	1,755.61
Leverage	241	0.52	0.35
Number of employees	788	2,026.61	7,435.39
Impact fund	1,371	0.03	0.18
ESG/impact fund	1,371	0.13	0.34

Panel D. Deal-level investor returns

Variable	*N*	Mean	Standard deviation	p5	p50	p95
Fund size (USD b)	351	3.49	4.58	0.21	1.29	14.68
Fund IRR quartile	333	2.73	0.76	1.00	3.00	4.00
Fund IRR	333	0.21	0.13	0.06	0.19	0.40
Deal amount invested (USD b)	351	0.15	0.19	0.01	0.09	0.55
Deal IRR	351	0.40	0.52	−0.15	0.29	1.36
Deal IRR rank within fund	351	0.52	0.27	0.01	0.56	0.91
Deal IRR rank within quarter	351	0.59	0.28	0.05	0.65	0.95
Deal TVM	351	3.03	2.74	0.22	2.28	8.21
Deal TVM rank within fund	350	0.56	0.28	0.01	0.61	0.93
Deal TVM rank within quarter	350	0.63	0.28	0.05	0.70	0.95
Deal PME vs. Russel 2000 style	338	2.24	2.09	0.12	1.66	5.68
Deal PME vs. Russel 2000 sector	338	2.02	1.93	0.02	1.53	5.35
Mimicking public co IRR	1,528	0.09	0.28	−0.33	0.09	0.51
Mimicking public co TVM	1,528	1.66	1.36	0.32	1.31	4.19
Mimicking public company PME	1,528	1.06	0.83	0.15	0.90	2.46

Notes. This table presents summary statistics. Panel A presents statistics on each Glassdoor rating dimension, reported pay, and the number of reviews at the company–quarter and review level. The ever-LBO sample is all the companies with LBO deals in our main analysis, including vanilla LBOs, public-to-private deals, and corporate divestitures (together, these include 1,371 deals). The control sample is nontargeted companies. Panel B presents statistics on employee characteristics. Reported tenure is the length of employment as reported on Glassdoor. Years of experience captures a worker's self-reported years of (relevant) experience. Whether a reviewer's reported job title is managerial and the work experience and education it typically requires are calculated as discussed in Section 2. Panel C presents deal characteristics of the PitchBook-Glassdoor matched deals in our main analysis. Panel D describes the investor return data from the StepStone SPI database that is matched to Glassdoor company and PitchBook deal information. The last three lines describe the returns metrics for the mimicking public equity investments. We follow Korteweg and Nagel (2016) to construct cash flows out of stock returns from CRSP to mimic the patterns observed in PE. We take the closest five matches for each LBO using the distance metric from Abadie and Imbens (2006). PMEs are calculated relative to the relevant Russell 2000 Sector Index.

Within this sample, we consider three mutually exclusive PE business models. The first is 3,572 LBOs. In an LBO, a PE fund takes a controlling stake in the target company. The PE firm typically uses mostly borrowed money to purchase the company, organizing the transaction so that the debt is placed on the target company's balance sheet. That is, the target company owes the money used to purchase it, not the PE fund. The second set is 484 management buyouts (MBOs). In an MBO, the existing managers of a company purchase a controlling interest with the help of a PE firm. MBOs typically increase company leverage as well but tend to be in the lower part of the LBO leverage distribution. The third set is 2,934 growth equity investments. Growth equity is closer to venture capital but for later stage companies and involves the PE firm taking a noncontrolling stake in a company. Here, the cash from the investment goes to the company rather than to selling shareholders. We retain only a company's first deal in each of these three categories.[10]

Our main analysis focuses on LBOs, which are, at a conceptual level, the strongest PE treatment as it normally entails a complete change in control unlike growth equity or management buyouts. We divide LBOs into three mutually exclusive categories based on PitchBook's deal classification scheme: public-to-private deals, corporate divestitures, and vanilla LBOs. In a public-to-private deal, PE investors purchase and take private a public company. Although small in number, these deals account for many of our reviews as the target companies tend to be large. Corporate divestitures occur when a PE firm acquires a subsidiary of a corporation and either holds it as a stand-alone firm or rolls it into another existing company. They have significant cultural and management implications because they typically cause the corporate form to change from

a diversified conglomerate to a more focused firm. We define a vanilla LBO as any other PE purchase of a stand-alone, privately held company.

We successfully match 2,762 (77%) of the qualifying LBOs to Glassdoor companies, 405 of the MBOs (84%), and 1,927 of the growth equity deals (66%). Of these matches, 1,371 LBO, 178 MBO, and 700 growth equity deals have at least one review by a current employee before and after the deal. Table 1, panel C, provides summary statistics about the final sample of matched LBOs that are used in our analysis. Online Table A.2 contains summary statistics about the growth equity and MBO samples. Online Table A.3 compares deal characteristics across all PitchBook deals, Glassdoor matched deals, and our analysis sample. The matched deals are reasonably representative of the full data set. They have a similar industry breakdown; for example, 16% (15%) of deals in our matched (full) sample are in healthcare.[11] Figure 2 shows that the matched deals are roughly uniformly distributed across our sample period. The coverage of these data for the broad U.S. economy offers a strength over previous work that has targeted small countries or specific industries.

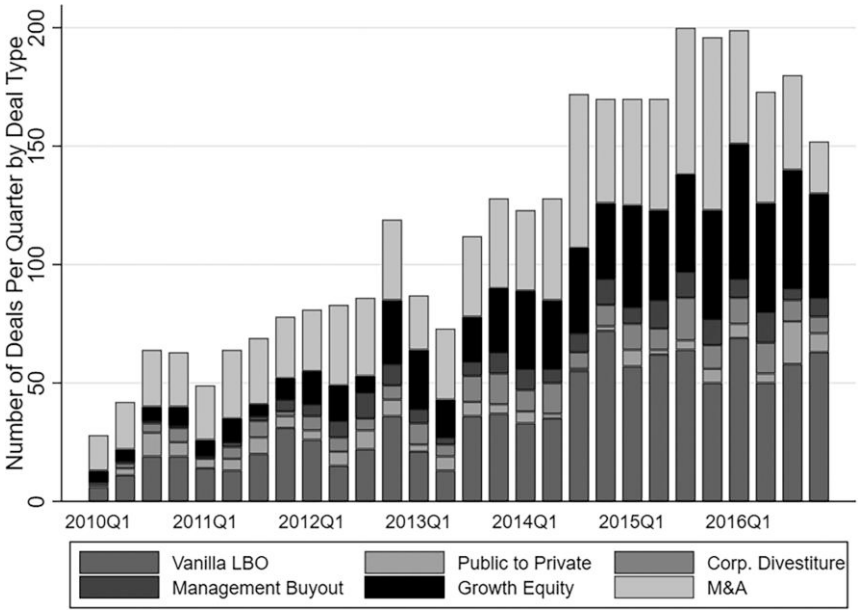
To understand the role of employee departures around LBOs, we obtain LinkedIn data from the analytics firm LIX on all employees who ever worked for a subset of the LBO targets. We restrict this analysis to the 618 targets in the LBO sample that have at least five reviews and a review by a current employee before and after the deal. We match 381 firms and record LinkedIn job titles and years for 457,087 of those firms' employees. We use this to create a firm–year panel of employment growth rates, departure rates, and hiring rates.[12]

2.3. Investor Return Data

We use investor return data from StepStone Group, which has built its StepStone Private Market Intelligence (SPI) database, providing fund-of-fund and advisory services in private markets since 2006. These data come from performing due diligence and monitoring investments, similar to other academic sources of deal-level PE return data (Robinson and Sensoy 2013, Degeorge et al. 2016, Braun et al. 2017). StepStone requires fund managers to report returns from all deals and reconcile them with fund-level performance, which mitigates the bias toward more successful deals that is suffered by data sets that allow selective reporting. We use deal-level internal rate of return (IRR), total value multiple (TVM), and public market equivalents (PMEs) with the latter being calculated against the industry-specific Russell 2000 Indexes using reported cash flows following Kaplan and Schoar (2005). As with Glassdoor ratings, we measure returns at or before March 2020 to avoid the confounding effects of the COVID-19 pandemic.[13]

We match 26% (351) LBOs in our main sample to SPI, a proportion broadly consistent with SPI's coverage of the LBO universe. Summary statistics for the matched sample are reported in panel D of Table 1. This matched sample shows comparable performance statistics to both the Preqin database and SPI itself.[14]

Figure 2. Deal Sample Overview



Notes. This figure presents the number of deals per quarter in the main LBO sample that we include in our analysis, which are PitchBook deals matched to Glassdoor that occur between 2010 and 2016 and comprise vanilla, public-to-private, and corporate divestiture LBOs. The figure also shows three other deal types: management buyouts, growth equity deals, and M&As.

2.4. Additional Data Sources

For supplemental analysis, we use data on stock returns from CRSP; data on conventional acquisitions from CapitalIQ; data on public firms from Compustat, which we use to identify public debt recapitalizations; and data on funds from Preqin's ESG module. These are introduced when they are used.

3. Empirical Approach

We are interested in the causal effect of LBOs on employee outcomes. The two major analytical challenges are nonrandom selection of targets by acquirers and nonrandom selection of employees into writing reviews. To address the first issue, we use a differences-in-differences design, test for pretrends using event studies, test for targeting drivers, and conduct a falsification test with canceled deals. Whereas there are likely unobservables that drive PE targeting, these steps support causality within the treated population. To address the second challenge, we construct measures at the company level, focus on cross-group heterogeneity, and test and control for composition changes in characteristics of interest.

Our baseline test is a differences-in-differences specification at the company–quarter level. The first difference compares targets before and after the buyout, whereas the second difference compares targets to firms that were never PE owned. The empirical specification is outlined by Equation (1):

$$\overline{Y}_{j,q} = \beta\ \mathbb{1}(\text{Post Deal}_{j,q}) + \alpha_j + \gamma_{n(j),q} + \varepsilon_{j,q}. \quad (1)$$

Here, $\overline{Y}_{j,q}$ is the year–quarter q average of a particular outcome for company j that operates in industry $n(j)$, such as the average compensation rating given to Dell Technologies in 2012Q3, which operates in the information technology industry. The indicator variable $\mathbb{1}(\text{Post Deal}_{j,q})$ is one if firm j is PE owned in year–quarter q and zero if not. The coefficient of interest, β, captures the relationship between PE ownership and $\overline{Y}_{j,q}$. We use company fixed effects, α_j, to control for cross-company differences and industry–quarter fixed effects, $\gamma_{n(j),q}$, to control for industry-level time trends. We use only reviews from employees who report working for the firm at the time of their review.

To understand how different types of employees are impacted, we construct company-by-quarter-by-cohort average outcomes, $\overline{Y}_{j,q,c}$, and test the interaction of the deal effect with the cohort characteristic. For example, to study short- versus long-tenured employees, we calculate the average outcome at the company–quarter level separately for these two groups of employees. Then, we use the model in Equation (2) to assess the interaction effect, where X_c represents an indicator for whether the group is composed of short- or long-tenured employees and δ, our coefficient of interest, represents the differential effect on that group:[15]

$$\overline{Y}_{j,q,c} = \delta\,\mathbb{1}(\text{Post Deal}_{j,q}) \times X_c + \beta\ \mathbb{1}(\text{Post Deal}_{j,q}) + \omega\ X_c + \alpha_j + \gamma_{n(j),q} + \varepsilon_{j,q,c}. \quad (2)$$

We also use event studies to scrutinize the identifying assumption that LBO-targeted and never-LBO-targeted companies exhibited parallel trends before the buyout. We plot the coefficients β_s from the following equation:

$$Y_i = \sum_{s \neq -1} \beta_s\ \mathbb{1}(\text{Deal in } q - s \text{ Quarter}_{j,q,s}) + \alpha_j + \gamma_{n(j),q} + \varepsilon_i. \quad (3)$$

Here, $\mathbb{1}(\text{Deal in } q - s \text{ Quarter}_{j,q,s})$ is an indicator variable equal to one if an LBO of company j occurred s quarters prior to quarter q and zero otherwise. The quarter before the deal is omitted.[16] In supplemental tests, we conduct the main analysis at the review level, which weights larger firms more heavily (note that the benchmark model weights all company–quarters equally).

