Private Equity Buyouts and Workplace Safety*

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Abstract

This paper presents evidence of a large, persistent decline in establishment-level workplace injury rates after private equity (PE) buyouts of publicly traded U.S. firms. We find that firms experience fewer OSHA safety violations after buyouts and that a larger decline in injury rates is associated with an increased probability of exit via IPO. Employment reductions after buyouts are concentrated in relatively low-injury-risk establishments. Overall, our results suggest that buyouts improve workplace safety and that PE acquirers benefit from this improvement. We explore possible causes of these changes through interviews with executives of companies acquired in buyouts and through cross-sectional analysis. (*JEL* G32, G34, J28)

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While private equity (PE) buyouts generate excess returns for investors (Harris, Jenkinson, and Kaplan, 2014), many commentators have expressed concerns that workers of acquired companies face layoffs and reduced wages after these transactions. However, employment risk and compensation are not the only margins on which a buyout might affect worker well-being. One particularly important and often-overlooked factor affecting production-level workers specifically is workplace safety. Labor historians generally attribute the rise of labor unions in the early 1900s primarily to concerns about dangerous working conditions (Dubofsky and McCartin, 2017). Despite substantial improvements in working conditions over the past century, U.S. private sector workers still experienced more than 100 million workplace injuries requiring treatment beyond first aid over the period 1990–2015.[1] Mounting evidence that may have profound implications for workplace safety indicates that substantial changes in operational structure and policies often follow buyouts (e.g. Davis et al., 2014). Yet, evidence on the effect of buyouts on this important margin of worker well-being is lacking.

This paper studies the evolution of workplace safety records after PE buyouts, analyzing establishment-level data from the Bureau of Labor Statistics' (BLS) Survey of Occupational Illnesses and Injuries (SOII). We find a large, sustained decline in workplace injury rates after buyouts of publicly traded companies. Compared to similarly sized control establishments in the same industry, annual injuries per employee fall by an average of 0.74 to 1.00 percentage points from the 4 years before to 4 years after an establishment's parent company is acquired in a buyout, or 11.1% to 15.0% of the prebuyout mean. For context, a comparable decline in workplace injury rates across all establishments in the United States would result in between 650,000 and 880,000 fewer workplace injuries per year. The decline appears the second year post-buyout, persists through at least the fourth year post-buyout, and is evident across

[1]The International Labour Organization (ILO) reports 430 million occupational injuries and illnesses and 355,000 fatalities globally per year (among 3 billion workers), which collectively cost an estimated 4% of global gross domestic product (ILO, 2003). Estimates of the compensating wage differential required for a statistical workplace injury in the United States range from US$(2018)20,000 to US$(2018)70,000 (Viscusi and Aldy, 2003).

multiple industries. While we lack the data to explore any compensating wage differentials, these findings suggest a novel dimension on which buyouts may positively affect workers.

We also explore the consequences of reductions in workplace injury rates after buyouts for firms. Cohn and Wardlaw (2016) find a negative association between workplace injury rates and firm value for publicly traded firms, suggesting that improvements in workplace safety could be a source of value creation for investors. Analyzing auxiliary data on workplace health and safety inspections from the Occupational Safety and Hazard Administration (OSHA), we find that inspected establishments are less likely to be cited for violations after buyouts. As violations carry fines, this finding points to a concrete dimension on which improvements in workplace safety after buyouts may benefit firms and their PE owners. This finding also helps allay concerns that the decline in workplace injuries we observe after buyouts could reflect changes in *reporting* practices rather than actual improvements in workplace safety. We also find that a firm is more likely to exit buyout status through an initial public offering (IPO) when its injury rate falls more post-buyout. As an IPO is typically the most profitable form of exit (Guo, Hotchkiss, and Song, 2011), this finding provides another piece of evidence that PE owners benefit from improvements in workplace safety.

We also use the workplace injury data to shed new light on employment dynamics after buyouts. We find that employment decreases after buyouts, confirming the findings of prior research (Davis et al., 2014). We also find that more dangerous establishments experience smaller decreases in injury rates after buyouts.[2] While we do not observe which specific jobs are eliminated, this finding suggests that job reductions are more likely to occur in establishments with excess back office staff, which are exposed to low levels of injury risk.[3]

To better understand the nature of the operational changes driving our results, we con-

[2]We also find that lower injury rate establishments are less likely to be reobserved in the data post-buyout. Because only a fraction of establishments is surveyed in the SOII in any given year, we note that we cannot distinguish between an establishment that closes and one that is simply not resurveyed.

[3]Consistent with this finding, Antoni, Maug, and Obernberger (2015) find that job reductions after buyouts in Germany are primarily concentrated in back-office jobs.

ducted interviews with both executives of companies acquired in buyouts and PE executives responsible for overseeing portfolio companies. We describe these interviews in detail in Section 6. The executives we interviewed broadly indicated that they were aware of post-buyout declines in workplace injury rates. Moreover, they indicated that these declines were a result of operational changes within the acquired company and, in some cases, were an explicit objective. Specific operational changes that executives linked to a decline in workplace injury rates include refocusing on core operations and increased monitoring at all levels of the organization.[4]

We also examine cross-sectional variation in the change in workplace injury rates after buyouts. We find that workplace injury rates decline more in companies with more physical assets, where the overall exposure to workplace injury risk is likely to be higher. The decline in injury rates is smaller after more highly levered buyouts, though our estimates here are less precise because we only observe post-buyout leverage for a subsample of buyouts. This finding dovetails both directionally and quantitatively with the conclusions of Cohn and Wardlaw (2016) that workplace injury rates in public firms increase with leverage. The fact that *average* injury rates decline after buyouts, which typically involve significant increases in leverage, suggests that other changes after buyouts, such as those discussed above, outweigh the effects of leverage on the balance.

One possible explanation for the decline in workplace injury rates after buyouts is the systematic automation or offshoring of dangerous jobs. A decrease in injury rates due simply to the elimination of dangerous jobs would be difficult to square with the concentration of employment reductions in low- rather than high-injury-rate establishments. However, automation or offshoring could lead to the replacement of high-injury-risk jobs with higher-skilled, low-injury-risk jobs in an establishment, even if total employment does not fall. While

[4]Increased monitoring may be both a motivation for and a by-product of the increase in information technology investment after buyouts that Agrawal and Tambe (2016) document.

we do not observe automation or offshoring directly, Autor and Dorn (2013) conclude that the jobs most susceptible to automation and offshoring involve the performance of "routine tasks." We find no relationship between the change in injury rates and industry-level routine-task intensity measured before a buyout. Our discussions with executives suggest that the role of automation in particular may be subtle. Many of the executives refer to what we characterize as "soft" forms of automation that streamline employee workflow and reduce physical touches, which make jobs safer rather than eliminate jobs.

We also consider the role that reductions in agency conflicts due to strengthening of governance might play in driving the decline in workplace injury rates after buyouts. The decline in workplace injury rates is not related to observable governance changes, such as replacement of the board chair, addition of directors to the board, or addition of the PE firm's own executives to the board, and is actually smaller when the CEO is replaced. However, as with automation, our interviews with executives suggest subtler changes in governance that might affect workplace safety. A common theme from these interviews is an increase in the amount of information collected, including workplace safety information specifically in some instances, and monitored throughout the organization after buyouts. Operational improvements due to stronger monitoring are likely to contribute to improvements in workplace safety.

The decline in workplace injury rates is greater in firms with positive abnormal accruals, high levels of analyst coverage, and significant holdings by high-turnover institutional shareholders. These findings could indicate a connection between the decline in workplace injury rates and reductions in "short-termism" as a result of the buyout, since workplace safety is best characterized as a long-term asset from a firm's standpoint. There is an ongoing debate about whether market scrutiny of short-term performance and executive compensation contracts with short horizons cause a bias toward short-term cash flows when publicly traded firms make decisions. Relatedly, we find that workplace injury rates after buyouts of the

publicly traded companies that constitute our primary sample decline *relative to* the change after a smaller sample of private company buyout establishments. While our interviews suggest a lengthening of decision horizons after buyouts in general, any conclusions here are speculative, since our proxies for short-termism could also proxy for other firm characteristics, and public and private buyout targets may differ along many dimensions. Nevertheless, our findings suggest that further investigation into changes in investment horizons after PE buyouts could be a fruitful direction for future research.

Our paper contributes to the literature on the impact of PE ownership on a firm's employees.[5] Existing work generally documents a reduction in employment and compensation after buyouts, consistent with the popular view that buyouts are harmful to workers.[6] Our analysis points to at least one dimension along which buyouts may actually benefit employees. Agrawal and Tambe (2016) find that exposure to information technology (IT) investment after buyouts increases the value of employee human capital. However, they find that these benefits hold only for white-collar workers and managers. In contrast, our evidence relates to the well-being of production-level workers, a larger and more vulnerable segment of the workforce.

Our paper also adds to recent work on changes in operating performance after PE buyouts. Davis et al. (2014) document significant improvements in total factor productivity after PE buyouts, most of it driven by reallocation of resources from low- to high-productivity establishments.[7] In contrast, we document substantial *within-establishment* improvements in a specific facet of operations. In the same general vein, Bernstein and Sheen (2016) find that restaurants' health ratings improve after their parent firms are acquired in PE buyouts.

[5]Papers studying the impact of PE buyouts on nonemployee stakeholders include those by Fracassi, Previtero, and Sheen (2017) (supermarket buyers) and Eaton, Howell, and Yannelis (2018) (for-profit college students).

[6]See, for example, Kaplan (1989), Muscarella and Vetsuypens (1990), Lichtenberg and Siegel (1990), Wright, Thompson, and Robbie (1992), Amess and Wright (2007), Boucly, Sraer, and Thesmar (2011), Davis et al. (2014), Antoni, Maug, and Obernberger (2015), and Davis et al. (2019).

[7]Brav, Jiang, and Kim (2015) find similar results following shareholder activism campaigns.

Our paper complements theirs by studying buyouts across a broad set of industries and focusing on a previously unexplored low-level dimension of operational improvements. Our interviews with executives of companies acquired in PE buyouts and PE executives responsible for overseeing portfolio companies also shed further light on the nature of operational changes after buyouts.[8]

1 Data and Sample Construction

In this section, we describe the data that we use in the paper as well as the process we use to match buyouts with establishment-level workplace injury data from the BLS' SOII. We also describe matched samples of establishments of acquired firms and control establishments that we use to conduct the difference-in-differences analysis.

1.1 Data sources

We obtain our sample of PE buyouts from Cohn, Mills, and Towery (2014). This paper builds a sample of whole-firm buyouts of publicly traded companies taking place between 1995 and 2007 using data from SDC Platinum and Dealogic, supplemented with news articles to remove improperly classified transactions. It consists of buyouts of nonbankrupt U.S. "C" corporations with at least $10 million in assets.[9] In later supplemental analysis, we also use a sample of private firm buyouts covering the same time period obtained from Cohn, Hotchkiss, and Towery (2015).

[8] A large literature studies changes in accounting measures of operating performance after PE buyouts, including work by Kaplan (1989), Muscarella and Vetsuypens (1990), Smith (1990), Wright, Thompson, and Robbie (1992), Smart and Waldfogel (1994), Amess and Wright (2007), Guo, Hotchkiss, and Song (2011), Boucly, Sraer, and Thesmar (2011), Cohn, Mills, and Towery (2014), and Cohn, Hotchkiss, and Towery (2015). Kaplan (1989), Denis (1994), and Kaplan (1994) describe specific operational improvements in case studies of four separate buyouts. Bernstein, Lerner, Sorensen, and Strömberg (2016) find that industries in which PE firms invest tend to grow as a whole, suggesting effects spill over within an industry.

[9] The restriction to "C" corporations excludes "pass-through entities," such as partnerships, "S" corporations, and limited liability companies (LLCs).

The BLS conducts the SOII each year by collecting injury and illness data based on Occupational Safety and Health Administration (OSHA) recordkeeping requirements. This process involves gathering data for hundreds of thousands of establishments in a stratified sampling process. Employers covered under the Occupational Safety and Health Act and employers selected to be part of the BLS survey are required to maintain a log recording any injuries "that result in death, loss of consciousness, days away from work, restricted work activity or job transfer, or medical treatment *beyond* first aid." These employers must make their injury logs available to OSHA inspectors and supply the data contained in the logs to the BLS. The SOII is primarily used to produce aggregate statistics on the state of occupational risk in various industries in the United States. Annual establishment-level SOII data are available starting in 1996.

