Private equity buyouts and workplace safety*

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This paper presents evidence of a large, persistent decline in establishment-level workplace injury rates after private equity (PE) buyouts of publicly-traded firms but not already-private firms. Cross-sectional evidence further links the public-firm post-buyout decline to alleviation of market pressure to focus on short-term performance. Employment drops more in low-injury risk establishments, and the fall in injury rates does not correlate with reductions in employment post-buyout, suggesting that systematic outsourcing of dangerous jobs or underreporting due to layoff concerns is unlikely to explain the results. Overall, our results suggest a novel dimension on which buyouts improve firms' fundamental operational competencies.

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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 firms but not already-private firms. Cross-sectional evidence further links the public-firm post-buyout decline to alleviation of market pressure to focus on short-term performance. Employment drops more in low-injury risk establishments, and the fall in injury rates does not correlate with reductions in employment post-buyout, suggesting that systematic outsourcing of dangerous jobs or underreporting due to layoff concerns is unlikely to explain the results. Overall, our results suggest a novel dimension on which buyouts improve firms' fundamental operational competencies.

A long-running debate over the role of private equity (PE) in the economy centers on the consequences of PE buyouts for the employees of acquired companies. Predominantly anecdotal evidence of large layoffs after buyouts suggests that PE ownership may be bad for workers. However, PE ownership could also affect employees who continue to work for an acquired company post-buyout. Agrawal and Tambe (2016) present evidence that increased IT investment after PE buyouts enhances the human capital of white-collar workers and managers, making them more employable in the long run. Far less is known about the long-term consequences of buyouts for production employees, who represent a majority of the workforce. In this paper, we shed light on those consequences using using establishment-level workplace safety records from the Bureau of Labor Statistics (BLS) to analyze the evolution of workplace injury rates after buyouts.

Estimates of the annual cost of workplace injuries and illnesses in the U.S. exceed $250 billion (Leigh, 2011).[1] The consequences of PE buyouts for workplace injury risk are unclear *a priori*. On the one hand, PE buyers may cut corners on workplace safety as part of efforts to reduce costs and increase cash flow. On the other hand, recent research suggests that PE buyouts are often followed by operational improvements (Davis et al., 2014; Bernstein and Sheen, 2016), and better workplace safety may be a byproduct or even an objective of such improvements. In addition, changes in organizational structure, production processes, product mix, and employee composition after buyouts may all affect workplace injury rates.

We document a large, sustained decline in establishment-level workplace injury rates after PE buyouts involving publicly-traded targets, both in absolute terms and relative to observationally-similar "control" establishments. Difference-in-differences estimates imply a 0.66 to 0.93 average percentage point decline in annual injuries per employee from the four

[1]The BLS reports 2.9 million U.S. private-sector workplace injuries and illnesses and 4,836 fatalities in 2015 (https://www.bls.gov/news.release/osh.nr0.htm). Globally, the International Labour Organization (ILO) reports 430 million occupational injuries and illnesses and 355,000 fatalities per year (among 3 billion workers), costing an estimated 4% of global GDP (https://www.ilo.org/legacy/english/protection/safework/worldday/facts_eng.pdf).

years before to four years after a buyout, or 10.3% to 14.5% of the pre-buyout sample mean. This decline begins the second year post-buyout and persists through at least the fourth year post-buyout. Similar patterns hold when we examine only injuries serious enough to result in days away from work, transfer, or reduced responsibilities, though the estimates are less precise. In contrast, we find no change in injury rates after buyouts of already-private firms. While our sample of private-target establishments is small, differences between public and private targets in post-buyout changes in injury rates are generally statistically significant.

We explore several possible explanations for the observed evolution of workplace injury rates after buyouts. One possible explanation for a drop in injury rates is that firms increase investment post-buyout on margins such as maintenance, equipment upgrades, training, and supervision that promote workplace safety. Models such as that of Stein (1988) suggest that publicly-traded companies, concerned about the perceptions of market participants, may forgo investment in activities that pay off in the long run in favor of those with more visible, shorter-term returns. Conversations with executives of companies acquired in buyouts suggest that market scrutiny may also lead firms into the pursuit of growth in non-core areas in a form of mission creep that may distract from day-to-day operational execution. By facilitating a lengthening of decision horizons and refocusing on core operations, a PE buyout may induce increased investment in margins that lead to workplace safety improvements.

We observe two sets of patterns that are consistent with this explanation for the fall in injury rates after public-firm buyouts, though we recognize that testing this explanation is challenging. First, the lack of a fall in injury rates after private-firm buyouts, both in absolute terms and relative to the fall in public-firm buyouts, could indicate that some aspect of going private may explain the fall in injury rates after public-firm buyouts. Second, we find a larger decline in injury rates when the target had more discretionary accruals, was followed by more stock analysts, and had a larger proportion of transitory institutional shareholders pre-buyout. More discretionary accruals could indicate that managers are more concerned

about short-term accounting metrics, more analysts may indicate a greater level of scrutiny of these metrics, and more transitory shareholders may increase pressure directly to focus on these metrics.[2]

Another possible explanation for the fall in injury rates after buyouts is a change in the composition of tasks performed in a business. For example, post-buyout automation may result in the replacement of dangerous hands-on production jobs with relatively safe jobs managing robotic equipment. Alternatively, firms may systematically outsource dangerous jobs to countries with weaker safety standards after buyouts. Without at least job-level (if not even finer) data, it is impossible to directly test for the effects of any changes in task composition. We nevertheless seek indirect evidence regarding the possible roles of automation and outsourcing in driving the evolution of injury rates after buyouts.

The labor literature argues that routine-task jobs are especially vulnerable to automation and outsourcing (Autor and Dorn, 2013). Using employee-level Swedish data, Olsson and Tåg (2017) present evidence that employees are more likely to lose their jobs after PE buyouts if they perform routine tasks. We find no evidence that the fall in injury rates after buyouts is greater at establishments in industries where a larger fraction of jobs involve routine tasks based on common measures, relative to controls. We also find that the fall in injury rates is actually slightly smaller after buyouts followed by reductions in employment. Of course, automation and outsourcing of dangerous tasks do not necessarily imply overall employment reductions, since both may entail the substitution of higher-skilled employees in safer jobs for lower-skilled employees.

Another possible explanation for the fall in injury rates after public-firm buyouts is that employees refrain from *reporting* injuries after these buyouts, perhaps because of an implicit threat of retaliation, without any change in actual injury risk. Analysis of auxiliary data on

[2]Caskey and Ozel (2017) document higher workplace injury rates in firms that just meeting quarterly earnings expectations.

OSHA inspections reveals a decrease in violations after public-firm buyouts, suggesting that factors affecting actual injury risk do change materially post-buyout. In addition, the fact that injury rates fall less after buyouts followed by employment reductions argues against a specific possible cause of disincentives to report injuries after buyouts - that employees are afraid to "rock the boat" and face retaliation when layoff risk is high.[3]

In addition to changing ownership form, most buyout transactions also involve significant increases in financial leverage. Cohn and Wardlaw (2016) find that establishment-level injury rates increase with parent firm financial leverage among publicly-traded companies and argue that this may reflect debt constraining firms' ability to invest in workplace safety. There are reasons to believe that buyout debt differs from public-firm debt. It is typically more concentrated and therefore potentially easier to renegotiate. It is also possible that other aspects of the transaction, such as increased free cash flow and capital injections more than offset the effects of increased debt load on investment capacity. We find some evidence that the drop in more serious injury rates after public-firm buyouts is attenuated when the acquired firm increases its debt more in the buyout transaction. However, we only observe post-transaction debt for about one-third of the buyouts in our sample, this sample may be special on many dimensions (Cohn et al., 2014), and we do not find any relationship between the leverage increase and the fall in overall injury rate.

We also find a smaller drop in injury rates after buyouts accompanied by CEO turnover. While open to multiple interpretations, this finding is at least at odds with injury rates falling after buyouts because of the systematic firing of managers who lack the operational skills to maintaing safe work environments. However, it could indicate the opposite - that PE buyers systematically replace CEOs who have an inherent tendency to overinvest in factors affecting workplace safety that cannot be addressed adequately through oversight and

[3]Consistent such a response being plausible, Boone et al. (2011) find that employees are less likely to report moderate injuries during times of high unemployment.

contracting. Perhaps a simpler explanation is that managerial turnover, even if desirable for other reasons, creates operational disruptions.

Finally, we consider the role of improved workplace safety as a source of value creation in buyouts. Cohn and Wardlaw (2016) document a strong negative association between firm value and injury rates. A back-of-the-envelope calculation using their estimates implies that the 0.66 to 0.94 percentage point relative fall in injury rates after public-firm buyouts would be associated with an average predicted increase in firm value of 4.1% to 5.3%. In interpreting the magnitude of this possible source of value, it is important to note that improvements in injury rate may proxy for broader operational improvements, and there is no way to isolate the pure effects of workplace safety improvements. We also find that PE owners are more likely to exit their investment in a company via an IPO — generally perceived as the most successful form of exit — when the company's injury rates fall more post-buyout. This finding suggests that PE firms are indeed rewarded for operational improvements in their portfolio companies.

Our paper contributes to the literature examining the impact of PE ownership on a firm's employees.[4] Much of this literature focuses on short-term changes in employment and compensation after PE buyouts.[5] One exception is a recent paper by Agrawal and Tambe (2016), who present evidence that systematic IT upgrades post-buyout increase the long-run value of employees' human capital. Their sample consists predominantly of white-collar workers, who are the most likely beneficiaries of IT-related knowledge transfers. In contrast, improvements in workplace safety disproportionately affect the well-being of front-line employees. In surveying the literature, Viscusi and Aldy (2003) conclude that employees ascribe a personal cost of $20,000 to $70,000 in 2015 dollars to an average workplace injury,

[4]Fracassi et al. (2017) examine the impact of PE buyouts on consumers, another important set of corporate stakeholders.

[5]See, for example, Kaplan (1989b), Muscarella and Vetsuypens (1990), Lichtenberg and Siegel (1990), Wright et al. (1992), Amess and Wright (2007), Boucly et al. (2011), Davis et al. (2014), and Antoni et al. (2015).

suggesting that the effect of improvements in workplace safety on these employees' well-being may be large.

Our paper also builds on recent work examining the establishment-level operational consequences of PE buyouts. Analyzing comprehensive establishment-level U.S. census data, Davis et al. (2014) document significant improvements in total factor productivity after PE buyouts, most of it driven by reallocation of resources from low-productivity to high-productivity establishments. In contrast, we document within-establishment improvements in a specific facet of operations, providing a lens into the nature of operational changes after buyouts. In the same general vein, Bernstein and Sheen (2016) find that restaurants' health ratings improve after their parent firms are acquired in PE buyouts. Relative to their paper, ours focuses on an aspect of operations more closely connected to the actual production process. It also studies firms in a broad set of industries, including manufacturing and distribution, rather than in a single service industry. We also add to this literature more generally by shedding light on the nature of the operational frictions that PE ownership helps to address.[6]

1 Workplace Safety and PE Buyouts

In this section, we discuss how firm's operational decisions impact workplace safety, explore how PE ownership potentially impacts these decisions, and describe additional insights from interviews we conducted with executives of both PE buyout specialists and firms acquired in PE buyouts. The discussion of workplace safety is adapted from and extends Section 1 of Cohn and Wardlaw (2016). That discussion is based largely on conversations

[6] A large literature studies changes in accounting measures of operating performance after PE buyouts, including work by Kaplan (1989b), Muscarella and Vetsuypens (1990), Smith (1990), Wright et al. (1992), Smart and Waldfogel (1994), Amess and Wright (2007), Guo et al. (2011), Boucly et al. (2011), Cohn et al. (2014), and Cohn et al. (2015). Kaplan (1989a), Denis (1994), and Kaplan (1994) describe specific operational improvements in case studies of four separate buyouts. Bernstein et al. (2016) find that industries in which PE firms invest tend to grow as a whole, suggesting spillover effects within industry.

with industrial safety practitioners and a case study on safety at Alcoa by Clark and Margolis (2000).

1.1 Workplace safety and operating policies and practices

Despite substantial improvements in workplace safety over the last century, especially in developed economies, many jobs remain inherently dangerous. Table I shows the percentage of injuries in the U.S. in 2012 by different causes (Panel A) and types (Panel B) as reported in the BLS' annual news release on employer-related workplace injuries and illnesses. The leading causes of workplace injuries are contact with objects, falls, and physical overexertion, while the most common injury types are sprains, strains or tears, soreness and pain, bruises and contusions, cuts and lacerations, and fractures.