4. Effect of LBOs on Job Quality

Our primary focus is testing the effect LBOs have on job quality. In Table 2, we show that LBOs have strong negative effects on all four satisfaction dimensions. Employees' satisfaction with compensation declines by 0.083 points after a buyout (column (1)). This is economically significant; for example, it is 57% larger than the 0.053 gap we observe between jobs that typically require a college degree and jobs that require only high school completion (Section 5.2). As a second benchmark, this effect is equivalent to the decline in satisfaction associated with a 33% lower IRR (Section 5.1). LBOs have a similar negative effect on employee perceptions of the firm's culture (column (3)); to our knowledge, this result offers the first evidence of how PE affects corporate culture. The negative effects on work–life balance and senior management are slightly smaller (columns (2) and (4)). Surprisingly, we find no effects on realized pay (columns (5)–(7)), and the discrepancy does not reflect a cut to benefits.[17] In Section 5, we propose a mechanism to explain why, if LBO investors do not reduce employee pay, we may observe a decline in satisfaction with compensation.

We use Equation (3) to test the identification assumption that LBO targets were not already on track to experience declines in employee satisfaction, in which case the declines would start before the LBO quarter. The results, shown in Figure 3, contain no pretrends for any of the four measures for both the overall sample (left) and the sample of LBOs with high leverage (right). Visually, these show clear, persistent decreases in satisfaction in the quarters immediately after the buyout. Event studies for reported pay also exhibit no pretrends (Online Figure A.3).

Table 2. Effect of LBOs on Job Quality

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.083***	−0.064***	−0.093***	−0.063***	0.011	0.119	0.012
	(0.018)	(0.019)	(0.025)	(0.022)	(0.011)	(0.095)	(0.011)
Observations	859,501	861,251	769,555	852,586	331,647	331,647	331,647
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.355	0.326	0.352	0.347	0.618	0.454	0.433
Outcome standard deviation	1.145	1.170	1.277	1.323	0.504	3.468	0.386

Notes. This table reports the effect of LBOs on job quality measures. We use company–quarter average reviews and reported pay as the dependent variable (Equation (1)). All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

The Borusyak et al. (2024) imputation estimator allows us to both formally test for pretrends and account for the potential bias created by heterogeneous treatment effects (see, for example, Goodman-Bacon 2021, Sun and Abraham 2021). Online Figure A.4 shows that our event studies are largely unchanged with negative impacts on all employee satisfaction measures appearing after the deal. As shown in Online Table A.6, none of the eight specifications shows pretrends that are statistically significant at the 5% level.[18] Further, Online Table A.7 shows that our results persist if we were to instead estimate a stacked regression—an additional approach to correct for possible biases in differences-in-differences models with staggered treatment adoption—as in, for example, Gormley and Matsa (2011) and Cengiz et al. (2019).

4.1. Layoffs

An immediate question is whether the results we document reflect layoffs. Although it is commonly asserted that LBOs are associated with layoffs, the academic evidence is conflicted.[19] We use a novel LinkedIn panel (see Section 2) to understand whether layoffs occur in our setting and the extent to which layoffs drive our results. Although the LinkedIn data do not necessarily capture all workers at the firm as administrative employee–employer data would, the LinkedIn data nevertheless appear to capture meaningful post-LBO dynamics. For instance, Davis et al. (2019) find that employment growth rises after two years following a private-to-private LBO and stumbles following a public-to-private LBO. In the LinkedIn data, we find a similar pattern: private-to-private LBOs exhibit significantly faster employment growth following the deal than public-to-private LBOs.[20]

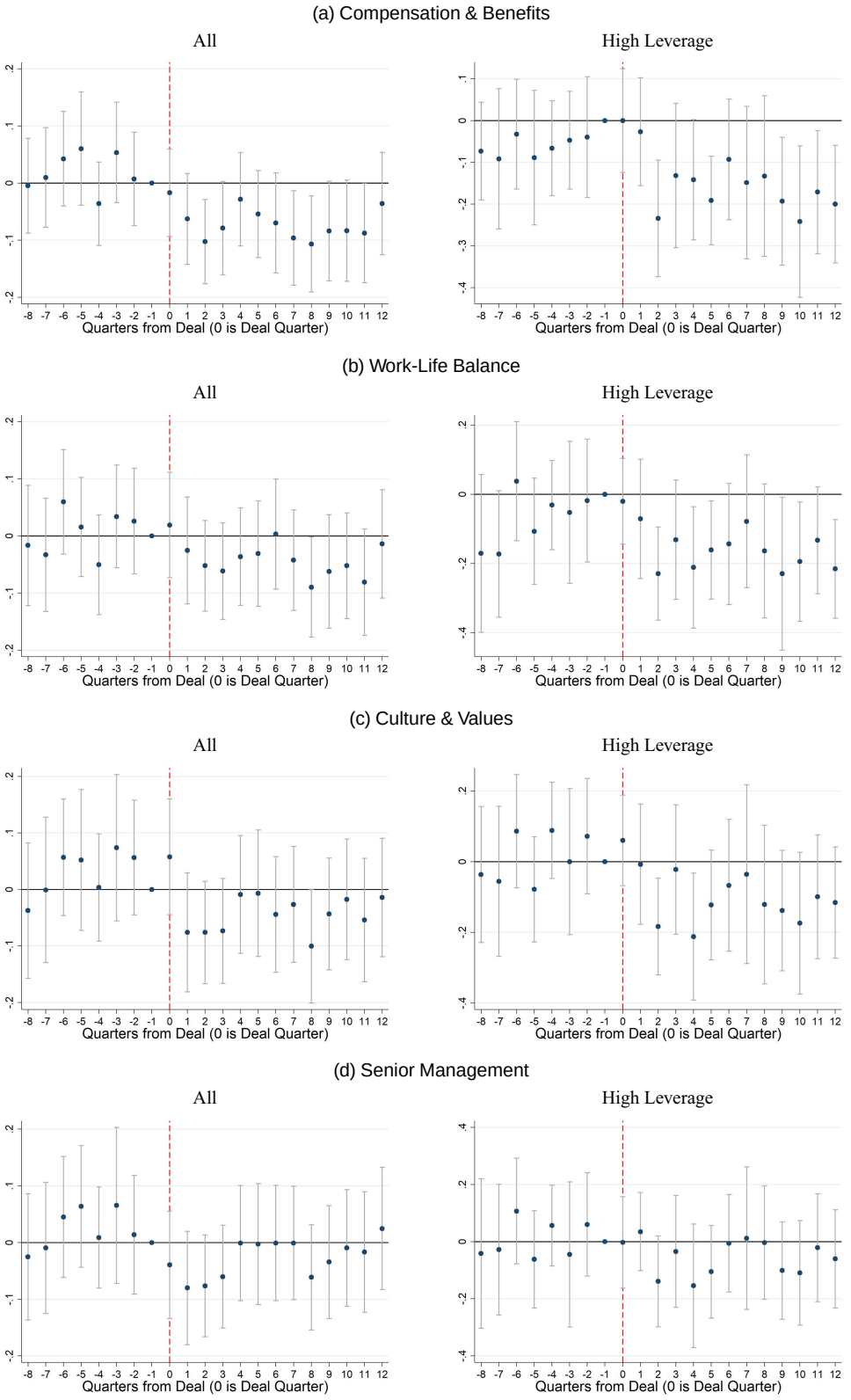
Within the LinkedIn data, Online Figure A.5, panel (a), shows that the departure rate increases in the first year after the buyout and returns to baseline during the second year. There is no increase in the hiring rate (panel (b)), and therefore, the overall employment growth rate declines (panel (c)). These patterns also appear in the number of reviews, in which there is some evidence of a short-term decline in the number of reviews by current employees (Figure 4(a)) and increases in the number of reviews by former employees (Figure 4(b)). After the first two quarters, however, the number of reviews reverts to the previous levels. Therefore, LBOs do not appear to alter the aggregate number of current employee reviews (Online Table A.8).

We first ensure that compositional changes related to layoffs are not fueling our results. First, we drop former employees from our main analysis to prevent an increase in disgruntled, recently laid-off employees from driving our results. Relatedly, in panel A of Table 3, we consider former employees separately and show they have a similarly sized negative effect, suggesting that satisfaction is not too contaminated by one's own unemployed, possibly by layoff, status.[21] Finally, in Section 6.2 we explicitly test for changes in observable characteristics.

Given these patterns, we also examine whether layoffs are driving the changes in satisfaction. It is natural that current layoffs could negatively affect stayers' welfare if, for example, the workplace becomes demoralized, current layoffs increase concerns over future ones, or remaining workers need to exert more effort to fill the slack left behind by those that left. Indeed, Ayas and Arslan (2023) show that Glassdoor ratings for work–life balance and culture fall faster for companies that experience large-scale layoffs compared with those that experience moderately sized layoffs.

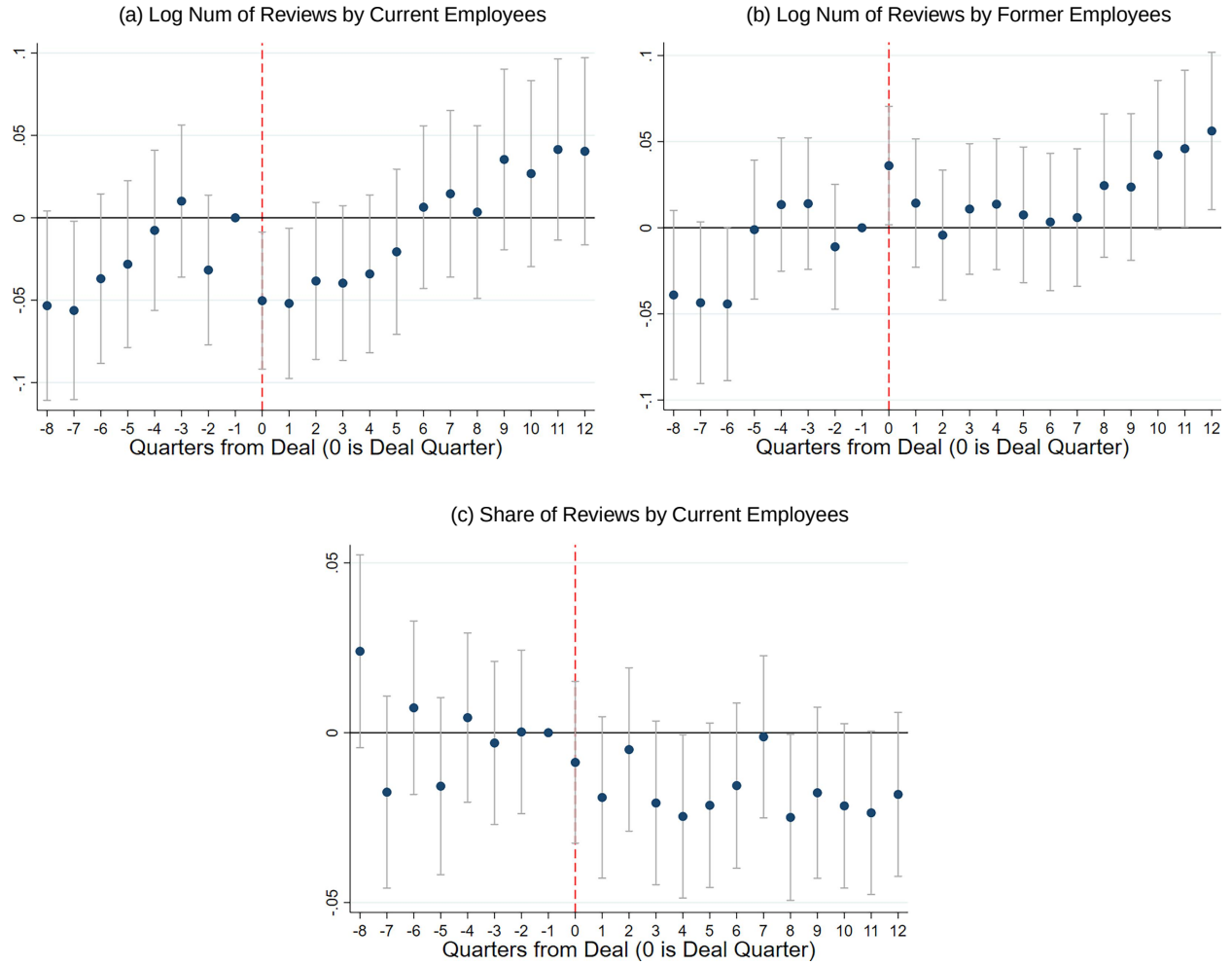
We test this in Online Table A.9 by interacting our post-LBO indicator with whether the deal had a below-median change in the LinkedIn departure rate between the four years prior to and after the deal. If layoffs are the principal factor underlying the decline in job quality, the effect should be concentrated in LBOs with relatively high layoffs. We observe a broadly significant decline in job quality following an LBO with high layoffs. But, whereas the interactions are positive, we cannot rule out