Each establishment in the SOII data has a unique identifier. Each establishment-year record contains the establishment's name, location, SIC code, number of injuries during the year ($Injuries$), number of injuries resulting in days away from work, restricted activity, or job transfer ($DARTInjuries$), average number of employees during the year ($Employees$), and total number of hours worked ($HoursWorked$). We use this data to construct annual measures of the injury rate at each establishment. Our primary injury rate measure is $Injuries/Employee$, which is $Injuries$ divided by $Employees$. We also construct the measure $DARTInjuries/Employee$, which is $DARTInjuries$ divided by $Employees$, and which captures the rate of relatively serious injuries. Finally, we compute $log(Employees)$, which is the natural log of an establishment's reported average employment over the year, and $HoursWorked/Employee$, which is $HoursWorked$ divided by $Employees$, further divided by 1,000 for convenience to reduce the number of significant digits we need to report. The only firm-level identifier in the SOII data is the parent firm's employer identification number (EIN).

The SOII microdata contain no additional information about the details of injury inci-

dents. However, Table 1 shows the percentage of injuries in the United States in 2014 by different causes (panel A) and types (panel B) as reported in the BLS' annual news release on employer-related workplace injuries and illnesses (BLS, 2015).[10] The leading causes of workplace injuries are contact with objects, falls, and physical overexertion, whereas the most common injury types are sprains, strains, or tears; soreness and pain; bruises and contusions; cuts and lacerations; and fractures.

— Insert Table 1 here —

We supplement our SOII injury data analysis using establishment-level data on safety inspections and violations from OSHA. OSHA conducts approximately 100,000 safety inspections annually. Data on these inspections and any resultant violations going back to 1970 can be found at the website of the Department of Labor.[11] The data include information on whether advance notice was given before the inspection, whether the inspection resulted in the finding of a violation, and, if so, whether the violation was considered serious or not. In our analysis of the OSHA inspections data, we only examine only surprise inspections, that is, inspections in which OSHA gave no advance notice.

Finally, we obtain data on various characteristics of each target firm, buyer, and transaction, which we use in cross-sectional analysis. We obtain financial data from Compustat, analyst coverage data from I/B/E/S, institutional investor holdings data from the Thomson Reuters 13(f) filings database, PE firm information from Capital IQ, and executive and director information from Captial IQ's People Intelligence database. See Appendix Appendix A. for definitions of all of the variables. We also identify IPOs of PE-backed firms using data from SDC Platinum that is hand-checked for accuracy.

[10] This information is available only for injuries resulting in at least one lost work day.
[11] https://enforcedata.dol.gov/views/data_summary.php

1.2 Linking PE buyouts to workplace safety records

Because establishment-level BLS data are available starting in 1996, we consider only buyouts taking place in 1997 and later. Thus, our buyout sample period is 1997–2007. This period includes the buyout wave of the mid-2000s. Before merging the buyout data with the BLS data, we remove buyouts of firms in the finance industry (12 buyouts) or that engage in franchising (20 buyouts). We make the latter determination by visiting company websites and searching for other internet-based information about franchising opportunities. Removing franchisers is important because a franchiser may have limited control over the operational practices of its franchisees.[12] This process results in a starting sample of 285 public-firm buyouts and 547 private-firm buyouts.

We start by using EINs from Compustat to match establishments in the BLS data to buyout firms. However, Compustat provides only a single EIN, while firms often have multiple EINs, and different establishments belonging to the same firm often report different EINs. An added challenge is that EINs are available in the BLS data only for the period 2002–2012. To address this limitation, we assign a parent firm to an establishment-year in the 1996–2001 period if the establishment is matched to that parent firm based on EIN after 2001.

After identifying establishments of firms acquired in buyouts via EIN, we obtain additional matches by manually comparing each buyout firm's name to establishment names in the BLS data. In addition to looking for obvious matches, we use information from corporate websites, Bloomberg Business, and news articles to identify other names under which a firm operates. If we cannot determine with near-certainty that an establishment belongs to a given buyout firm, we do not create the match. For the supplemental sample of private firm

[12]Motivated by the same logic, Bernstein and Sheen (2016) treat franchised restaurants as a control group in their analysis of the effect of PE buyouts on restaurant health code violations. We cannot employ this approach because our data do not allow us to identify whether a given location is firm or franchisee owned. In addition, the number of franchisers in our sample is small.

buyouts, we can only match based on name.[13] We refer to establishments in the BLS data belonging to PE-acquired firms as "buyout establishments."

We match 13,452 unique establishments to 244 unique public buyout targets (approximately 55 establishments per buyout) and 2,051 unique establishments to 316 unique private buyout targets (approximately 6.5 establishments per buyout). It is not surprising that we match more establishments to public targets than to private targets, as public buyout targets tend to have far more establishments than private targets. Davis et al. (2019) report approximately 112 establishments per public buyout target and 16 establishments per private buyout target based on census data. It is also not surprising that we identify fewer establishments per firm than they do, as only a fraction of establishments is surveyed in the SOII in any given year. We match establishments in the OSHA inspection data to buyout firms based on establishment name.

1.3 Matched treatment and control sample formation

Our primary empirical strategy, which we describe in Section 2, is a generalized difference-in-differences approach. Specifically, we compare changes in injury rates at establishments of acquired firms from the 4 years before to 4 years after buyouts to changes over the same period for matched control establishments. Thus, we only consider buyout establishments in the SOII in at least one of the 4 years before and at least one of the 4 years after the buyout. We form matched buyout and control samples by matching each buyout establishment to up to five establishments that were never acquired in buyouts during the sample period. We consider only potential controls that are (a) in the same four-digit SIC industry as the buyout establishment, (b) present in the SOII in at least the same years in the 8-year window around the buyout as the buyout establishment, and (c) within 50% to 200% of the public

[13]The resultant link files for both public and private buyout establishments are stored at the BLS and can be made available to researchers on-site.

establishment's size based on number of employees. Within that set of candidate controls, we choose those closest in terms of $log(Employees)$. For each selected control establishment, we only retain establishment-years that coincide with the years the buyout establishment is in the SOII.[14]

In our main analysis, we restrict attention to buyout establishments (and their matched controls) with at least 100 employees in the most recent prebuyout year in the injury data. Meaningful injury rates are difficult to calculate for small establishments because the inability of an employee to suffer a fractional injury results in a preponderance of both zero and very high injury rates for these establishments, adding noise to ordinary least squares (OLS) regressions where injury rate is the dependent variable. We also do not require that control establishments matched to establishments of public (private) firms acquired in buyouts belong to public (private) firms themselves since doing so makes it more difficult to obtain matches and therefore considerably shrinks the overall sample size. We relax both of these assumptions in robustness checks, which we describe at the end of this section, and our estimates change little. Table 2 summarizes the sample formation.

— Table 2 here —

Panel A reports the number of firms, establishments, and establishment-years at each step in the formation process for both public and private buyout samples. The need for an establishment to appear in the data in both the pre- and post-buyout windows produces the most attrition in the final sample of buyout establishments. Because the BLS only surveys a fraction of establishments each year, many establishments go 4 years without being surveyed. While, as a result, the sample we can analyze represents only a fraction of

[14]As an example, if the establishment of a firm acquired in a 2003 buyout is in the SOII in 1999, 2002, 2005, and 2007, then we only consider potential control establishments also observed in the SOII in at least 1999, 2002, 2005, and 2007. A control establishment also might be in the SOII in additional years during the 8-year window around the buyout, say, 2001 and 2008, but we exclude these nonoverlapping years when forming our data set.

all acquired establishments, we see no reason why the largely random survey-based limitation would result in estimation bias.

Panel B reports the number of establishments in the final sample matched based on EIN and name. We match 64% of public target buyout establishments in the main sample on the basis of EIN and 36% based on name. Because we do not have an EIN for private buyout targets, 100% of private target buyout establishments in the main sample are matched on the basis of name. Panel C reports the number of buyout establishment-year observations in the final sample by year relative to the year of the buyout. Attrition in the post-buyout period appears fairly minimal. Finally, panel D reports the number of control establishments for each buyout establishment in the sample. It shows that most buyout establishments are matched to five control establishments (the maximum number possible).

Appendix Appendix B. presents alternative variants of our main analysis. These variations include matching establishments of publicly traded firms to establishments of other publicly traded firms only (B1), identifying public buyout establishments on the basis of name only, that is, without the use of EINs (B3), matching each buyout establishment to one control establishment, not five (B4), using propensity score matching to match based on multiple establishment characteristics instead of just industry and size (B2), and lowering the minimum establishment size for inclusion in the sample from 100 to 50 employees (B5). We also estimate count models where we impose no restriction on establishment size (B6).

Table 3 presents summary statistics for the characteristics of the firms and establishments in our final sample. Panel A reports means of several characteristics for buyouts establishments and their matched control establishments. The means of all of the observable characteristics we examine are similar for the buyout and control samples, despite the fact that we match only on industry and establishment size. While we cannot rule out the possibility that buyout and control establishments vary along unobserved dimensions, the homogeneity between treatment and control establishments provides some assurance that

the as-if random assignment assumption for valid difference-in-differences estimation is likely satisfied. It is also worth noting the similarity in the size (number of employees) of public- and private-target buyout establishments that make it into the final sample; size similarity makes comparisons between the two groups at least somewhat meaningful.

— Table 3 here —

Panel B reports the breakdown of establishments into four broad industry categories. These categories are manufacturing (SIC codes in the 2000s and 3000s), transportation (SIC codes in the 4000s), trade (SIC codes in the 5000s), and services (SIC codes in the 7000s and 8000s).[15] Panel C reports financial characteristics of public-firm buyout targets in the main sample, calculated using Compustat data as of the last fiscal year-end prior to the buyout. For comparison, the means and medians of these characteristics for the Compustat universe during the sample period are shown to the right. Buyout firms in our sample tend to be significantly larger than Compustat firm in terms of medians but slightly smaller in terms of means. They also tend to have lower Tobin's q. Along other dimensions, buyout targets are similar to Compustat firms in general.

We use a similar approach to assign control establishments to each buyout establishment in the supplemental OSHA inspection data. Specifically, we match each inspected buyout establishment to inspected control establishments in the same four-digit SIC code experiencing the same inspection type and scope. We retain the five closest-sized establishments to the buyout establishment in terms of log employment immediately prior to the buyout. Within our sample, approximately 60% of inspections are safety related, with the remainder health related. Approximately 65% of inspections uncovered a violation and 41% a serious violation.

[15] Disclosure limitations prevent us from providing a superfine breakdown.

2 Empirical Methodology

We employ a standard generalized difference-in-differences approach to estimate changes in establishment-level injury rates after PE buyouts, relative to changes at control establishments. Denoting establishment by i, year by t, and four-digit SIC code industry by j, our primary regression specification is the following:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} + \epsilon_{it}. \quad (1)$$

We use $Injuries/Employee$ and $DARTInjuries/Employee$ as measures of $InjuryRate$ in estimating Equation (1). The indicator $BuyoutFirm$ equals one for buyout establishments and zero for control establishments. The indicator $PostBuyout$ equals zero in the 4-year prebuyout period and one for observations in the 4-year post-buyout period. We exclude establishment-year observations from the buyout year itself because the parent firm is independent part of that year and under PE ownership part of the year. We include both establishment fixed effects (α_i) and four-digit SIC code-by-year fixed effects (ϕ_{jt}) to account for any unobserved time-invariant establishment factors and time-varying industry factors that might affect injury rates.[16] The coefficient γ captures the estimated change in injury rate from before to after a buyout for buyout establishments relative to control establishments.