— Insert Table I here —

Firms expend resources on a variety of activities that reduce the risk of on-the-job injury. Some of these activities involve capital expenditures on the acquisition and upkeep of physical assets involved in production. These activities include maintaining existing equipment, replacing old and worn parts and machines, buying equipment with better safety features, and automating dangerous tasks. The physical assets involved can include both sophisticated machinery as well as simpler equipment. As an example of the latter, replacing steel cable used for hoisting objects with (more expensive) synthetic fiber cable can reduce injury risk by decreasing recoil and the incidence of sharp edges upon breakage.

Firms also expend managerial time, effort, and attention on less tangible activities that reduce injury risk. These activities include devising and implementing policies and procedures that promote safety, training and monitoring employees, and fostering a safety culture. For example, lockout-tagout procedures prevent faulty machinery from being used until prop-

erly repaired.[7] Alcoa introduced a forklift speed limit of four miles per hour on a production floor to reduce the risk of collisions (Clark and Margolis, 2000). While such a policy may seem mundane, the leading source of workplace injuries in 2012 was floors, walkways, and ground surfaces.[8] Such policies require effort to implement, monitor, and enforce. Many plants create safety committees to devise safety improvements, though there is variation in the amount of authority vested in these committees. Perhaps the biggest innovation in safety management in the last few decades is the real-time, automated collection of data on a firm's production processes, which allows for real-time monitoring and expedites the mitigation of potential hazards.

While safety-related activities are implemented at the establishment level, they are driven by firm-level decisions through budgetary and policy initiatives. An establishment may cut spending on safety in order to meet short-run budgeted cost targets. Safety practitioners with whom we spoke repeatedly mentioned that budget constraints were an important impediment to implementing workplace safety measures. Anecdotally, the Chemical Safety Board (CSB) blamed a catastrophic explosion at BP's Texas City Refinery in 2005 that killed 15 employees at least in part on an explicit decision not to replace a worn valve due to corporate-level cost-cutting pressures.[9] Firm-level policy initiatives include hiring safety consultants to help improve safety practices, setting safety targets and holding managers accountable for achieving them, and implementing a safety culture. As an extreme example, Alcoa completely reorganized its entire operations around workplace safety in the 1990s.[10]

Improvements in workplace safety yield a number of important benefits for the firm. These benefits include reduced workmen's compensation premia and litigation costs. They

[7]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.

[8]Source: http://stats.bls.gov/news.release/archives/osh2_11262013.pdf.

[9]Source: http://www.csb.gov/assets/1/19/CSBFinalReportBP.pdf.

[10]See DuPont case study on Norfolk Southern: `http://www2.dupont.com/Sustainable_Solutions/en_US/assets/downloads/case_studies/NorfolkSouthern_CaseStudy.pdf`.

may also include reduced wages, as evidence suggests that employees demand compensation for exposure to injury risk. Perhaps the most important benefits relate to increased productivity. An injured employee may be unable to return to work immediately or may operate at reduced efficiency. 45% of workplace injuries in the U.S. result in at least one day away from work, restricted work activity, or job transfer (BLS, 2015). Operations may be idled while the cause of the injury is investigated and mitigated. Unsafe work conditions may also lead to low employee morale, which can adversely affect productivity.

The benefits of improved workplace safety can be substantial. While these benefits are difficult to quantify, Cohn and Wardlaw (2016) find a strong negative association between firm value, as measured by Tobin's Q, and firm-level injury rates. They estimate a reduction in firm value of approximately $99,000 per additional workplace injury.

One important characteristic of the returns from efforts to improve workplace safety is that they may take time to materialize. These efforts may necessitate comprehensive redesign of workflow, equipment upgrades, and other steps that take time to implement. Employees need time to adapt to new work routines and often initially resist safety-enhancing procedural changes 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. Finally, reduced injury risk yields a stream of benefits over time. Thus, one should think of workplace safety as a long-term asset.

1.2 Impact of PE buyouts on workplace safety

PE buyouts are typically accompanied by substantial changes in a firm's operations. How buyouts affect workplace safety depends on the nature of these operational changes. There is considerable debate among researchers, policymakers, practitioners, and critics of the PE industry about the nature of the changes buyouts engender in acquired firms' operations.

However, there is little evidence on the subject to date.

A traditional view is that PE owners focus on cost-cutting in order to increase cash flow. Facing pressure to cut costs, managers of portfolio companies may reduce spending on activities such as maintenance, training, and supervision that enhance workplace safety. Reductions in spending on these activities would compromise workplace safety, resulting in increased injury risk.

Cuts to spending on safety-promoting activities after buyouts could reflect several underlying phenomena. Firms may overinvest in safety pre-buyout, in the sense that the marginal shareholder return from investment in safety is negative. Such overinvestment could reflect managerial altruism or preference for the quiet life, but may also represent the fulfillment of previously-agreed implicit contracts between managers and employees regarding workplace safety. Jensen (1989) argues that the primary purpose of the traditional leveraged buyout (LBO) structure is to prevent managers from making investments that destroy shareholder value by committing the firm to disburse free cash flow to creditors. Critics of PE buyouts argue that this cost-cutting imposes costs on non-financial stakeholders, whose interests may not be accounted for in cost-cutting decisions. Such actions are consistent with the argument that acquirers may expropriate rents from non-financial stakeholders by ignoring implicit commitments previously made by a firm's managers (Shleifer and Summers, 1988).

Taken to an extreme, PE owners may underinvest in workplace safety. If such behavior is difficult to observe and the consequences take years to materialize, PE owners may be able to sell a firm before the adverse financial impact of such underinvestment is realized. Of course, a rational expectations-forming buyer who understands the incentives of PE owners would discount the value of the firm accordingly. Nevertheless, if the behavior is unobserved, PE owners could, in principle, still underinvest in safety in equilibrium.

A more modern view of PE buyouts — one that the PE industry itself promotes — is that PE ownership results in fundamental improvements in a firm's operations. Workplace safety

represents one dimension on which such improvements might occur. Mounting evidence suggests that PE ownership is accompanied by increased productivity. However, the question of why PE ownership might result in operating improvements remains largely unanswered. Interviews with executives of firms acquired in PE buyouts, which we describe in detail shortly, suggest that the patience of PE owners relative to public markets offers one plausible explanation.

Public corporate executives face pressure from market participants to meet earnings expectations and generate stock price increases. This pressure can bias executives towards projects with short payoff horizons (Stein, 1988) and can cause an excessive pursuit of high-visibility growth (Bebchuk and Stole, 1993). Baber et al. (1991), Roychowdhury (2006), Bhojraj et al. (2009), and Asker et al. (2014), among others, present evidence that managers distort decisions towards those that improve operating metrics in the short run. Corporate executives also state in surveys that they are willing to turn down positive NPV investments if necessary to meet earnings expectations (Graham et al., 2005). Given the unglamorous, long-term nature of investments in improving operating competencies, managers of firms facing market pressure to meet earnings and growth expectations in the short run may neglect such investments. A buyout, then, potentially allows for greater investment in these competencies by alleviating the source of pressure on managers to behave myopically. It is important to note that such an explanation applies to buyouts of public firms but not those of already-private firms, which are not subject to market pressures pre-buyout.

Another view is that PE buyouts help to relax financing constraints and therefore address an underinvestment problem. PE buyouts often result in the infusion of capital. Boucly et al. (2011) present evidence that non-listed European firms increase growth rates substantially after PE buyouts. Cohn et al. (2015) find similar increases in the U.S. Davis et al. (2014) find evidence of expansion at the plant level after buyouts of non-listed firms. A relaxation of financing constraints may allow a firm to increase investment in operating competen-

cies, including those that impact workplace safety. In theory, buyouts could relax financing constraints in public firms. However, the evidence to date primarily links buyouts to the relaxation of financing constraints in firms that are private pre-buyout, and these firms are generally considered to have less access to capital then publicly-traded firms. Thus, changes in injury rates after buyouts of private firms are likely to be more informative about any impact of buyouts on workplace safety through the relaxation of financing constraints.

1.3 Summary of interviews with PE and target-firm executives

To gain further insights into how PE buyouts might affect the operations of acquired firms, we interviewed several executives of PE buyout firms as well as firms acquired in PE buyouts. All of the acquired firms involved are in the sample we study in our empirical analysis. The firms span a broad array of sectors, including manufacturing, construction, logistics, retail, and health services. Taken broadly, these discussions appear to validate a lengthening of planning horizons as a plausible mechanism by which PE buyouts might lead to improvements in operating competencies in general and workplace safety in particular.

One common refrain from the interviews was that operating changes after buyouts often took the form of refocusing on core operations rather than dramatic changes in mission. This refocusing effort is generally described as a combination of eliminating non-core operations, some of which were built up in a form of mission creep, and shifting more attention to the reliability and efficiency of core operations. One executive characterized the changes as "getting back to the basics" and focusing on the "boring stuff." A number of the executives explicitly identified improvements in workplace safety as a specific plank in a broader platform of core operational improvements. These executives typically pointed to the belief that a safer work environment would allow the firm to contain labor costs in the long run as the primary motive for efforts to improve safety. For example, a former PE executive 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 recognized that safe working environments increase morale, decrease turnover, and impact wage negotiations."

One commonly mentioned component of efforts to improve basic operational efficiency after buyouts was an expansion of the set of metrics used to evaluate business unit-level performance. This expansion of metrics was generally intended to provide PE owners and managers with a more nuanced view of performance and a better understanding of where to target efforts at improving efficiency. In some cases, workplace safety-related metrics were explicitly added to the set of measures used to evaluate performance. Other commonly implemented metrics relating to throughput, machinery uptime, and employee absenteeism also provide insights into workplace safety. A couple of executives specifically mentioned the formal introduction of "six sigma" management, a data-driven approach to eliminating defects in the production process.

The introduction of more granular performance metrics after buyouts appears to be part of a broader shift in focus from short-term profitability to long-term operational efficiency. Several of the executives stated that PE buyers were willing to accept lower profitability in the short-run if necessary to implement efficiency-enhancing changes, and that such changes were often difficult to implement while a firm was still publicly-traded and subject to market scrutiny. A former executive with Welsh, Carson, Anderson and Stowe, a large buyout specialist, 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. You just need a number of years to invest in these kinds of improvements," referring to the in-depth operational improvements targeted by the firm. Others described specific changes implemented with a longer-run focus that were likely to incidentally reduce workplace injury risk, even if this reduction may not have been the primary objective. For example, an execu-

tive with the PE firm Three Cities Research described introducing an employee-retention scheme after the buyout of Garden Ridge Pottery, a home decor retailer, that reduced annual employee turnover from 400% to 20%. More experienced employees may suffer fewer on-the-job injuries because of better awareness of workplace hazards and greater familiarity with equipment and machinery.

2 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 the matched sample of establishments of acquired firms and control establishments that we use to conduct difference-in-differences analysis.

2.1 Linking PE Buyouts to Workplace Safety Records

We obtain samples of public-firm PE buyouts from Cohn et al. (2014) and private-firm buyouts from Cohn et al. (2015). These papers build samples of whole-firm buyouts using data from SDC Platinum and Dealogic, supplemented with news articles to remove improperly classified transactions. Both samples consist of buyouts of non-bankrupt U.S. "C" corporations with at least $10 million in assets.[11] The public buyout sample contains buyouts between 1995 and 2007, while the private buyout sample contains buyouts between 1995 and 2011.

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 each year in a stratified sampling process. Employers covered under the Occupational Safety and Health

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

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 log to the BLS. The SOII is used primarily to produce aggregate statistics on the state of occupational risk in various industries in the United States. Annual establishment-level SOII data is 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 $Employees$. The only firm-level identifier in the SOII data is the parent firm's employer identification number (EIN).

