M&A and Workplace Safety: Evidence from U.S. Mines

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Abstract

Using data from the U.S. Mine Safety and Health Administration, this paper provides the first evidence on the effect of M&A on workplace safety. Compared to mines that were never involved in any acquisition, injury rates increase in mines that underwent changes of control. The estimated effect is more pronounced when the new owners have poorer workplace safety records and are expected to gain greater monopsony power in local labor markets. In contrast to other acquisitions, injury rates tend to fall following private equity buyouts.

Keywords: mergers and acquisitions, control, ultimate ownership, labor and finance, implicit contracts, workplace safety

JEL Codes: G34, J24, J28.

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1 Introduction

Workplace safety is important for workers' well-being, and the lack of it imposes substantial societal costs. For example, in 2022, around 3 out of 100 full-time equivalent workers in the U.S. private industry experienced work-related injuries and illnesses, with the total economic cost estimated at $1.3 trillion.[1] Due to its importance, federal agencies that regulate competition have started to pay attention to the interaction between mergers and acquisitions (M&As) and workplace safety.[2] On June 29, 2023, the two primary competition regulators, the Federal Trade Commission (FTC) and the antitrust division of the Department of Justice (DOJ), proposed changes to the pre-merger notification process implementing the Hart-Scott-Rodino (HSR) Act. One proposed change would have created a new Worker and Workplace Safety Information section and required merging parties to report their prior five-year record of violations of employees' occupational health and safety rights.[3] The FTC and DOJ proposal stated that a history of labor rights violations could indicate employers' monopsony power, and such information is relevant to identifying potential labor market effects of transactions.

Despite the importance of workplace safety and the increased regulatory emphasis on the effects of M&A on the labor markets, the empirical evidence on how changes of control affect workplace safety remains limited. Partly for this reason,

[1] https://www.bls.gov/news.release/pdf/osh.pdf and https://injuryfacts.nsc.org/all-injuries/costs/societal-costs/data-details

[2] A July 2021 presidential executive order expressed the administration's policy, "to enforce the antitrust laws to combat the excessive concentration of industry, the abuses of market power, and the harmful effects of monopoly and monopsony especially as these issues arise in labor markets...."https://www.whitehouse.gov/briefing-room/presidential-actions/2021/07/09/executive-order-on-promoting-competition-in-the-american-economy/.

[3] https://www.federalregister.gov/documents/2023/06/29/2023-13511/premerger-notification-reporting-and-waiting-period-requirements.

the FTC and DOJ did not adopt the proposal at the time, opting to return to the issue in the future if stronger evidence emerges to support the theory.[4] This paper helps fill this gap in the literature with evidence on the effects of changes of control on workplace safety.[5]

The view that firms should provide employees with broad protection against job-related risk has its foundation in the implicit contract theory of Baily (1974) and Azariadis (1975). According to this theory, risk-neutral entrepreneurs insure risk-averse workers by insulating their wages and jobs from adverse shocks to production. In exchange for greater job stability, employees might be more willing to invest in firm-specific skills. Although regulations exist to enforce minimum health and safety standards and penalties are imposed on firms caught violating these standards, firms vary in the extent to which they ensure a safe, healthy working environment for employees. In this sense, the provision of workplace safety is a form of "implicit" (as opposed to explicit or enforceable) contract. Implicit contracts are at risk when control changes. Shleifer and Summers (1988) made this point, arguing that the gains from breaching implicit contracts might even be a key driver of M&A activities. For instance, a corporate raider might make short-term gains by cutting wages once their investment in firm-specific human capital is sunk.[6] Building on this argument, we develop a simple model of how changes of control affect workplace safety. In the model, we assume that a change of con-

[4] "Although the Commission has determined not to require specific information about workers or workplace safety information in the HSR Filing at this time, as the Agencies acquire more experience with conducting competition analyses of labor markets, the Commission may revisit the issue in future rulemakings" (Federal Trade Commission, 2024).

[5] A large literature in economics studies how changes in ownership and control affect labor, but attention focuses on employment, layoff risks, and compensation, not workplace safety. For a recent survey, see Pagano (2020).

[6] Pagano and Volpin (2005) propose a model that shows that the Shleifer-Summers story can be fully incentive compatible.

trol disrupts a worker's owner-specific human capital, and the worker needs to exert more effort to maintain the same productivity with the new owner. Because of the increased effort, the injury rates increase after a change of control; the effect also depends on the differences in workplace safety practices and corporate governance between new and old owners, as well as the value of workers' outside options after changes of control. We proceed to test these hypotheses in the U.S. mining sector.

The U.S. mining industry makes a compelling empirical laboratory to study how M&A affects workplace safety for three main reasons. First, the federal government mandates that each U.S. mine report work-related accidents, injuries, and illnesses along with employment and working hours every quarter to the Mining Safety and Health Administration (MSHA). Second, MSHA has tracked the party in control (the ultimate owners) of each mine since January 1st, 1950. Third, unlike many other sectors, the mining sector's product markets are fairly competitive. This implies that changes of control are less likely to affect producers' product market power, and thus changes in product market power are unlikely to be a primary driver of our findings.

To estimate the effects of changes of control on workplace safety, we use a stacked difference-in-differences (DiD) design that compares average changes in injury rates of mines that experienced changes of control (treated mines) to average changes in mines that were never involved in any acquisitions in the sample period (control mines). Our main finding is consistent with the implicit contract theory. Specifically, changes of control are associated with an 11% increase in injury rates per labor hour of treated mines relative to control mines. The baseline result is robust to different types of injuries, variations in the design of the set of control mines, and alternative estimation methods, including propensity score

matching and synthetic difference-in-differences (Arkhangelsky et al., 2021). The estimated effect on the number of cited regulatory violations is similar: we find that the number of reported violations increases by 8.6% in the treated as compared with the control mines.

According to the model, changes of control affect workplace safety because workers lose owner-specific human capital, and the new owner needs the workers to exert more effort to rebuild it. Empirically, we cannot measure employee effort directly but we use working intensity, measured as working hours per employee, as an imperfect proxy for it. We find that employees in treated mines on average experience a 5% increase in working hours (equivalent to 1.7 hours per week) after a change of control.

As further support for the model, we show that it is the change in control, not merely a change in management, that drives the effect. The dataset allows us to distinguish between changes in the identity of the ultimate owner of a mine and changes in the mine management. While the increase in injury rates is larger when both the owner and the management change, there is no effect when only management changes, and there is a significantly positive increase in injury rates when only the owner changes. This suggests that ownership plays a more critical role than management in sustaining implicit contracts, or that managers primarily act in accordance with owners' preferences.

Leveraging cross-sectional heterogeneity in our sample, we test three more predictions of the model. First, our model implies that the post-acquisition change in injury rates depends on the difference in workplace safety practices between the new and previous owners. Empirically, we use owner fixed effects that are estimated using pre-acquisition injury rate data as a proxy for an owner's workplace safety practices. We find that the estimated effect on injury rates is larger when

new owners have worse workplace safety practices than previous owners.

Second, our model predicts the increase in injury rate to be higher if the worker's outside option decreases after a change of control (due to increased monopsony power in the local labor market). We find evidence that supports this prediction. We define a local labor market in the mining sector as a commuting zone. For each acquirer that made acquisitions in a local labor market, we follow Arnold (2021) and calculate the predicted change in employment-based Herfindahl-Hirschman index (HHI) as a proxy for the predicted change in the local monopsony power of the acquirer. Consistent with the model's prediction, the magnitude of the estimated effect on injury rates is larger for acquisitions that are predicted to increase local employer monopsony power more.