While estimates of regression Equation (1) capture the average change in injury rates from the 4 years before to 4 years after a buyout, they do not indicate the timing of these changes. We explore how injury rates evolve over time after buyouts in more detail by

[16] Because the buyout year varies across establishments, we can distinguish industry-year fixed effects from the treatment effect. Note that the main effect of $BuyoutFirm$ is not included, because it does not vary within establishment and is therefore absorbed by the establishment fixed effects.

estimating the following regression:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \sum_{K \in (-4,1) \cup (1,4)} \beta_K YearRelBuyoutK_{it} + \sum_{K \in (-4,1) \cup (1,4)} \gamma_K BuyoutFirm_i * YearRelBuyoutK_{it} + \epsilon_{it}. \quad (2)$$

Here, $K = -4, -3, -2, -1, 1, 2, 3, 4$ represents the number of years an observation occurs relative to the year of the buyout year. We include buyout-year observations (i.e., $K = 0$) in estimating this regression, unlike our estimation of Equation (1), where we do not. The buyout year is the omitted year in the regression. The γ_K coefficients capture the difference between injury rate in year K relative to the buyout year and injury rate in the buyout year.

Finally, we examine how changes in workplace injury rates vary with observable characteristics of the target firm, the buyer, and the transaction itself. While the results from this analysis may be open to multiple interpretations, it provides insight into the scenarios in which changes in workplace injury rates are more likely. This cross-sectional analysis involves estimating regressions of the following form:

$$\begin{aligned} InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} \\ + \eta PostBuyout_t * Characteristic_i \\ + \delta BuyoutFirm_i * PostBuyout_{it} * Characteristic_i + \epsilon_{it}, \end{aligned} \quad (3)$$

where $Characteristic$ is a firm- or transaction-level characteristic.[17] The coefficient δ on the triple interaction term $BuyoutFirm * PostBuyout * Characteristic$ captures the cross-sectional variation of the change in injury rates with the given characteristic.

[17]The main effects of $BuyoutFirm$ and $BuyoutFirm * Characteristic$ are both fully absorbed by the establishment fixed effects α_i and are therefore omitted from the regression equation.

3 Evolution of Injury Rates around PE Buyouts

We begin our analysis by presenting a series of plots of workplace injury rates at buyout and control establishments in each year around the buyout year. We then turn to formal estimation based on the methodology described in Section 2.

3.1 Graphical analysis of injury rates around buyouts

Figure 1 plots the mean injury rates for our sample. Figures 1a and 1b plot the mean $Injuries/Employee$ and $DARTInjuries/Employee$, respectively. Figures 1c and 1d plot the industry-adjusted rates, where we first subtract the mean rate for all establishments in the same year and the four-digit SIC code industry. The points in these latter two plots are equivalent to the mean residuals from a regression of injury rates on industry-year fixed effects.

— Figure 1 here —

The plots reveal similar patterns. A comparison of prebuyout injury rate trends in buyout and control establishments reveals no obvious differences, suggesting that the parallel trends assumption required for valid difference-in-differences estimation is likely to be satisfied. The plots also show that injury rates for public-firm buyout establishments fall below those of control establishments in the second year post-buyout and remain below through the fourth year after the buyout. These patterns hold for both the overall injury rate and the rate of more serious DART injuries. The patterns are consistent with injury rates declining after public-firm buyouts with a short lag, as one would expect if operational changes implemented after buyouts take time to translate into observable outcomes.

3.2 Difference-in-differences estimates

Table 4 presents estimates based on regression Equation (1). We report standard errors, clustered at the firm level, below each point estimate, both in this table and in all of the remaining tables in the paper. The dependent variable in columns 1 through 3 in each panel is $Injuries/Employee$. Column 1 reports estimates excluding establishment fixed effects (industry-year fixed effects are included). This exclusion allows us to estimate the main effect of $BuyoutFirm$. Columns 2 and 3 report estimates of Equation (1) with establishment fixed effects, first excluding and then including establishment-level controls. Columns 4 through 6 present the same three regressions, where the dependent variable is $DARTInjuries/Employee$.

— Table 4 here —

The small and statistically insignificant coefficients for $BuyoutFirm$ in columns 1 and 4 suggest no differences in prebuyout injury rates in public-firm buyout and control establishments. The statistically insignificant coefficients for $PostBuyout$ in all columns suggest that control establishments do not experience unexplained changes in injury rates from before to after the buyout year. The negative coefficients for the interaction between $BuyoutFirm$ and $PostBuyout$ in columns 1 through 3 support a decline in injury rates at buyout establishments relative to control establishments after buyouts. The interaction coefficient is statistically significant at the 5% or better level in all three regressions.

The point estimates indicate an average fall in annual injuries per employee of 0.74 to 1.00 percentage points, or 11.1% to 15.0% of the prebuyout mean of 0.0669 for buyout establishments (see Table **??**). For the average size public buyout establishment (448 employees), this fall translates into 3.3 to 4.5 fewer workplace injuries per year, or 13.3 to 17.9 over the first 4 years post-buyout (the post-buyout estimation window). To contextualize these estimates, we note that a comparable decline in injury rates across all private-sector estab-

lishments in the United States would result in between approximately 650,000 and 880,000 fewer workplace injuries per year, based on the size of the private-sector workforce in 2002, the middle year of our buyout sample period.[18] Noting that the aggregate rate of workplace injuries per employee in the United States has steadily fallen by about 0.25 percentage points per year over the last 40 years, the reduction in injury rates after buyouts is comparable to advancing 3 to 4 years relative to the aggregate trend.

The coefficients for the interaction between $BuyoutFirm$ and $PostBuyout$ are also negative in columns 4 through 6 and are statistically significant at the 1%, 10%, and 5%levels, respectively. The point estimates indicate an average decline in injuries requiring days away from work or temporary restrictions or transfer per employee of 7.0% to 12.9% relative to the prebuyout mean of 0.0341 for buyout establishments. Thus, the decrease in workplace injury rates after buyouts holds even when we restrict attention to only the most serious injuries.

The positive coefficient for $HoursWorked/Employee$ in both panels is not surprising, since time spent working represents an employee's exposure to the arrival risk of injuries. Note that, because we normalize $HoursWorked/Employee$ by dividing it by 1,000, the coefficient of -0.0137 indicates an expected decrease of 0.0274 injuries per year per full-time (i.e., 2,000 hours per year) employee. Of course, less-mechanical reasons could explain this association as well. Employees who work too hard may experience a higher injury risk. Alternatively, employees who work too little may become rusty and thereby increase their injury risk. The negative coefficients for $log(Employees)$ could reflect the relative sophistication of larger establishments' operations, which are likely to be more heavily automated due to economies of scale in automation.

[18]We calculate the hypothetical response implied by a coefficient of 0.0074 as $(0.0074/0.053) \times 4.7M = 656,226$, where 4.7M is the number of nonfatal workplace injuries in 2002, and 0.053 is the number of injuries per equivalent full-time worker, both per a BLS (2002) news release on workplace injuries and illnesses.

3.3 Timing of changes in injury rates after buyouts

The results in Table 4 indicate a fall in injury rates after public-firm buyouts but do not give any indication of the exact timing of the fall. Operational changes generally take time to produce observable improvements in workplace safety outcomes (Clark and Margolis, 2000). It therefore would be difficult to attribute a decline in injury rates taking place immediately after a buyout to workplace safety improvements due to post-buyout operational changes. Table 5 presents estimates of the evolution of injury rates relative to controls each year around the buyout based on regression Equation (2), with six specifications mirroring those of Table 4.

— Table 5 here —

Here, the patterns are consistent with those shown in Figure 1. No clear patterns emerge for either treatment or control establishments prebuyout. The small, statistically insignificant coefficient for $BuyoutFirm * PostBuyoutYr1$ in the first column indicates that injury rates in public-firm buyout establishments remain effectively unchanged relative to those of nonbuyout establishments the first year after the buyout. The remaining interaction terms indicate that injury rates at acquired establishments fall substantially below those of control establishments the second year after the buyout and remain low through at least the fourth year after the buyout. Again, no discernible pattern materializes for the period after private-firm buyouts.

Workplace injuries decrease after public-firm buyouts on average. To assess the breadth of this phenomenon, we next break the full sample into four broad industry categories based on the SIC code of the buyout establishment. These categories are manufacturing (SIC 2000s and 3000s), transportation (4000s), trade (5000s), and services (6000s). We estimate regression Equation (1) for each of the four subsamples. Table 6 presents the results.

— Table 6 here —

The coefficients for $BuyoutFirm*PostBuyout$ are negative and large in magnitude across all four industry-category subsamples, ranging from 0.80 to 1.22 percentage points. These coefficients are statistically significant for the trade and services categories at the 5

4 Consequences for PE Investors

In this section, we consider the consequences of reductions in workplace injuries for investors. Reductions in injury rates may benefit firms in several ways: decreased downtime, fewer lawsuits, lower compensating wage differentials, and increased employee morale and productivity. Studying publicly traded firms, Cohn and Wardlaw (2016) find a negative relationship between firm value, as measured by Tobin's q, and workplace injury rates. Their estimates, applied to the firms in our sample, imply that the estimated fall in injury rates after public-firm buyouts would be associated with an average predicted increase in firm value of 1.2% to 1.6%.[19] However, workplace injury rates could proxy for operational performance more generally, and whether these estimates apply to PE-owned firms is unclear.

We conduct two sets of tests to shed further light on the implications of reductions in workplace injury rates for PE owners. First, we examine changes in the incidence of OSHA safety violations after buyouts. While most of the costs of an unsafe workplace to the employer are unobserved and in many cases intangible, OSHA violations result in fines and can create litigation risk and compliance problems that make conducting business more difficult. An added advantage of examining OSHA violations is that they represent the conclusions of an OSHA inspector and are not reported by the firm itself, sidestepping concerns that changes in reporting behavior might drive the decline in workplace injury

[19] Cohn and Wardlaw (2016) estimate that a one-unit increase in injuries per 1,000 hours worked is associated with a 3.19-unit decrease in Tobin's q in the following year. Noting that mean hours worked per employee in our sample and Tobin's q are 1,751 and 1.16, respectively, a 0.0074 decrease in injuries per employee would translate into a predicted $[3.19 \times (1,000/1,751) \times 0.0074]/1.16 = 1.2\%$ increase in firm value.

rates we document in Section 3. Second, we examine the relationship between the likelihood that a PE-acquired firm exists buyout status through an IPO, often considered a sign of a successful buyout, and the change in its workplace injury rate post-buyout.

4.1 Analysis of OSHA violations data

Here, the sample consists of establishment-years in which OSHA conducted an inspection of a given establishment. We estimate a linear probability model (LPM) variant of the generalized difference-in-differences Equation (1). The dependent variable is an indicator equal to one if the given inspection resulted in the finding of a violation and zero otherwise. We include industry-year fixed effects. Because of the infrequency of repeat observation of establishments in the data, we do not include establishment fixed effects. However, we do include inspection-type fixed effects.[20] We estimate four specific regressions based on combinations of using either one or five matched control establishments and using all violations or only serious violations to determine the dependent variable. Table 7 presents the regression estimates.

— Table 7 here —

The negative coefficients for the $BuyoutFirm \times PostBuyout$ interaction terms in all of the regressions indicate that the probability of an OSHA violation declines at buyout establishments post-buyout, relative to control establishments. The coefficients in the first and third columns imply an 8.8% or 11.6% decline in the probability of a violation relative to the prebuyout mean probability of 60.2%, while those in the second and fourth columns imply a 22.1% or 29.3% decline in serious violations relative to the prebuyout mean probability of 33.1%. It appears, then, that a reduction in OSHA violations represents one specific tangible

[20] OSHA identifies 12 inspection types: accident, complaint, referral, monitoring, variance, follow-up, unprogrammed related, planned, programmed related, unprogrammed other, programmed other, other, and fatality/catastrophe. We exclude planned inspections from our analysis.

dimension on which a firm itself may benefit from a buyout. This finding also provides some comfort that the reduction in reported workplace injury rates is not a change in the reporting rather than the actual incidence of workplace injuries.

4.2 Changes in workplace injury rate and probability of an IPO

For each buyout firm in the final matched sample, we compute the average number of injuries per employee across establishment-years in the 4 years before and 4 years after the buyout, as well as the comparable numbers for all control establishments matched to that firm's establishments. We then compute $InjuryRateChange$ as the change in average injury rate for the buyout firm minus the change for its control establishments. We also compute $IndAdjInjuryRateChange$ as an alternative measure, substituting injury rates relative to four-digit SIC code-year means for raw injury rates. We then estimate OLS regressions where the dependent variable is an indicator for whether the firm exited buyout status via IPO and the explanatory variable is one of the two measures of injury rate changes. Table 8 presents the results from these regressions.