Because establishment-level BLS data is available starting in 1996, we consider only buyouts taking place in 1997 and later. Because the public buyout sample ends in 2007, we consider only buyouts taking place in 2007 and before in both the public- and private-firm buyout samples for consistency. 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 public-firm buyouts

and four private-firm buyouts) or that engage in franchising (20 public-firm buyouts and four private-firm buyouts). We make the latter determination by visiting company websites and searching for other information on the Internet regarding 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 attempt to match each of these acquired firms to its establishments in the BLS injury data. For public-firm buyouts, we are able to use EINs from Compustat to match some establishments in the BLS data to buyout firms. However, Compustat provides only a single EIN, and different establishments belonging to the same firm may 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 for any year 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. As Compustat generally only covers publicly-traded firms, we do not have an EIN with which to match private firms acquired in buyouts to BLS establishments. We therefore can only match these based on name.[13]

[12]Bernstein and Sheen (2016) compare restaurants owned by PE-backed firms and franchisees within the same restaurant chain in their assessment of the impact of PE buyouts on restaurant health code violations in order to control for chain-specific factors. 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.

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

For simplicity, we refer to establishments in the BLS data belonging to PE-acquired firms as "buyout establishments." We are able to match 244 public firms and 316 private firms acquired in PE buyouts to at least one establishment in the BLS data. We match 13,140 establishments in total to the 244 public firms and 2,051 establishments to the 316 private firms.

Both the higher match rate of public-target firms and of establishments to these firms are likely due to two causes. First, as noted, we can use a firm-level identifier in the matching process only for public-target firms. For reference, we use EIN to match approximately 65% of the establishments that we are able to match to public-target firms. Second, public-target firms are generally larger than private-target firms and therefore generally have more associated establishments that we can potentially match. We cannot investigate this second potential cause further, as we lack independent data on the size of private-target firms.

2.2 Matched treatment and control sample formation

Our primary empirical strategy, described in Section 3, is a difference-in-differences approach. Specifically, we compare changes in injury rates at establishments of acquired firms from the four years before to four years after buyouts to changes over the same period for matched control establishments.

For each establishment belonging to each buyout target firm in our sample, we retain only observations from year $t-4$ through year $t+4$, where t is the year of the buyout. We then eliminate any establishment that is not present in the BLS data at least once in the period from year $t-4$ through $t-1$ and at least once in the period from year $t+1$ through $t+4$, as we observe no information about changes in injury rates from before to after the buyout for these establishments. This requirement reduces the usable size of buyout establishments considerably, to 1,437 establishments belonging to 152 unique public-target firms and 199 establishments belonging to 121 unique private-target firms.

This attrition is due mainly to the fact that the BLS samples a relatively small fraction of establishments each year and does so without the intent of constructing a longitudinal data set. The small size of the usable sample does raise some concerns about selection bias and the degree to which our results can be generalized. The data collection methodology used by the BLS tilts the survey towards establishments that are economically meaningful to a given industry and size cohort. Additionally, the BLS more heavily samples industry-size cohorts for which there is greater variation in injury rates. As a result, our sample may favor larger firms and, to some degree, industries for which workplace safety protocols are more important.

For each buyout establishment, we form a pool of candidate control establishments. These are establishments in the same 4-digit SIC code as the buyout establishment and that are present in at least any year from $t-4$ through $t+4$ that the buyout establishment is present.[14] We then choose as matched controls the five establishments from the candidate pool with the smallest absolute differences in $Log(Employees)$ in the last pre-buyout year in the data, with the further restriction that the control's $Employees$ at that time must be between 50% and 200% of the buyout establishment's $Employees$. If the candidate pool for a given buyout establishment has fewer than five potential matches, we choose all of the establishments in the candidate pool as matched controls for that buyout establishment. The set of all matched buyout and control establishments represents the sample we use to conduct difference-in-differences analysis. We are able to match virtually all buyout establishments

[14]To see how the requirement that control establishments appear in the injury data in at least the same years as the buyout establishment impacts the sample, note that if the establishment of a firm acquired in a 2003 buyout is in the injury data in 1999, 2002, 2005, and 2007, then we include only control establishments that are also observed in the BLS data in the same four years in the set of possible controls. A candidate control establishment fitting this criterion might also be in the BLS data in additional years, say 2001 and 2008, but we ignore these non-overlapping years. This restriction is necessary because the establishments sampled by the BLS vary substantially from year to year. Despite the fact that establishments often appear in multiple years in the data during the sample period, the data is not designed to be longitudinal per se. In principle, we could match control establishment observations to buyout establishment observations in different years, but an overall downward trend in injury rates during the sample period would make the results difficult to interpret.

to at least one control establishment.

In our main analysis, we restrict attention to buyout establishments (and their matched controls) with at least 100 employees in the most recent pre-buyout 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. This screen reduces the usable sample to 397 establishments belonging to 114 unique public-target firms and 108 establishments belonging to 78 private-target firms (along with their matched control establishments). In a robustness check, we relax the minimum threshold to 50 employees, which results in a usable sample of 519 establishments belonging to 134 unique public-target firms and 152 establishments belonging to 104 unique private-target firms. Table 2 reports information about sample formation.

— Table 2 here —

Panel A shows the number of firms, establishments, and establishment-years at each step in the formation process. As noted, the primary cause of attrition is the requirement that an establishment be present at least once in both the four-year window before and four-year window after the buyout. We examine this issue further in Table 3, comparing our buyout sample against the larger Compustat universe.

We necessarily make a number of choices when deciding how to construct the sample used in the analysis we present in the paper. However, we also use several alternative approaches to constructing the matched sample to ensure that the results we present are robust to various approaches. These alternatives include matching establishments of publicly-traded firms only to establishments of other publicly-traded firms, matching each buyout establishment to only one control establishment instead of five, using propensity score matching to match based on multiple establishment characteristics instead of just industry and size, and lowering the

minimum establishment size for inclusion in the sample from 100 to 50 employees. Appendix A presents the difference-in-differences estimates that are the centerpiece of our analysis for each of these different alternative samples. The estimates are all similar to those we obtain using the main sample, which are presented in Section 4.

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. 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, somewhat allaying concerns about survivorship bias. 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).

Table 3 provides summary statistics for the characteristics of the firms and establishments in our final sample. Panel A reports means of several characteristics for the establishments of firms acquired in PE buyouts and their matched control establishments. The buyout and control samples are remarkably homogenous on all observable characteristics, despite the fact that we match only on industry and establishment size. It does not appear that PE acquirers select buyout targets based on characteristics such as a firm's injury rate. While we cannot rule out the possibility that buyout and control establishments vary on 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.

— Table 3 here —

Panel B reports broad industry breakdowns of the establishments in the main sample using the Fama and French (1997) five-industry categorization.[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. On other dimensions, public-firm buyout targets are similar to Compustat firms in general.

3 Empirical Methodology

We employ a standard difference-in-differences methodology 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 4-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). Angrist and Pischke (2008) caution against including potentially endogenous regressors as controls when estimating difference-in-differences, as the inclusion of such variables can lead to biased estimates. Thus, we do not include control variables in (1). However, in the empirical analysis, we also estimate specifications of (1) where we include $Log(Employees)$ and $HoursWorked/Employee$ as establishment-level controls.

[15] Disclosure limitations prevent us from providing this breakdown at a finer level.

These variables may help to explain injury rates and could therefore increase the precision of the difference-in-differences estimates.

The indicator $BuyoutFirm$ equals one for buyout establishments and zero for control establishments. The indicator $PostBuyout$ equals zero for buyout and control establishment observations in the four-year pre-buyout period and one for observations in the four-year post-buyout period. We exclude establishment-year observations from the buyout year itself because it is a blended year: The parent firm is independent part of the year and under PE ownership part of the year. We include both establishment fixed effects (α_i) and industry-year fixed effects (ϕ_{jt}) to account for any unobserved time-invariant establishment factors and time-varying industry factors that might impact injury rates. Because the buyout year varies across establishments, we can separately identify industry-year fixed effects from the treatment effect itself. 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. The coefficient γ captures the estimated change in injury rate from before to after a buyout for establishments of buyout firms relative to control establishments and is the object of interest in the regressions.

While estimates of regression equation (1) capture the average change in injury rates from the four years before to four 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 estimating the following regression:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \sum_{K \in (-4,4) \setminus 0} \beta_K PostBuyoutYrK_{it} + \sum_{K \in (-4,4) \setminus 0} \gamma_K BuyoutFirm_i * YearRelBuyoutK_{it} + \epsilon_{it}. \quad (2)$$

This regression equation allows the difference in injury rates across buyout and control

establishments relative to the pre-buyout period to vary across each of the post-buyout years. 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. The buyout year (i.e., $K = 0$) is the omitted year relative to the buyout. The γ_K coefficients capture injury rate in year-relative-to-the-buyout K relative to injury rate in the buyout year.

One question of interest is how changes in workplace injury rates differ after buyouts of publicly-traded and already-private firms. We assess these differences by estimating the following regression:

$$\begin{aligned} InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} \\ + \theta PostBuyout_t * WasPublic_i \\ + \lambda BuyoutFirm_i * PostBuyout_{it} * WasPublic_i + \epsilon_{it}, \end{aligned} \tag{3}$$

where $WasPublic$ is an indicator taking a value of one for establishments in the matched public-firm buyout sample and zero for observations in the matched private-firm buyout sample. The coefficient λ from this triple difference regression captures the change in injury rates after public-firm buyouts (relative to controls) compared to the relative change after private-firm buyouts.

Finally, to explore the causes of any changes in injury rates after buyouts, we examine how these changes vary with several observable characteristics of the acquired firm, measured prior to the time of the buyout. Our regressions take the following form:

$$InjuryRate_{it} = \alpha_i + \phi_{jt} + \beta PostBuyout_t + \gamma BuyoutFirm_i * PostBuyout_{it} + \eta PostBuyout_t * Characteristic_i + \delta BuyoutFirm_i * PostBuyout_{it} * Characteristic_i + \epsilon_{it}, \quad (4)$$

where $Characteristic$ is a firm- or transaction-level characteristic. 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. 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 and is the object of interest in these regressions. We are only able to estimate regressions based on equation (4) for the public buyout sample, as we do not generally observe firm-level characteristics for private firms.

Our interviews with executives of firms acquired in PE buyouts suggest that alleviation of market pressure to focus on short-term performance may lead to improvements in workplace safety after buyouts. We consider four proxies for the extent to which a firm faces pressure to focus on short-run performance. The first two are the fraction of a firm's shares held by institutional investors as a whole ($AllInstHoldings$), computed from Thomson 13(f) holdings data, and by "transient" institutional investors more specifically ($TransientInstHoldings$) as identified by Bushee (1998). Transient investors are those that either have high portfolio turnover or engage in momentum trading strategies. Bushee argues that managers face less pressure to behave myopically when they have more institutional ownership in general because these owners are ostensibly more "patient" than retail investors, but that transient investors may apply more pressure to focus on short-term performance. Consistent with these arguments, he presents evidence that firms with more overall institutional (transient)

ownership cut research and development spending less (more) in order to reverse an earnings decline.

The third proxy for short-term pressure is the number of stock analysts covering a firm. Greater analyst coverage is likely to focus more attention on the firm's short-term financial performance. We calculate the number of analysts issuing at least one earnings forecast for a firm in a given year from the I/B/E/S earnings forecast database. We then create an indicator variable $HighAnalystCoverage$, which equals one if at least six analysts (the median number in the I/B/E/S universe) cover the firm and zero otherwise.

The fourth proxy is based on the firm's earnings management practices. A firm facing strong pressure to focus on short-term performance may engage in aggressive accounting practices in order to boost reported earnings. We employ a commonly used measure of accounting aggressiveness — abnormal discretionary accruals as computed using the modified "Jones" model (Dechow et al., 1995). We create an indicator variable $PosAbnormalAccruals$, which equals one if abnormal accruals are positive and zero otherwise. While the first three proxies are intended to capture characteristics influencing managers to focus on the short run, the fourth proxy is intended to capture a response to such influence.

Another important characteristics of most PE buyouts is the use of debt added to the target firm's balance sheet to finance the transaction. Cohn and Wardlaw (2016) present evidence that workplace injury rates increase with financial leverage in publicly-traded firms, likely because high leverage limits the resources firms have available to devote to improving workplace safety. We explore the impact of buyout leverage by setting $Characteristic$ in (4) to $LeveageChange$ — the change in the firm's book debt-to-assets from the last fiscal year end before to the first fiscal year end after the buyout, computed using data from Compustat. Note that this variable is only available for buyout firms in Compustat the year after the buyout, either because they have public bonds outstanding or because they subsequently went public again and were required to publish financial statements covering the two years

before they did so.