Figure 3. (Color online) Event Study of Effects of LBOs on Dimensions of Employee Satisfaction: All Deals and High-Leverage Deals



Notes. This figure presents differences-in-differences event studies of the effect of LBOs on four dimensions of employee satisfaction. Under each panel, we plot event studies using all deals (left) and high-leverage deals (right). The unit of observation is a review, and we present separate coefficients for 8 quarters before and 12 quarters after the buyout. The regression is fully saturated, including dummies for all quarters around the buyout. We omit quarter −1 (the quarter before the buyout). All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

Figure 4. (Color online) Event Study of Effects of LBOs on Number/Share of Reviews by Employment Status



Notes. This figure presents differences-in-differences event studies of the effect of LBOs on number/share of reviews by employment status. The unit of observation is a company-quarter, and we present separate coefficients for 8 quarters before and 12 quarters after the buyout. The regression is fully saturated, including dummies for all quarters around the buyout. We omit quarter −1 (the quarter before the buyout). All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

that low-layoff LBOs exhibit similar decreases along each dimension. Along the same lines, our key results reflect long-term changes that would occur after the temporary layoffs documented above (comparing Figure 3 and Online Figure A.5). Thus, whereas one-time layoffs may explain some of the negative effects on employee satisfaction, they cannot fully rationalize the declines we document.

We cannot rule out that one-time layoffs may drive some of the negative effects on average satisfaction. That said, our results persist for LBO targets with low layoffs, declines in satisfaction persist long after the immediate layoffs, and the LBO-associated layoffs themselves do not seem to affect current employees' propensity to review.

Beyond the effects of one-time layoffs, it seems plausible that lower satisfaction may stem from a permanent decrease in job security, which would constitute a genuine decrease in job quality. Importantly, sentiment may decline even if future layoffs do not ultimately occur if workers exhibit heightened concern that such layoffs will eventually occur. Below, we introduce a mechanism in which greater concern about future layoffs should the firm experience distress (e.g., bankruptcy) drives lower satisfaction, especially with compensation. Here, a key mediating force would be leverage because it increases the chance of distress and, thus, future layoffs. Layoffs at the time of the buyout are orthogonal to this future job security mechanism, and thus, through this lens, it follows that we may not observe meaningful heterogeneity in layoffs at the time of the buyout.

The natural complement of layoffs is new hires. In panel B of Table 3, we look at how LBOs impact new hires compared with existing employees. More specifically, using review-level data, we compare the effect of an LBO on current employees who started after the PE

Table 3. Effect of LBOs on Job Quality of New Hires and Former Employees

Panel A. Former employee sample

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.040*	−0.056**	−0.034	−0.064**	0.010	0.031	0.005
	(0.021)	(0.024)	(0.030)	(0.025)	(0.024)	(0.189)	(0.022)
Observations	500,529	501,717	455,935	498,064	49,141	49,141	49,141
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.441	0.437	0.457	0.421	0.672	0.524	0.507
Outcome standard deviation	1.137	1.225	1.329	1.296	0.503	3.303	0.370

Panel B. Interaction with whether employee is hired after deal

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.081***	−0.044*	−0.083**	−0.058*	0.004	−0.000	0.002
	(0.026)	(0.024)	(0.033)	(0.033)	(0.014)	(0.130)	(0.015)
𝟙(Post LBO) × 𝟙(New Hire)	0.059***	0.063***	0.089***	0.070***	0.008	0.048	0.001
	(0.019)	(0.017)	(0.022)	(0.023)	(0.012)	(0.125)	(0.012)
Observations	2,096,939	2,101,478	2,089,269	2,074,669	714,874	714,874	714,874
Tenure fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.241	0.206	0.220	0.241	0.505	0.321	0.300
Outcome standard deviation	1.273	1.328	1.391	1.450	0.582	4.221	0.462

Notes. Panel A repeats our main analysis using former employees instead of current employees. Employee reviews are assumed to be as of the employee's departure if that is reported. Panel B shows the effect of an LBO interacted with whether the employee is hired after the deal, using review-level data. We use review date, deal date, and job tenure to determine if an employee is hired after the deal. An employee is hired after the deal if the distance between the deal date and review date is longer than that employee's job tenure could be. We include tenure fixed effects to control for bias. Panel A is a company–year panel and includes company and industry–year fixed effects as we only observe an employee's year of departure. Panel B includes company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

deal (new hires) with current employees who started before the deal (predeal employees).[22] The first row of coefficients documents large negative effects among current employees who were hired before the deal. Adding these estimates to the significantly positive coefficients for new hires in the second row indicates that new hires are unaffected. This points away from employee-unfriendly operational changes as a mechanism in that new hires would be exposed to such changes as well.

4.2. Models of Surplus Transfer

Our core result is that employees are less satisfied with their jobs after LBOs. A natural question is the extent to which employee losses are offset by gains to capital. In the most extreme negative case, LBOs could both make employees unhappy and destroy value for capital and other stakeholders. Such a comovement of satisfaction and returns would be consistent with much of the existing employee satisfaction literature. Broadly, this literature shows an association between firm outperformance and positive employee outcomes (Edmans 2011, Welch and Yoon 2023). Perhaps closest to us, Gadgil and Sockin (2020) argue that corporate scandals decrease both employee satisfaction and firm value. However, LBOs hurting both capital and labor is inconsistent with the significant outperformance of PE investments shown in the literature. Whereas there is ongoing debate on whether the net-of-fee performance to buyout fund investors is high enough on the risk-adjusted basis (see Kaplan and Sensoy 2015, Korteweg 2023 for recent surveys), the evidence of on average high and persistent gross-of-fee returns is not disputed (Braun et al. 2017, Phalippou 2020) and points to the skill-based explanations thereof (Berk and Van Binsbergen 2015). If anything, that makes the decrease in employee satisfaction even more surprising.

Although capital benefits and labor suffers, it is unclear ex ante whether this is efficient. There are numerous channels through which inefficient, negative-sum value transfers could occur. Leverage is perhaps a central example with a major benefit of leverage being tax savings (a transfer) (Miller 1977) and the major costs

of leverage being created for other stakeholders, including employees facing a higher risk of job loss (Graham et al. 2023), government losing tax revenue, and counterparties facing the loss of a relationship (although other benefits of leverage such as incentives may also play a role). Price increases and aggressive negotiation are other potential actions, consistent with evidence on healthcare (Gupta et al. 2021, Liu 2021) and colleges (Eaton et al. 2020). Wages are a type of price that is particularly relevant to our setting; if private equity firms are better able to exploit monopsony power or holdup over their employees, they could create value for capital, lowering overall production because of morale and incentives.

Alternatively, LBOs could engage in actions that both create value in aggregate and make employees worse off. Layoffs offer a central example as they clearly produce negative externalities yet are a natural part of socially valuable Schumpeterian creative destruction. Classic agency stories offer another example: if PE firms reduce wasteful shirking or diversion, employees might be hurt, yet the gains to capital might outweigh that loss. Incentive pay is potentially another example, potentially increasing productivity but making workers unhappy because of risk bearing.

Such transfers from labor to capital are possible because of the sticky nature of worker–firm relationships. Although workers may lose, they may not lose enough to switch to a new job. For one, switching jobs is costly. Relocating to a new employer not only requires time and effort applying to vacancies, but could necessitate a pay cut, a longer commute, or dislocating from valuable social networks. Second, low-wage workers tend to be overly pessimistic about the wages they would earn from their outside options, which would contribute to increased firm entrenchment (Jäger et al. 2024). Third, workers struggle to perceive the nonpecuniary aspects of other employers (Sockin and Sojourner 2023), which renders jobs experience—rather than inspection—goods (Menzio and Shi 2011) and ultimately induces uncertainty in the match quality promised by outside options. Fourth, workers differ in how much they value job amenities with low- and middle-income workers placing comparatively less monetary value on such attributes (Sockin 2022). Taken together, the reduction in amenity value workers experience following an LBO may be more tolerable than undergoing the costly and uncertain process of switching employers.

Even if these transfers benefited capital, there are several reasons firms may not have implemented these changes. The most direct story is different objectives, by which private equity owners might simply value worker happiness less than the prior owners. Even more cynically, perhaps poor incentives led to managers living Hicks' (1935) "quiet life" (Schoar 2002, Bertrand and Mullainathan 2003) and a deferral of unpleasant reforms. Related is the view of Shleifer and Summers (1988) that acquirers can exploit opportunities to break implicit promises to employees that were established by the previous owners. Finally, more positively, the new managers may have the skill or capital to implement operational improvements that some employees experience negatively (Kaplan 1989, Bloom et al. 2015). Our results that new hires are essentially unimpacted are most consistent with a Shleifer and Summers (1988) model. Under such a model, only previous employees would suffer, whereas it is more natural to think all employees would suffer from employee-unfriendly operational changes. These channels are, of course, inherently linked. A PE firm buying a mature family firm might care less about reputation and social impact, offer stronger incentives to more skilled managers, and cut wages or staff in violation of implicit contracts (Sraer and Thesmar 2007, Ellul et al. 2018).

There is also a natural question of what form these transfers take. Reduced pay seems like the most natural channel and has been a focus of previous work, yet we observe minimal effects on pay. Layoffs are another commonly studied impact; however, we exclude former employees and find only weak associations between our effects and current layoffs. Work environment changes, ranging from higher expectations to cultural changes, are another natural channel and often understudied. In the following section, we focus on risk as a core explanation of our findings. Whereas risk is key to finance, it is understudied in relation to labor and PE and, as we show, is most consistent with our findings.

5. Risk Sharing

Since the rise of agency theory, the degree to which employees and employers share risk has been a topic of great interest (Ross 1973, Holmström 1979). Intuitively, exposing employees to greater risk, whether through incentive pay, firm leverage, or profit sharing, can strengthen incentives and increase productivity (Lazear 2000). At the same time, employees are generally more risk averse than the firm, and incentive pay increases the risks that employees bear. Higher leverage increases the chances of firm distress, which leads to layoffs, which, in turn, imposes large costs on workers who are not fully insured and face large switching costs to find an equivalent job (Cantor 1990, Asquith et al. 1994, Sharpe 1994). Under risk-bearing models, workers demand compensating wage differentials to offset that risk (Abowd and Ashenfelter 1981, Hamermesh and Wolfe 1990).

Aligning incentives is one of the key ways that PE managers claim to add value. We focus on three ways these transactions impact owner–employee risk sharing. First, we show that LBOs increase the sensitivity of employee compensation satisfaction to LBO investor returns, and the satisfaction decreases that we observe are driven by poorly performing deals. Second, we show that our LBO effects are driven by high-leverage deals

and workers with relatively poor outside options. Third, we show direct evidence of an increase in the use of managerial incentive pay and an offsetting decline in reported work–life balance. Together, these results are most consistent with greater risk bearing driving the employee satisfaction decline though other determinants might be at play.