— Table 8 here —

The coefficients for the change in injury rate variables are negative and statistically significant at the 10

5 Employment Dynamics after Buyouts

One of the most salient observable within-establishment changes after buyouts with clear evidence is a substantial reduction in employment (Davis et al., 2014; Antoni et al., 2015; Davis et al., 2019). In this section, we explore the connections between employment changes and injury risk around buyouts. We also consider their implications for interpretation of the results in Section 3.

We begin by estimating difference-in-differences models where $log(Employees)$ and $HoursWorked/Employee$ are dependent variables using the same matched sample we have used throughout. We first estimate the relative changes in the variables unconditionally after buyouts. We then estimate triple-difference regressions using two measures of establishment-level injury risk. The injury-risk measures are the establishment's injury rate the most recent year in the data prior to the buyout ($EstabInjuryRate$) and the four-digit SIC industry-average injury rate for that year ($IndustryInjuryRate$). Table 9 presents the results.

— Table 9 here —

The dependent variables are $log(Employees)$ in the first three columns of each panel and $HoursWorked/Employee$ in the final three columns. The estimates in column 1 imply a 13% average within-establishment reduction in employment relative to prebuyout levels, almost identical to the estimate of Davis et al. (2019). The estimates in columns 2 and 3 suggest that employment falls more in relatively low-injury-risk establishments. This result is consistent with PE owners primarily laying off administrative staff, which generally faces low workplace injury risk, rather than production workers, consistent with the conclusions of **?** for German buyouts.

Column 4 reveals that hours worked per employee increases slightly unconditionally post-buyout. Any decline in hours worked per employee could help explain the decline in injuries per employee after buyouts, since fewer hours worked implies less exposure to injury risk. The fact that hours per employee increases is evidence against this idea. Column 5 reveals that hours worked per employee increase less in establishments with previously high injury rates. However, column 6 reveals that it increases slightly more in establishments in industries with high injury rates historically. It is therefore unclear whether hours worked per employee decreases more or less in more dangerous establishments within a firm.

The greater fall in employment in relatively safe establishments that we document in

columns 2 and 5 of Table 9 at least partly offsets the negative effects of declines in injury rates within establishment on overall injury rates. However, this offsetting effect proves immaterial. Figure 2 shows that the aggregate injury rates for the buyout and control groups (injuries summed across all establishments divided by employees summed across all establishments) in the sample, which account for changes in establishment size over time, exhibit patterns similar to those in Figure 1.

— Figure 2 here —

While changes in employment and hours worked per employee represent changes on the intensive margin of labor activity, firms also make changes on the extensive margin after PE buyouts, by closing and creating establishments (Davis et al., 2014). We also examine the probability with which an establishment sampled prebuyout is subsequently resampled post-buyout. We do so using the full set of establishments belonging to PE-acquired firms that are present in the BLS data in at least 1 of the 4 years immediately prior to the buyout. We regress an indicator variable equal to one if an establishment reappears in the BLS data in year $t + n$ post-buyout on its prebuyout injury risk measures, for each of $n = 1, 2, 3, 4$ separately. Table 10 presents the results.

— Table 10 here —

Reobservation rates are higher for establishments with higher prebuyout workplace injury rates, compared to controls. One important caveat is that establishments in our sample are not surveyed each year, so we cannot distinguish between a closed establishment from one that, by chance, is not reobserved in the data.

6 Discussion

In this section, we discuss how a firm's operational policies and practices affect workplace safety and how a PE buyout might change workplace safety by altering these practices. Subsection 6.1 discusses the impact of operational practices on workplace safety. This discussion is based largely on conversations with industrial safety practitioners and a case study on safety at Alcoa by Clark and Margolis (2000) and borrows from Cohn and Wardlaw (2016). Subsection 6.2 discusses the nature of operational changes after buyouts, with a focus on changes with important implications for workplace safety. This discussion is based in part on interviews we conducted with executives working for firms acquired in buyouts as well as PE executives responsible for overseeing the operations of portfolio companies.

6.1 Workplace safety and operating policies and practices

Factories, warehouses, stores, and other places of business make myriad operational decisions over time. Even in developed economies, such as the U.S. economy, many workers still toil in inherently physical jobs, such as, in construction, manufacturing, servicing, distribution (e.g., warehouses), and even many retail jobs. Risk of on-the-job injury is real for these workers, and the safety of the conditions in which they work is a first-order driver of their well-being.

Corporate objectives and policies influence the operational decisions of individual establishments within a corporation. For example, establishments may respond to corporate cost-cutting initiatives by cutting corners on maintenance, training, supervision, repair work, and other operational policies that promote workplace safety. As an extreme example, regulators investigating the 2005 explosion at BP's Texas City Refinery that killed 15 workers and injured 180 others found that management had removed replacement of a pressure valve from draft budgets in each of the 2 years before the explosion because of intense cost-cutting

pressure.[21] They concluded that failure of this pressure value contributed to the accident. More generally, Cohn and Wardlaw (2016) present evidence that an establishment's injury rate rises when its parent company has fewer financial resources available to fund operational spending.

Other corporate objectives and policies may contribute positively to workplace safety. The adoption of modern production practices aimed at increasing productivity are likely to improve workplace safety as well. For example, lockout-tagout procedures that prevent the operation of machinery in need of repair not only reduce the risk of damage to machinery and extended downtime but also reduce the risk of employees being injured by malfunctioning equipment.[22] The intense monitoring of production processes necessary to implement "six sigma" production reduces the risk of undetected faults that could cause injuries. In general, minimizing movement of employees, inventory, and equipment with an establishment in order to reduce overhead costs also reduces employee exposure to injury risk.

Firms also make operating decisions with the explicit aim of reducing workplace injuries, such as mandating the use of safety equipment, holding regular safety meetings, and establishing written procedures for handling dangerous equipment or toxic materials. As a specific example, recoil from cables that break under tension is a common hazard in fields such as shipping and distribution. While more expensive, most firms in these industries now use synthetic fiber rather than traditional steel cable because synthetic fiber cables have fewer sharp edges when they fracture, reducing hazards due to snapping cables. Firms benefit from improved workplace safety in a variety of ways. Fewer workplace injuries mean less lost work time. Forty-five percent of workplace injuries in the United States result in at least 1 day away from work, restricted work activity, or job transfer (BLS, 2016), and operations

[21] See the U.S. Chemical Safety Board's report at https://www.csb.gov/file.aspx?DocumentId=5596.

[22] Lockout procedures involve isolating and disabling power sources in dangerous machinery in a systematic, step-by-step way. Tagout procedures ensure that only specific employees can unlock and untag a machine, ensuring that malfunctioning equipment is not accidentally brought back online before it is repaired.

may be idled while the cause of the injury is investigated and mitigated. Safer workplaces also mean fewer lawsuits and OSHA violations, lower compensation wage differentials and workmen's comp insurance premiums, and higher employee morale.

In a well-chronicled example, Alcoa Corporation reorganized its entire operational architecture around reducing workplace injuries and fatalities in the 1970s. This process involved compensating managers based on workplace safety records and implementing numerous low-level changes designed to reduce workplace accident risk. For example, Alcoa introduced a forklift speed limit of 4 miles per hour on its production floors after an employee was killed in a forklift collision (Clark and Margolis, 2000). The leading source of workplace injuries in the United States in 2014 was floors, walkways, and ground surfaces (BLS, 2015). While Alcoa explicitly targeted workplace safety improvements, management expected that steps to improve workplace safety would also increase productivity. According to Charles Duhigg, who has written extensively about workplace safety, the focus on worker safety at Alcoa "led to an examination of an inefficient manufacturing process - one that made for suboptimal aluminum and danger for workers" (Clark and Margolis, 2000).[23]

6.2 Buyouts, operations, and workplace safety

When raising funds, PE firms increasingly emphasize operational improvements made in portfolio companies and downplay other potential sources of value creation, such as financial engineering or multiple expansions. Recent research finds evidence that firms do, indeed, implement considerable operational changes after buyouts. Observed changes include closing efficient establishments and shifting production to more productive establishments.

Historically, many politicians and journalists have characterized PE firms as aggressive

[23]The coupling of workplace safety and production quality seems to have been understood throughout the company. Bert Harris, a smelting department superintendent in Alcoa at the time observed, "In many ways, what we are facing in safety is exactly like the problems we face in quality-there are a lot of ways to foul up; it's only through attention to details that we find the right way to get the job done, and it's only through discipline that we get the job done that way" (Clark and Margolis, 2000).

cost-cutters who seek to squeeze cash flow out of the companies they acquire without regard to the long-term consequences. Such arguments can be difficult to square with rational expectations, as investors to whom a PE firm eventually sells a portfolio company should account for worse expected long-run performance when they value the company. A more nuanced view is that firms may extract additional cash flow by expropriating workers. Shleifer and Summers (1988) argue that an acquirer may expropriate workers by abrogating implicit contracts between workers and prior owners. While concerns usually center on layoffs and reduced wages, firms may also cut corners on workplace safety in order to reduce costs, at least in the short run, for example, by deferring maintenance, shrinking training budgets, and eliminating supervisory positions.

Our finding that workplace injuries *decline* after buyouts is at odds with this view. PE firms themselves often argue that they focus on fundamentally improving the operations of the firms they acquire rather than cutting costs. This argument could be self-serving, since it is likely to appeal to investors and to serve PE firms' public relations objectives. However, evidence suggests that operations do fundamentally improve after buyouts. For example, Bernstein and Sheen (2016) find a reduction in health code violations after restaurant buyouts. More broadly, Davis et al. (2014) find a significant increase in total factor productivity after buyouts.

To better understand the nature of operational changes after buyouts, we interviewed both executives working for companies acquired in buyouts and PE firm executives responsible for overseeing portfolio companies. We started with the list of firms in our sample. From this list, we attempted to contact people who were involved with either the target company or the buyout firm. We were able to speak to several individuals on both sides of buyout transactions, some of whom were willing to speak on background and some of whom were willing to be quoted directly.

Executives at multiple companies describe a renewed emphasis on operational execu-

tion after buyouts, with an emphasis on "the boring stuff," accompanied by divestiture of potentially distracting noncore assets built up over time in a form of "mission creep." This narrative is consistent with the closing of inefficient establishments and redistribution of jobs after public-firm buyouts that Davis et al. (2014) document and improvements in health ratings after restaurant buyouts that Bernstein and Sheen (2016) document. As a specific example, Garden Ridge Pottery closed urban locations to focus on its traditional suburban markets after its 2000 buyout. An executive there also described adoption of an employee retention plan that reduced annual turnover among store employees substantially and replacement of peg board displays with shelves and racks, which require less time to stock.

Restructuring may also result in a significant shift in the mix of labor and capital a firm employs. Olsson and Tåg (2017) find evidence of systematic elimination of "routine-task" jobs, which are most exposed to automation and outsourcing risk (Autor and Dorn, 2013), after PE buyouts in Sweden. However, the specific types of routine-task jobs that are lost is unclear. Antoni et al. (2015) find that German buyouts are followed by a loss of predominantly administrative jobs. Our conclusion that employment reductions are greater in establishments with lower injury rates suggests that the focus of job reductions after U.S. buyouts may be similar. As an anecdotal example, we offer the experience of Garden Ridge Pottery, where an executive described a reduction in administrative staff from 350 to 60 after the buyout there.[24]

Executives at multiple companies also described increased organization-wide data collection and monitoring after buyouts to support efforts to improve operational execution, which required managers to "be on their game a lot more." Many executives described an expansion of the "scorecard" used to evaluate operations to incorporate more detailed operational

[24] A comparison of U.S. and non-U.S. buyouts is challenging, since buyouts outside the United States almost exclusively involve private companies. Independent private companies may be ripe for capital-intensive investments that improve efficiency since they have limited access to capital markets.

metrics relating to throughput, downtime, and production variances.[25] An increased emphasis on data collection and monitoring is broadly consistent with the increased investment in information technology after buyouts that Agrawal and Tambe (2016) document. At a higher level, the Garden Ridge Pottery executive described a general partner from Three Cities Research, the PE buyer, working various retail wage jobs throughout the company after the buyout to generate a more complete picture of operations.