A PE buyout may also be accompanied by management turnover. The replacement of weak managers with better managers could improve operations and decrease workplace injury risk. We examine this possible explanation for the fall in injury rates after public-firm buyouts by setting $Characteristic$ to $CEOTurnover$, an indicator variable equal to one if the CEO was replaced at the time of the buyout and zero otherwise.

Finally, we consider two aspects of the acquiring group. The first is whether management participates in the acquisition, which we measure with the indicator variable $MgmtPartic$. The second is whether the buyout group includes a PE firm that specializes in buyouts of publicly-traded firms. We measure this with the indicator variable $FrequentPEBuyer$, which is equal to one if the buying group includes a PE firm involved in at least six public-firm buyouts in our full buyout sample and zero otherwise.[16]

4 The Evolution of Injury Rates Around PE Buyouts

We begin our analysis by presenting a series of plots of 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 3.

4.1 Graphical analysis of injury rates around buyouts

Figure 1 shows plots of mean injury rates for the public buyout sample. Figures 1a and 1b plot mean $Injuries/Employee$ and $DARTInjuries/Employee$, respectively. Figures 2c and 1d plot industry-adjusted rates, where we first subtract the mean rate for all establishments in the same year and 4-digit SIC code industry. The points in these latter two plots are

[16]PE buyers involved in at least six public-firm buyouts in our sample are KKR, Texas Pacific Group, Apollo Management, Goldman Sachs Capital Partners, Carlyle Group, Blackstone Capital Partners, Leonard Green & Partners, Bain Capital, Thomas H. Lee Partners, Kelso & Co., and Welsh Carson Anderson & Stowe.

equivalent to the mean residuals from a regression of injury rates on industry-year fixed effects.

— Figure 1 here —

The plots all show similar patterns. A comparison of pre-buyout 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 take time to implement and manifest in observable outcomes after buyouts.

Figure 2 shows the analogous plots for the private-firm buyout sample. These plots also show no obvious differences in trends or levels of injury rates in the pre-buyout period across buyout and control establishments. However, unlike the plots for the public buyout sample, these plots also show no evidence of a change in injury rates (either upwards or downwards) post-buyout. Thus, simple injury rate plots appear to support a decline in injury rates after public- but not private-firm buyouts.

— Figure 2 here —

While these figures are suggestive, they do not account for other factors that may be important drivers of injury risk. We next turn to formal estimation of the change in injury rates around buyouts.

4.2 Difference-in-differences estimates

Table 4 presents estimates based on regression equation (1). Panel A presents estimates for public-firm buyouts, while Panel B presents estimates for private-firm buyouts. Standard errors clustered at the firm level are reported 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, excluding and including establishment-level controls, respectively. Columns (4) through (6) present the same three regressions, where the dependent variable is $DARTInjuries/Employee$.

— Table 4 here —

In Panel A, the small and statistically insignificant coefficients on $BuyoutFirm$ in columns (1) and (4) suggest no differences in pre-buyout injury rates in public-firm buyout and control establishments. The statistically insignificant coefficients on $PostBuyout$ 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 on the interaction of $BuyoutFirm$ 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 1% level in all three regressions. The point estimates indicate an average fall in injuries per employee of 10.3% to 14.5% relative to the pre-buyout mean of 0.0641 for buyout establishments (see Table 3).

The coefficient on the interaction of $BuyoutFirm$ and $PostBuyout$ is also statistically significant in column (4), where $DARTInjuries/Employee$ is the dependent variable and no establishment fixed effects are included. However, it is statistically insignificant in columns

(5) and (6). Because DART injuries represent only about half of total injuries, the power to reject the null hypothesis of no change in injury rates is likely lower with this dependent variable. We revisit the lack of statistical significance when we compare changes in injury rates after buyouts of public and private firms in Table 6.

As in Panel A, the coefficients on the separate variables $BuyoutFirm$ and $PostBuyout$ lack statistical significance in the results in Panel B. However, unlike in Panel A, the coefficients on $BuyoutFirm * PostBuyout$ are also small and statistically insignificant (and positive) in Panel B. Thus we fail to reject the null hypothesis that workplace injury rates do not change after PE buyouts of already-private firms. One caveat is that the private-firm buyout sample is much smaller than the public-firm buyout sample, which may limit the statistical power of the tests conducted on the private-firm buyout sample. Reflecting this difference, the standard errors of the estimates are generally somewhat larger in Panel B than in Panel A.

4.3 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. As operational changes generally take time to produce observable improvements in workplace safety outcomes, it would be surprising if the fall takes place immediately after the buyout. Table 5 presents estimates based on regression equation (2), with six specifications mirroring those of Table 4. Panel A presents estimates for public-firm buyouts, and Panel B presents estimates for private-firm buyouts.

— Table 5 here —

The patterns here are consistent with those shown in Figure 1. The small, statistically insignificant coefficients on $BuyoutFirm*PostBuyoutYr0$ and $BuyoutFirm*PostBuyoutYr1$ in Panel A indicate that injury rates in public-firm buyout establishments fall slightly, if at

all, relative to those of non-buyout establishments in the year of the buyout and the first year after. 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.

The coefficients on the $BuyoutFirm * PostBuyoutYrK$ interactions in Panel B, where we examine buyouts of already-private firms, vary in sign and are generally statistically insignificant. If anything, there is some indication of elevated injury rates the fourth year post-buyout. However, it would be difficult to attribute an increase in injury rates with such a long lag to the buyout itself.

4.4 Differences between public- and private- firm buyouts

The estimates presented in Tables 1 and 5 suggest that the response of injury rates to a buyout differs depending on whether the acquired firm was public or private pre-buyout. We formally test whether the difference in response is statistically significant by estimating regression equation (3). Table 6 presents the results, with six specifications mirroring those of Table 4.

— Table 6 here —

The coefficients on the triple interaction term $BuyoutFirm * PostBuyout * WasPublic$ are negative, large, and statistically significant at least at the ten percent level in all six specifications. The differential response in workplace injury rates suggests that the going-private aspect of a public-firm buyout may play a role in driving the observed decline in injury rates after these transactions. It is worth noting that, even though we cannot reject the null hypothesis that the change in DART injury rates is zero after public-firm buyouts (Table 4, Panel A), we are able to reject the null hypothesis of no change relative to a private-firm buyout benchmark based on the results in Table 6.

4.5 Analysis of OSHA violations data

Next, we present analysis of the OSHA inspections and violations data. One possible explanation for the apparent decline in injury rates after public-firm buyouts is that employees report fewer injuries, even holding fixed the actual rate of injuries. One useful feature of OSHA violations is that they are determined by an OSHA inspector and are not simply reported by the firm itself.

The sample here consists of establishment-years in which OSHA conducted an inspection of a given establishment. We estimate a variation 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 the frequency of repeat observation of an establishment in the data, we do not include establishment fixed effects. However, we do include inspection type fixed effects. 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 on the $BuyoutFirm \times PostBuyout$ interaction terms in all of the regressions indicate that the probability of an OSHA violation declines post-buyout at buyout establishments, relative to control establishments. That is, a measure of conditions likely conducive to actual injury risk declines after buyouts.

4.6 Cross-sectional Variation in the Decline in Injury Rates After Public-Firm buyouts

This section examines some of the factors that correlate with a higher or lower fall injury rates after buyouts. While we are careful not to draw strong conclusions from this analysis,

it does help to shed some light on the underlying causes of the overall decline in injury rates after buyouts. We focus here on publicly-targets, both because of data availability and because we only observe a significant change in injury rates around buyouts of public targets.

We begin by simply breaking the full sample into four subsamples based on the industry of the buyout establishment, with control establishments continuing to be grouped with the corresponding buyout establishment. The subsamples represent broad industry categories: 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). We then estimate regression (1) for each of the four subsamples. This analysis is exploratory in the sense that we do not have strong reasons to believe that the fall in injury rates should be higher or lower in any particular industry. Even workers in the services industry are subject to workplace injury risk and would benefit from efforts to improve safety. Table 8 presents the results.

— Table 8 here —

The coefficients on $BuyoutFirm * PostBuyout$ are negative and of fairly large (and similar) magnitude across all four industry-category subsamples. These coefficients are statistically significant for the trade category subsample at the ten percent level and services category subsample at the five percent level, though it should be noted these two subsamples are the two with the largest number of observations. Overall, the patterns that this table depicts suggest that the fall in injury rates is general and not specific to any particular industry group.

Next, we analyze variation in the change in injury rates after buyouts, in buyout establishments relative to controls, with a number of target, buyer, and transaction characteristics. Table 9 reports estimates of regression equation (4), using the characteristics described in Sec-

tion 2. Panel A reports estimates where the dependent variable is $Injuries/Employee$. Panel B reports estimates where the dependent variable is $DARTInjuries/Employee$. Only the coefficients on $BuyoutFirm * PostBuyout$ and $BuyoutFirm * PostBuyout * Characteristic$ are shown in the table for the sake of brevity.

— Table 9 here —

Panel A reports results using all injuries to compute the dependent variable, while Panel reports results using only relatively serious injuries. Row (1) of each panel shows that injury rates decline more after buyouts with positive abnormal accruals pre-buyout than those with negative abnormal accruals. This differential response is statistically significant at the five percent level when we examine all injuries but is statistically insignificant when we examine only serious injuries. To the extent that the positive abnormal accruals are a hint that a firm is relatively concerned about the strength of reported earnings, the differential response could indicate a bigger fall in injury rates after buyouts of firms for which alleviation of pressure to focus on short-term accounting measures is the greatest.

We also find a larger drop in injury rates after buyouts of targets with high pre-buyout analyst coverage (row (2)) and relatively transitory institutional investors bases (row (3)). These results lend some support to an even broader view that direct pressure from analysts and investors to focus on short-term accounting performance may constrain investment in workplace safety, which could be relieved through a PE buyout. Of course, any of these first three variables could proxy for a number of other firm characteristics that may relate to injury risk, and the results should be interpreted accordingly. Nevertheless, these results add to the evidence that injury rates fall after public- but not private-target buyouts in suggesting that escaping market scrutiny may be a factor in facilitating a reduction in injury rates after buyouts. The relative change in injury rates after buyouts does not appear correlated with parent-firm KZ index (row (4)).

Of the buyer characteristics (rows (5) through (7)), only whether or not the buyers included multiple PE firms (club deals) have explanatory power over the fall in injury rates after buyouts. These rates falling more after club deals than after single-PE buyer transactions.

Of the transaction characteristics (rows (8) through (14)), only whether the firm replaced its CEO at the time of the buyout has explanatory power over the fall in injury rates based on all injuries post-buyout. These rates fall less when the CEO is replaced. This result argues against an interpretation based on the replacement of operationally ineffective CEOs, since injury rates would likely fall more after buyouts rather than less in those cases. One possible interpretation is that CEO turnover, while beneficial on other dimensions, is disruptive to firm operations. Such turnover may also be accompanied by other significant organizational changes that could themselves be temporarily disruptive. However, we lack the data to assess the correlation with more detailed organizational changes.

Among transaction characteristics, only the increase in a firm's leverage from the year prior to the buyout to the year of the buyout, which we interpret as the additional of debt as part of the buyout structure, has any explanatory power over the fall in relatively serious injury rates. Injury rates fall less after buyouts in which debt load increases more. This result is at least loosely consistent with the finding of Cohn and Wardlaw (2016) that workplace injury rates increase with financial leverage in a sample of publicly-traded firms. However, the coefficient on the triple interaction is only statistically significant at the one percent level (and is not significant at all when we examine all injuries, as in Panel A).

The final row of the table examines whether the change in injury rates after public firm buyouts is correlated with the fraction of jobs in a company's industry that involve routine tasks. Studying variation with this routine-task job share could help shed light on the role of compositional changes due to automation or outsourcing of dangerous jobs in explaining the fall in injury rates after these buyouts. If routine tasks are especially vulnerable to

automation or outsourcing, as prior research suggests, then a greater fall in injury rates after buyouts of establishments in high routine task share industries would lend some support to specific sources of compositional changes as drivers of the overall fall in injury rates.

We do not see any evidence that the decline in injury rates after buyouts is either larger or smaller for establishments in industries with high routine task job shares. Of course, compositional changes having nothing to do with routine task jobs could also occur after buyouts.