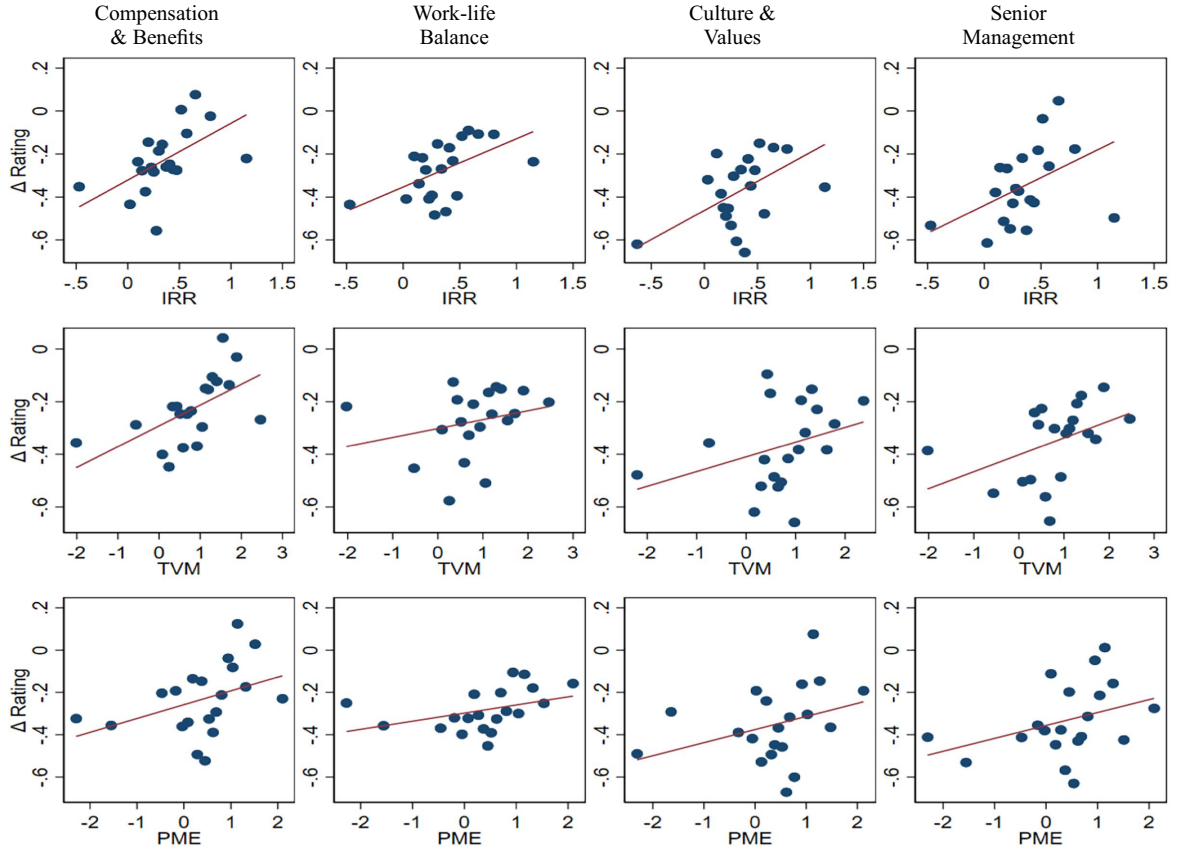
5.1. Investor Returns

Do employees win when LBO investors win? Edmans (2011), for instance, shows that higher returns coincide with more satisfied employees for public firms, but PE could have different effects for several reasons. If investor returns in PE come largely from extracting employee surplus, as discussed in Section 4.2, we might see a negative relationship between returns and job quality changes; the more effective PE is at extracting surplus, the worse workers do. Alternatively, there might be no correlation if PE firms always extract employee surplus in the same way regardless of deal success. Finally, we might see a positive relationship if there is a risk-sharing mechanism in which investor successes and failures are passed through to employees, similar to the way that currency exchange rate variation passes through to wages and prices (Gopinath et al. 2010, Bussiere 2013). Because deal performance is determined well after the deal date and occurs simultaneously with satisfaction changes, we do not seek to establish causality here.[23]

5.1.1. Satisfaction and Deal Returns. We first show the raw relationship between returns and job quality changes for the return-matched deals. Figure 5 uses bin-scatters to show that there is a positive association between investor returns and changes in employee satisfaction, particularly for changes in compensation and IRR. Figure 6 shows that, whereas there is no relationship for base pay, there is a markedly positive slope for variable earnings across all three return measures.

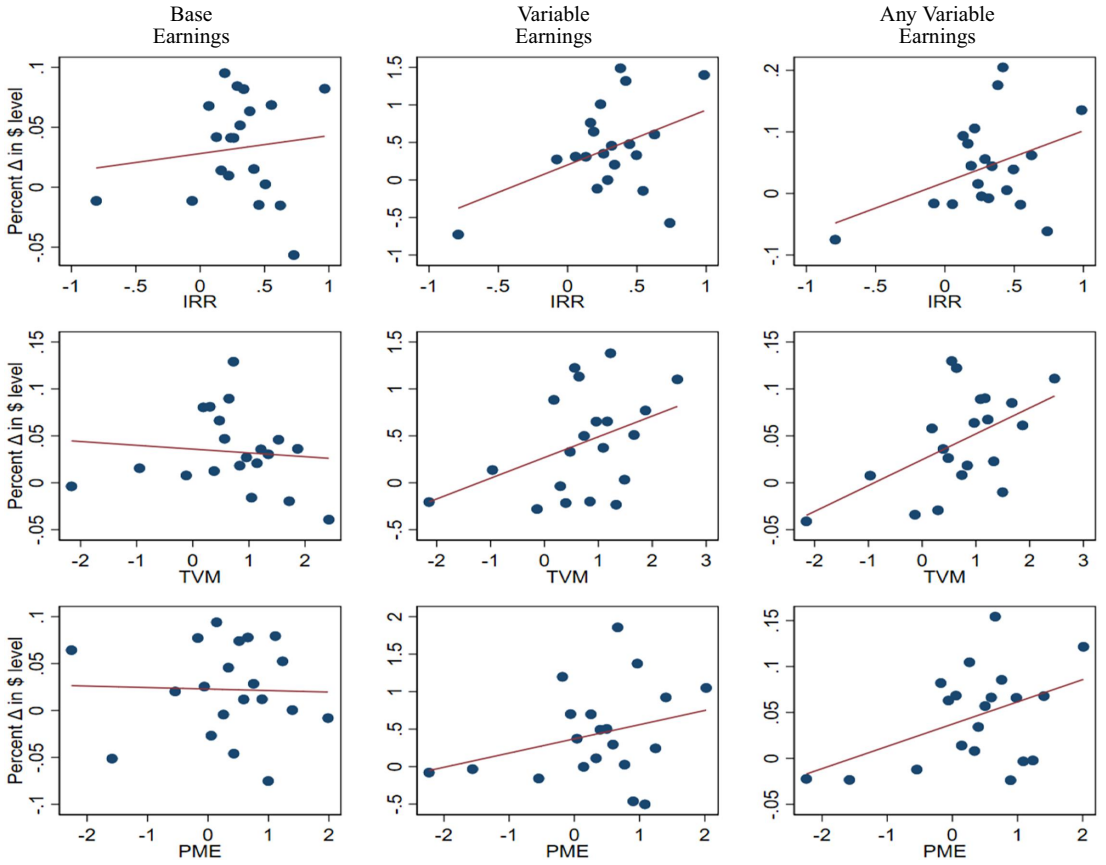
Next, we examine this association after removing company- and time-specific confounders using the following

Figure 5. (Color online) Investor Return and Changes in Employee Satisfaction



Notes. The figure presents bin-scatter plots of the changes in residualized average quarterly ratings of employees on the deal-level gross-of-fee returns in the LBO-SPI matched sample. The rating category is indicated at the top of each column. Returns are measured as either the deal's IRR (top row), TVM (middle), or PME (bottom) as indicated by the *x*-axis title. PMEs are computed against the style and target firm's industry sector of the Russell 2000 index. The returns are transformed by taking the natural log of one plus the return value for IRR and 0.1 plus value for multiples before taking the average within the respective return quantile.

Figure 6. (Color online) Investor Return and Changes in Employee Pay



Notes. The figure presents bin-scatter plots of the changes in residualized average quarterly pay of employees on the deal-level gross-of-fee returns in the LBO-SPI matched sample. The pay category is indicated at the top of each column. Returns are measured as either the deal's IRR (top row), TVM (middle), or PME (bottom) as indicated by the *x*-axis title. PMEs are computed against the style and target firm's industry sector of the Russell 2000 index. The returns are transformed by taking the natural log of one plus the return value for IRR and 0.1 plus value for multiples before taking the average within the respective return quantile.

linear regression model:

$$\Delta\overline{Y}_j = \beta\,\text{Return}_j + \omega\,X_j + \eta_t + \varepsilon_j. \quad (4)$$

Here, there is no causal interpretation, and we attempt to identify only the residual correlation between investor outcomes and the changes in job quality at the target firm. The dependent variable $\Delta\overline{Y}_j$ is the change in average ratings from before to after the LBO with both ratings adjusted for industry–quarter fixed effects. The vector X_j controls for the predeal average rating (to demean each firm's outcome) and the investment amount. Finally, we include deal–year fixed effects, η_t. In untabulated analyses, we find that the predeal characteristics do not significantly predict returns. Consistent with the low return predictability of publicly traded equities, kitchen sink regressions of returns to the LBO equity sponsors on characteristics result in the determination coefficients of at most 4%.

Our estimates are presented in Table 4, panel A. We use IRR, which is most closely tied to a buyout fund carry (Robinson and Sensoy 2013, Gredil 2022) and is traditionally the central evaluation metric for both deals and funds (Gompers et al. 2016, Da Rin and Phalippou 2017).[24] There is a large and robustly positive relation for each satisfaction measure and for incentive pay. For example, a 10% increase in the annualized return is associated with a 7% increase in employee variable pay (column (6)). The exception is base earnings (column (5)), for which we see no effect. Online Table A.10 shows the same pattern for the other two return measures: TVM, which captures the total return to investors without any time or risk adjustment (panel A), and PME, which captures idiosyncratic returns after accounting for what the comparable public market investments would deliver (panel B). Online Table A.4 shows that the relationship is stronger in fully resolved deals but is similar for deals that were not yet fully resolved at the end of our sample. Furthermore, we find no relationship between the average predeal ratings and the deal returns both in raw terms and as a percentile rank within the fund (Online Table A.11).

Overall, it is clear that higher investor returns in LBOs are not associated with larger deteriorations in employee

ratings. Instead of a zero-sum reallocation of employee surplus to investors, there instead appears to be a positive relationship, and the deals that are the worst for job quality are typically also the worst for investors.

5.1.2. Relative to Matched Public Firm Investments. An association between LBO deal returns and employee satisfaction is perhaps not surprising as public firms show a similar relationship (Edmans 2011). This leads us to ask whether the relationship between financial performance and job quality for PE-backed firms is different from that in public firms. The positive association between returns and job quality could be weaker for PE-backed firms if PE investors are less capital constrained on the downside and more effective at keeping rents to themselves on the upside. On the other hand, the relation could be stronger if PE firms offer more performance pay on the upside and pass more of the downside to employees through leverage, greater willingness to fire, or costly default.

We compare the sensitivity of public and private firms by creating a set of mimicking public trades for each PE deal. Our approach has two steps. First, we match each LBO target to the five closest publicly traded peers at the time of the deal.[25] Second, for each public company, we construct a hypothetical trade that mimics the cash flow pattern of the respective LBO. We follow Korteweg and Nagel (2016) and assume that investment amounts in the mimicking deals exactly match those of the actual deals, the interim distributions are a function of the time since the deal or previous distribution, and the residual value is paid off at the terminal distribution of the actual deal. Deviating from Korteweg and Nagel (2016), we use an individual stock's return rather than market-wide returns, so our mimicking trades capture both idiosyncratic and systematic risk, just as the comparable LBO's returns.

These mimicking trades package public stock returns in PE-like cash flows. We use them to calculate comparable IRRs, TVMs, and PMEs against style and industry sectors, just as we do for the actual LBO sponsor fund cash flows. We can then estimate the sensitivity of satisfaction to returns for LBO targets relative to public companies:

$$\Delta\overline{Y}_j = \beta\,\text{Return}_j + \delta\,\mathbb{1}(\text{LBO}_j) \times \text{Return}_j + \gamma\,\mathbb{1}(\text{LBO}_j) + \omega\,\text{X}_j + \alpha_d + \varepsilon_j. \quad (5)$$

The coefficient of interest, δ, is on the interaction between the return measure and being an LBO rather than a mimicking trade, and α_d are fixed effects for each LBO target and its mimicking trades. We present the results in Table 4, panel B.[26] Better performance is associated with more satisfied employees for both public and private companies. Looking first at raw performance in the odd columns, compensation satisfaction is more related to investor returns for employees of LBO targets than for employees of public companies. Specifically, a 0.25 increase in log IRR is associated with a 0.095-point increase in employee ratings of compensation—about three times as large as the 0.033-point increase for the mimicking public investments. Decreases in satisfaction are correspondingly more severe. Even columns show the same results for IRR percentile rank, which adjust for the higher mean and volatility of LBO returns (see Table 1, panel D).