Several executives we interviewed indicated that PE buyers were willing to accept lower profitability in the short run as a part of restructuring and implementation of increased monitoring. For example, a former executive with the large mid-market buyout specialist Welsh, Carson, Anderson, and Stowe suggested that "earnings were sometimes explicitly projected to go down for the first two years after a buyout, reflecting investments that would drive up earnings and growth in years four or five" and that "you don't have that luxury in a public environment." While the view that PE ownership allows for longer decision horizons than public market ownership is controversial, it does reconcile significant improvements in operational efficiency that Davis et al. (2014) document with a lack of improvements in profitability in at least the first couple of years after buyouts (Guo et al., 2011; Cohn et al., 2014).

Several of the executives we interviewed expressed awareness of improvements in workplace safety under PE ownership. Some executives identified improvements in workplace safety as a specific plank in a broader platform of operational improvements. A former PE executive we interviewed in the energy industry who is now with Total Safety, a safety consultancy, characterized the view on improved workplace safety as follows: "Fewer compliance problems, less scrutiny from regulators, sure, but the really good companies recognize that safe working environments increase morale, decrease turnover, and impact wage negotia-

[25] A couple of executives we interviewed specifically mentioned the formal introduction of data-intensive six sigma management.

tions."

An important factor to consider is the timing of any improvements in workplace safety due to operational changes after buyouts. Large operational changes generally take time to implement. Employees need time to adapt to new work routines and often initially resist procedural changes, including those that improve workplace safety, because of the extra effort required to abide by them (Clark and Margolis, 2000). Even after such changes are successfully implemented, a period of learning and refinement may be required before injury risk declines substantively. The timing of decline in workplace injury rates we observe after buyouts appears consistent with such a delay, since the decline appears primarily starting in the second year after a buyout.

7 Cross-Sectional Analysis

This section explores the sensitivity of the change in workplace injury rates after PE buyouts to various firm, acquirer, and transaction characteristics by estimating variations of regression equation (3). Our primary sample consists of establishments belonging to publicly traded companies acquired in PE buyouts. However, we also have a smaller matched sample of establishments, described in Section 1, belonging to private companies acquired in buyouts. Recent research finds significant differences in the nature of operational changes after buyouts of public and private companies (e.g., Davis et al., 2014).

To shed further light on possible differences between public and private firm buyouts, we compare the change in workplace injury rates after buyouts of publicly traded and private firms.[26] We do so by combining the public and private buyout matched samples and esti-

[26]Because the private firm buyout sample of establishments is small, we do not independently analyze it in detail. However, we do present difference-in-differences estimates mirroring those in Table 4 based on the private firm buyout sample in Table B7 in the appendix. This table reveals no change in workplace injury rates after these buyouts, though we would expect the tests to have limited statistical power because of the small sample size.

mating the triple interaction regression defined in (3), where $Characteristic$ in the triple interaction is $WasPublic$. This variable takes a value of one if the observation is in the matched public firm buyout sample and zero if it is in the matched private firm buyout sample. Table 11 presents the results, with six specifications mirroring those of Table 4.

— Table 11 here —

The coefficients for the triple interaction term $BuyoutFirm * PostBuyout * WasPublic$ are negative, large, and statistically significant at least at the 10% level in all six specifications. This negative coefficient implies that workplace injuries fall in establishments of public companies acquired in buyouts not just relative to control firms, but also relative to establishments of private companies acquired in buyouts. While we cannot observe the causes of these differences, and public and private firms acquired in buyouts could differ in many ways, this finding nevertheless adds to the evidence of important differences in operational changes after buyouts of public and private firms.

In our final analysis, we examine cross-sectional differences in the changes in workplace injury rates after public firm buyouts by estimating (3), using just the public firm buyout matched sample. Here, we set $Characteristic$ in the triple interaction to various observable firm, acquirer, and transaction characteristics. Table 12 reports the results. Panel A reports estimates where the dependent variable is $Injuries/Employee$. Panel B reports estimates where the dependent variable is $DARTInjuries/Employee$. We only report coefficients for $BuyoutFirm * PostBuyout$ and $BuyoutFirm * PostBuyout * Characteristic$ in the table for the sake of brevity. We are careful not to draw strong conclusions, since many of the cross-sectional variables could proxy for multiple underlying target, acquirer, and transaction characteristics.

— Table 12 here —

Workplace injury rates decline more after buyouts of firms with more tangible assets.

This finding is intuitive - the scope for reducing injury rates is likely to be higher in firms where production is more physical. The change in injury rates is not related to the size of the acquired firm. It is also not related to the fraction of employees in the firm's industry involved in routine task work. This nonresult may shed light on the role that changes in job composition due to restructuring play in the reduction in workplace injury rates after buyouts. Autor and Dorn (2013) argue that routine-task jobs are most prone to automation and offshoring. If workplace injury rates fall after buyouts because PE buyers systematically target firms with large potential savings through automation or offshoring, then one would expect a larger drop in workplace injury rates in firms where these opportunities are larger, that is, those in industries in which routine-task jobs are prevalent. However, one would need job-level data to more definitely test the role of such restructuring or of changes in job composition more generally. Moreover, our executive interviews suggest that "soft" forms of automation that reduce physical touches in the production process could be important, even if replacement of employees with robots is not a first-order driver of the change in injury rates. Workplace injury rates decline more after buyouts of firms with positive abnormal accruals and high levels of analyst coverage and high-turnover shareholders prebuyout. These characteristics could be interpreted as proxies for a greater tendency toward short-termism, defined as overweighting of short-term cash flows. Efforts to reduce workplace injury rates are likely to be costly in the short run, even if they create value in the long run. A reduction in short-termism as a result of going private could also explain the decrease in workplace injury rates after buyouts of public firms relative to private firms (Table 11). However, whether public firms are subject to a short-termism bias that a PE buyout might alleviate is itself subject to debate, and testing this hypothesis is challenging.[27] Our proxies for

[27]See Graham, Harvey, and Rajgopal (2005), Edmans, Fang, and Huang (2017), Edmans, Fang, and Lewellen (2017), and Ladika and Sautner (2020) for evidence in support of the hypothesis and Jiang (2018) for evidence against. Jiang (2018) also points out that evidence from Van Binsbergen et al. (2012), Schulz (2016), and Cohen et al. (2013) is at odds with investors overweighting short-term cash flows.

short-termism are coarse and may capture other firm characteristics. For example, analyst coverage is highly correlated with firm size, while abnormal accruals could proxy for growth. The way in which PE firms structure operational change in these classes of firms may be different. Nevertheless, the results are consistent with views expressed about the lengthening of decision horizons after buyouts expressed by many of our executive interviews.

The decline in workplace injury rates after buyouts shows no relationship with observable buyer characteristics. However, it is worth making two observations here. First, we observe only a limited set of buyer characteristics and cannot rule out the possibility that the decline in injury rates varies with unobserved characteristics. Second, even within the set of observable characteristics, we are limited in our analysis by disclosure restrictions, which, for example, would prevent us from estimating a change in workplace injury rates specific to each PE firm or small subset of PE firms.

Workplace injury rates decrease less after buyouts in establishments that reduce employment post-buyout. Simple downsizing, then, does not appear to explain our results. The decline in injury rates does not differ depending on whether the firm replaces its board chair, adds new directors, or adds directors employed by the PE firm post-buyout. Moreover, the decline is, if anything, smaller when the firm replaces its CEO post-buyout. We therefore cannot link the decline in workplace injury rates to observable governance-related changes. However, the increased monitoring throughout the organization after buyouts that many of our interviews describe could be certainly interpreted as strengthening oversight.

Finally, workplace injuries decline less after buyouts involving larger increases in debt, though the relationship is only statistically significant (at the 10

8 Conclusion

Overall, the results presented in this paper suggest a positive effect of PE buyouts on workplace safety. While recent research provides evidence that at least some workers experience increased unemployment risk and lower wages after buyouts, those that remain employed do appear to experience an improvement in working conditions. Thus, our evidence helps to paint a more nuanced picture of how PE buyouts affect production-level workers. Of course, buyouts are not random events, and one must be careful in reaching conclusions about causality. Nevertheless, the results suggest a "bright side" of PE buyouts for production-level workers and lend further support to the argument that buyouts of public firms improve operational performance. Future work considering how injury rates and wages evolve together around buyouts would be useful for further understanding the impact of these transactions on employees.

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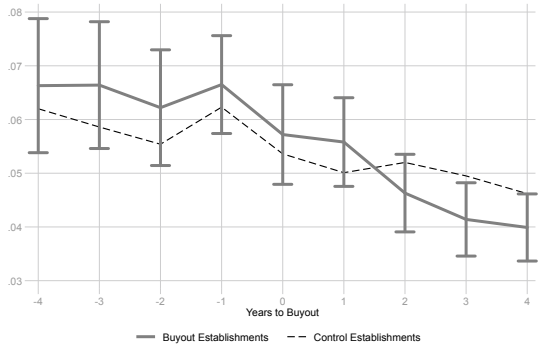
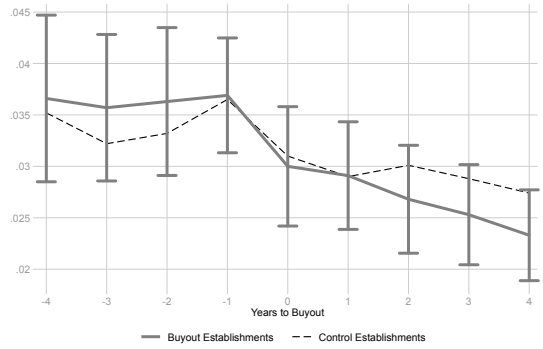
Figure 1: Injury rates around public firm buyouts

This figure presents the mean injury rates and DART injury rates for public firm buyouts and control establishments around the buyout year. Figure 1a presents $Injuries/Employee$. Figure 1b presents $DARTInjuries/Employee$. Figure 1c presents four-digit SIC code industry-adjusted $Injuries/Employee$. Figure 1d presents four-digit SIC code industry-adjusted $DARTInjuries/Employee$. Error bars represent a 95% confidence interval around the difference between the two series.

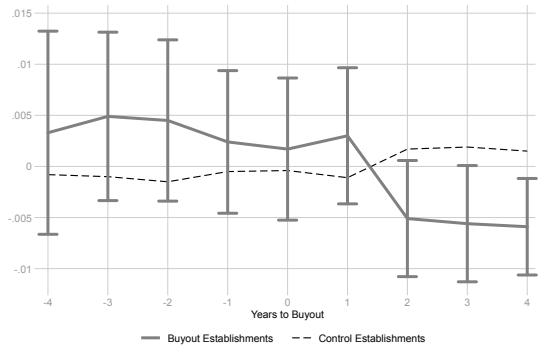
(a) $Injuries/Employee$



(b) $DARTInjuries/Employee$



(c) Industry-adjusted $Injuries/Employee$



(d) Industry-adjusted $DARTInjuries/Employee$

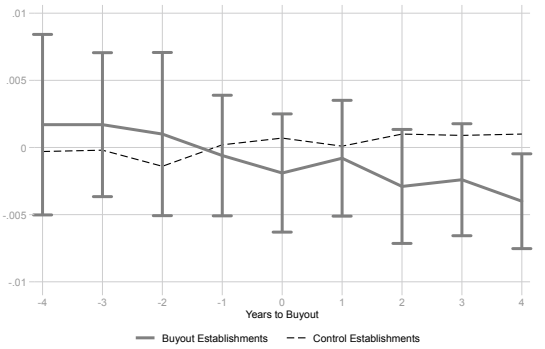


Figure 2: Pooled injury rate around public firm buyouts

This figure presents pooled injury rates across buyout and control establishments around the buyout year. These pooled injury rates are calculated by summing *Injuries* and *Employees* separately for all buyout and control establishments in each year relative to the buyout year and then dividing the summed injuries by the summed employees. Note that the figure does not depict error bands because the data are collapsed to a single observation per year for each of the buyout and control establishment samples.