Finally, we examine whether the change in injury rates after a buyout differs in cases where the buyout is followed by reductions in employment. Employees may be especially reluctant to report injuries, even if they occur, when layoff risk is high. If employees can at least partly anticipate layoffs, buyouts followed by layoffs may therefore induce reduced injury reporting. Reductions in employment may also induce changes in job composition, if this reduction is concentrated in certain types of jobs. To shed light on the role of employment reductions in explaining changes in injury rates after buyouts, we once again estimate triple differences, where here the interacted characteristic is an indicator for a net reduction in employment at the establishment from the last year in the data pre-buyout to the first year in the data post-buyout. Table 10 presents the results.

— Table 10 here —

Each panel present results from six regressions mirroring those in Table 4. The coefficient on the triple interaction of $BuyoutFirm$, $PostBuyout$, and $EmpDecrease$ is statistically insignificant in all specifications in Panel A and is positive in four of the six. The drop in injury rates after public-firm buyouts does not appear to be concentrated among companies reducing employment.

Ultimately, we cannot draw firm conclusions based on the cross-sectional analysis in this section, since cross-sectional characteristics could proxy for too many unobserved factors

driving injury risk. Nevertheless, it is interesting to note that the fall in injury rates after buyouts appears widespread across various industries and that the fall is concentrated in firms for which there might be at least some indication of high pressure to focus on short-term accounting performance.

5 Employment Dynamics After Buyouts

The analysis in this study focuses primarily on examining within-establishment changes in injury rates after buyouts. To add further color to this analysis, this section investigates employment dynamics around buyouts, exploring how changes in employment and work intensity differ across establishments with differing levels of injury risk. The results in Section 4 support a decrease in injury risk within establishment after public-firm buyouts. However, post-buyout changes in the distribution of employment across establishments with differing levels of injury risk could also impact overall levels of injury risk, as could changes in employee utilization. Figure 3 plots trends in employment and employee utilization at buyout and control establishments in the years around the buyout.

— Figure 3 here —

Figure 3a plots $Log(Employees)$ for the public buyout sample. This figure shows an average decrease in employment after buyouts, consistent with the findings of Davis et al. (2014) from analysis of census data. After declining over the first year after the buyout, employment remains below control establishment levels for the three years after but exhibits no further declines during these years. Figure 3b plots $HoursWorked/Employee$ for the public buyout sample. There are no consistent patterns here. Figures 3c and 3d show the analogous figures for private-firm buyouts. There is no indication of a change in either $Log(Employees)$ or $HoursWorked/Employee$ after these buyouts.

We test these changes more formally using the same difference-in-differences approach described in Section 3. Table 11 presents this analysis. Panel A presents results for public-firm buyouts, while Panel B presents results for private-firm buyouts.

We begin by simply estimating a variant of equation (1), substituting $log(Employees)$ for $Injuries/Employee$. Column (1) reports the results from this regression. In Panel A, the coefficient on $BuyoutFirm * PostBuyout$, which is statistically significant at the one-percent level, implies a 13% average reduction in employment relative to pre-buyout levels. This is consistent with the estimates of Davis et al. (2014). This similarity provides some comfort that the sample of buyout establishment surveyed by the BLS is not too unusual. We observe no fall in employment after private-firm buyouts in column (1) in Panel B.

— Table 11 here —

Next, we examine how the fall in employment after buyouts varies across establishments with different levels of injury risk. We use two measure of this risk. The first is the injury rate of the establishment in the final year reported before the buyout year. The second is the average 4-digit SIC code injury rate over the sample period computed using all of the SOII data. We then estimate regressions of the form of equation (4), setting $Chararteristic$ to each of these two measures. Columns (2) and (3) present the results. In Panel A, the positive coefficients on both of the triple interaction terms (statistically significant at the one-percent level) indicate that employment falls by less in relatively dangerous establishments than in relatively safe establishments pre-buyout. The coefficients are also positive in Panel B but are statistically insignificant.

Rather than being caused by specific jobs become safer, the overall fall in injury rates after public-firm buyouts documented in Table 4 could be driven by the systematic elimination of relatively dangerous jobs. Without employee-level data, we cannot directly test this explanation, which would be interesting in its own right. However, the fact that employment

actually falls more in relatively safe establishments suggests that such an explanation cannot be linked to a more general strategy of outsourcing dangerous work.

We also examine how work intensity changes around buyouts. Buyout owners might compensate for reduced employment by requiring remaining employees to work longer hours. Column (4) presents estimates of equation (1) where $HoursWorked/Employee$ is the dependent variable. The coefficient on $BuyoutFirm * PostBuyout$ in Panel A is positive and statistically significant, though only at the ten percent level of significance. The estimated 4% increase in hours worked per employee partially offsets the impact of the average 13% reduction in headcount on a firm's total productive effort. The coefficient is statistically insignificant in Panel B. Columns (5) and (6) show that any change in hours worked per employee does not vary with the establishment's level of injury risk in either public- or private-firm buyouts.

The results in Panel A suggest that a relative shift in employment towards higher-injury-rate establishments after public-firm buyouts could at least partly offset the negative impact of the within-establishment decrease in injury rates on overall injury risk. To assess this possibility, we examine how overall injury rates—pooling employees across all buyout establishments and across all control establishments—evolve after buyouts. To do so, we sum $Injuries$ and $Employment$ for all public-firm buyout establishments in our final sample and also for all control establishments, and then divide summed injuries by summed employment for each group. This is equivalent to calculating mean injury rates for buyout and control establishments, weighting each establishment-year observation by the establishment's reported employment in that year. Figure 4 presents a plot of these pooled injury rates in each year relative to the buyout year.[17]

— Figure 4 here —

[17]Note that, unlike the plots in Figures 1 and 2, this plot does not show error bands around the means. Because we are pooling all buyout and control establishments in this plot, we observe only a single aggregate injury rate for each group.

The patterns here are consistent with those shown in Figure 1. Overall injury rates fall considerably for public-firm buyout establishments but not control establishments starting in the second year post-buyout, even after accounting for changes in employment across establishments. Thus, the impact of the fall in injury rates within establishment after buyouts on overall injury rates appears to dwarf the impact of the relative decrease in employment in lower-injury-rate establishments.

6 Injury Rate Reductions as a Source of Value in Buyouts

In this section, we examine the possible role of injury rates reductions as a source of value creation for PE buyers. One important caveat to the discussion in this section is that we cannot distinguish changes in injury rates themselves from information that they contain about broader operational changes that may also have value implications. As a starting point, we use estimates of the association between injury rates and firm value from Cohn and Wardlaw (2016) to estimate the possible amount of firm value created by the fall in injury rates after buyouts. A back-of-the-envelope calculation using their estimates implies that the 0.66 to 0.94 percentage point relative fall in injury rates after public-firm buyouts would be associated with an average predicted increase in firm value of 4.1% to 5.3%.

We also examine how the mode of exit from a PE buyout varies with reductions injury rates after buyouts. The unit of observation in this analysis is a buyout firm. We compute estimates of firm-level change in injury rates relative to control establishments after buyouts using two approaches. 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 firmâĂŹs establishments. In doing so, we use establishment-year observations in the four

years before and four 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.

The second approach is the same as the first, except that we use the residuals from an OLS regression of injuries per employee on industry-year indicators rather than the raw injuries per employee as the input. We refer to the resulting change in industry-adjusted injury rates as $IndAdjInjuryRateChange$. We then estimate OLS regressions of an indicator for whether the firm exited buyout status via IPO on a constant and each of the two measures of injury rate changes separately. Table 12 presents the results from these regressions.

— Table 12 here —

The coefficients on the change in injury rate variables are negative and statistically significant at the ten percent level in both regressions. That is, a firm is more likely to exit buyout status via an IPO if its injury rate declines post-buyout. To the extent that an IPO represents the most desirable form of exit, this could indicate that improvements in workplace safety after buyouts — or at least operational improvements more generally that they reflect — create value for PE owners.

7 Conclusion

Overall, the results presented in this paper suggest a positive effect of buyouts on workplace safety, at least in buyouts of publicly traded firms. This effect appears to be driven in part by the alleviation of pressure that public-market scrutiny creates to focus on short-term performance and high-visibility activities. Of course, buyouts are not random events, and one must be careful in reaching conclusions about causality. Nevertheless, the results

lend 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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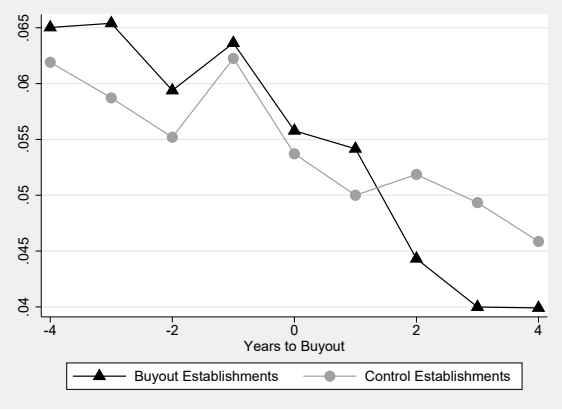
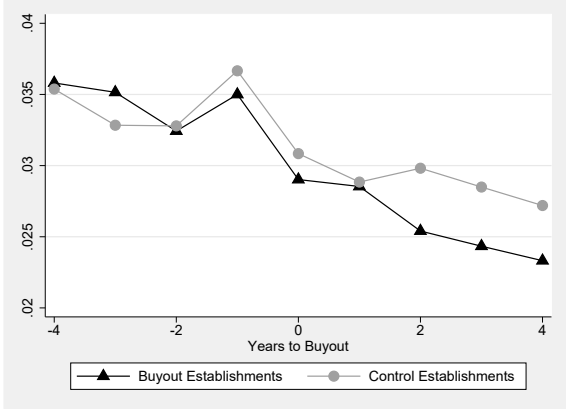
Figure 1: Injuries over event time: public firms

This figure presents mean injury rates and DART injury rates for public-firm buyout and control establishments around the buyout year. Figure 1a presents $Injuries/Employee$. Figure 1b presents $DARTInjuries/Employee$. Figure 1c presents 4-digit SIC code industry-adjusted $Injuries/Employee$. Figure 1d presents 4-digit SIC code industry-adjusted $DARTInjuries/Employee$.

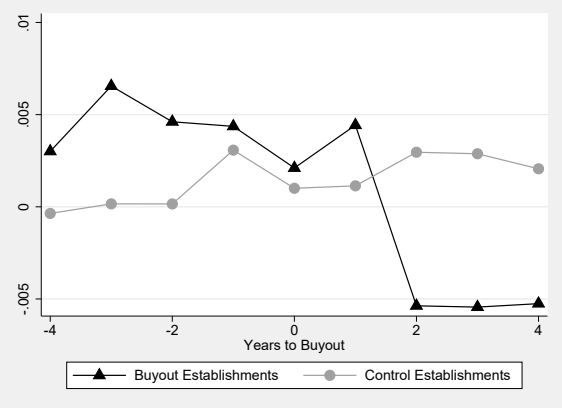
(a) $Injuries/Employee$



(b) $DARTInjuries/Employee$



(c) Industry-Adjusted $Injuries/Employee$



(d) Ind-Adjusted $DARTInjuries/Employee$

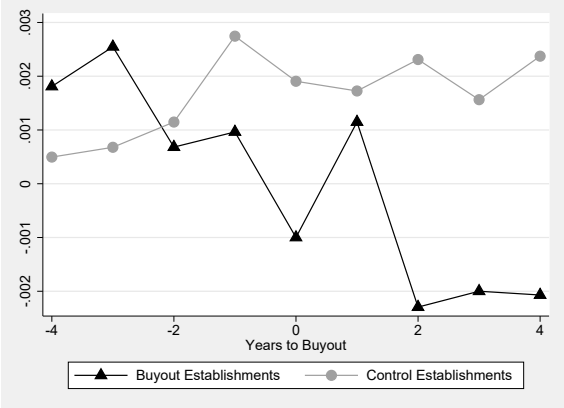
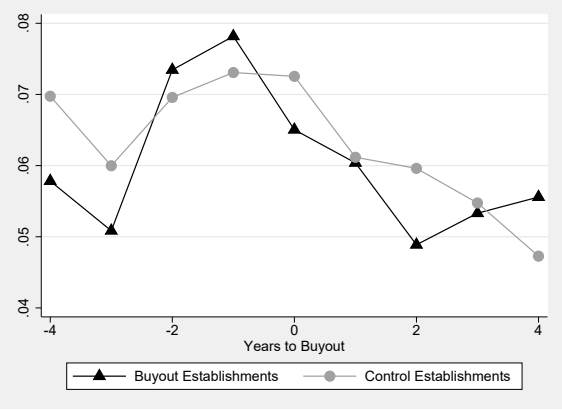


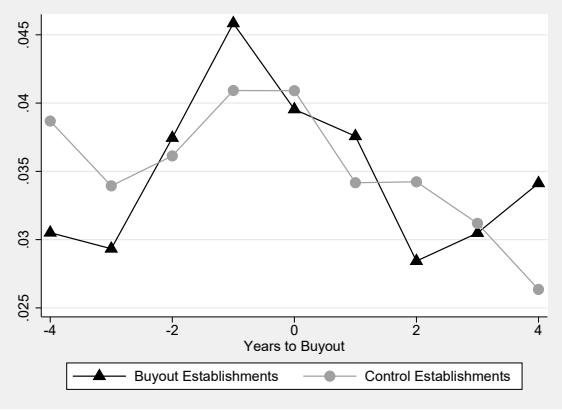
Figure 2: Injuries over event time: private firms

This figure presents mean injury rates and DART injury rates for private-firm buyout and control establishments around the buyout year. Figure 2a presents $Injuries/Employee$. Figure 2b presents $DARTInjuries/Employee$. Figure 2c presents 4-digit SIC code industry-adjusted $Injuries/Employee$. Figure 2d presents 4-digit SIC code industry-adjusted $DARTInjuries/Employee$.