Note that the higher pass-through to compensation relative to public companies coexists with lower satisfaction on average after LBOs; the −0.22 coefficient on the stand-alone LBO indicator implies that LBO investor returns would need to be at least 1.1 standard deviations above the sample mean for the target's employees to experience a larger increase in satisfaction than if they were working for a comparable public company with the same stock return, whereas the estimates in column (2) imply that the expected increase in LBO employee satisfaction is lower than that of a median public firm benchmark for deals returning below the 74th percentile to investors.[27] Importantly, the results in Table 4 are not driven by predeal employee satisfaction ratings as shown in Online Table A.11. Finally, we show that, for all models in panel B, there is significant positive LBO pass-through (as per the reported p-values for the F-test on the sum of Return and LBO × Return) as in panel A even though they are not significantly different from those at the public companies.

In sum, the negative changes in employee satisfaction after buyouts are endemic (but not limited) to low-performing deals for investors.[28] LBOs exhibit more pass-through of returns to employee satisfaction with compensation than publicly traded firms (though they have a lower average), consistent with surplus-sharing theories. This could arise from more use of explicit performance-based employment contracts or could reflect another mechanism. However, we find no evidence that predeal employee satisfaction ratings explain the variation in returns to LBO investors. Whereas these correlations shed light on how PE creates value, we reiterate that there is no sense of causal determination.

5.2. Deal Leverage

High leverage characterizes LBOs and differentiates them from most other corporate transactions. Leverage magnifies both returns and the risk of financial distress and is seen as instrumental to value creation in PE (Kaplan and Stromberg 2009, Guo et al. 2011, Gompers et al. 2016). We look at how changes in leverage are associated with changes in compensation. Specifically, we look at the association between the new debt taken on in the deal relative to the overall size of the deal (right-hand side of the equation) and the change in job quality (left-hand side of the equation). Although default probability is related to total debt (and the firm's unobservable debt

Table 4. Association of Job Quality Changes with Investor Returns

Panel A. Within the LBO sample

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
log(Return)	0.252***	0.208***	0.248**	0.227**	0.015	0.690***	0.075***
	(0.079)	(0.076)	(0.100)	(0.089)	(0.026)	(0.235)	(0.024)
Observations	350	351	228	350	240	240	240
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.558	0.590	0.558	0.606	0.553	0.595	0.627
Outcome standard deviation	0.798	0.793	0.865	0.935	0.259	2.850	0.327

Panel B. Relative to public matches

	Compensation and benefits		Work–life balance		Culture and values		Senior management	
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LBO × log(Return)	0.248**		0.227		0.065		0.113	
	(0.123)		(0.161)		(0.151)		(0.191)	
LBO × Return percentile		0.220*		0.220		−0.011		0.092
		(0.125)		(0.151)		(0.133)		(0.134)
log(Return)	0.132*		0.054		0.318***		0.234	
	(0.079)		(0.101)		(0.106)		(0.153)	
Return percentile		0.229***		0.104*		0.360***		0.353***
		(0.065)		(0.059)		(0.078)		(0.076)
LBO	−0.220***	−0.106***	−0.099	−0.009	−0.103	−0.017	−0.080	0.008
	(0.055)	(0.037)	(0.065)	(0.045)	(0.066)	(0.049)	(0.069)	(0.051)
Observations	1,846	1,846	1,851	1,851	1,199	1,199	1,846	1,846
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.600	0.605	0.668	0.669	0.660	0.662	0.681	0.688
Outcome standard deviation	0.676	0.676	0.767	0.767	0.825	0.825	0.880	0.880
LBO return standard deviation	0.323	0.290	0.323	0.290	0.337	0.287	0.323	0.290
Pub. return standard deviation	0.243	0.290	0.243	0.290	0.268	0.302	0.243	0.290
Pr{R+LBO× R=0}	0.0004	0.0004	0.0108	0.0164	0.0004	0.0115	0.0012	0.0003

Notes. This table reports tests of the association between deals' gross-of-fee returns and the changes in job quality around the deal. The dependent variables are the change in the residualized quarterly average ratings or reported wages (as indicated in the column of the table) from the predeal quarters average to the postdeal quarters average. The independent variable of interest are (i) a natural log of 1.1 plus the deal's IRR and (ii) the percentile rank of the deal's IRR. Panel A reports analysis that uses only the sample of LBOs, whereas in panel B, the sample is augmented with mimicking investments in public companies. *LBO* is a dummy variable that takes a value of one for actual LBOs and zero for the mimicking public investments, each entering as a separate observation. For each LBO we match with replacement up to five public companies based on industry, year founded, log number of reviews, share of jobs requiring only a high school diploma, and share of reviews by employees with at least three years of tenure. The IRRs of the mimicking investments in public companies are constructed from their respective stock returns and the cash flow pattern of the matching LBO (see Section 5.1 for details). The IRR percentile ranks are computed separately within the LBO sample and within the mimicking public investments sample and normalized to be between −0.5 and 0.5. In panel A, the control variables include the year of the deal, the amount invested, and the average rating or salary data before the deal that corresponds to the dependent variable. In panel B, the control variables include the predeal rating averages and the deal case fixed effects that indicate each LBO with the mimicking investments matched to it. Standard errors are clustered at the industry subsector of the LBO as indicated in SPI. In panel B, we double-cluster by the company identifier and the LBO industry subsector.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

capacity), our differences-in-differences design means what matters for our tests is the change in default probability, and thus, the change in debt associated with the LBO is our closest proxy. Although leverage is by nature endogenous, Online Table A.13 shows that our measures of debt do not correlate significantly with any predeal characteristic that we observe in the data.

We study whether leverage is related to the effect PE deals have on employees by reestimating Equation (1) with $\mathbb{1}(\text{Post LBO}_{j,q})$ interacted with deal leverage. We first partition the LBO dummy into the leverage terciles. As is evident from Table 5, panel A, high-leverage deals drive the negative effects on job satisfaction, and the decrease is largely monotone in leverage. For compensation and work–life balance, the statistically significant coefficients on the highest leverage tercile are about 1.5 times as large as those on the middle tercile and about three to six times as high as those on the lowest tercile, which are negative but statistically insignificant. In panel B, we consider a continuous leverage variable that reflects the fraction of debt in the transaction's overall size. The strong negative relation with leverage persists with the effects of zero-leverage deals being economically small and not statistically different from zero.

Finally, to examine whether our measure of leverage is simply capturing cross-industry variation, we consider an alternative measure of leverage in which we subtract the industry-specific median change in leverage for our deal sample. This measure, whereas 0.84-correlated with unadjusted leverage, is constructed so that zero reflects an increase in leverage that is typical for deals in this industry. We then repeat the analysis in panel B of Table 5, the results of which are reported in panel C. Again, we find significantly negative effects on satisfaction for the average LBO and starkly more negative effects among deals involving more leverage. The effects are also comparable in magnitude to those obtained for unadjusted leverage in panels A and B. Whereas the interaction coefficient increases and becomes statistically significant for culture, the lack of uniformly sharper sorting with industry-adjusted leverage may reflect noise introduced by our somewhat-fine industry controls. It is also plausible that a raw increase in leverage is more salient from the perspective of the target's employees. We also note that our industry adjustments reduce the dispersion of the leverage measure. Thus, given the same coefficient estimates, the marginal effect of a one standard deviation increase in industry-adjusted leverage is about 12% larger than that of raw leverage.

Overall, the results in Table 5 support financial leverage being an important predictor of the post-LBO change in employee satisfaction. To further this point, we repeat our main event study analyses for deals with above-median leverage (see Online Table A.14 for regression analyses with median cuts). The results for each measure of satisfaction are presented on the right side of Figure 3. Despite the smaller sample sizes, we observe that the discontinuous declines after the deal are steeper for high-leverage deals than for the overall sample. Although leverage is endogenous and associated with other factors, these event studies show that high-leverage deals do not target firms with pretrends and, together with Table 5, suggest that leverage has a causal effect in LBOs.

We examine several alternative explanations. First, an association with layoffs does not seem to subsume the leverage results, and in fact, controlling for layoffs seems to strengthen the leverage results (Online Table A.15). Second, in Online Appendix B, we show that other deal-level variables are only weakly predictive and, thus, cannot explain our leverage results. These analyses include dividing the LBO sample into three transaction types: public-to-private deals, corporate divestitures, and vanilla LBOs. The negative effects are of roughly similar magnitude across the three deal types and are not significantly different from one another. The exception is that corporate divestitures are associated with larger work–life balance declines—when employees at the targeted subsidiary may have previously benefited from managers who enjoyed the quiet life and suffered from the agency issues inherent in multiunit corporations (Scharfstein and Stein 2000, Schoar 2002, Bertrand and Mullainathan 2003)—but the results are weak relative to the leverage-based cuts. Deal size has no strong effect, alleviating concerns about size biasing the availability of data on debt. Finally, fund ESG status does not make a major difference, and LBOs led by ESG-focused funds are associated with the same negative effect on employees as other deals. In sum, although we cannot rule out the possibility that an omitted variable drives the leverage relationship, we show that common deal-level characteristics do not drive it.

5.2.1. Leverage, Satisfaction, and Employee Vulnerability. The link between leverage and satisfaction has intuitive theoretical roots. Titman (1984), Chang (1992), and Berk et al. (2010) argue that financial distress is costly to workers and workers should, thus, internalize the costs of firm leverage. Indeed, empirical work supports the view that employees price in default risk (see, for instance, Agrawal and Matsa 2013, Chemmanur et al. 2013, Simintzi et al. 2015, Baghai et al. 2021, Graham et al. 2023).

In such a world, a leverage increase is equivalent to an important amenity being cut: job security. The value of that amenity is highest for the employees with the most to lose, either because they have worse outside options or because they are entrenched and have captured more surplus. We consider different measures of this slack, looking at characteristics that would predict outside options and entrenchment. Perhaps the most natural measure of outside options is labor market conditions. If unemployment is relatively high, workers face higher

Table 5. Heterogeneity in Effect of LBOs on Job Quality by Deal Leverage

Panel A. Leverage terciles

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO) × 1(Low Leverage)	−0.070	−0.030	0.001	−0.043	−0.001	0.373	0.041
	(0.049)	(0.054)	(0.070)	(0.059)	(0.027)	(0.234)	(0.027)
𝟙(Post LBO) × 1(Middle Leverage)	−0.136**	−0.125**	−0.108	−0.127*	0.017	0.119	0.002
	(0.053)	(0.060)	(0.095)	(0.070)	(0.021)	(0.249)	(0.029)
𝟙(Post LBO) × 1(High Leverage)	−0.194***	−0.191***	−0.120	−0.105*	−0.001	0.111	0.010
	(0.052)	(0.055)	(0.075)	(0.062)	(0.034)	(0.261)	(0.029)
Observations	842,903	844,632	755,134	836,066	326,076	326,076	326,076
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.356	0.327	0.353	0.349	0.618	0.456	0.435
Outcome standard deviation	1.146	1.170	1.278	1.323	0.504	3.464	0.386

Panel B. Continuous leverage

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.036	−0.009	0.032	−0.041	0.007	0.236	0.023
	(0.050)	(0.059)	(0.081)	(0.065)	(0.028)	(0.244)	(0.028)
𝟙(Post LBO) × Leverage	−0.191**	−0.208**	−0.212	−0.101	−0.003	−0.076	−0.012
	(0.082)	(0.092)	(0.129)	(0.103)	(0.052)	(0.407)	(0.046)
Observations	842,903	844,632	755,134	836,066	326,076	326,076	326,076
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.356	0.327	0.353	0.349	0.618	0.456	0.435
Outcome standard deviation	1.146	1.170	1.278	1.323	0.504	3.464	0.386