Third, according to our model, post-acquisition injury rates may be lower when the new owners have significantly better corporate governance, that is, greater alignment between managers and owners, compared to the previous owners. We test this prediction by comparing private equity (PE) and non-PE acquirers. PE firms have better corporate governance than traditional corporations (Jensen, 1989; Kaplan and Stromberg, 2009), and recent evidence shows that PE owners actively monitor the companies that they invest in (Herkenhoff et al., 2025). We therefore argue that PE-owners are likely to provide higher-power incentives to managers so that their preferences are more aligned with owners and they invest more in workplace safety. In line with the results in Cohn et al. (2021), we find that mines acquired by PE firms experience a decline in injury rates.[7]

This paper contributes to the literature on corporate finance and workplace safety. Using establishment-level data from the Bureau of Labor Statistics (BLS)

[7]The establishments in Cohn et al. (2021) are in the manufacturing, transportation, trade, and services sectors. The mining sector is not in their sample.

Survey of Occupational Injuries and Illnesses (SOII), Cohn and Wardlaw (2016) shows that establishments have higher injury rates if their parent companies are more financially constrained, suggesting that a firm's financial condition is an important determinant of workplace safety. Charles et al. (2022) studies how product demand shocks affect workplace safety in the metal mining industry. Positive demand shocks relax financial constraints but also increase the opportunity cost of investing in workplace safety. Using the quarterly changes in commodity prices to measure demand shocks, they find that these shocks lead to worse workplace safety. However, they further find that demand shocks that *only* increase a mine's cash flow lead to lower injury rates, consistent with the evidence in Cohn and Wardlaw (2016). Two papers in this literature, Cohn, Nestoriak, and Wardlaw (2021) and Chen and Wittry (2024), are closely related to ours. Using the SOII data as well as data on health and safety violations from the Occupational Safety and Hazard Administration (OSHA), Cohn et al. (2021) shows that private-equity-sponsored buyouts of public companies lead to an improvement in workplace safety. Chen and Wittry (2024) uses data from the U.S. coal mines and find that mines that transited from privately held to publicly traded experienced a significant increase in production and safety violations. Our paper complements these two papers. While these authors focus on ownership changes from publicly traded to privately held and vice versa, we focus on all changes of control due to mergers and acquisitions.

Our paper also contributes to the literature on the M&As and labor. In the corporate finance literature, studies find that human capital-related factors are important drivers of M&As (see, e.g., Ouimet and Zarutskie, 2020; Chen et al., 2020; Tate and Yang, forthcoming). In mergers between hospitals, Prager and Schmitt (2021) finds that the wages of skilled workers and nursing & pharmacy

workers grew slower in commuting zones that experienced a large increase in merger-induced concentration. Arnold (2021) uses microdata from the U.S. Census Bureau and finds that M&As that increase local labor market concentration lead to lower worker earnings and lower employment. Arnold, Milligan, Moon, and Tavakoli (2023) studies the effects of M&As on worker outcomes in Canada. The authors also find that employees in target firms suffer earnings losses. The effect is primarily driven by employees who leave target firms, and their match qualities with new employers are much worse. This evidence is consistent with the evidence on how M&As affect workers in Brazil in Lagaras (forthcoming). Bach, Baghai, Bos, and Silva (forthcoming) assesses the non-monetary impacts of M&As on workers and shows that mergers lead to an increased incidence of mental illness among workers in both targets and acquirers. Ma, Ouimet, and Simintzi (2024) shows that M&As lead target establishments to adopt more technology and become less routine-intensive. Their evidence highlights the role of M&As in facilitating technology adoption and affecting labor reallocation.

We contribute to this literature by studying how M&As affect non-wage job amenities, focusing on workplace safety. To the best of our knowledge, this is the first empirical evidence documenting the effect of M&As on workplace safety. Our results provide support for the arguments in Shleifer and Summers (1988) that shareholder gain sometimes comes at the expense of other non-financial stakeholders. In conjunction with prior evidence on the negative effects on wage, this paper's analysis produces evidence that workers in target firms also experience worse workplace safety after M&As, consistent with the views that wages and job amenities are positively correlated in Mortensen (2003) and empirical evidence in Sockin (2022) and Marinescu, Qiu, and Sojourner (2023).

2 Model

In this section, we build a theoretical framework for our empirical analysis based on the implicit contract theory (e.g., Shleifer and Summers (1988)). We follow and adapt Pagano and Volpin (2005) to rationalize why implicit contracts, or the alliance between management and workers, are incentive-compatible in the context of a change of control. The setup of the model is the following.

2.1 Setup

The game involves four players (an initial and a new owner, a manager, and a worker) and six stages:

- At $t = 0$, the initial owner hires the worker.
- At $t = 1$, one of two things happens: (i) there is no change of owner (NC); or (ii) there is a change of control (CC): the new owner replaces the initial one.
- At $t = 2$, the owner in control (the initial or the new owner, depending on whether there is a change of control at $t = 1$) hires the manager and writes an incentive contract in which the manager is given a fraction α of the profits.[8]
- At $t = 3$, the manager chooses the wage w to pay to the worker conditional on output y. The worker's outside option at this stage is $\overline{U}$.
- At $t = 4$, the manager chooses how much effort to invest in health and safety $h \geq 0$ at a personal cost h and the worker chooses to invest in effort $\epsilon \in \{0, 1\}$. Both investments are unobservable.

[8] The manager's outside option is set to 0 so that the participation constraint can be ignored.

- At $t = 5$, the output y is produced, the wage w is paid, and the profits are distributed to the owner and the manager. With probability e, $y = Y$. The worker is injured with probability $p = [\rho + (1-\rho)\epsilon]\overline{p}/(1+h)$, where $\rho < 1$. p is increasing in the worker's effort ϵ (i.e., the effort is costly) and decreasing in the manager's investment in health and safety, h.

There are three critical assumptions. First, we assume that the worker's productivity e depends on whether there is a change of control or not. If there is no change of control, $e = 1$; if there is a change of control, $e = \epsilon$. This captures the idea that the worker's human capital is owner-specific and is a substitute for effort: the change of control disrupts worker productivity and requires an investment in effort to maintain the same productivity. As effort increases the probability of an injury, the worker needs to be compensated for providing effort. Second, the investment in health and safety, h, is made by a manager who gives a weight $\alpha \in [0,1]$ to shareholder value. The value of α depends on the corporate governance of the firm. Specifically, the cost to the owner of providing an incentive contract α to the manager is $(1-\lambda)\alpha$, where $\lambda \in [0,1)$ measures the alignment between the owner and the manager. This feature will be useful in distinguishing between corporate and private equity acquirers. Finally, we assume that the worker suffers a loss $L > 0$ in case of injury and is otherwise risk-neutral.

The model is solved by backward induction starting at stage $t = 3$. There are two subgames to consider.