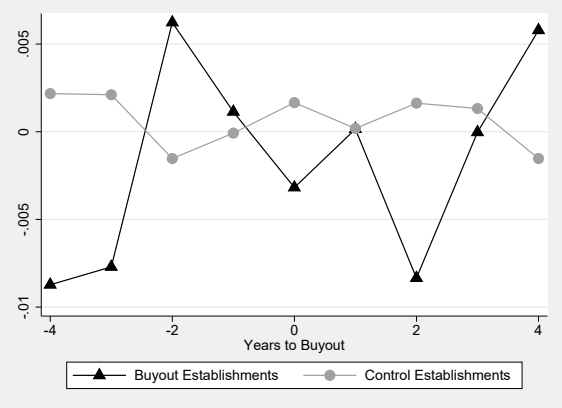
(a) $Injuries/Employee$



(b) $DARTInjuries/Employee$



(c) Industry-Adjusted $Injuries/Employee$



(d) Ind-Adjusted $DARTInjuries/Employee$

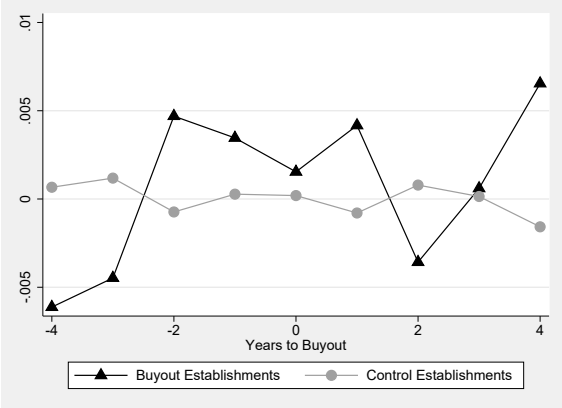
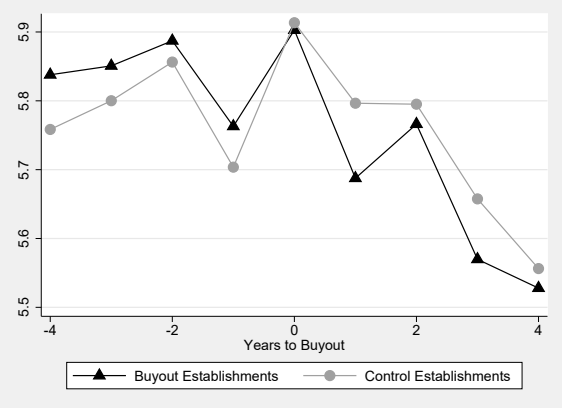


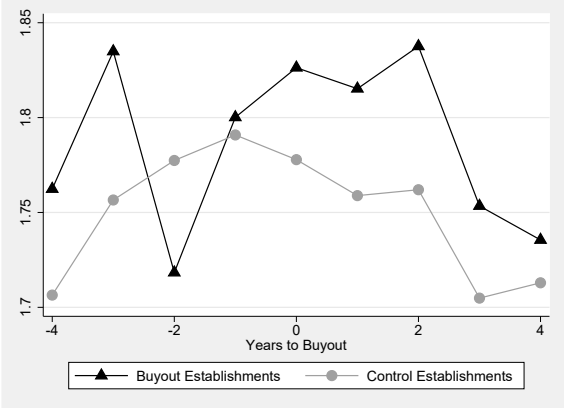
Figure 3: Employment dynamics

This figure presents mean $Log(Employees)$ and $HoursWorked/Employee$ for buyout and control establishments around the buyout year. Figures 3a and 3b present $Log(Employees)$ and $Hours/Employee$, respectively, for public-firm buyouts. Figures 3c and 3d present $Log(Employees)$ and $Hours/Employee$, respectively, for private-firm buyouts.

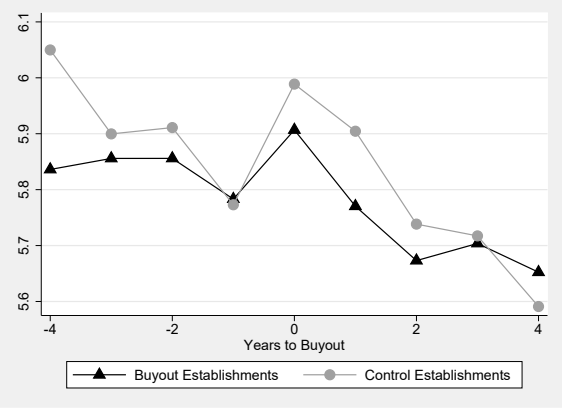
(a) $Log(Employees)$ - public firms



(b) $HoursWorked/Employee$ - public firms



(c) $Log(Employees)$ - private firms



(d) $HoursWorked/Employee$ - private firms

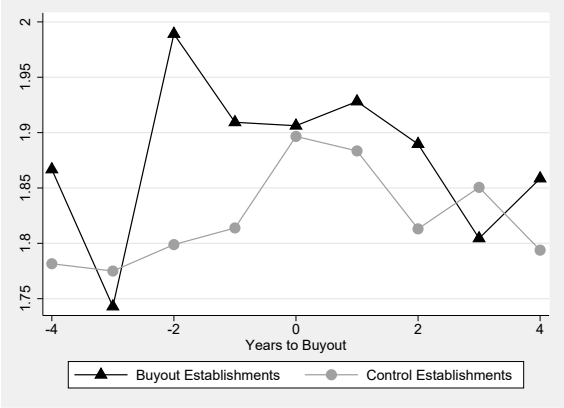


Figure 4: Pooled injuries over event time: public firms

This figure presents pooled injury rates across for 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.

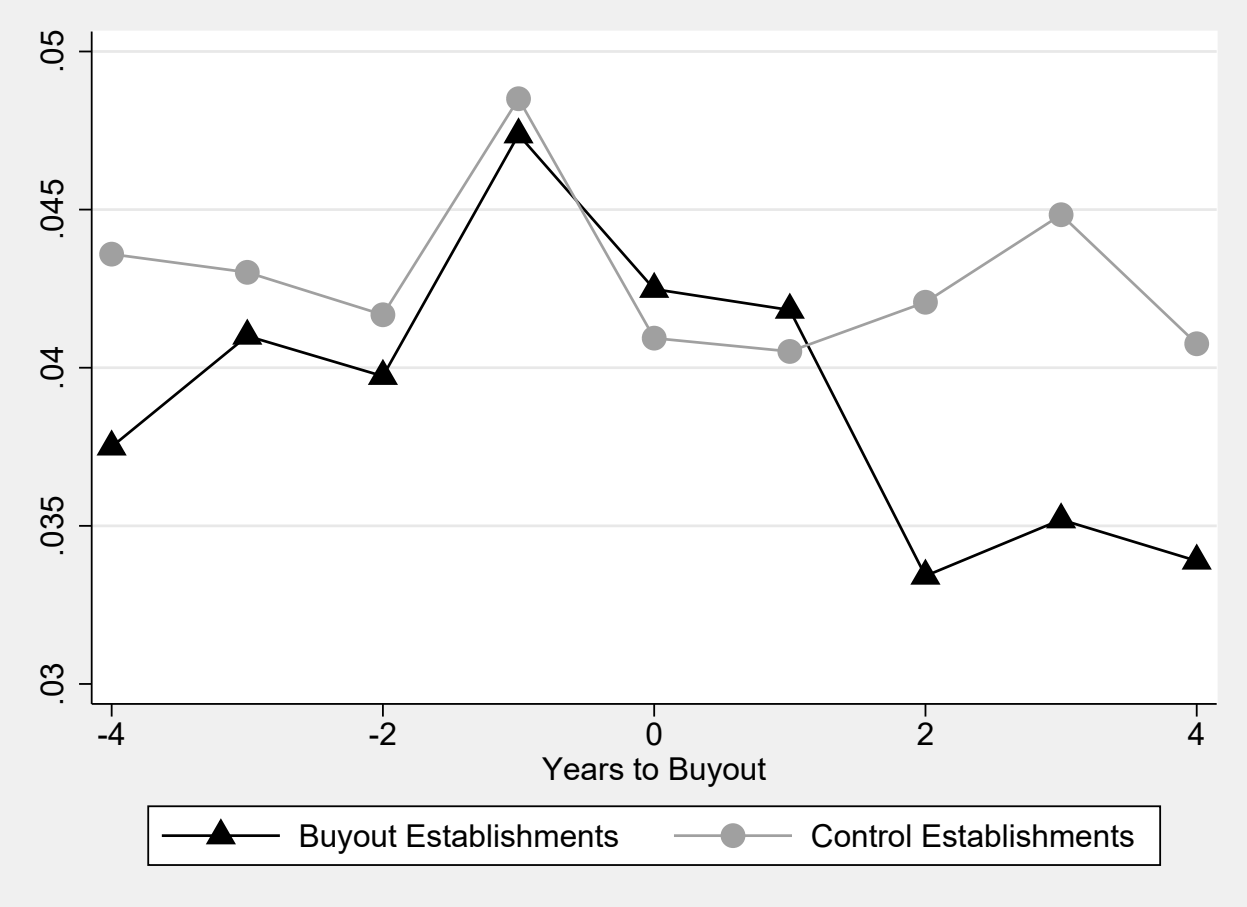


Table 1: Injuries by Cause and Type

This table shows the percentage of private sector U.S. workplace injuries in 2012 by nature (Panel A) and cause (Panel B), as reported by the BLS. These percentages were computed from incident rates available at http://www.bls.gov/news.release/pdf/osh2.pdf.

Panel A: Percent injuries by nature

Nature of injury	Percent
Sprains, strains, tears	38.16
Soreness, pain, including back	14.67
Bruises, contusions	8.33
Fractures	8.03
Cuts, lacerations	8.03
Multiple traumatic injuries and disorders	3.07
Heat (thermal) burns	1.49
Carpal tunnel syndrome	0.89
Amuptations	0.59
Chemical burns	0.40
Tendonitis (other or unspecified)	0.30
All other natures	16.06

Panel B: Percent injuries by cause

Cause of injury	Percent
Contact with objects	29.69
Fall on same level	19.56
Overexertion in lifting/lowering	14.44
Violence and other injuries by persons or animal	8.38
Transportation incidents	6.64
Fall to lower level	6.29
Exposure to harmful substances or environments	5.82
Slips or trips without fall	5.47
Repetitive motion	3.49
Fires and explosions	0.23

Table 2: Sample Formation

This table presents information about the buyout firms in the sample. Panel A describes how the sample was constructed. 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. *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.