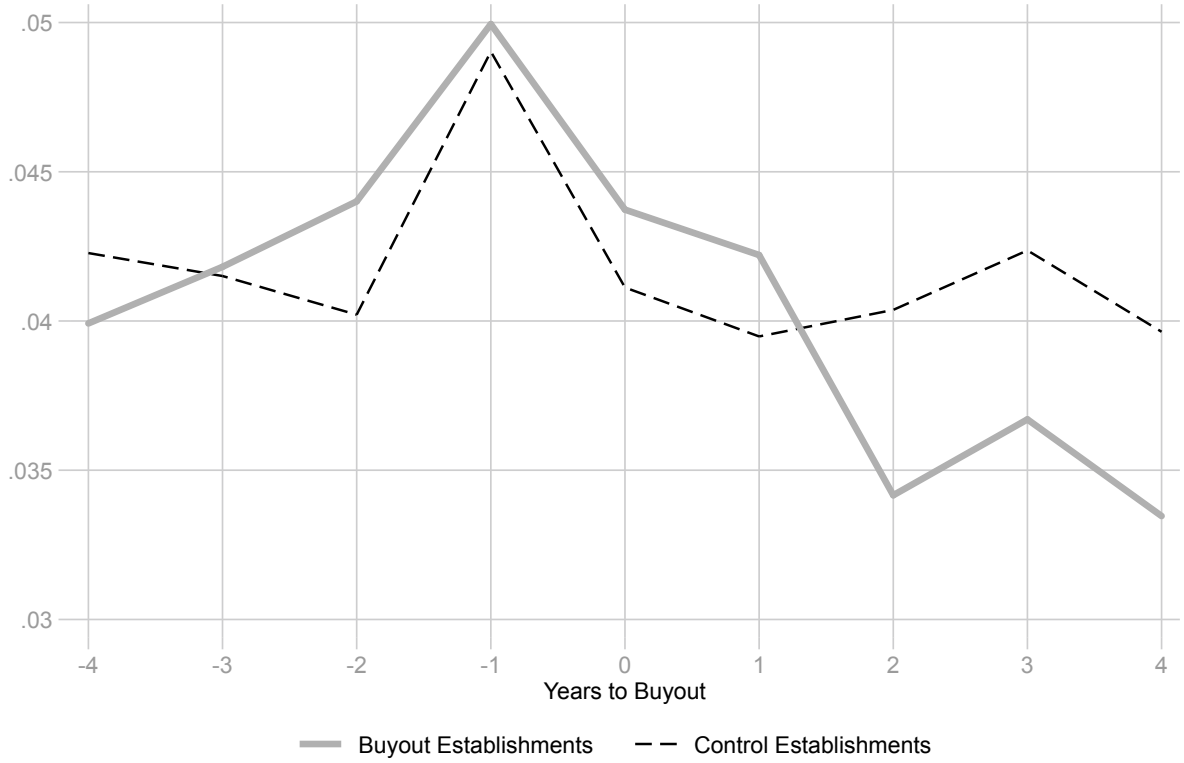


Table 1: Injuries by type and cause

This table shows the percentage of private sector U.S. workplace injuries in 2014 by nature (panel A) and cause (panel B), as reported by the BLS. We computed these percentages from the incident rates available at https://www.bls.gov/news.release/archives/osh2$_1$1192015.*pdf*.

A. Percentage of injuries by nature

Nature of injury	%
Sprains, strains, tears	36.7
Soreness, pain, including back	16.6
Fractures	8.9
Bruises, contusions	7.9
Cuts, lacerations	7.6
Multiple traumatic injuries and disorders	2.9
Heat (thermal) burns	1.5
Carpal tunnel syndrome	0.7
Amputations	0.5
Chemical burns	0.4
Tendonitis (other or unspecified)	0.2
All other natures	15.5

B. Percentage of injuries by cause

Cause of injury	%
Contact with objects	31.4
Fall on same level	21.7
Overexertion in lifting/lowering	14.0
Fall to lower level	6.9
Transportation incidents	6.7
Exposure to harmful substances or environments	5.2
Slips or trips without fall	5.2
Violence and other injuries by persons or animal	5.2
Repetitive motion	3.5
Fires and explosions	0.1

Table 2: Sample formation

This table presents information about the buyout firms in the sample. Panel A describes the sample's construction. Panel B reports the sources of matches with the BLS injury data. Panel C tabulates the number of control establishments for each establishment belonging to a PE-acquired firm ("buyout establishment") in the sample. Panel D reports the number of establishment-year observations for buyout establishments by year relative to the buyout year.

A. Buyout sample formation

	Public-firm buyouts			Private-firm buyouts		
	Firms	Estabs	Estab-years	Firms	Estabs	Estab-years
Starting buyout sample	285			547		
Buyout-BLS data matches	244	13,452	24,213	316	2,051	5,384
Observations in (-4,+4) window around buyout	228	10,356	16,493	288	1,345	2,792
Present in (-4,-1) AND (+1,+4)	152	1,565	5,227	121	199	743
At least one valid control	149	965	3,256	120	194	713
Employment $\geq$ 50 at buyout	134	614	2,341	104	152	606
Employment $\geq$ 100 at buyout (main sample)	114	395	1,639	78	108	474

B. Types of buyout establishment matches

Type of match	Public-firm buyouts	Private-firm buyouts
EIN	253	0
Name	142	108
Total	395	108

D. Control establishments per buyout establishment

	Number of control establishments					
	1	2	3	4	5	Total
Public-firm buyouts	53	38	32	13	259	395
Private-firm buyouts	24	12	10	8	54	108

E. Establishment-year observations by year relative to buyout year

	Number of observations									
	$t-4$	$t-3$	$t-2$	$t-1$	t	$t+1$	$t+2$	$t+3$	$t+4$	Total
Public-firm buyouts	115	141	160	245	182	216	203	194	183	1,639
Private-firm buyouts	32	38	38	64	59	62	63	59	59	474

Table 3: Summary statistics

This table presents information about the establishments in the sample. Panel A reports means of various establishment characteristics the last year in the sample prior to the buyout for establishments of PE-acquired firms ("buyout establishments") and control establishments. Panel B reports the number of buyout establishments in the final sample in each of the Fama and French (1997) five-industry categories. Panel C reports summary statistics for characteristics of buyout firms in the sample from the year prior to the buyout, along with means and medians for the Compustat universe during the sample period. *Assets* equals total reported assets. *Sales* equals total reported sales. *Debt/Assets* equals book debt divided by book assets. $Tobin's\ q$ equals the ratio of the firm's market value to its book value. *CashFlow/Assets* equals the sum of income before extraordinary items and depreciation, divided by lagged assets. *Capex/Assets* equals capital expenditures divided by lagged assets. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

A. Means of buyout and control establishment characteristics prebuyout

	Public-firm buyouts			Private-firm buyouts		
	Buyout estabs	Control estabs	t-stat	Buyout estabs	Control estabs	t-stat
Number	395	1,583		108	380	
Employees	426.3	415.4	-0.29	372.42	398.86	-0.71
log(Employees)	5.607	5.546	-1.32	5.6390	5.6831	-0.53
HoursWorked/Employee	1.751	1.744	-0.29	1,628.28	1,643.52	-0.51
Injuries/Employee	0.06686	0.0637	-0.94	0.0708	0.0701	0.11
DARTInjuries/Employee	0.03406	0.0333	-0.42	0.0405	0.0389	0.75

B. Buyout establishments by broad industry category

Industry category	Public-firm buyouts	Private-firm buyouts
Consumer durables, Nondurables, Wholesale, retail, and some services (laundries, repair shops)	166	39
Manufacturing, energy, and utilities	59	23
Business equipment, telephone, and television transmission	40	6
Health care, medical equipment, and drugs	48	18
Other	82	22

C. Public buyout firm prebuyout characteristics

	Sample firms					Compustat universe	
	Mean	SD	10th pctile	Median	90th pctile	Mean	Median
Assets	$1,370M	$3,562M	$73M	$387M	$2,929M	$1,592M	$107M
Sales	$1,220M	$2,037M	$83M	$391M	$3,197M	$1,251M	$90M
Debt/assets	0.251	0.223	0.000	0.217	0.587	0.262	0.205
Tobin's q	1.168	0.728	0.543	0.910	2.158	2.332	1.250
CashFlow/assets	0.090	0.084	0.016	0.083	0.184	0.065	0.060
Capex/assets	0.071	0.096	0.013	0.049	0.141	0.079	0.044

Table 4: Injury rate changes after PE buyouts: Difference-in-differences estimates

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms in our sample relative to control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, we include only observations from the 4 years before and 4 years after the buyout in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0057			0.0011		
	(0.0038)			(0.0018)		
PostBuyout	0.0034	0.0003	0.0012	0.0035	0.0000	0.0002
	(0.0059)	(0.0033)	(0.0031)	(0.0045)	(0.0038)	(0.0036)
BuyoutFirm * PostBuyout	-0.0100***	-0.0074**	-0.0091***	-0.0044***	-0.0024*	-0.0032**
	(0.0025)	(0.0029)	(0.0029)	(0.0015)	(0.0013)	(0.0015)
log(Employees)			-0.0083***			-0.0043***
			(0.0024)			(0.0015)
HoursWorked/Employee			0.0137***			0.0045*
			(0.0032)			(0.0028)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,942	6,942	6,942	6,942	6,942	6,942
Adjusted R^2	.3153	.6624	.6703	.2505	.6234	.6266

Table 5: Evolution of injury rates after PE buyouts

This table presents estimates of variation in establishment-level injury rates over the 4 years before and 4 years after PE buyouts for establishments of PE-acquired public firms relative to control establishments. Separate results are shown for the public- and private-target samples). In each case, the sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before, year of, and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. We estimate the following regression:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \sum_{K \in (-4,1) \cup (1,4)} \beta_K YearRelBuyoutK_{it} + \sum_{K \in (-4,1) \cup (1,4)} \gamma_K BuyoutFirm_i * YearRelBuyoutK_{it} + \epsilon_{it}.$$

$BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $YearRelBuyoutK$ is the year relative to the buyout year. $YearRelBuyout0$ (i.e., the buyout year) is excluded from the regressions. That is, all estimates are relative to the buyout year. Standard errors, clustered at the firm level, are shown to the right of each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	Public targets		Private targets	
YearRelBuyout-4	.0231	(0.0122)	-0.0156*	(0.0093)
YearRelBuyout-3	0.0195**	(0.0091)	-0.0170	(0.0151)
YearRelBuyout-2	0.0070	(0.0060)	-0.0047	(0.0072)
YearRelBuyout-1	0.0041	(0.0031)	-0.0155	(0.0212)
YearRelBuyout1	-0.0027	(0.0033)	0.0137	(0.0231)
YearRelBuyout2	-0.0105*	(0.0060)	-0.0285	(0.0339)
YearRelBuyout3	-0.0141*	(0.0081)	-0.0041	(0.0208)
YearRelBuyout4	-0.0157	(0.0108)	-0.0111	(0.0294)
BuyoutFirm * YearRelBuyout-4	0.0018	(0.0053)	-0.0121	(0.0078)
BuyoutFirm * YearRelBuyout-3	-0.0002	(0.0033)	-0.0123*	(0.0064)
BuyoutFirm * YearRelBuyout-2	0.0011	(0.0027)	-0.0068	(0.0072)
BuyoutFirm * YearRelBuyout-1	-0.0028	(0.0039)	0.0027	(0.0053)
BuyoutFirm * YearRelBuyout1	-0.0033	(0.0030)	-0.0011	(0.0052)
BuyoutFirm * YearRelBuyout2	-0.0103***	(0.0037)	-0.0094*	(0.0052)
BuyoutFirm * YearRelBuyout3	-0.0099***	(0.0034)	-0.0048	(0.0061)
BuyoutFirm * YearRelBuyout4	-0.0102***	(0.0028)	0.0061	(0.0062)
Observations	7,789		2,092	
Adjusted R^2	.6747		.7302	

Table 6: Injury rate changes after PE buyouts by industry category

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments across four different industry categories. The buyout sample includes only public-target buyouts. In each case, the sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable is $Injuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. All regressions include establishment and industry-year fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

Industry category SIC codes	Manufacturing (2000s & 3000s)	Transportation (4000s)	Trade (5000s)	Services (7000s & 8000s)
PostBuyout	-0.0005	-0.0258***	-0.0004	0.0116*
	(0.0034)	(0.0084)	(0.0038)	(0.0071)
BuyoutFirm * PostBuyout	-0.0080	-0.0122	-0.0084**	-0.0093**
	(0.0052)	(0.0111)	(0.0042)	(0.0041)
Observations	1,518	1,320	1,631	2,430
Adjusted R^2	.7867	.5608	.6222	.6679

Table 7: OSHA inspections and violations

This table presents difference-in-differences estimates of OSHA inspection violation incidence changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to inspected establishments of firms acquired in buyouts between 1995 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched with up to either five control establishments (first two columns) or one control establishment (last two columns) matched on establishment SIC code, inspection year, inspection type, inspection scope, and owner type. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. The dependent variable in columns 1 and 3 is an indicator equal to one if any violation was reported and zero otherwise. The dependent variable in columns 2 and 4 is an indicator equal to one if a serious violation was reported and zero otherwise. *BuyoutFirm* is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. *PostBuyout* is an indicator equal to one in the year after the buyout year and zero before. All regressions include industry-year and inspection type fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$ (based on a two-tailed t-test).