2.2 *No Change of Control*

If there is no change of control at $t = 1$, the worker's incentive compatibility condition is irrelevant and the manager chooses (h, w) to maximize:

$$\alpha(Y - w) - h \tag{2.1}$$

subject to the participation constraint :

$$w \geq \frac{\rho \overline{p} L}{1 + h} + \overline{U}. \tag{2.2}$$

In equilibrium, the participation constraint is binding and thus problem (2.1) becomes:

$$\max_{h \geq 0} \alpha \left(Y - \frac{\rho \overline{p} L}{1 + h} - \overline{U} \right) - h \tag{2.3}$$

From the first order condition (which is a necessary and sufficient condition because the objective function is concave in h), we have that the probability of injury is:

$$p_{NC} = \sqrt{\frac{\rho \overline{p}}{\alpha L}}. \tag{2.4}$$

Proceeding backwards to $t = 2$, the initial owner chooses α to maximize:

$$\max_{\alpha \in [0,1]} [1 - \alpha(1 - \lambda)] \left(Y - p_{NC} L - \overline{U} \right), \tag{2.5}$$

where p_{NC} is given in Equation 2.4. It is convenient to express this problem as a choice of p_{NC}. Since $\alpha = \frac{\rho \overline{p}}{L} \left(\frac{1}{p_{NC}} \right)^2$ based on Equation 2.4, the problem (2.5) becomes:

$$\max_{p_{NC}\in[0,\overline{p}]} \left[1-(1-\lambda)\frac{\rho\overline{p}}{L}\left(\frac{1}{p_{NC}}\right)^2\right]\left(Y-p_{NC}L-\overline{U}\right), \tag{2.6}$$

From the first order conditions (which are necessary and sufficient because the problem is concave in p_{NC}), we obtain our first result. The details of all proofs are available in Appendix A.

Result 1: *The equilibrium injury rate in case there is no change of control p^*_{NC} is decreasing in λ, $\overline{U}$, and L; and increasing in ρ and $\overline{p}$.*

2.3 *Change of Control*

If there is a change of control at $t=1$, the manager chooses (h,w) to maximize:

$$\alpha(Y-w)-h \tag{2.7}$$

subject to the incentive compatibility condition

$$w \geq \frac{\overline{p}L}{1+h}, \tag{2.8}$$

and the participation constraint

$$w \geq \frac{\overline{p}L}{1+h}+\overline{U}, \tag{2.9}$$

The first constraint is redundant, and in equilibrium, the second constraint is binding. Using this result, problem (2.7) becomes:

$$\max_{h\geq 0}\alpha\left(Y-\frac{\overline{p}L}{1+h}-\overline{U}\right)-h \tag{2.10}$$

From the first order condition, the probability of injury is:

$$p_{CC} = \sqrt{\frac{\overline{p}}{\alpha L}}. \tag{2.11}$$

Proceeding backwards to $t = 2$, the new owner chooses α to maximize:

$$\max_{\alpha \in [0,1]} [1 - \alpha(1 - \lambda)] \left(Y - p_{CC}L - \overline{U}\right), \tag{2.12}$$

where p_{CC} is given in Equation 2.11. As done before, it is convenient to express this problem as a choice of p_{CC}. Since $\alpha = \frac{\overline{p}}{L}\left(\frac{1}{p_{CC}}\right)^2$ based on equation Equation 2.11, the problem (2.12) becomes:

$$\max_{p_{CC} \in [0,\overline{p}]} \left[1 - (1 - \lambda)\frac{\overline{p}}{L}\left(\frac{1}{p_{CC}}\right)^2\right] \left(Y - p_{CC}L - \overline{U}\right) \tag{2.13}$$

From the first order conditions (which are necessary and sufficient because the problem is concave in p_{CC}), we obtain our second result.

Result 2: *The equilibrium injury rate p^*_{CC} in case of a change of control is decreasing in λ, $\overline{U}$, and L; and increasing in $\overline{p}$.*

2.4 *Impact of a change of control on the injury rate*

Noting from Result 1 that p^*_{NC} is strictly increasing in ρ, and that $p^*_{CC} = p^*_{NC}$ when $\rho = 1$, holding all other parameters constant, it is immediate to conclude:

Prediction 1: *Holding all other parameters fixed, the injury rates in the target mine increase after a change of control: $p^*_{CC} > p^*_{NC}$.*

The intuition for this result is that the new owner needs the worker to exert more effort to achieve the same productivity due to the loss of owner-specific

human capital. This implies that the change of control increases the risk of injury for the worker as compared to the case of no change of control.

2.5 Differences between New and Initial Owners

In this section we will allow for differences between the new and the inini-tial owners in terms of workplace safety standards (as measured by $\overline{p}$), workers' outside options (as measured by $\overline{U}$), and the cost of incentivizing managers (as measured by λ). We examine the impacts of these differences on the injury rate. In each case, we will indicate with a subscript 0 for the initial owner and with a subscript 1 for the new owner.

2.5.1 Workplace Safety Practices

The parameter $\overline{p}$ associated with an owner can be interpreted as the owner's workplace safety practices, and a higher $\overline{p}$ implies that an owner has worse workplace safety practices. From Result 2, we know that p^*_{CC} is increasing in $\overline{p}$. This implies:

Prediction 2: *Holding all other parameters as constant, the effect of change of control on the injury rates in the target mine is larger when the new owner has worse workplace safety practices relative to the previous owner (i.e., if* $\overline{p}_1 > \overline{p}_0$*).*

2.5.2 Workers Outside Option

From Result 2, p^*_{CC} is decreasing in the worker's outside option $\overline{U}$. This implies that the injury rates in the target mine will increase more following an acquisition if the worker's participation constraint becomes more slack (i.e., if $\overline{U}_1 < \overline{U}_0$). This is a testable prediction if we have a suitable proxy for the labor market conditions:

Prediction 3: *Holding all other parameters constant, if an acquisition makes the local labor market less competitive (i.e., if $\overline{U}_1 < \overline{U}_0$), then the injury rates in the target mine increase more.*

2.5.3 Private Equity Acquirers

An important parameter in the model is λ: $1 - \lambda$ measures how much it costs to the owner to share a dollar of profit with the manager. It is reasonable to assume that private equity acquirers have a higher λ. There are different ways to justify this assumption. First, it is generally accepted that private equity firms have better corporate governance than traditional corporations (Jensen, 1989; Kaplan and Stromberg, 2009). Therefore, under private equity ownership, the incentives of managers and shareholder value are more aligned, and λ captures the alignment between the two. A second way to support this assumption is to follow Mueller and Panunzi (2004) and notice that PE acquirers tend to use more debt than corporate buyers to fund their acquisitions. If λ measures financial leverage (i.e., debt is raised and used to fund the acquisition), the profits net of managerial compensation (and including the proceeds from the debt issuance) for a PE owner is $[\lambda + (1-\lambda)(1-\alpha)](Y - pL - \overline{U}_1) = [1 - \alpha(1-\lambda)](Y - pL - \overline{U}_1)$.

Let λ_{CO} and λ_{PE} denote the values of λ for corporate and PE acquirers, respectively, and let p_{CO} and p_{PE} represent the injury probabilities for corporate and PE acquirers, respectively. From Result 2, we know that $\lambda_{CO} < \lambda_{PE}$ implies that $p_{CO} > p_{PE}$.