Panel 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	13,140	25,019	316	2,051	5,384
Observations in (-4,+4) window around buyout	228	9,123	14,661	288	1,345	2,792
Present in (-4,-1) AND (+1,+4)	152	1,437	5,241	121	199	743
At least one valid control	149	1,417	5,133	120	194	713
Employment $\geq$ 50 at buyout	134	519	2,352	104	152	606
Employment $\geq$ 100 at buyout (main sample)	114	397	1,654	78	108	474

Panel B: Types of buyout establishment matches

Type of match	Public-firm buyouts	Private-firm buyouts
EIN	255	0
Name	142	108
Total	397	108

Panel D: Control establishments per buyout establishment

	Number of control establishments					
	1	2	3	4	5	Total
Public-firm buyouts	52	39	32	13	261	397
Private-firm buyouts	24	12	10	8	54	108

Panel 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	118	144	160	246	184	218	203	195	186	1,654
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. ***, **, and * after an buyout establishment mean indicates that it is statistically different than the mean for control establishments at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Means of buyout and control establishment characteristics pre-buyout

	Public-firm buyouts			Private-firm buyouts		
	Buyout estabs	Control estabs	*t*-stat	Buyout estabs	Control estabs	*t*-stat
Number	397	1,583		108	380	
Employees	448.05	441.60	-0.15	372.42	398.86	-0.71
Log(Employees)	5.6230	5.5638	1.24	5.6390	5.6831	-0.53
HoursWorked/Employee	1,751.40	1,750.26	0.05	1,628.28	1,643.52	-0.51
Injuries/Employee	0.0641	0.0643	-0.07	0.0708	0.0701	0.11
DARTInjuries/Employee	0.0362	0.0383	-1.05	0.0405	0.0389	0.75

Panel 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
Healthcare, Medical Equipment, and Drugs	50	18
Other	82	22

Panel C: Public buyout firm pre-buyout characteristics

	Sample firms					Compustat universe	
	Mean	Std. Dev	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'sQ	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 firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0050			0.0006		
	(0.0041)			(0.0021)		
PostBuyout	0.0033	0.0002	0.0012	0.0036	0.0001	0.0005
	(0.0068)	(0.0052)	(0.0050)	(0.0053)	(0.0050)	(0.0049)
BuyoutFirm * PostBuyout	-0.0093***	-0.0066**	-0.0074**	-0.0039**	-0.0018	-0.0026
	(0.0040)	(0.0032)	(0.0029)	(0.0018)	(0.0016)	(0.0017)
Log(Employees)			-0.0084***			-0.0044***
			(0.0024)			(0.0017)
HoursWorked/Employee			0.0152***			0.0060**
			(0.0030)			(0.0025)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,025	7,025	7,025	7,025	7,025	7,025
Adjusted R^2	0.3153	0.6604	0.6662	0.2512	0.6209	0.6251

Panel B: Buyouts of private firms

	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	0.3349	0.6975	0.7011	0.2964	0.6603	0.6645

Table 5: Evolution of Injury Rates After PE Buyouts

This table presents estimates of variation in establishment-level injury rates over the four years before and four years after PE buyouts for establishments of PE-acquired 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 four years before, year of, and four 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 $Injuries/Employee$. YearRelBuyout $t \pm K$ is the year of the buyout relative to the buyout year t. YearRelBuyout t (i.e., the buyout year) is excluded from the regressions. That is, all estimates are relative to the buyout year t. Standard errors clustered at the firm level are shown to the right of each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

	Public Targets		Private Targets	
YearRelBuyout -4	-0.0014	(0.0077)	-0.0156*	(0.0093)
YearRelBuyout $t-3$	-0.0026	(0.0070)	-0.0170	(0.0151)
YearRelBuyout $t-2$	-0.0102**	(0.0051)	-0.0047	(0.0072)
YearRelBuyout $t-1$	-0.0017	(0.0033)	-0.0155	(0.0212)
YearRelBuyout $t+1$	-0.0002	(0.0034)	0.0137	(0.0231)
YearRelBuyout $t+2$	0.0056	(0.0053)	-0.0285	(0.0339)
YearRelBuyout $t+3$	0.0023	(0.0064)	-0.0041	(0.0208)
YearRelBuyout $t+4$	0.0065	(0.0079)	-0.0111	(0.0294)
BuyoutFirm * YearRelBuyout $t-4$	0.0019	(0.0055)	-0.0121	(0.0078)
BuyoutFirm * YearRelBuyout $t-3$	0.0001	(0.0038)	-0.0123*	(0.0064)
BuyoutFirm * YearRelBuyout $t-2$	0.0024	(0.0032)	-0.0068	(0.0072)
BuyoutFirm * YearRelBuyout $t-1$	-0.0025	(0.0037)	0.0027	(0.0053)
BuyoutFirm * YearRelBuyout $t+1$	-0.0045	(0.0033)	-0.0011	(0.0052)
BuyoutFirm * YearRelBuyout $t+2$	-0.0114***	(0.0044)	-0.0094*	(0.0052)
BuyoutFirm * YearRelBuyout $t+3$	-0.0110***	(0.0039)	-0.0048	(0.0061)
BuyoutFirm * YearRelBuyout $t+4$	-0.0107***	(0.0033)	0.0061	(0.0062)
Observations	7,887		2,092	
Adjusted R^2	0.6820		0.7302	

Table 6: Injury Rate Changes After PE Buyouts: Public- vs 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0015			-0.0016		
	(0.0042)			(0.0030)		
PostBuyout	0.0075	0.0058	0.0044	0.0075*	0.0051	0.0044
	(0.0067)	(0.0051)	(0.0047)	(0.0042)	(0.0039)	(0.0037)
WasPublic	0.0031			0.0040		
	(0.0046)			(0.0028)		
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	0.0065			0.0008		
	(0.0058)			(0.0036)		
PostBuyout * WasPublic	-0.0027	-0.0058*	-0.0044	-0.0044	-0.0047**	-0.0041**
	(0.0042)	(0.0034)	(0.0034)	(0.0024)	(0.0020)	(0.0020)
BuyoutFirm * PostBuyout * WasPublic	-0.0105**	-0.0111**	-0.0096*	-0.0066*	-0.0056*	-0.0067**
	(0.0051)	(0.0052)	(0.0053)	(0.0035)	(0.0033)	(0.0033)
Log(Employees)			-0.0059**			-0.0029**
			(0.0023)			(0.0015)
HoursWorked/Employee			0.0146***			0.0064***
			(0.0026)			(0.0022)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8,862	8,862	8,862	8,862	8,862	8,862
Adjusted R^2	0.3217	0.6709	0.6755	0.2625	0.6315	0.6347

Table 7: OSHA Inspections and Violations

This table presents difference-in-differences estimates of OSHA inspection violation incidence changes at establishments of PE-acquired 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 to 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

N Matches	5 per buyout establishment		1 per buyout establishment	
	All	Serious	All	Serious
Buyout Firm	-0.00830	-0.0308	-0.0267	-0.0452**
	(0.0178)	(0.0199)	(0.0218)	(0.0219)
Post Buyout	0.0479	0.0837**	0.0791	0.132**
	(0.0400)	(0.0378)	(0.0514)	(0.0535)
Buyout Firm × Post Buyout	-0.0619**	-0.0784**	-0.0643**	-0.0894*
	(0.0300)	(0.0352)	(0.0319)	(0.0478)
Observations	5212	5212	2276	2276
Adjusted R^2	0.044	0.026	0.056	0.035

Table 8: 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 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Industry category SIC codes	Manufacturing (2000s & 3000s)	Transportation (4000s)	Trade (5000s)	Services (7000s & 8000s)
PostBuyout	-0.0006	-0.0222***	-0.0004	0.0116*
	(0.0055)	(0.0071)	(0.0056)	(0.0071)
BuyoutFirm * PostBuyout	-0.0079	-0.0122	-0.0084*	-0.0093**
	(0.0051)	(0.0117)	(0.0043)	(0.0043)
Observations	1,518	1,320	1,631	2,430
Adjusted R^2	0.3328	0.2552	0.2692	0.1600

Table 9: 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 previously-public PE-acquired 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 four years before and four years after the buyout are included in the sample. Establishments with fewer than 100 employees at the time of the buyout are excluded. All column 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 Table XXX for definitions of characteristics. The coefficients on $PostBuyout$ 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: All injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target chars						
(1) PosAbnormalAccruals	−0.0175*	(0.0120)	−0.0090**	(0.0043)	6,293	0.6757
(2) HighAnalystCoverage	−0.0026	(0.0015)	−0.0148**	(0.0068)	7,025	0.6639
(3) TransitoryHoldingPct	−0.0035	(0.0041)	−0.0168**	(0.0081)	7,025	0.6641
(4) RoutineTaskShare	−0.0072	(0.0065)	−0.0013	(0.0016)	6,815	0.6631
(5) KZIndex	−0.0058	(0.0068)	−0.0015	(0.0077)	5,481	0.7075
Buyer chars						
(6) FrequentBuyer	−0.0223	(0.0135)	−0.0086	(0.0059)	7,025	0.6637
(7) ClubDeal	−0.0033	(0.0025)	−0.0102**	(0.0052)	7,025	0.6640
(8) IndustrySpecialist	−0.0072**	(0.0030)	0.0038	(0.0079)	7,025	0.6631
Transaction chars						
(9) LeverageChange	−0.0055	(0.0245)	0.0051	(0.0102)	3,898	0.6344
(10) CEOTurnover	−0.0084***	(0.0032)	0.0110**	(0.0045)	7,025	0.6639
(11) ChairTurnover	−0.0082**	(0.0032)	0.0068	(0.0054)	7,025	0.6631
(12) BuyerExecJoinsBoard	−0.0052***	(0.0017)	−0.0108	(0.0139)	7,025	0.6632
(13) NewDirector	−0.0065***	(0.0023)	−0.0011	(0.0059)	7,025	0.6629
(14) MgmtParticipation	−0.0164	(0.0127)	0.0018	(0.0065)	7,025	0.6634
(15) EmploymentReduction	−0.0112*	(0.0062)	0.0065	(0.0070)	7,025	0.6627

Table 9: Injury Rates Around buyouts: Variation with Pre-Buyout Firm Characteristics (Continued)

Panel B: DART injuries

	BuyoutFirm * PostBuyout		BuyoutFirm * PostBuyout * Characteristic		Obs	Adj R^2
Target chars						
(1) PosAbnormalAccruals	−0.0001	(0.0065)	−0.0031	(0.0031)	6,293	0.6329
(2) HighAnalystCoverage	−0.0065	(0.0075)	−0.0079**	(0.0039)	7,025	0.6206
(3) TransitoryHoldingPct					7,025	
(4) RoutineTaskShare	−0.0084	(0.0065)	0.0013	(0.0105)	6,815	0.6224
(5) KZIndex	−0.0081	(0.0139)	0.0003	(0.0071)	5,481	0.7075
Buyer chars						
(6) FrequentBuyer	−0.0030	(0.0073)	−0.0021	(0.0037)	7,025	0.6203
(7) ClubDeal	−0.0039	(0.0062)	−0.0012	(0.0026)	7,025	0.6224
(8) IndustrySpecialist					7,025	
Transaction chars						
(9) LeverageChange	−0.0058	(0.0132)	0.0097*	(0.0051)	3,898	0.5978
(10) CEOTurnover	−0.0009	(0.0063)	0.0046	(0.0038)	7,025	0.6203
(11) ChairTurnover					7,025	
(12) BuyerExecJoinsBoard					7,025	
(13) NewDirector					7,025	
(14) MgmtParticipation	−0.0019	(0.0069)	0.0030	(0.0030)	7,025	0.6203
(15) EmploymentReduction	−0.0030	(0.0029)	0.0020	(0.0036)	7,025	0.6212

Table 10: Injury Rate Changes After PE Buyouts: Variation with Change in Employment