N matches	5 controls per buyout establishment		1 control per buyout establishment	
	All	Serious	All	Serious
Buyout firm	-0.00847	-0.0231	-0.0247	-0.0374*
	(0.0170)	(0.0173)	(0.0208)	(0.0203)
PostBuyout	0.0358	0.0734**	0.0700	0.119**
	(0.0373)	(0.0368)	(0.0508)	(0.0525)
BuyoutFirm × PostBuyout	-0.0532*	-0.0733**	-0.0699**	-0.0971**
	(0.0279)	(0.0313)	(0.0300)	(0.0458)
Observations	6,208	6,208	2,410	2,410
Adjusted R^2	.047	.025	.065	.036

Table 8: Probability of exit via IPO and changes in injury rates after PE buyouts

This table presents estimates from probit regressions of whether or not a firm exited buyout status via an initial public offering (IPO) on the change in its injury rate after the buyout relative to controls. Observations are at the firm level. The dependent variable is an indicator equal to one if the firm exited buyout status via IPO and zero otherwise. The explanatory variable $InjuryRateChange$ is constructed as follows. For each buyout firm in the final matched sample, we compute the average number of injuries per employee across establishment-years before and after the buyout, as well as the comparable numbers for all control establishments matched to that firm's establishments. In doing so, we use establishment-year observations in the 4 years before and 4 years after the buyout, as in our difference-in-differences analysis. We then compute $InjuryRateChange$ as the change in average injury rate for the buyout firm from before to after the buyout, minus the change in average injury rate for the control establishments. We compute the explanatory variable $IndAdjInjuryRateChange$ similarly, using the residuals from an OLS regression of injuries per employee on industry-year indicators rather than the raw injuries per employee as the input. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	(1)	(2)
InjuryRateChange	-5.7863*	
	(3.2184)	
IndAdjInjuryRateChange		-5.5713*
		(3.2088)
Constant	-1.0299***	-1.0290***
	(0.1576)	(0.1580)
Observations	114	114
Pseudo-R^2	.0250	.0230

Table 9: Employment and employee utilization changes after PE buyouts

This table presents difference-in-differences estimates of post-buyout employment and employee utilization changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of public firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $log(Employment)$. The dependent variable in columns 4 through 6 is $HoursWorked/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $EstabInjuryRate$ equals the establishment's $Injuries/Employee$ the last year observed prior to the buyout. $IndustryInjuryRate$ equals the mean four-digit SIC code $Injuries/Employee$ for the full BLS sample. $EmpDecrease$ is an indicator equal to one if an establishment's employment declines from the last year observed prebuyout to the first year observed post-buyout and zero otherwise. Standard errors, clustered at the firm level, are shown below each point estimate. $^*p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

Dep var	log(Empl)	log(Empl)	log(Empl)	HoursWorked/ Employee	HoursWorked/ Employee	HoursWorked/ Employee
PostBuyout	-0.0679	-0.0760	-0.2339	-0.1068	-0.1142	-0.1292
	(0.1210)	(0.1197)	(0.1426)	(0.0676)	(0.0684)	(0.0947)
BuyoutFirm * PostBuyout	-0.1395***	-0.2517***	-0.2035***	0.0366*	0.0836***	0.0071
	(0.0371)	(0.0667)	(0.0502)	(0.0206)	(0.0319)	(0.0303)
PostBuyout * EstabInjuryRate		0.4272			0.0956	
		(0.3207)			(0.1494)	
BuyoutFirm * PostBuyout * EstabInjuryRate		1.710***			-0.7141**	
		(0.4959)			(0.3336)	
PostBuyout * IndustryInjuryRate			5.6282**			0.8179
			(2.4147)			(2.1093)
BuyoutFirm * PostBuyout * IndustryInjuryRate			1.1578**			0.5349
			(0.4752)			(0.3706)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,942	6,942	6,942	6,942	6,942	6,942
Adjusted R^2	.8770	.8779	.8772	.6734	.6639	.6734

Table 10: Injury rates and post-buyout reobservation of buyout establishments

This table presents estimates from a linear probability model of the likelihood that a public buyout establishment in the BLS data in the 4-year window prior to the buyout is reobserved in the data in each of the 4 years after the buyout. The sample consists of all establishments matched to the public buyout sample in at least one of the 4 years before the buyout. The dependent variable is an indicator for whether the establishment is reobserved in a given post-buyout year. The explanatory variable is $Injuries/Employee$ measured in the last year the establishment is in the BLS data prior to the buyout. The regressions include firm fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

Reobserved at	Year $t+1$	Year $t+2$	Year $t+3$	Year $t+4$
Injuries/employee	0.2661***	0.1900***	0.2122***	0.1823**
	(0.0695)	(0.0720)	(0.0793)	(0.0964)
Observations	1,353	1,353	1,353	1,353
Adjusted R^2	.4583	.4591	.4581	.4572

Table 11: Injury rate changes after PE buyouts: Public- versus private-firm buyouts

This table presents triple difference estimates of the difference in post-buyout injury rate changes at establishments of PE-acquired firms (first difference), relative to control establishments (second difference), between previously public and previously private target firms (third difference). The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $WasPublic$ is an indicator equal to one for public-firm buyout establishments and zero for private-firm buyout establishments. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0016			-0.0015		
	(0.0044)			(0.0031)		
PostBuyout	0.0075	0.0053	0.0038	0.0066*	0.0042	0.0035
	(0.0073)	(0.0049)	(0.0045)	(0.0039)	(0.0037)	(0.0035)
WasPublic	0.0022			0.0039		
	(0.0057)			(0.0032)		
BuyoutFirm * PostBuyout	0.0011	0.0015	0.0025	0.0027	0.0038	0.0043
	(0.0045)	(0.0046)	(0.0045)	(0.0031)	(0.0030)	(0.0029)
BuyoutFirm * WasPublic	0.0073			0.0009		
	(0.0058)			(0.0036)		
PostBuyout * WasPublic	-0.0018	-0.0061*	-0.0044	-0.0036	-0.0050**	-0.0043**
	(0.0047)	(0.0034)	(0.0035)	(0.0024)	(0.0021)	(0.0021)
BuyoutFirm * PostBuyout * WasPublic	-0.0112**	-0.0092*	-0.0116**	-0.0071**	-0.0066**	-0.0076**
	(0.0052)	(0.0054)	(0.0054)	(0.0035)	(0.0032)	(0.0032)
log(Employees)			-0.0059**			-0.0029*
			(0.0025)			(0.0015)
HoursWorked/Employee			0.0136***			0.0055**
			(0.0026)			(0.0024)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,779	8,779	8,779	8,779	8,779	8,779
Adjusted R^2	.3224	.6751	.6811	.2612	.6339	.6365

Table 12: Injury rate changes after PE buyouts: Variation with firm characteristics

This table presents estimates of cross-sectional differences in post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. All columns show estimates from OLS regressions of the following form:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} + \theta PostBuyout_t * Characteristic_i + \lambda BuyoutFirm_i * PostBuyout_{it} * Characteristic_i + \epsilon_{it}.$$

In panel A, $InjuryRate$ is $Injuries/Employee$. In panel B, $InjuryRate$ is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. See Appendix A. for definitions of the characteristics. The coefficients for $PostBuyout$ and $PostBuyout * Characteristic$ are not shown for the sake of brevity. All regressions include establishment and industry-year fixed effects. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

A. All injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target characteristics						
(1) log(Assets)	−0.0198	(0.0122)	0.0016	(0.0015)	6,942	0.6623
(2) TangibleAssetRatio	0.0001	(0.0027)	−0.0239**	(0.0117)	6,942	0.6640
(3) RoutineTaskShare	−0.0072	(0.0065)	−0.0013	(0.0016)	6,942	0.6631
(4) PosAbnormalAccruals	−0.0175*	(0.0120)	−0.0090**	(0.0043)	6,360	0.6757
(5) HighAnalystCoverage	−0.0026	(0.0015)	−0.0148**	(0.0068)	6,942	0.6639
(6) TransitoryHoldingPct	−0.0035	(0.0041)	−0.0168**	(0.0081)	6,922	0.6641
Buyer characteristics						
(7) FrequentBuyer	−0.0223	(0.0135)	−0.0086	(0.0059)	6,942	0.6627
(8) ClubDeal	−0.0034	(0.0026)	−0.0077	(0.0056)	6,942	0.6630
(9) IndustrySpecialist	−0.0072**	(0.0030)	0.0038	(0.0079)	6,942	0.6621
(10) MgmtParticipation	−0.0164	(0.0127)	0.0018	(0.0065)	6,942	0.6624
Transaction characteristics						
(11) EmpDecrease	−0.0152**	(0.0068)	0.0134*	(0.0076)	6,942	0.6637
(12) ChairTurnover	−0.0082**	(0.0032)	0.0068	(0.0054)	6,942	0.6621
(13) NewDirector	−0.0065***	(0.0023)	−0.0011	(0.0059)	6,942	0.6619
(14) BuyerExecJoinsBoard	−0.0052***	(0.0017)	−0.0108	(0.0139)	6,942	0.6622
(15) CEOTurnover	−0.0084***	(0.0032)	0.0110**	(0.0045)	6,942	0.6629
(16) LeverageChange	−0.0055	(0.0245)	0.0051	(0.0102)	3,809	0.6344

Table 12: Injury rates around buyouts: Variation with prebuyout firm characteristics (Continued)

B. DART injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target characteristics						
(1) log(Assets)	−0.0039	(0.0066)	0.0002	(0.0008)	6,942	0.6232
(2) TangibleAssetRatio	0.0000	(0.0020)	−0.0074	(0.0065)	6,942	0.6235
(3) RoutineTaskShare	−0.0084	(0.0065)	0.0013	(0.0105)	6,815	0.6224
(4) PosAbnormalAccruals	−0.0001	(0.0065)	−0.0031	(0.0031)	6,293	0.6329
(5) HighAnalystCoverage	−0.0065	(0.0075)	−0.0079**	(0.0039)	6,942	0.6240
(6) TransitoryHoldingPct	0.0024	(0.0022)	−0.0172***	(0.0057)	6,922	0.6276
Buyer characteristics						
(7) FrequentBuyer	−0.0030	(0.0073)	−0.0021	(0.0037)	6,942	0.6231
(8) ClubDeal	−0.0039	(0.0062)	−0.0012	(0.0026)	6,942	0.6232
(9) IndustrySpecialist	−0.0070	(0.0070)	−0.0010	(0.0052)	6,942	0.6233
(10) MgmtParticipation	−0.0019	(0.0069)	0.0030	(0.0030)	6,942	0.6236
Transaction characteristics						
(11) EmpDecrease	−0.0033	(0.0026)	0.0017	(0.0035)	6,942	0.6238
(12) ChairTurnover	−0.0031**	(0.0013)	0.0042	(0.0034)	6,942	0.6243
(13) NewDirector	−0.0033**	(0.0015)	0.0018	(0.0025)	6,942	0.6240
(14) BuyerExecJoinsBoard	−0.0030	(0.0013)	0.0037	(0.0039)	6,942	0.6234
(15) CEOTurnover	−0.0009	(0.0063)	0.0046	(0.0038)	6,942	0.6230
(16) LeverageChange	−0.0058	(0.0132)	0.0097*	(0.0051)	3,898	0.5978