The comparison between p_{NC} and p_{PE} is more complex because there are two conflicting effects at play when the new owner is a private equity firm. First, the change in control requires more effort by the worker to remain productive. This

increases the injury rates. The second effect is that the manager is more aligned with shareholder value under private equity ownership, which increases the investment in health and safety. The net effect depends on the difference in corporate governance between the PE acquirers and the initial owners, $\Delta\lambda \equiv \lambda_{CO} - \lambda_{PE}$: the larger $\Delta\lambda$, the greater the difference in governance between the two. Based on Result 2, we have the following prediction:

Prediction 4: *Holding all other parameters constant, if the new owner is a private equity firm, then the injury rates in the target mine decrease ($p^*_{PE} < p^*_{NC}$) provided that the governance gap between the private equity owner and the previous owner $\Delta\lambda$ is sufficiently large.*

3 Data on Mines in the U.S.

For each mine, we observe the evolution over time of its owners, occupational injuries, employment, labor hours, and other characteristics using the following five Mining Safety and Health Administration (MSHA) datasets.[9] Our data ends in 2023Q2.

- **Mine Attributes**: For each mine, this data reports its location (county, state, latitude, longitude), regulation district, current status, and mine type (facility, underground, surface).

- **Quarterly Employment Data**: This data reports employment and working hours at the mine×subunit code×year×quarter level. The data is available

[9]Specifically, in the order listed below, they are the msha_mine, msha_qtrly_oprtr_emplymnt, msha_accident, msha_inspection, msha_violation, msha_controller_history, and msha_operator_history files available at https://enforcedata.dol.gov/views/data_summary.php.

since 1983Q1. We aggregate data to the mine×year×quarter level by summing employment and working hours across all subunits within a mine. We drop mines that have missing or zero employment or working hours from our sample.

- **Accident Data**: This data are extracted from the Mine Accident, Injury and Illness Report form (MSHA Form 7000-1). It contains information on all accidents, injuries, and illnesses reported by mine operators and contractors and are available since 1983Q1. The data provides information on the number of injuries at the mine×subunit code×year×quarter level. We aggregate data to mine×year×quarter level by summing the number of injuries across subunits within a mine. If there are no reported injuries at a mine in a quarter, then the number of injuries is zero.

- **Inspection Data**: The data provides information on each inspection of a mine. The data provides information on the event number, the beginning and ending dates of an inspection, and the description and code of the type of inspection. Underground mines are required to receive four regular inspections per year and surface mines are required to receive two regular inspections per year. Other inspections could be driven by workers' complaints. We focus on the regular inspections by MSHA.

- **Violation Data**: The data provides information on each violation associated with an inspection, and we calculate the total number of violations in each inspection. If there is no recorded violation in an inspection, then the number of total violations is assigned to be zero. We merge data on violations with data on inspections using the event number of each inspection. We fur-

ther calculate the number of inspections and total number of violations in a mine in a quarter, and then calculate the average number of violations per inspection in a mine in a quarter.

- **Data on Owners and Managers of Mines**: To identify whether a mine is involved in an acquisition in a quarter, we use the data on the history of the owner and manager of a mine provided by MSHA. A manager of a mine operates the mine on behalf of the owner. For each mine, the data provides the starting and ending dates of each owner and manager. An owner could be a firm or a person.

In Section 5 we will adopt an estimation window of [-12, +12] quarters around an acquisition. Given the choice of the estimation window and that the injury data starts in 1983Q1 and ends in 2023Q2, we focus on acquisitions made between 1986Q1 and 2020Q3. During this period, we identify 18,711 mine-quarter observations that experienced changes of control, which involve 12,160 unique target mines and 8,165 unique acquirers.[10] On average, around 1% of mines experienced changes of control in a quarter. Cumulatively, around 25% of mines experienced at least one change of control during this period. Most of the acquisitions in our sample are asset sales, that is, an owner sells some mines in its portfolio to an acquirer. There are 1,940 changes of control associated with mergers between two owners, and it accounts for around 10% of all changes of control. We report the trend of the share of mines that experienced changes of control at the quarterly level in Figure 1.

[10]There are 18,860 acquisitions. More than one acquirer can be associated with an acquisition.

4 Empirical Strategy

This section describes the empirical strategy used to estimate the effects of changes of control on injury rates. We identify a cohort as all acquisitions that happen in a quarter. For each cohort, we choose the estimation window to be 12 quarters before and after the event quarter (25 quarters in total). Given that the employment data starts in the first quarter of 1983 and our current sample ends in the second quarter of 2023, we use cohorts between the first quarter of 1986 and the second quarter of 2020.

A mine is defined to be a treated mine in a cohort if it experienced a change of control during the event quarter. The potential set of control mines includes those that were not involved in any acquisition events in the event quarter, that is, they did not experience control changes and their owners did not make any acquisitions. For a cohort with event quarter t, we keep mines with strictly positive employment and strictly positive working hours.

To cleanly estimate the effects, we further require the treated mines do not experience other control changes and that their owners do not make any other acquisitions within the estimation window. We further require that control mines do not experience any change of control and their owners do not make any acquisitions during the whole sample period between 1983Q1 and 2023Q2. In the sample, there are 4,836 unique treated mines and 17,497 unique control mines.

We use a stacked difference-in-differences (DiD) design and compare the changes in injury rates in treated mines after control changes to ones in control mines. Following Cengiz, Dube, Lindner, and Zipperer (2019), we first stack the observations across cohorts and then run the following regression in Equation 4.1. An

advantage of this specification is that it aligns treated and control mines by event time, and this is equivalent to a setting in which all the events happen at the same time rather than being staggered over time. This specification mitigates concerns raised by Goodman-Bacon (2021) with the canonical two-way fixed effects model of effects of a staggered treatment and is in a similar spirit to the method proposed in Callaway and SantAnna (2021).

$$y_{kit} = Treated_{ki} \times \sum_{e=-12 \& e \neq -1}^{12} \beta_e \times \mathbb{1}(t - t_k = e) + \mu_{ki} + \xi_{kj(i)t} + \eta_{kd(k,i)t} + \epsilon_{kit}, \tag{4.1}$$

where k, i, and t index a cohort, a mine, and a quarter, respectively. t_k is cohort k's quarter of control change. y_{kit} is the injury rate in mine i in quarter t in cohort k. We measure injury rate as the number of injuries per 1,000 full-time equivalent workers. Following Charles et al. (2022), the number of full-time equivalent employees in a mine in a quarter is calculated as the number of hours worked divided by 500, which is the number of hours a full-time worker (40 hours per week) works in a quarter. μ_{ki} is a set of cohort×mine fixed effects. $\xi_{kj(i)t}$ is a set of cohort×commodity×year×quarter fixed effects. For mine i, $j(i)$ represents the commodity it produces. This set of fixed effects aims to control for the effects of demand shocks on injury rates (Charles et al., 2022). $\eta_{kd(k,i)t}$ aims to control for the change in injury rates before treatment. For each mine i in cohort k, we define $\Delta InjuryRate_{ki}$ as the difference between a mine's injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Within each cohort k, mine i is assigned to a decile of $\Delta InjuryRate$, denoted as $d(k,i)$. We term $\eta_{kd(k,i)t}$ as cohort×change decile×year×quarter fixed effects.

Beyond including the fixed effects above, the trends of outcomes in quarters

before treatment are not mechanically required to be similar between treated and control mines in each quarter; instead, we test whether the parallel assumption is satisfied before treatment based on the coefficients in pre-treatment quarters. The coefficients of interest are β_e. The estimated coefficients capture the dynamics of the relative injury rates between treated and control mines over time. The omitted category is $e = -1$, the quarter immediately before a control change event. β_e is interpreted as the average relative change in an outcome between treated and control mines across all cohorts during elapsed time e, relative to time -1. If outcomes in treated and control mines are on similar trends before control changes, then β_{-12}, β_{-11},..., β_{-3}, and β_{-2} would be small in magnitude and statistically insignificant. Conversely, our model would predict that β_0, β_1,..., β_{11}, and β_{12} are positive and statistically different from zero.