Panel C. Industry-adjusted continuous leverage

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.149***	−0.136***	−0.094*	−0.101***	0.007	0.207	0.017
	(0.030)	(0.033)	(0.048)	(0.038)	(0.017)	(0.155)	(0.018)
𝟙(Post LBO) × Leverage	−0.163*	−0.218**	−0.304**	−0.075	−0.011	−0.181	−0.024
	(0.091)	(0.105)	(0.143)	(0.122)	(0.058)	(0.455)	(0.052)
Observations	842,795	844,524	755,049	835,958	325,998	325,998	325,998
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.356	0.327	0.353	0.349	0.618	0.456	0.435
Outcome standard deviation	1.146	1.170	1.278	1.323	0.504	3.464	0.386

Notes. This table shows whether deal leverage is associated with different effects relative to a base of nontargeted companies, using Equation (1). We use company–quarter-level data and omit LBOs for which the deal's leverage is not observed. The leverage is measured as the new debt added to the capital structure scaled by the deal's total size (i.e., debt plus equity). Panel A divides the sample of LBOs into leverage terciles. In panel B, we use continuous leverage that is bound between zero (if no new debt was issued) and one. In panel C, we also use continuous leverage but adjusted for the median increase in debt across all LBO targets in the same industry per our sample. Thus, the coefficients on 𝟙(Post LBO) indicate the effect for deals with zero-leverage in panel B, and the target's industry typical increase in leverage in panel C. The number of observations in panel C is smaller as we omit three deals for which there are no other deals in the same industry. All models include company and industry-quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

costs of job loss and, thus, higher costs of firm risk. We assess whether the effects are stronger when the industry has a higher unemployment rate in Table 6. Indeed, we see that the effects on compensation and culture are twice as large in industry years with above-median unemployment, consistent with risk being more costly when workers have worse outside options.[29]

Next, we consider job tenure, which is related both to switching costs and entrenchment. We partition current employees into those with less than three years and those

Table 6. Heterogeneity in Effect of LBOs on Job Quality by Labor Market Tightness

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.075***	−0.071***	−0.102***	−0.073***	0.004	0.034	0.003
	(0.021)	(0.022)	(0.029)	(0.026)	(0.013)	(0.123)	(0.014)
𝟙(Post LBO) × 𝟙(High Unemployment)	−0.073**	−0.028	−0.123**	−0.064	0.052**	0.108	0.013
	(0.035)	(0.037)	(0.048)	(0.043)	(0.022)	(0.185)	(0.021)
Observations	559,723	560,447	475,634	555,655	204,661	204,661	204,661
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.372	0.344	0.383	0.363	0.634	0.469	0.448
Outcome standard deviation	1.125	1.160	1.274	1.305	0.512	3.567	0.396

Notes. This table shows the effect of an LBO interacted with the unemployment rate in the firm's industry. We use company–quarter-level data (Equation (1)); 𝟙(High Unemployment) takes the value of one if an industry's unemployment rate is in the top tercile among all industries in the same year and zero otherwise. Industry-level unemployment data are from the U.S. Bureau of Labor Statistics. We manually map NAICS three-digit industry to Glassdoor industry. All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

with more than three years of tenure at the firm. In Table 7, we show that the negative effects for all four satisfaction dimensions are driven by workers with longer tenure with near-zero coefficients for short-tenure workers. For example, the negative effect on compensation is more than 10% of a standard deviation for long-tenured workers (column (1)). This result is robust to using review-level data and to controlling for education and experience requirements. In Online Table A.16, panel A, we report the results at the review level, now exploiting the higher statistical power to consider four tenure categories. We find that, in general, the most negative effects are among workers with at least four years of tenure though there are also significant negative effects among workers with two to three years of tenure. In panel B, we find that these results are essentially unchanged when we add controls for worker's education, experience, and manager status.

The final employee characteristics we consider are proxies for skill because less-skilled workers may be

Table 7. Effect of LBOs on Job Quality by Employee Tenure at Firm

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.011	0.018	0.010	0.007	0.004	0.100	0.013
	(0.026)	(0.026)	(0.029)	(0.030)	(0.013)	(0.127)	(0.014)
𝟙(Post LBO) × 𝟙(3+ Years Tenure)	−0.121***	−0.121***	−0.115***	−0.100***	−0.001	−0.007	−0.001
	(0.018)	(0.018)	(0.020)	(0.021)	(0.010)	(0.098)	(0.010)
𝟙(3+ Years Tenure)	0.043***	−0.074***	−0.066***	−0.132***	0.210***	0.954***	0.099***
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.015)	(0.002)
Observations	756,552	758,113	755,296	751,730	286,768	286,768	286,768
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.343	0.318	0.329	0.342	0.603	0.439	0.417
Outcome standard deviation	1.185	1.211	1.302	1.362	0.519	3.645	0.403

Notes. This table reports how the effect of an LBO varies with the number of years an employee has been at the firm, using company–quarter–cohort level data (Equation (2)). For each company-quarter, we compute average ratings and reported pay for reviewers with 0–3 years of tenure and 3+ years of tenure separately. The interaction with 0–3 years of tenure is omitted so that the coefficient on 𝟙(Post LBO) represents the effect for that group. All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

more exposed to firm-specific changes, for example, employer bankruptcies (Baghai et al. 2021). To test whether these workers are more negatively impacted by an LBO, in Table 8, panel A, we compare effects across jobs that typically require less and more than three years of experience, and in panel B, we compare the effects across jobs based on the education level typically required. The negative effects on compensation, culture, and senior management are generally driven by lower skill workers, whereas the negative effect on work–life balance is driven by higher skill workers, consistent with the relationship for managers.

In these tables, the independent average effects offer benchmarks for assessing the magnitude of the LBO effects. For example, workers in jobs that require a college degree are about 0.10 points more satisfied with their firm's culture (Table 8, panel B). Therefore, the average negative effect of LBOs on satisfaction with culture, at −0.09 points, represents nearly the average difference in satisfaction with culture associated with a college degree. To provide a second benchmark, this negative effect is about half the average extra satisfaction that managers have with firm culture relative to nonmanagers (0.2 points per Table 9).

In sum, the decline in satisfaction with pay is concentrated among less-skilled, entry-level workers and those who have been at the firm for a longer period: the workers most vulnerable to firm fragility.

Table 8. Effect of LBOs on Job Quality by Job Requirements

Panel A. Interaction with job's required work experience

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.089***	−0.030	−0.054	−0.055*	0.008	0.032	−0.005
	(0.027)	(0.029)	(0.034)	(0.032)	(0.017)	(0.145)	(0.016)
𝟙(Post LBO) × 𝟙(Req. 3+ Years Exp.)	0.033	−0.047**	0.006	0.020	0.014	0.039	0.010
	(0.022)	(0.023)	(0.024)	(0.025)	(0.017)	(0.123)	(0.013)
𝟙(Req. 3+ Years Exp.)	0.093***	0.090***	0.096***	0.095***	0.287***	0.897***	0.090***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.021)	(0.002)
Observations	539,588	540,449	484,881	536,066	225,651	225,651	225,651
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.349	0.321	0.345	0.353	0.626	0.446	0.424
Outcome standard deviation	1.178	1.217	1.306	1.360	0.515	3.691	0.405

Panel B. Interaction with job's required education

Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.103***	−0.036	−0.038	−0.050	0.000	−0.072	−0.018
	(0.032)	(0.034)	(0.038)	(0.037)	(0.022)	(0.180)	(0.020)
𝟙(Post LBO) × 𝟙(Req. College)	0.035	−0.035	−0.024	0.003	0.019	0.149	0.022
	(0.025)	(0.028)	(0.027)	(0.029)	(0.020)	(0.147)	(0.016)
𝟙(Post LBO) × 𝟙(Req. Masters/Prof)	0.111**	0.028	0.014	0.093*	−0.006	0.213	0.026
	(0.047)	(0.046)	(0.051)	(0.055)	(0.071)	(0.362)	(0.035)
𝟙(Req. College)	0.053***	0.092***	0.105***	0.098***	0.305***	1.029***	0.105***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.027)	(0.003)
𝟙(Req. Masters/Prof)	0.112***	0.125***	0.140***	0.142***	0.585***	1.534***	0.144***
	(0.007)	(0.008)	(0.008)	(0.008)	(0.009)	(0.047)	(0.005)
Observations	536,006	536,914	481,774	532,309	221,163	221,163	221,163
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.346	0.318	0.342	0.350	0.629	0.450	0.427
Outcome standard deviation	1.181	1.221	1.311	1.364	0.523	3.687	0.404

Notes. This table reports how the effect of an LBO on an employee varies with the work experience (panel A) and education (panel B) that an employee's job typically requires, using company–quarter–cohort level data (Equation (2)). The work experience and education each reviewer's reported job title typically requires is calculated as described in Section 2. For each company-quarter, we compute average ratings and reported pay for reviewers in each group separately. The coefficient on 𝟙(Post LBO) in represents the effect on the least qualified group (jobs typically not requiring >3 years of work experience or college). All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

5.2.2. Reduced Compensation Satisfaction Without Reduced Compensation. Following an LBO, we document a decline in compensation satisfaction despite no discernible change in base pay. Although perhaps initially puzzling, these results are entirely consistent with theories of costly employee risk-bearing. If workers are risk averse, they demand higher wages from firms that impose more risk on them. Postbuyout, workers earn the same pay at a riskier firm, so they perceive themselves to be underpaid relative to their new job and its new associated level of risk.

These compensation results are harder to square with other models of leverage impacting employees. For example, in some contexts, firms strategically increase leverage to extract wage concessions from labor (Perotti and Spier 1993, Matsa 2010).[30] These models generally predict lower wages overall but do not provide clear predictions on passthrough, whereas we find no average wage change but large increases in passthrough, including much higher use of incentive pay for managers as we discuss in the following section. Therefore, in the case of LBOs, it does not seem that new owners are using leverage as a strategic bargaining tool.

5.3. Managerial Compensation

PE sponsors often claim that they create value through stronger incentives. Our data allow us to directly measure incentive pay for both frontline workers and managers. Managers in Glassdoor data are generally lower and mid-level rather than C-suite executives. Nevertheless, managers generally receive large premiums in variable pay compared with nonmanagerial rank-and-file employees. As evident from Table 9, column (6), managers receive fivefold more in variable earnings than their nonmanagerial peers in part reflecting a 17-percentage-point greater likelihood of receiving variable income. After an LBO, this managerial premium widens further though mostly along the extensive margin. In column (7) of the table, we show that the likelihood that a manager receives any performance pay rises by about three percentage points (12% of the mean). This pay increase is isolated to incentive pay with base pay going up only slightly. In Online Table A.17, we break down performance pay into its four component categories and show this effect is driven entirely by a shift toward cash bonuses rather than stock or profit sharing.

Perhaps because of these stronger incentives, managers report larger declines in work–life balance and, in fact, drive the entire negative effect of LBOs on work–life balance. For the other satisfaction dimensions, managers are not differentially affected relative to frontline workers. After LBOs, managers appear to be pushed to work harder but are compensated more generously via incentive pay. This is consistent with findings by Gompers et al. (2016) about the importance of incentives in PE deals. More generally, higher incentive compensation particularly for managers is related to the increase in overall within-firm pay inequality (Piketty 2013) and also a greater connection between higher and more volatile pay for managers and higher firm productivity (Bloom et al. 2021). Our results suggest that the dramatically increasing footprint of the LBO business model may, in part, account for these trends.