Appendix A. Variable Definitions

Injuries/Employee: Annual number of injuries divided by the reported average number of employees, measured at the establishment-year level, from the SOII

DARTInjuries/Employee: Annual number of "days away, restricted, transfer" injuries (injuries so severe the employee could not return to work at a normal capacity) divided by reported average number of employees, measured at the establishment-year level, from the SOII

HoursWorked/Employee: Total hours worked at an establishment scaled by the reported average number of employees, measured at the establishment-year level, from the SOII

log(Assets): The natural log of the Compustat item *at* for the buyout firm immediately prior to the buyout

TangibleAssetRatio: Compustat item *ppent* divided by *at*

RoutineTaskShare: Percentage of employees in an industry performing "routine task" labor, measured at the industry-year level, based on OES and DOT data and following the procedure of Autor and Dorn (2013) (we thank Ben Zhang for sharing the routine task share data)

AbnormalAccruals: Abnormal accruals from a modified Jones' model based on Compustat data. Total annual accruals, $ta = (oancf - ibc)/at_{t-1}$, are regressed on the change in cash sales $((sale - sale_{t-1}) - (rect - rect_{t-1}))/l12_{t-1}$ $1/at_{t-1}$, tangible assets $ppent/at_{t-1}$, and inverse assets $1/at_{t-1}$ for each year and two-digit SIC industry. The residual is then calculated as the abnormal accrual.

PosAbnormalAccruals: Indicator equal to one if $AbnormalAccruals > 0$ and zero otherwise

AnalystCoverage: # of analysts covering each stock, from I/B/E/S

HighAnalystCoverage: Indicator equal to one if *AnalystCoverage* is greater than the sample median (13) and zero otherwise

TransitoryHoldingPct: Shares held by institutional investors identified as "transitory" investors by Bushee (1998) divided by all shares held by institutional investors per the Thompson 13(f) holdings data

FrequentBuyer: The acquirer group includes a PE firm with at least six buyouts in the full sample

ClubDeal: Indicator for whether multiple PE firms are part of a buyer group in a transaction

IndustrySpecialist: Indicator equal to one if at least 50% of buyer's buyout targets in same two-digit SIC industry as target

MgmtParticipation: Indicator equal to one if management was part of acquirer group and zero otherwise

EmpDecrease: Reduction in reported average employment, from the SOII

LeverageChange: The change in leverage $(dlc+dltt)/at$ from before the year before buyout to the year after the buyout, from Compustat

CEOTurnover: CEO changes at time of or within first year after buyout, per Capital IQ People Intelligence database

ChairTurnover: Board chair changes at time of or within first year after buyout, per Capital IQ People Intelligence database

BuyerExecJoinsBoard: PE acquirer executive joins board at time of or within first year after buyout, per Capital IQ People Intelligence database

NewDirector: New director joins board at time of or within first year after buyout, per Capital IQ People Intelligence database

Appendix B. Additional Tables

Table B1: Injury rate changes after PE buyouts: Public firm controls only

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. Each buyout establishment is matched to one control establishment (instead of up to five, as in the main sample) in the same industry and belonging to a publicly traded firm with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. $^*p<.1$; $^{**}p<.05$; $^{***}p<.01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0108*			0.0022		
	(0.0062)			(0.0027)		
PostBuyout	-0.0008	0.0081	0.0083*	-0.0049	0.0002	0.0004
	(0.0054)	(0.0056)	(0.0044)	(0.0052)	(0.0046)	(0.0041)
BuyoutFirm * PostBuyout	-0.0121***	-0.0122**	-0.0134***	-0.0062***	-0.0040*	-0.0046**
	(0.0040)	(0.0055)	(0.0052)	(0.0023)	(0.0020)	(0.0019)
log(Employees)			-0.0066			-0.0032
			(0.0041)			(0.0026)
HoursWorked/Employee			0.0214***			0.0118***
			(0.0055)			(0.0031)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,547	2,547	2,547	2,547	2,547	2,547
Adjusted R^2	0.4707	0.7766	0.7852	0.3794	0.7322	0.7384

Table B2: Injury rate changes after PE buyouts: Matching on multiple characteristics

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry using propensity score matching, where $log(Employees)$, $HoursWorked/Employee$, and $Injuries/Employee$ are used to estimate an establishment's propensity to be acquired as part of a buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. $*p$ <.1; $**p$ <.05; $***p$ <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0068*			0.0017		
	(0.0035)			(0.0015)		
PostBuyout	0.0041	0.0034	0.0038	-0.0007	-0.0009	-0.0009
	(0.0053)	(0.0048)	(0.0042)	(0.0040)	(0.0044)	(0.0040)
BuyoutFirm * PostBuyout	-0.0115***	-0.0083***	-0.0099***	-0.0050***	-0.0026**	-0.0034**
	(0.0025)	(0.0026)	(0.0026)	(0.0014)	(0.0013)	(0.0014)
log(Employees)			-0.0083***			-0.0047***
			(0.0029)			(0.0017)
HoursWorked/Employee			0.0139***			0.0057***
			(0.0032)			(0.0022)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,615	6,615	6,615	6,615	6,615	6,615
Adjusted R^2	.3394	.6633	.6710	.2630	.6257	.6296

Table B3: Injury rate changes after PE buyouts: Matching on name only

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Unlike in Table 4, the sample includes public buyout firms matched only on firm name (i.e., no EIN matches). Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p<.1$; $^{**}p<.05$; $^{***}p<.01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0053			0.0009		
	(0.0040)			(0.0023)		
PostBuyout	0.0034	0.0002	0.0014	0.0037	0.0000	0.0002
	(0.0068)	(0.0033)	(0.0042)	(0.0045)	(0.0038)	(0.0036)
BuyoutFirm * PostBuyout	-0.0096***	-0.0070**	-0.0082***	-0.0042**	-0.0022*	-0.0031*
	(0.0029)	(0.0031)	(0.0026)	(0.0018)	(0.0013)	(0.0017)
log(Employees)			-0.0083***			-0.0046***
			(0.0024)			(0.0017)
HoursWorked/Employee			0.0137***			0.0055**
			(0.0032)			(0.0025)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,860	6,860	6,860	6,860	6,860	6,860
Adjusted R^2	.3140	.6612	.6700	.2498	.6221	.6260

Table B4: Injury rate changes after PE buyouts: Single control establishment

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to one control establishment (instead of up to five, as in the main sample) in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. $^*p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0070			0.0021		
	(0.0040)			(0.0021)		
PostBuyout	-0.0075	-0.0101	-0.0075	-0.0031	-0.0099	-0.0087
	(0.0101)	(0.0075)	(0.0080)	(0.0073)	(0.0062)	(0.0064)
BuyoutFirm * PostBuyout	-0.0099***	-0.0069**	-0.0092***	-0.0035*	-0.0015	-0.0027
	(0.0035)	(0.0032)	(0.0035)	(0.0019)	(0.0016)	(0.0018)
log(Employees)			-0.0103***			-0.0055***
			(0.0027)			(0.0015)
HoursWorked/Employee			0.0161***			0.0077***
			(0.0040)			(0.0019)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,914	2,914	2,914	2,914	2,914	2,914
Adjusted R^2	.3322	.6258	.6401	.2303	.6020	.6105

Table B5: Injury rate changes after PE buyouts: Difference-in-differences estimates with a lower minimum establishment size

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired public firms relative to control establishments. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 50 employees at the time of the buyout (instead of 100 as in the main sample) are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, multiplied by 1,000 to show more significant digits of the estimates. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0087**			0.0028		
	(0.0042)			(0.0018)		
PostBuyout	-0.0021	0.0014	0.0019	0.0016	0.0032	0.0033
	(0.0060)	(0.0045)	(0.0044)	(0.0042)	(0.0042)	(0.0040)
BuyoutFirm * PostBuyout	-0.0096***	-0.0067**	-0.0076***	-0.0038**	-0.0020	-0.0024*
	(0.0029)	(0.0028)	(0.0029)	(0.0015)	(0.0012)	(0.0013)
log(Employees)			-0.0084***			-0.0034***
			(0.0019)			(0.0012)
HoursWorked/Employee			0.0117***			0.0033
			(0.0038)			(0.0029)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,026	10,026	10,026	10,026	10,026	10,026
Adjusted R^2	.2686	.6012	.6078	.2396	.5949	.5967

Table B6: Injury rate changes after PE buyouts: Estimates from count models

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms relative to control establishments based on count models. The sample consists of establishment-years belonging to establishments of firms acquired in buyouts between 1997 and 2007 ("buyout establishments") and those of matched control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. The dependent variable in columns 1 through 4 is $Injuries/Employee$. The dependent variable in columns 5 through 8 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. *p <.1; **p <.05; ***p <.01 (based on a two-tailed t-test).

	All injuries				DART injuries			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
BuyoutFirm	0.0102	0.1766			-0.0256	0.0518		
	(0.2149)	(0.1469)			(0.2293)	(0.1843)		
PostBuyout	0.3610**	0.0676	0.0279	0.0230	0.3000*	0.0219	0.0493	0.0471
	(0.1436)	(0.1153)	(0.0472)	(0.0490)	(0.1572)	(0.1359)	(0.0705)	(0.0721)
BuyoutFirm * PostBuyout	-0.1335	-0.1463*	-0.1390*	-0.1469**	-0.1427	-0.1003	-0.1187*	-0.1225*
	(0.0988)	(0.0843)	(0.0781)	(0.0718)	(0.1281)	(0.0954)	(0.0629)	(0.0638)
log(Employees)				-0.1498***				-0.1007**
				(0.0352)				(0.0435)
HoursWorked/Employee				0.3067***				-0.2400***
				(0.0494)				(0.0780)
Model	Poisson	nbreg	Poisson	Poisson	Poisson	nbreg	Poisson	Poisson
Establishment FE	No	No	Yes	Yes	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,223	14,223	12,376	12,376	13,698	13,698	10,947	10,947

Table B7: Injury rate changes after PE buyouts: Private-to-private buyout sample

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms in our auxiliary sample of private-to-private buyouts relative to control establishments. Each buyout establishment is matched to up to five control establishments in the same industry with the closest values of $log(Employees)$ the last reported year prior to the buyout. For each buyout establishment and its associated controls, only observations from the 4 years before and 4 years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. The dependent variable in columns 1 through 3 is $Injuries/Employee$. The dependent variable in columns 4 through 6 is $DARTInjuries/Employee$. $BuyoutFirm$ is an indicator equal to one if the establishment belongs to a firm acquired in a PE buyout and zero otherwise. $PostBuyout$ is an indicator equal to one in the year after the buyout year and zero before. $log(Employees)$ equals the log of the establishment's average reported employment for the year. $HoursWorked/Employee$ equals reported hours worked divided by reported average employment, further divided by 1,000 for convenience. Standard errors, clustered at the firm level, are shown below each point estimate. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (based on a two-tailed t-test).

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0014			0.0001		
	(0.0050)			(0.0034)		
PostBuyout	-0.0089	-0.0100	-0.0104	-0.0092	0.0009	-0.0104
	(0.0155)	(0.0191)	(0.0187)	(0.0117)	(0.0149)	(0.0187)
BuyoutFirm * PostBuyout	0.0004	0.0014	0.0022	0.0023	0.0037	0.0022
	(0.0050)	(0.0049)	(0.0049)	(0.0034)	(0.0032)	(0.0049)
log(Employees)			0.0042			0.0042
			(0.0036)			(0.0036)
HoursWorked/Employee			0.0136***			0.0136***
			(0.0040)			(0.0040)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,837	1,837	1,837	1,837	1,837	1,837
Adjusted R^2	.3349	.6975	.7011	.2964	.6603	.6645