To measure the average treatment effect, we estimate the following regression:

$$y_{kit} = \beta \times Treated_{ki} \times \mathbb{1}(t - t_k > 0) + \mu_{ki} + \xi_{kj(i)t} + \eta_{kd(k,i)t} + \epsilon_{kit} \tag{4.2}$$

The parameter of interest, β, captures the average post-treatment change in injury rates among treated mines relative to that in control mines. Our model's prediction is that β is positive and statistically different from zero. Identification requires that the units follow parallel trends absent the treatment.

The first two rows of Table 1 report the means of injury rate and the natural logarithm of working intensity for treated and control mines before changes of control. We define working intensity as working hours per worker. The third and fourth rows report the means of the changes in injury rate and the natural logarithm of working intensity before acquisitions for treated and control mines, conditional on cohort×commodity and cohort×change decile fixed effects. For an

acquisition, the change in injury rate (the natural logarithm of working intensity) before treatment is the difference between a mine's injury rate (the natural logarithm of working intensity) in the quarter just before the acquisition and its average injury rate (the natural logarithm of working intensity) over the 11 preceding quarters. Although the levels of injury rates and working intensity are statistically different between treated and control mines, the changes in injury rate and the natural logarithm of working intensity before acquisitions are not statistically significant between treated and control mines.

5 Results

In this section, we present the findings of our empirical analysis. Our basic result that changes of control are associated with an increase in injury rates is presented in section 5.1. Section 5.2 explores the mechanisms at work. Additional analysis and robustness tests are discussed in section 5.4.

5.1 Baseline Analysis

To examine the effects of changes of control on target mines' injury rates, our baseline analysis estimates Equation 4.1. Estimated coefficients during the pre-treatment period are small and statistically insignificant, adding credibility to the maintained parallel trends assumption post-treatment (Figure 2 and Table 2). The trajectories of injury rates between treated and control mines only start to diverge after treatment. Injury rates of treated mines, on average, increase by 0.994 per 1,000 full-time employees relative to control mines after changes of control, and it is statistically significant at the 1% level. Given that the average injury rate

of treated mines before treatment is 8.914 per 1,000 full-time employees, the estimated average treatment effect is economically meaningful and represents an 11.1% increase. This result is consistent with Prediction 1 of our model.

5.2 Mechanism

In this section, we provide evidence for the proposed mechanisms through which changes of control affect injury rates.

5.2.1 Working Intensity

In the model in Section 2, one channel through which a change of control affects workplace safety is that the new owner needs the worker to exert more effort because the acquisition disrupts the owner-specific human capital. If a worker works longer hours after a change of control, then the worker is more likely to experience occupational injuries due to fatigue or inattention (Dembe et al., 2005). This could contribute to why changes of control lead to increased injury rates per hour of labor in target mines.

Empirically, we use working intensity, measured as working hours per employee, as a proxy for worker effort. We estimate Equation 4.1 using the natural logarithm of working intensity as the dependent variable and report the results in Figure 3 and Table 3. The results show that the estimated coefficients in pre-treatment periods are small and statistically insignificant, and the trajectories of the natural logarithm of working intensity between treated and control mines only start to diverge after treatment. During the quarter of treatment, working intensity dips, possibly due to the disruptions caused by acquisitions. After the first post-treatment quarter, working intensity starts to increase. The working inten-

sity of treated mines, on average, increases by 5% more relative to control mines after changes of control, and this is statistically significant at the 1% level. Given that the working intensity of an average employee in treated mines before treatment is 447 hours per quarter (34.3 hours per week), the estimated effect suggests that an employee, on average, works 22 hours more in a quarter (1.7 hours more per week) after a change of control.

5.2.2 Owners vs. Managers

The literature has shown that management practices play a critical role in firm performance (see, for example, Bloom et al., 2013, 2014, 2020). This raises an important question: if managers also change following acquisitions, are the estimated effects in Table 2 driven by changes in ownership or by changes in management? To answer this question, we use MSHA's data and track the changes of managers in a mine. For the set of treated mines, we further examine whether there are heterogeneous effects for the following two cases after an acquisition: (1) both the owner and manager of a mine change, and (2) only the owner of a mine changes while the manager remains the same. For the set of control mines, we also differentiate whether the manager of a mine changes during the acquisition quarter.

Table 4 reports the results. In column (1), the estimation sample includes treated mines that experience changes in both owners and managers and control mines that do not experience changes in managers during the acquisition quarters. In column (2), the estimation sample includes treated mines in which managers do not change and control mines that do not experience changes in managers during the acquisition quarters. In column (3), the estimation sample only includes

control mines. The *Treated* dummy variable in column (3) is equal to one if control mines experience changes in managers. In each subsample, the dependent variable is injury rate scaled by the pre-treatment sample average injury rate of mines with *Treated* dummy equal to one.

The estimate in column (1) shows that, relative to control mines that do not experience changes in managers, the injury rates of treated mines that experience changes in both owners and managers increase 12.9% relative to the pre-treatment sample mean. The estimate is statistically significant at the 1% level. When only the owners of treated mines change, the estimated effect is smaller, and the injury rates increase 7.8% relative to the pre-treatment sample mean, as shown in column (2). The estimate is statistically significant at the 5% level. Column (3) reports the estimated effect of changing managers, conditional on no changes of control. The estimated effect is much smaller and statistically insignificant. Overall, the results in Table 4 suggest that it is the changes in owners, rather than just the changes in managers, that drive the baseline results.

5.3 Cross-Sectional Heterogeneity

In this section, we examine several empirical predictions of the model, leveraging the cross-sectional heterogeneity in our sample of mines.

5.3.1 Owner Workplace Safety Practices

Prediction 2 of the model in Section 2 predicts that the change in injury rate after a change of control depends on the difference in workplace safety practices between the new and the previous owners. The change in the injury rate after a change of control is expected to be larger if the new owner has worse practices rel-

ative to the previous owner. To proxy for an owner's workplace safety practices, we follow Bertrand and Schoar (2003) and estimate owner fixed effects based on the pre-acquisition injury rate data. A higher estimated owner fixed effect would suggest that the owner has worse workplace safety practices. Specifically, for each cohort of acquisitions in quarter t, we run the following regression using all the data before quarter t:

$$y_{ifcm} = X'_{im}\beta + \eta_f + \psi_{cm} + \epsilon_{ifcm}, \tag{5.1}$$

where i, f, m index mine, owner, and time, respectively, and $m < t$. η_f and ψ_{cm} are owner and commuting zone$\times$year$\times$quarter fixed effects, respectively. y_{ifcm} is the injury rate of mine i belonging to owner f in commuting zone c in time m. X_{im} is a vector of mine characteristics, including the natural logarithm of employment, the natural logarithm of working hours per employee, and a dummy variable indicating whether a mine is a coal mine.

For each cohort t, we then extract the estimated owner fixed effects, $\widehat{\eta_f}$, which expresses f's average residual contribution to injury rates above other factors. For each treated mine, we calculate the difference in the owner fixed effects of new and old owners as $\Delta\eta = \widehat{\eta_{f,new}} - \widehat{\eta_{f,old}}$ and term it as the change in owner-specific occupational injury record. $\Delta\eta$ can be interpreted as the change in owner workplace safety practices after changes of control. When $\Delta\eta > (<)0$, it means the new owner brings in worse (better) practices relative to the previous owner.