This table presents estimates of variation in post-buyout injury rates changes at establishments of previously-public PE-acquired firms relative to control establishments with whether or not the establishment decreased employment post-buyout. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 four years before and four 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. $EmpDecrease$ is an indicator equal to one if the establishment's employment decreases from the last reported year before to the first reported year after the buyout and zero otherwise. $Log(Employees)$ 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0116			0.0010		
	(0.0078)			(0.0035)		
PostBuyout	0.0010	-0.0016	-0.0005	0.0017	-0.0019	-0.0014
	(0.0070)	(0.0057)	(0.0054)	(0.0055)	(0.0053)	(0.0051)
EmpDecrease	-0.0043			-0.0032*		
	(0.0028)			(0.0019)		
BuyoutFirm * PostBuyout	-0.0159***	-0.0150**	-0.0112*	-0.0054*	-0.0030	-0.0017
	(0.0059)	(0.0071)	(0.0062)	(0.0033)	(0.0029)	(0.0029)
BuyoutFirm * EmpDecrease	-0.0109			-0.0006		
	(0.0086)			(0.0042)		
PostBuyout * EmpDecrease	0.0036	0.0025	0.0025	0.0028	0.0032*	0.0031*
	(0.0029)	(0.0029)	(0.0028)	(0.0018)	(0.0018)	(0.0017)
BuyoutFirm * PostBuyout * EmpDecrease	0.0108*	0.0143*	0.0065	0.0026	0.0020	-0.0015
	(0.0065)	(0.0078)	(0.0070)	(0.0038)	(0.0036)	(0.0036)
Log(Employees)			-0.0077***			-0.0043****
			(0.0024)			(0.0017)
HoursWorked/Employee			0.0151***			0.0060**
			(0.0030)			(0.0025)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,025	7,025	7,025	7,025	7,025	7,025
Adjusted R2	0.3166	0.6617	0.6664	0.2514	0.6212	0.6252

Table 10: Injury Rate Changes After PE Buyouts: Variation with Change in Employment (Continued)

Panel B: Buyouts of private firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0041			-0.0005		
	(0.0063)			(0.0049		
PostBuyout	-0.0118	-0.0083	-0.0091	-0.0168	-0.0040	-0.0045
	(0.0174)	(0.0203)	(0.0199)	(0.0129	(0.0156)	(0.0155)
EmpDecrease	-0.0068			-0.0095		
	(0.0083)			(0.0062		
BuyoutFirm * PostBuyout	-0.0018	0.0003	0.0013	0.0001	0.0023	0.0027
	(0.0056)	(0.0043)	(0.0046)	(0.0043	(0.0033)	(0.0034)
BuyoutFirm * EmpDecrease	0.0057			0.0014		
	(0.0104)			(0.0070)		
PostBuyout * EmdDecrease	-0.0039	-0.0020	-0.0015	0.0034	0.0044	0.0047
	(0.0069)	(0.0063)	(0.0063)	(0.0049)	(0.0047)	(0.0048)
BuyoutFirm * PostBuyout * EmpDecrease	-0.0053	0.0024	0.0018	0.0049	0.0029	0.0031
	(0.0111)	(0.0105)	(0.0106)	(0.0071)	(0.0065)	(0.0066)
Log(Employees)			0.0043			0.0037*
			(0.0037)			(0.0022)
HoursWorked/Employee			0.0135***			0.0081***
			(0.0039)			(0.0028)
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 R2	0.3358	0.6970	0.7006	0.2980	0.6601	0.6637

Table 11: 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 firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 four years before and four 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 4-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 pre-buyout to the first year observed post-buyout and zero otherwise. Standard errors clustered at the firm level are shown below each point estimate. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

Dep var	Log(Empl)	Log(Empl)	Log(Empl)	HoursWorked/ Employee	HoursWorked/ Employee	HoursWorked/ Employee
PostBuyout	-0.0975	-0.1075	-0.2658*	-0.1130	-0.1186	-0.1337
	(0.1110)	(0.1102)	(0.1415)	(0.0811)	(0.0824)	(0.0940)
BuyoutFirm * PostBuyout	-0.1308***	-0.2457***	-0.2074***	0.0409*	0.0909***	0.0060
	(0.0354)	(0.0597)	(0.0507)	(0.0211)	(0.0334)	(0.0318)
PostBuyout * EstabInjuryRate		0.4818			0.0528	
		(0.3357)			(0.2187)	
BuyoutFirm * PostBuyout * EstabInjuryRate		1.8220***			-0.8117**	
		(0.4540)			(0.3970)	
PostBuyout * IndustryInjuryRate			5.7313**			0.7872
			(2.8088)			(1.9766)
BuyoutFirm * PostBuyout * IndustryInjuryRate			1.4057***			0.5816
			(0.5248)			(0.4152)
Establishment FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,025	7,025	7,025	7,025	7,025	7,025
Adjusted R^2	0.8846	0.8846	0.8846	0.6679	0.6676	0.6681

Panel B: Buyouts of private firms

Dep var	Log(Empl)	Log(Empl)	Log(Empl)	HoursWorked/ Employee	HoursWorked/ Employee	HoursWorked/ Employee
PostBuyout	0.0406	-0.1038	-0.6908*	0.0125	0.0118	-0.1465
	(0.0984)	(0.1526)	(0.3524)	(0.1054)	(0.1346)	(0.3848)
BuyoutFirm * PostBuyout	0.0011	-0.0102	-0.0683	-0.0633*	-0.0638	-0.0883
	(0.0466)	(0.0713)	(0.0841)	(0.0354)	(0.0595)	(0.0679)
PostBuyout * EstabInjuryRate		1.6372			0.0067	
		(1.3864)			(0.9748)	
BuyoutFirm * PostBuyout * EstabInjuryRate		0.1939			0.0075	
		(0.7021)			(0.5488)	
PostBuyout * IndustryInjuryRate			9.5667*			2.0803
			(4.9359)			(5.1264)
BuyoutFirm * PostBuyout * IndustryInjuryRate			1.0108			0.3642
			(0.9037)			(0.7450)
Establishment FE	Yes	Yes	Yes	Yes	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	0.8531	0.8531	0.8531	0.6358	0.6353	0.6351

Table 12: 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

InjuryRateChange	-5.7863*	
	(3.2184)	
IndAdjInjuryRateChange		-5.5713*
		(3.2088)
Constant	-1.0299***	-1.0290***
	(0.1576)	(0.1580)
Observations	114	114
Pseudo R-squared	0.0250	0.0230

A Additional Tables

Table A1: 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 firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0099*			0.0019		
	(0.0060)			(0.0026)		
PostBuyout	-0.0007	0.0078	0.0081*	-0.0047	0.0001	0.0003
	(0.0054)	(0.0056)	(0.0044)	(0.0052)	(0.0046)	(0.0041)
BuyoutFirm * PostBuyout	-0.0108***	-0.0112**	-0.0124**	-0.0057***	-0.0037*	-0.0043**
	(0.0039)	(0.0053)	(0.0051)	(0.0022)	(0.0020)	(0.0019)
Log(Employees)			-0.0063*			-0.0031
			(0.0036)			(0.0023)
HoursWorked/Employee			0.0211***			0.0114
			(0.0053)			(0.0031)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,645	2,645	2,645	2,645	2,645	2,645
Adjusted R^2	0.4713	0.7769	0.7853	0.3811	0.7326	0.7385

Table A2: Injury Rate Changes After PE Buyouts: Difference-in-Difference Estimates Matching on Multiple Characteristics

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0059			0.0008		
	(0.0038)			(0.0020)		
PostBuyout	0.0017	0.0012	0.0014	0.0013	-0.0012	-0.0011
	(0.0066)	(0.0052)	(0.0050)	(0.0047)	(0.0045)	(0.0043)
BuyoutFirm * PostBuyout	-0.0102***	-0.0070**	-0.0077***	-0.0040**	-0.0016	-0.0021
	(0.0029)	(0.0030)	(0.0028)	(0.0018)	(0.0016)	(0.0017)
Log(Employees)			-0.0071***			-0.0031*
			(0.0026)			(0.0018)
HoursWorked/Employee			0.0141***			0.0053**
			(0.0027)			(0.0021)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,025	7,025	7,025	7,025	7,025	7,025
Adjusted R^2	0.3484	0.6741	0.6773	0.2708	0.6457	0.6491

Panel B: Buyouts of private firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	-0.0034			-0.0025		
	(0.0045)			(0.0034)		
PostBuyout	-0.0177	-0.0212	-0.0207	-0.0197**	-0.0107	-0.0105
	(0.0138)	(0.0165)	(0.0166)	(0.0096)	(0.0156)	(0.0155)
BuyoutFirm * PostBuyout	-0.0009	0.0012	0.0014	0.0031	0.0046	0.0048
	(0.0050)	(0.0044)	(0.0045)	(0.0035)	(0.0030)	(0.0030)
Log(Employees)			0.0072*			0.0062**
			(0.0037)			(0.0025)
HoursWorked/Employee			0.0089**			0.0054*
			(0.0044)			(0.0031)
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	0.4853	0.7420	0.7437	0.4584	0.7351	0.7377

Table A3: Injury Rate Changes After PE Buyouts: Difference-in-Differences Estimates with a Single Control Establishment

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0064			0.0017		
	(0.0041)			(0.0024)		
PostBuyout	-0.0083	-0.0110	-0.0086	-0.0036	-0.0105*	-0.0091
	(0.0099)	(0.0082)	(0.0084)	(0.0071)	(0.0062)	(0.0065)
BuyoutFirm * PostBuyout	-0.0085**	-0.0052	-0.0070**	-0.0026	-0.0034	-0.0018
	(0.0038)	(0.0036)	(0.0035)	(0.0022)	(0.0021)	(0.0019)
Log(Employees)			-0.0107***			-0.0047**
			(0.0026)			(0.0020)
HoursWorked/Employee			0.0169***			0.0064**
			(0.0045)			(0.0027)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,940	2,940	2,940	2,940	2,940	2,940
Adjusted R^2	0.3310	0.6206	0.6251	0.2279	0.5982	0.6078

Panel B: Buyouts of private firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0029			0.0030		
	(0.0060)			(0.0045)		
PostBuyout	-0.0020	-0.0102	-0.0104	0.0061	0.0118	0.0116
	(0.0061)	(0.195)	(0.0206)	(0.0139)	(0.0224)	(0.0229)
BuyoutFirm * PostBuyout	-0.0071	-0.0057	-0.0052	0.0009	0.0013	0.0017
	(0.0061)	(0.0050)	(0.0051)	(0.0043)	(0.0037)	(0.0038)
Log(Employees)			0.0042			0.0025
			(0.0042)			(0.0027)
HoursWorked/Employee			0.0118**			0.0086**
			(0.0057)			(0.0043)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	830	830	830	830	830	830
Adjusted R^2	0.3764	0.7159	0.7183	0.3206	0.6519	0.6549

Table A4: Injury Rate Changes After PE Buyouts: Difference-in-Differences Estimates with Lower Minimum Establishment Size

This table presents difference-in-differences estimates of post-buyout injury rates changes at establishments of PE-acquired firms relative to control establishments. Panel A presents results for buyouts of publicly-traded firms. Panel B presents results for buyouts of private firms. 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 four years before and four 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. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% level, respectively, based on a two-tailed t-test.

Panel A: Buyouts of public firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0008			0.0025		
	(0.0045)			(0.0022)		
PostBuyout	-0.0021	0.0014	0.0019	0.0017	0.0033	0.0036
	(0.0063)	(0.0056)	(0.0053)	(0.0046)	(0.0052)	(0.0050)
BuyoutFirm * PostBuyout	-0.0092***	-0.0062**	-0.0064**	-0.0035**	-0.0017	-0.0020
	(0.0032)	(0.0032)	(0.0031)	(0.0017)	(0.0015)	(0.0016)
Log(Employees)			-0.0083***			-0.0033**
			(0.0021)			(0.0014)
HoursWorked/Employee			0.0128***			0.0044*
			(0.0031)			(0.0026)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	10,103	10,103	10,103	10,103	10,103	10,103
Adjusted R^2	0.2681	0.5996	0.6018	0.2383	0.5919	0.5941

Panel B: Buyouts of private firms

	All injuries			DART injuries		
	(1)	(2)	(3)	(4)	(5)	(6)
BuyoutFirm	0.0045			0.0031		
	(0.0051)			(0.0033)		
PostBuyout	-0.0126	-0.0096	-0.0101	-0.0118	0.0008	0.0005
	(0.0121)	(0.0191)	(0.0187)	(0.0094)	(0.0149)	(0.0147)
BuyoutFirm * PostBuyout	-0.0041	-0.0001	0.0008	0.0010	0.0040	0.0044
	(0.0052)	(0.0048)	(0.0048)	(0.0032)	(0.0029)	(0.0029)
Log(Employees)			0.0059*			0.0047**
			(0.0036)			(0.0022)
HoursWorked/Employee			0.0170***			0.0088***
			(0.0041)			(0.0028)
Establishment FE	No	Yes	Yes	No	Yes	Yes
Year x Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,387	2,387	2,387	2,387	2,387	2,387
Adjusted R^2	0.2896	0.6559	0.6613	0.2527	0.6289	0.6332