The rise in manager incentive pay raises the possibility that LBOs reduce satisfaction with compensation by increasing within-firm pay inequality (Bandiera et al. 2007,

Table 9. Effect of LBOs on Job Quality by Manager Status

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.061***	−0.031	−0.017	−0.040	0.002	0.033	−0.001
	(0.022)	(0.024)	(0.030)	(0.027)	(0.012)	(0.103)	(0.012)
𝟙(Post LBO) × 𝟙(Manager)	0.009	−0.121***	−0.040	−0.005	0.022	0.171	0.032**
	(0.020)	(0.025)	(0.025)	(0.026)	(0.015)	(0.121)	(0.013)
𝟙(Manager)	0.256***	0.056***	0.203***	0.216***	0.435***	1.757***	0.170***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.023)	(0.002)
Observations	673,506	675,028	606,678	667,498	296,783	296,783	296,783
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.363	0.327	0.353	0.358	0.685	0.473	0.447
Outcome standard deviation	1.170	1.201	1.288	1.347	0.529	3.635	0.398

Notes. This table shows the effect of an LBO interacted with each reviewer's manager status, using company–quarter–cohort level data (Equation (2)). We identify managers using the reviewer's job title. For each company-quarter, we compute average ratings and reported pay for managers and nonmanagers separately. The interaction with nonmanager status is omitted so that the coefficient on 𝟙(Post LBO) represents the effect for that group. All models include company and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Breza et al. 2017). To assess whether this may be the case, we construct three standard measures of within-firm pay inequality: the standard deviation, 90–10 ratio, and 90–50 ratio, all at the company–quarter level.[31] We first examine whether LBOs increase inequality in either base pay or total pay. Online Table A.18, panel A, shows that there is no statistically significant effect within our data. Next, we create an indicator for LBOs that have above-median changes in inequality. Panel B shows these deals do not have different effects from deals with below-median inequality changes (the interaction coefficients are near zero and insignificant). Therefore, higher within-firm inequality does not appear to explain the overall decline in satisfaction.

6. Validation and Robustness Tests

In this section, we conduct an array of tests that validate and confirm our main results. First, we compare LBOs to other types of deals. Second, we show that our results are robust to employee composition changes. Finally, we discuss matching tests and show that our results are robust to a variety of alternative specifications.

6.1. Other Deal Types

6.1.1. Recapitalizations. Because leverage is endogenous, our leverage results could reflect something about high-leverage LBOs that is not related to new debt though our approach is in line with previous literature, which shows only correlations between employment or pay and leverage.[32] To further test the importance of leverage for job quality, we consider situations outside of PE in which firms quickly take on large amounts of new debt. We focus on recapitalizations, which occur when a public firm increases its leverage by issuing debt, simultaneously buying back shares or issuing large dividends (Kovenock and Phillips 1997). There are several reasons for these transactions, including fending off a hostile takeover and facilitating the exit of a large shareholder. Using Compustat data, we define recapitalizations as occurring when a firm increases debt by at least 5% of assets, at the same time buying back shares and paying out cash worth a combined total of more than 5% of assets.[33] This filter captures large capital structure changes with the average transaction leading to a 37% increase in book leverage from a 22% debt issue, an 8% share buyback, and an 8% dividend, all expressed as percentages of firm assets. We match these firms to our Glassdoor data, yielding 258 leveraged recapitalizations from 2010 to 2016. Although these firms undergo large capital structure changes, assets increase just 4%, suggesting that much of the change is financial reorganization rather than expansion. We follow the capital structure literature (e.g., Korteweg et al. 2022) by considering firms whose assets always exceed $10 million.

Table 10 reports the association between public recapitalizations and our seven job quality measures. Higher debt recapitalizations are associated with larger decreases in compensation satisfaction, consistent with our results from LBOs. The coefficient is large and of strikingly similar size to the leverage coefficient on PE deals (Table 5, panel B), affirming the importance of risk as a driver of satisfaction. Whereas the effect of leverage may be similar, the overall change is not: buyouts with relatively low levels of debt are associated with no change in compensation satisfaction, whereas recapitalizations with relatively low levels of debt are associated with an increase. Our evidence here, whereas tentative, suggests that either LBOs affect employee satisfaction

Table 10. Effect of Leveraged Recapitalization on Job Quality Among Public Firms

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
1(Post Leveraged Recap.)	0.055*	0.006	−0.047	−0.007	−0.011	−0.100	−0.012
	(0.030)	(0.034)	(0.050)	(0.038)	(0.014)	(0.151)	(0.016)
1(Post Leveraged Recap.) × Leverage	−0.197**	0.005	0.020	−0.095	0.058	0.980	0.128*
	(0.097)	(0.136)	(0.233)	(0.136)	(0.044)	(0.713)	(0.076)
Observations	77,252	77,273	62,211	76,932	44,266	44,266	44,266
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.322	0.260	0.304	0.247	0.614	0.387	0.360
Outcome standard deviation	0.878	0.934	1.029	1.030	0.486	3.582	0.385

Notes. This table reports the effect of leveraged recapitalizations on job quality measures among public firms interacted with the ratio of debt issuance over total assets. The sample includes 258 leveraged recapitalization events from Glassdoor-Compustat matched U.S. public firms during 2010–2016. Control firms are public firms that did not do leveraged recapitalizations. We use company–quarter average reviews and reported pay as the dependent variable (Equation (1)). All models include company and industry-quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

through channels other than leverage or that LBOs increase leverage more than the increase associated with recapitalizations, and the effects of leverage are only felt when the risk of financial distress is particularly acute.

6.1.2. Canceled Deals. A central concern with our main analytical approach is that, because PE firms do not target companies at random, there may be unobservables at the targeted firms that lead them to have the effects we see in the absence of the buyout. This raises the question of what the counterfactual would be in the absence of the deal. Above, we addressed this concern with event studies, which establish that targets were not on track toward the negative effects before the deal. We push our identification further by studying canceled deals. We obtain 108 canceled LBOs from PitchBook and Capital IQ. Online Table A.19, panel A, presents summary statistics on reviews for these firms in the same manner as Table 1, panel A. Panel B reports the differences-in-differences effect of canceled LBOs on employee satisfaction measures relative to the same set of control firms as in the main analysis (LBO-targeted firms are omitted from the sample). We observe no negative effects following a canceled LBO, further supporting our key identifying assumption. The coefficients are generally small and insignificant except for a positive effect at the 10% significance level on work–life balance.

6.1.3. Generic Acquisition Restructuring. The effects of LBOs might be common to acquisitions more generally. To address this concern, we gather data on conventional acquisitions from Capital IQ to compare their effects with those of PE acquisitions. We require M&A transactions to have a public or private corporate buyer and to occur between 2010 and 2016; have a U.S. target; have a value of $100 million or above; and not be LBO, MBO, or bankruptcy transactions. Out of a total of 5,672 deals meeting these requirements, we match 2,040 to Glassdoor. Among these, we eliminate deals in which the acquirer did not take a majority stake or we do not observe reviews by current employees both before and after the deal. This requires acquired companies to become subsidiaries rather than be subsumed into the acquirer and disappear. As a result, the matched deals tend to be large relative to the average acquisition, which is advantageous because it makes them more comparable to the LBO targets. This leads to 1,010 M&A deals in the final analysis sample, summary statistics for which are available in Online Table A.20.

We present our results in Table 11. These models use Equation (1) except that the independent variables of interest are postdeal indicators and should be interpreted relative to control firms that are never acquired, not relative to one another. The first row in Table 11 indicates that LBOs negatively affect all four satisfaction dimensions. In contrast, traditional corporate acquisitions have weaker and mixed effects. Specifically, M&A negatively affects satisfaction with compensation but with a less precise and smaller magnitude than LBOs. There is a strong negative effect of M&A on senior management ratings, but no effect on work–life balance or culture. Importantly, M&A deals are themselves often associated

Table 11. Effect of Alternative Ownership Changes on Job Quality

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
𝟙(Post LBO)	−0.083***	−0.064***	−0.092***	−0.062***	0.017	0.092	0.010
	(0.018)	(0.019)	(0.025)	(0.022)	(0.011)	(0.094)	(0.011)
𝟙(Post M&A)	−0.047**	−0.019	−0.039	−0.070***	−0.020	0.063	0.006
	(0.019)	(0.021)	(0.028)	(0.024)	(0.013)	(0.110)	(0.012)
𝟙(Post Management Buyout)	−0.088*	0.026	0.017	0.003	0.006	−0.229	−0.031
	(0.048)	(0.052)	(0.070)	(0.057)	(0.028)	(0.237)	(0.026)
𝟙(Post Growth Equity)	−0.009	−0.010	−0.013	−0.013	0.022	−0.060	−0.005
	(0.026)	(0.027)	(0.036)	(0.030)	(0.014)	(0.138)	(0.015)
Observations	874,801	876,572	782,966	867,847	357,633	357,633	357,633
Company–deal fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.354	0.325	0.351	0.346	0.614	0.450	0.428
Outcome standard deviation	1.144	1.169	1.276	1.321	0.505	3.473	0.387
p-value (Post LBO = Post M&A)	0.166	0.096	0.154	0.807	0.017	0.837	0.806

Notes. This table reports the effect of four types of ownership changes on job quality measures. The sample includes 1,371 LBOs, 1,010 M&As, 178 management buyouts, and 700 growth equity deals. We use company–quarter average reviews and reported pay as the dependent variable (Equation (1)). The bottom row reports the p-values for the F-tests for the equality of LBO and M&A coefficients. All models include company–deal and industry–quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

with increases in both risk (e.g., medium-term restructuring) and leverage (e.g., debt-financed deals). Compared with M&A deals, LBOs are associated with significantly (at the 10% level) more negative changes in work–life balance and significantly more positive changes in base pay. Pay and satisfaction moving in opposite directions is consistent with our risk narrative: increased risk from LBOs reduces satisfaction despite flat pay.

6.1.4. Other PE Deal Types. We are interested in whether our main effects reflect simply the event of any PE transaction or are particular to the mixture of changes—notably a change in control and high leverage—that occur following LBOs. The subsequent rows of Table 11 consider two other deal types. First, MBOs have negative effects on compensation but no effects on the other three dimensions. Recall that, in an MBO, the existing managers of a company purchase a controlling interest in a company with the help of a PE firm. Whereas leverage increases, management is generally constant, helping to explain the zero effect on senior management.

In a typical growth equity deal, the PE firm takes a nonmajority stake in a company to fund investments by that company. If the LBO effects are common to any new investment, we would expect to see similar results for growth equity, but the last row shows that these deals exhibit no effects on satisfaction. Overall, deals characterized by increases in leverage (LBO and MBO) experience the largest declines in satisfaction with compensation.

6.2. Employee Composition and Distribution Effects

Our results could potentially be driven by changes to employee composition around LBOs. We examine whether this is the case in Online Figure A.6, in which we plot changes in our employee characteristics around the time of the deal. These charts show no obvious deal effects, something we confirm in Online Table A.8. In Online Table A.21, we control for five aspects of employee composition and find that our results are robust to including these measures.[34]

The company–quarter-level analysis further obviates concerns about compositional changes along the dimensions we study; for example, the possibility that fewer managers submit reviews after the buyout and managers are in general correlated with higher ratings. This is because the data are constructed as the average review for each cohort (e.g., managers versus nonmanagers) after versus before the buyout and relative to the differences across cohorts at control firms. Therefore, it does not seem that employee heterogeneity explains the results.

A related concern is that the results could reflect a shift in the tail of the satisfaction distribution at LBO targets with no effect in the middle of the distribution. Online Figure A.7 presents quantile regression estimates of the effect of LBOs on each dimension of employee satisfaction at five different quantiles (5th, 25th, 50th, 75th, and 95th). The coefficients of $\mathbb{1}$(Post LBO) are all negative at different points of the satisfaction distribution though we see different magnitudes for the effect. The results are not driven by a change to the tail of the distribution but rather to declines across all percentiles, especially toward the middle and upper middle of the distribution.

6.3. Robustness Checks

Our results are robust to several alternative specifications. First and foremost, in Table 12, we show that our results are robust to a matching estimator constructed in

Table 12. Effect of LBOs on Job Quality: Matching Estimation

	Satisfaction data				Pay data		
Variable	Compensation and benefits (1)	Work–life balance (2)	Culture and values (3)	Senior management (4)	Base earnings (5)	Variable earnings (6)	Any variable earnings (7)
$\mathbb{1}$(Post) × $\mathbb{1}$(LBO)	−0.075***	−0.067***	−0.086***	−0.045*	0.014	0.043	0.002
	(0.021)	(0.023)	(0.029)	(0.026)	(0.014)	(0.119)	(0.013)
Observations	125,670	125,767	107,552	125,002	54,584	54,584	54,584
Company fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Deal–quarter fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.527	0.501	0.519	0.500	0.785	0.623	0.603
Outcome standard deviation	1.002	1.043	1.141	1.159	0.523	3.481	0.381

Notes. This table reports the effect of an LBO on job quality using a matching estimator. We construct the control sample using matched nontreated firms. We match each LBO target to five never-targeted companies with at least one review in the three years prior to the deal using founded year, industry, average percentage of reviewers with >3-year tenure (over last three years), average percentage of reviewers in jobs that typically require only high school (over last three years), and log number of reviews (over last three years). We use the Abadie and Imbens (2006) distance metric that weights each dimension by its standard deviation. We use company–quarter-level data. All models include company and deal-quarter fixed effects. Standard errors are clustered at the company level.