Within each cohort of acquisitions, we sort treated mines into quartiles based on $\Delta\eta$ and estimate Equation 4.2 for each subsample. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines. Table 5 reports the estimated results. The estimates suggest that if new owners have

worse workplace safety practices than previous owners, treated mines experience a higher increase in the injury rates after changes of control. Specifically, among treated mines with bottom-quartile $\Delta\eta$, the estimated effect relative to the pre-treatment sample mean is 5%, and is not statistically significant. Among treated mines with top-quartile $\Delta\eta$, injury rates increase significantly after changes of control. The average effect of changes of control on injury rates represents a 77% increase relative to the pre-treatment sample mean. The difference between the estimates in the two subsamples is statistically significant at the 1% level. Overall, the results are consistent with Prediction 2 of the model and also suggest that changes in owners' workplace safety practices could play an important role in affecting injury rates.

5.3.2 Employer Market Power

Another explanation for the increased injury rates among treated mines after changes of control is that employers' market power increases in a local labor market, and workers' outside option value decreases. Prediction 3 of our model in Section 2 predicts that the increase in injury rate is expected to be higher if workers' outside option ($\overline{U_1}$) decreases after an acquisition. We test this prediction in this subsection.

We define a local mining labor market as a commuting zone and measure mining employers' monopsony power in a commuting zone as the employment-based Herfindahl-Hirschman index (HHI) across mine owners (Azar et al., 2020). To evaluate the importance of acquisitions made by an owner in a commuting zone, we follow Arnold (2021) and calculate the *predicted* change in commuting zone-level employment HHI ($\widehat{\Delta HHI}$) due to acquisitions and then examine how

the estimated effects on injury rates vary with it. Some acquisitions are between mines in different commuting zones; their $\widehat{\Delta HHI}$ will be zero. Others share a commuting zone and generate $\widehat{\Delta HHI} > 0$.

We next discuss how to measure $\widehat{\Delta HHI}$. For each owner that made acquisitions in a commuting zone in quarter t, we assume the events happened in quarter $t-1$ and there were no other acquisitions, and then calculate the pseudo employment HHI at the commuting zone level in quarter $t-1$. $\widehat{\Delta HHI}$ is then defined as the difference between the pseudo employment HHI and the actual employment HHI in the commuting zone in quarter $t-1$.

Within each cohort of acquisitions, we sort the treated mines into quartiles of $\widehat{\Delta HHI}$ and estimate Equation 4.2 for each subsample. The average $\widehat{\Delta HHI}$ of mines with $\widehat{\Delta HHI}$ in the bottom quartile is close to zero (-0.0007) while the average $\widehat{\Delta HHI}$ of mines with $\widehat{\Delta HHI}$ in the top quartile is much larger (0.043). The top quartile's average implied a 430-point increase in HHI is far above the 100-point increase considered significant under the FTC and DOJ Merger Guidelines. We report the estimated effects in Table 6. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines.

The estimated effects monotonically increase with $\widehat{\Delta HHI}$, evidence supporting the employer market power mechanism. Among treated mines with $\widehat{\Delta HHI}$ falling in the top quartile, the results show that the average injury rates in treated mines increase 23% relative to the pre-treatment sample mean. The estimate is statistically significant at the 1% level. Among treated mines with $\widehat{\Delta HHI}$ falling in the bottom quartile, the estimated coefficient on $Treated \times Post$ is much smaller (a 5% increase relative to the sample mean) and is not statistically significant. The difference between the two estimates is statistically significant at the 1% level.

5.3.3 Private Equity Acquisitions

Prediction 4 of the model in Section 2 predicts that the injury rate in the target mine could decrease after a change of control if the acquirer's corporate governance is sufficiently better than the previous owner. It is generally accepted that private equity (PE) firms have better corporate governance than traditional corporations (Jensen, 1989; Kaplan and Stromberg, 2009). Therefore, under private equity ownership, the incentives of manager and shareholder values are more aligned, and the injury rates of target mines may decrease after PE acquisitions. In this subsection, we examine the effects of acquisitions by PE firms and by non-PE acquirers.

To compile a list of PE firms, we extract the acquirers from the SDC Platinum with the SIC code equal to 6799. We perform a fuzzy name match procedure to match mine owners to the list of PE firms. In our sample, 70 mines experienced PE acquisitions. We estimate the effects of PE and non-PE acquisitions on the injury rates of target mines based on Equation 4.2. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines. We report the results in Table 7. Column (1) reports the estimated effect of PE acquisitions. The result shows that target mines' injury rates decrease by 16.3% relative to the pre-treatment sample mean, but it is not statistically significant at the conventional level with a p-value of 0.189. Column (2) reports the estimated effect of non-PE acquisitions and shows that target mines' injury rates increase by 11.6% relative to the pre-treatment sample mean of target mines, and it is statistically significant at the 1% level. Overall, these estimates are consistent with the model prediction. The estimate in column (1) is also consistent with the evidence in Cohn, Nestoriak, and Wardlaw (2021), which shows that PE acquisitions improve workplace safety

using the Bureau of Labor Statistics (BLS) Survey of Occupational Injuries and Illnesses (SOII).

5.4 Robustness Checks

In this section we explore several robustness checks of our main results.

5.4.1 Traumatic vs. Non-Traumatic Injuries

A concern with using the total injury rate is that it not only reflects workplace safety in a mine but may also be driven by workers' willingness to report. To mitigate the concern that our results are driven by reporting biases, we further examine the effects of changes of control on traumatic injury rates and non-traumatic injury rates separately. Previous studies argue that traumatic injuries are the least subject to reporting biases and more reflect workplace safety. To classify injuries, we follow the definition of traumatic in Morantz (2013).[11] Both traumatic and non-traumatic injury rates increase in treated mines after changes of control relative to control mines. Columns (1) and (2) of Table 8 report their average treatment effects, respectively. In particular, traumatic injury rates increase 0.299 per 1,000 full-time equivalent employees, representing an 11.1% relative to the pre-treatment sample mean of treated mines. Non-traumatic injury rates increase 0.695 per 1,000 full-time equivalent employees, representing an 11.2% relative to the sample mean of treated mines before treatment.

[11]Specifically, an injury is defined to be traumatic if it is a fatality or its nature meets the following criteria: "Amputation or Enucleation", "Fracture,Chip", "Dislocation", "Cut,Lacer,Punct-Open Wound", "Burn or Scald (Heat)", "Crushing", "Electric Burn-Cntact Burn", "Burn,Chemicl-Fume,Compoun", "Laser Burn", "Dust in Eyes". All the other injuries are classified are non-traumatic.

5.4.2 Safety Violations

We further use data on MSHA inspections to examine the effects of control changes on the number of violations of safety standards during an inspection. We focus on regular inspections, rather than complaint-driven inspections, by MSHA. Underground mines are inspected four times per year, and surface mines are inspected twice per year. In our sample, 94% of mines are surface mines.

We use the baseline sample and merge it with data on safety violations. We use the number of violations during an inspection in a quarter as the outcome variable and estimate the effect using Poisson regression. Column (3) of Table 8 shows that the number of violations during an inspection of treated mines on average increases 8.6% relative to control mines after control changes, and it is statistically significant at the 1% level.

5.4.3 Synthetic Difference-in-Differences

To assess the robustness of the empirical strategy, we also estimate the baseline effects using the synthetic difference-in-differences (synthetic DiD) estimator of Arkhangelsky et al. (2021). This estimator requires balanced panel data. Specifically, for each cohort of acquisitions, we estimate using the synthetic DiD estimator and then aggregate estimates across cohorts weighted by the number of target mines in each cohort (Callaway and SantAnna, 2021). We compute standard errors based on bootstraps with 300 resamplings. We report the result in column (4) of Table 8. The estimated average treatment effect is 0.897, with a standard error of 0.025. The magnitude of the estimated effect is similar to the baseline result.

5.4.4 Estimates Based on Matched Treated and Control Mines

To mitigate the concern that the probability of being targeted in an acquisition is endogenous, we estimate the effect of changes of control on the injury rates of target mines based on a matched sample of treated and control mines. Specifically, for each cohort of acquisitions, we match a treated mine with a control mine based on the estimated propensity scores. The propensity score is estimated by predicting the dummy of a treated mine using a linear probability model with a quadratic natural logarithm of employment, a quadratic natural logarithm of hours per employee, a quadratic natural logarithm of mine age, the average change in the natural logarithm of employment, owner-specific occupational injury record estimated from Equation 5.1, a dummy variable indicating whether a mine belongs to an owner that controls multiple mines, a dummy variable indicating whether a mine employs at least one contractor, and the injury rate as well as cohort×commuting zone and cohort×commodity fixed effects. For acquisitions in cohort t, the average change in the natural logarithm of employment is defined as the difference between a mine's natural logarithm of employment in quarter $t-1$ and its average natural logarithm of employment over the 11 preceding quarters. Except for the average change in the natural logarithm of employment before treatment, all independent variables in the prediction regression are measured in the quarter just before treatment. We report the prediction regression results in Table A.2. In each cohort of acquisitions, we match a treated mine with one control mine with the closest propensity score and form a matched pair. We include cohort×matched pair×mine and cohort×matched pair×year×quarter fixed effects in the estimation. The estimated effect based on the matched sample is reported in column (5) of Table 8. The estimated effect is similar to the baseline result.

5.4.5 Control for Local Shocks

One concern with the baseline results is that local economic conditions may affect the injury rate in a mine. To mitigate this concern, we further control for local shocks by including the cohort×commuting zone×year×quarter fixed effects in the baseline analysis. We report the average treatment effect in column (6) of Table 8. Our baseline results are robust.

5.4.6 Cleaner Control Mines

In the baseline results, we assume the Stable Unit Treatment Value Assumption (SUTVA) holds. However, this assumption could be violated if there are spillovers from target mines to control mines in the same commuting zone. To mitigate this concern, we use a set of cleaner control mines in this analysis. Specifically, we only use control mines in commuting zones where there were no mines involved in any acquisition within the estimation window. The cost of focusing on this set of cleaner control mines is that there are only 2,180 (12% of) control mines that satisfy this condition. Column (7) of Table 8 reports the estimated effect using this alternative set of control mines. Relative to control mines, the injury rates in target mines on average increase by 1.225 per 1,000 full-time employees. The estimated effect is similar to the baseline result.

5.4.7 Excluding Commodities Competing in Local Product Markets

To further mitigate the concern that our results could be driven by changes in the product market power of mines after acquisitions, we exclude mines that

produce commodities that compete in local product markets.[12] Around 27% of mines in our sample produce commodities that compete in national or international product markets. We re-estimate Equation 4.2 and report the estimated effect in column (8) of Table 8. Our result is robust.

6 Conclusion

In this paper, we use the U.S. mining sector as an empirical setting and present the first evidence on the effects of changes of control on workplace safety. We find that changes of control lead to a significant increase in injury rates on average.

We next perform tests to explore mechanisms. First, we find that working intensity, defined as working hours per employee, increases in target mines after changes of control. The increased working hours per employee could lead workers to experience more occupational injuries due to fatigue or inattention (Dembe et al., 2005). This could contribute to why changes of control lead to increased injury rates in target mines. Second, we show that the change in control, not the change in management, is what matters. This suggests that ownership plays a more critical role than management in sustaining implicit contracts, or that managers primarily act in accordance with owners' preferences.

We further conduct cross-sectional heterogeneity tests. First, we find that the estimated effect on injury rates is larger when new owners have worse workplace safety practices than previous owners, which suggests that the changes in the owner's workplace safety practices after changes of control play an important role in workplace safety. Second, we find that the estimated effects on injury rates

[12] We thank Chris Marks for sharing the list of commodities that compete in local product markets or not.

are larger when new owners are expected to enjoy greater monopsony power in a local labor market. This evidence suggests that the potential increase in monopsony power after changes of control plays an important role in affecting workplace safety. Third, we further find that private equity (PE) acquirers behave differently from non-PE acquirers. In contrast to other types of acquirers, injury rates tend to decline following private equity buyouts.

Our paper has important policy implications for evaluating the potential labor market effects of mergers and acquisitions (M&As). Specifically, our evidence lends some support to the recently proposed changes to the premerger notification process, which would require merging parties to report their prior five-year record of violations of employees' occupational health and safety rights. M&As where changes of control towards an owner with a worse safety record have much more negative effects on workplace health and safety than M&As where the new owner has a similar or better record. While this highlights an important dimension of the employment relationship, it is only one aspect among many that warrants consideration.

A Proofs for Result 1 and Result 2 in Section 2

A.1 Proof of Result 1

We first prove that p^*_{NC} decreases in λ, and other claims are proved using a similar approach. The first order condition (F.O.C) for problem (2.6) is:

$$F(p^*_{NC};\lambda,\overline{U},L,\rho,\overline{p}) \equiv 2(1-\lambda)\frac{\rho\overline{p}}{L}(1/p^*_{NC})^3(Y-p^*_{NC}L-\overline{U})-L[1-(1-\lambda)\frac{\rho\overline{p}}{L}(1/p^*_{NC})^2]=0 \tag{A.1}$$

By the implicit function theorem, $\frac{\partial p^*_{NC}}{\partial\lambda}=-(\frac{\partial F}{\partial\lambda})/(\frac{\partial F}{\partial p^*_{NC}})$.

Based on Equation A.1, we have:

$$\frac{\partial F}{\partial p^*_{NC}}=-2(1-\lambda)\frac{\rho\overline{p}}{L}[3(1/p^*_{NC})^4(Y-p^*_{NC}L-\overline{U})+L(1/p^*_{NC})^3]-2(1-\lambda)\rho\overline{p}(1/p^*_{NC})^3<0$$

$$\frac{\partial F}{\partial\lambda}=-2\frac{\rho\overline{p}}{L}(1/p^*_{NC})^3(Y-p^*_{NC}L-\overline{U})-\rho\overline{p}(1/p^*_{NC})^2<0$$