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

the same manner as our matching in Section 5.1.[35] Although this estimator yields similar results to our main differences-in-differences specification, we do not incorporate it as our benchmark approach because we do not believe this matching procedure imparts value beyond our industry–quarter controls. Company-level data are an intuitive choice for matching, but they are not the focus of Glassdoor and, therefore, are noisy and coarse, particularly for the private firms on which our study focuses. Industry is the most reliably reported data point at the company level; however, as our specification already includes industry–quarter controls, matching on industry would accomplish little. Review-level data could be used for matching, yet it creates challenges because we must either use reviews from close to the deal (creating bias, especially when evaluating the possibility of pretrends) or use reviews from well before the deal (which are noisy and often unavailable because of the recency of the site). Although we use matching to construct cash-flow benchmarks for the return tests, we do not focus on causality there and must use some form of mapping as we lack an inherent control group. For all other regressions, we believe the problems introduced by matching outweigh the benefits, and so we present results based on a matching estimator as a robustness check.

Beyond that, we show that our results do not appear to be driven by endogenous target selection and hold for different aggregation methods, different cuts of the sample, and additional rating measures. In the interests of space, we discuss these in Online Appendix C.

7. Conclusion

This paper offers the first analysis, to our knowledge, of the effect of PE buyouts on job quality as perceived by employees. We show that LBOs are associated with significant perceived declines in job quality and increases in the use of managerial incentive pay. In contrast to a narrative of broad-based cost-cutting, we find no effects on average pay. Instead, the evidence is most consistent with risk transfer playing a central role. Relative to comparable public firms, LBOs lead to more return pass-through to employees. This mechanism predicts that risk-averse employees report job quality declines because of a perceived loss of job security. For example, LBOs with relatively high leverage appear to have much stronger negative effects on employee satisfaction. In further support of this mechanism, we show that predictors of worker entrenchment and unemployment cost, such as industry unemployment rates, long job tenure, and less skill, are associated with larger negative effects on job satisfaction. Although immediate postdeal layoffs doubtless contribute to feelings of insecurity, they do not fully rationalize our effects.

LBOs appear to be able to reallocate surplus away from more replaceable and entrenched employees, achieving better matches with new hires. Our results point to a need for further research on how ownership type affects employees, potentially using alternative measures of culture and comprehensive measures of compensation. Future research that could disentangle the role of ex ante commitment by PE sponsors in sharing gains with the employees of LBO-targeted firms from the ex post incentive pay (and perk) hikes if the deal turns successful would be especially helpful in characterizing how LBOs alter labor relations. This work ultimately sheds new light on how PE affects the nature of the firm, pushing beyond the existing literature on employment, separations, and wages.

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Endnotes

[1] Authors' calculations based on PitchBook and Dealogic 2018–2019 data.

[2] See, for example, articles from *The Guardian* or *The Atlantic*, accessed August 3, 2021.

[3] Related work that touches more broadly on employee welfare around acquisitions or takeovers includes Pontiff et al. (1990), Pagano and Volpin (2005), and Dessaint et al. (2017).

[4] For instance, Oyer (2004) finds that the average survey respondent overestimates their earnings by about 5%.

[5] Among public firms that appear in Compustat and, thus, have employment data, the reviews as a share of firm U.S. employees was 2.5% in 2015.

[6] Values below 200, for instance, could reasonably correspond to percentage points, dollars, thousands of dollars, or tens of thousands of dollars.

[7] In Online Appendix C, we consider six additional measures, including benefits ratings. We exclude these from our main analysis because they are either highly correlated with our main variables or are not well populated. Glassdoor's reviewing policies changed over time. In May 2012, the platform added culture and values as a dimension and eliminated half-point scores as well as partial reviews. In September

2020, it added a score for diversity and inclusion. Glassdoor has also varied the number of reviews one can view before being required to post a review. Our quarterly fixed effects control for these dynamics.

[8] Retrieved from https://occupationdata.github.io/ on November 1, 2020. O*NET data from the U.S. Department of Labor, Employment and Training Administration provide rich information on the nature of each of these. Only 39% of reviews are matched because job titles are missing for many reviews and are ambiguous (e.g., associate) for others.

[9] The industries are accounting & legal, aerospace & defense, agriculture & forestry, arts, entertainment & recreation, biotech & pharmaceuticals, business services, construction, repair & maintenance, consumer services, education, finance, government, healthcare, information technology, insurance, manufacturing, media, mining & metals, nonprofit, oil, gas, energy & utilities, real estate, restaurants, bars & food services, retail, telecommunications, transportation & logistics, and travel & tourism.

[10] For example, RestorixHealth was taken private in 2010 by Cressey & Company and Leonard Green & Partners, raised growth equity twice in 2014, and then was sold to a different PE consortium in 2015. It enters our sample twice: the initial LBO enters our main sample, and the first growth round enters our growth equity sample. This example is illustrative but far from typical as less than 1% of our companies have multiple LBOs. Some deal types fit into multiple classifications. In these cases, we classify deals in the following order of priority (as mentioned above, a deal in our data is assigned to only one type): growth equity, MBO, public-to-private, corporate divestiture, and vanilla LBO. We compare our deal samples against a control group of never-treated firms that do not have an LBO, MBO, growth equity deal, or secondary buyout in our PitchBook sample.

[11] Matched deals are slightly more likely to be public-to-private and less likely to be corporate divestitures, reflecting the difficulty of obtaining high-quality matches to subsidiaries before and after the LBO. Because we focus on companies with employee reviews, our matched deals tend to be larger and have more employees than the typical PE deal. This focus on companies with meaningful employment is of little concern because we are interested in the impact on employees.

[12] Specifically, we calculate the employment change of firm j in year t as the number of LinkedIn users who report working for that firm in year t divided by the number of users who report working for that company in year $t-1$, minus one. The departure (hiring) rate is the number of users whose last (first) year of employment at that company is year t, divided by the number of users reporting employment at the company in year $t-1$.

[13] This restriction and long PE holding periods mean that only about 40% of the deals in our sample are fully resolved with the remaining data points relying partially on valuations reported by fund managers. In Online Table A.4, we show that our results are comparable or stronger among fully resolved deals and that unresolved deals are not stale (the variance is only modestly lower, none are carried at cost) but are more likely to contain deals that became distressed and endured more adverse changes in employee satisfaction.

[14] We show that the matched sample IRR quartiles as well as PME and TVM quartiles are close to their population means of 2.5. This similarity holds when comparing matched deals to others in their funds "within fund" or quarter and fund size ("within quarter"). Our matched sample appears to be very slightly better performing; in unreported analysis, we show this is driven by our exclusion of secondary buyouts. Online Table A.5 shows that the SPI-matched deals have similar leverage but are more likely to be public-to-private transactions or corporate divestitures than a typical deal in the main LBO sample.

[15] When testing for heterogeneous effects of PE ownership across worker or job characteristics, we could incorporate firm–quarter fixed effects in lieu of company and industry–quarter fixed effects. Doing so markedly reduces the sample size as it drops company-quarters without members of each cohort. Whereas this increases standard errors significantly, it does not meaningfully alter point estimates.

[16] We use review-level data to ensure that we are not artificially smoothing pretrends by aggregating to the company–quarter level and to exploit the greater power though the results are similar with company–quarter-level event studies.

[17] We find no effects on incidence in reviewer comments of the words "health insurance," shown in Online Figure A.2. Further, we find no effect on the overview rating for fringe benefits among reviewers in our sample with satisfaction ratings and a pay report (see Online Appendix C).

[18] Ratings for senior management do show statistically significant pretrends at the 10% level in our high-leverage sample; however, multiple hypothesis testing means that we should expect one or more of eight tests to fail at the 10% level because of chance. Specifically, if the null hypothesis held and the tests were independent, the probability of at least one test being statistically significant at the 10% level is $57\% = 1-(1-0.1)^8$.

[19] Olsson and Tåg (2017) look most directly at layoffs and find that, in Sweden, there is no increase in worker unemployment after buyouts. At U.S. manufacturing firms, Davis et al. (2014) also find limited net change in employment after buyouts. However, Antoni et al. (2019) find evidence of one-time layoffs after buyouts in Germany with employment declining by about 9%. Though, Davis et al. (2019) show that employment growth after an LBO varies by type of buyout and the postbuyout macroeconomic and credit conditions.

[20] Among LBOs in our LinkedIn sample, public-to-private LBOs exhibit an average growth rate of 7.3%, whereas private-to-private LBOs exhibit an average growth rate of 10.9%. With a t-statistic of 2.01, the 3.6-percentage-point gap is significant at the 5% level.

[21] To identify former employees, we make use of the employee's job ending year in Glassdoor. When job ending year is missing, we use the year of review.

[22] We use review date, deal date, and job tenure to determine if an employee is hired after the deal. An employee is a new hire if the distance between the deal date and review date is longer than the job tenure (upper bond in days). We use tenure controls to address any association between tenure and satisfaction.

[23] We also do not seek to establish the role of PE manager skill as our small sample and the high degree of randomness in deal level returns make that impossible. Korteweg and Sorensen (2017) show that the luck-related variance in buyout fund returns is six times that of the skill-related variance.

[24] To reduce the effect of outliers (including a handful of observations with near −100%), we compute the natural log of IRR plus 1.1. This results in a largest (smallest) value of our explanatory variable of 1.36 (−2.3) and a mean (standard deviation) of 0.32 (0.52).

[25] For LBO targets and for each public company at the time of the LBO, we match on year founded, industry, log number of reviews, share of jobs requiring only a high school diploma, and share of reviews by employees with at least three years of tenure. We calculate the final three variables using reviews in the three years prior to the LBO. Online Table A.12 shows the companies in our mimicking trades have similar compensation ratings to our LBO sample but somewhat worse ex ante ratings on the other dimensions. We control for the predeal average rating in estimation, which helps to address this.

[26] We focus only on satisfaction outcomes for sample consistency (i.e., not all matched public controls that have ratings have salary reports and vice versa) and for statistical power considerations.

[27] This is equal to the negative effect of LBOs on ratings (0.106) divided by the combined effect of return percentile increase on LBO ratings (0.220 + 0.229) and recentered: 0.106/(0.220 + 0.229) + 0.5.

[28] In unreported results, we find similar interactions for TVM but weaker ones for PME. Also, we find a positive but not significant difference in the pay passthrough of LBO returns relative to public companies.

[29] Data on unemployment are from the U.S. Bureau of Labor Statistics and at the North American Industry Classification System (NAICS) three-digit year level, which we match manually to Glassdoor industries.

[30] See also Bronars and Deere (1991), Bae et al. (2011), Benmelech et al. (2012), and Michaels et al. (2019).

[31] We caveat that our measures of within-firm pay inequality are calculated only on the subset of workers for which we have pay reports. Because this introduces possible bias by which types of workers choose to report their income on Glassdoor, we first residualize pay by a quadratic in years of experience along with job title and industry–quarter fixed effects and then calculate the standard deviation, 90–10 ratio, and 90–50 ratio of these residuals.

[32] The few examples of exogenous variation, such as Agrawal and Matsa (2013) and Simintzi et al. (2015), come from the side of worker bargaining power, not leverage (which could be related to worker bargaining power through other channels). One way our analysis is differentiated from the existing literature on the relationship between capital structure and labor is that we take the perspective of the employee responding to a change in leverage, whereas a commonality of the diverse existing literature is a focus on the firm's perspective, examining how the firm reacts to worker demands.

[33] Specifically, we calculate debt as dlcq plus dlttq, use atq for assets, and define buying back shares and paying out cash as quarterized prstkcy − sstky + dvy.

[34] Specifically, we add five continuous controls for the percentage of reviews in each company-quarter by employees who report the following characteristics: a tenure of less than three years, a job title that indicates the employee is a manager, a job title that typically requires masters or professional degrees, a job title that typically requires above-median work experience, and a location in the company headquarter's metropolitan statistical area.

[35] We provide more detail on this in Online Appendix C.

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