Therefore,

$$\frac{\partial p^*_{NC}}{\partial\lambda}<0.$$

Next, we show how p^*_{NC} varies with $\overline{U}$, L, ρ, and $\overline{p}$. Based on Equation A.1, we have:

$$\frac{\partial F}{\partial\overline{U}}=-2(1-\lambda)\frac{\rho\overline{p}}{L}(1/p^*_{NC})^3<0$$

$$\frac{\partial F}{\partial L}=-2(1-\lambda)\rho\overline{p}(1/p^*_{NC})^3[(1/L)^2(Y-p^*_{NC}L-\overline{U})+\frac{p^*_{NC}}{L})]-1<0$$

$$\frac{\partial F}{\partial \rho} = 2(1-\lambda)\frac{\overline{p}}{L}(1/p^*_{NC})^3(Y - p^*_{NC}L - \overline{U}) + (1-\lambda)\overline{p}(1/p^*_{NC})^2 > 0$$

$$\frac{\partial F}{\partial \overline{p}} = 2(1-\lambda)\frac{\rho}{L}(1/p^*_{NC})^3(Y - p^*_{NC}L - \overline{U}) + (1-\lambda)\rho(1/p^*_{NC})^2 > 0$$

Therefore, by the implicit function theorem, we have:

$$\frac{\partial p^*_{NC}}{\partial \overline{U}} < 0, \frac{\partial p^*_{NC}}{\partial L} < 0, \frac{\partial p^*_{NC}}{\partial \rho} > 0 \text{ and } \frac{\partial p^*_{NC}}{\partial \overline{p}} > 0.$$

A.2 Proof of Result 2

This follows from the proof of Result 1 after setting $\rho = 1$ and observing that problem (2.6) is identical to problem (2.13) when $\rho = 1$.

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Main Figures and Tables

Figure 1: Trend of Changes of Control

This figure reports the trend of the percentage of mines that experience changes of control in each quarter between the first quarter of 1986 and the second quarter of 2020. In each quarter *t*, each dot represents the sum of the number of changes of control between quarters t-3 and t divided by the average number of mines with strictly positive employment between quarters t-3 and t.

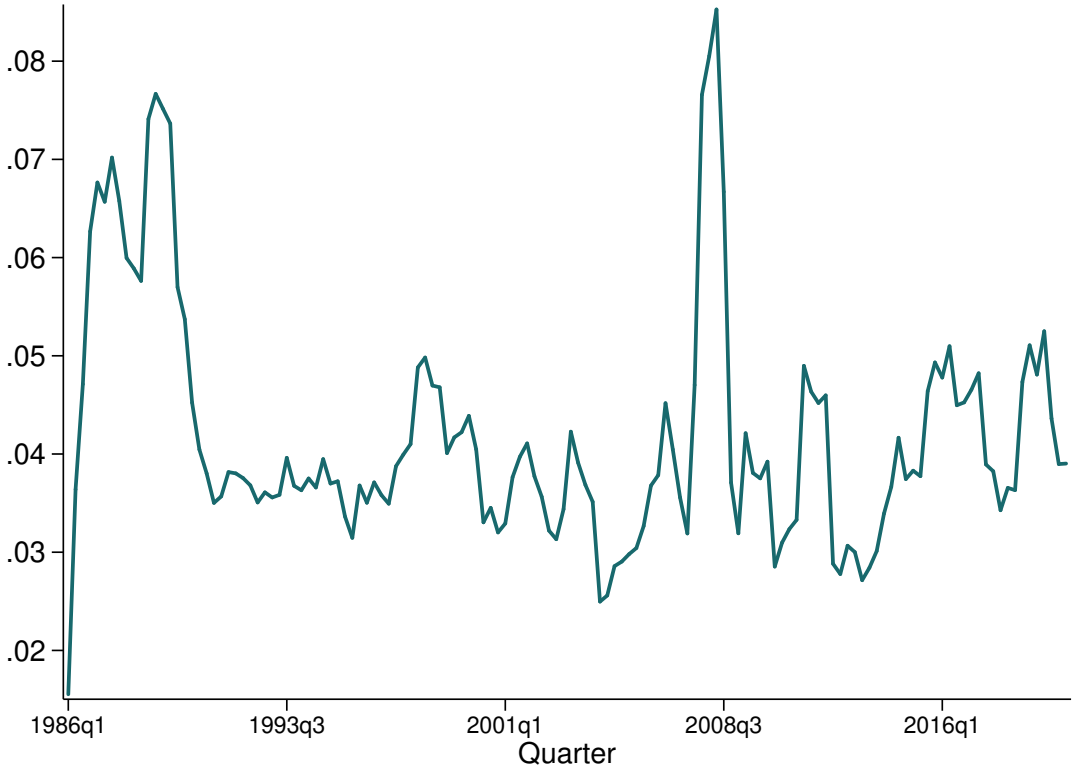


Figure 2: Effects of Changes of Control on Injury Rates of Target Mines

This figure reports the dynamic treatment effects of changes of control on injury rates of target mines. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

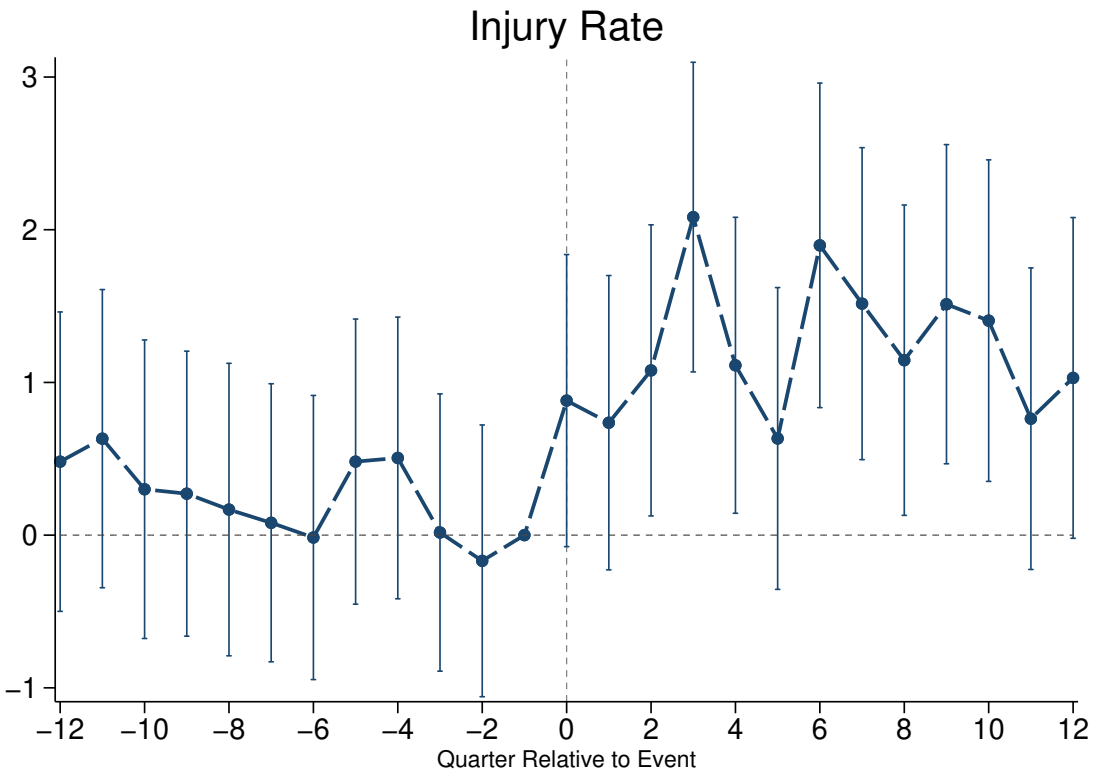


Figure 3: Effects of Changes of Control on Working Intensity of Target Mines

This figure reports the dynamic treatment effects of changes of control on the natural logarithm of the working intensity of target mines. Working intensity is defined as working hours per employee. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

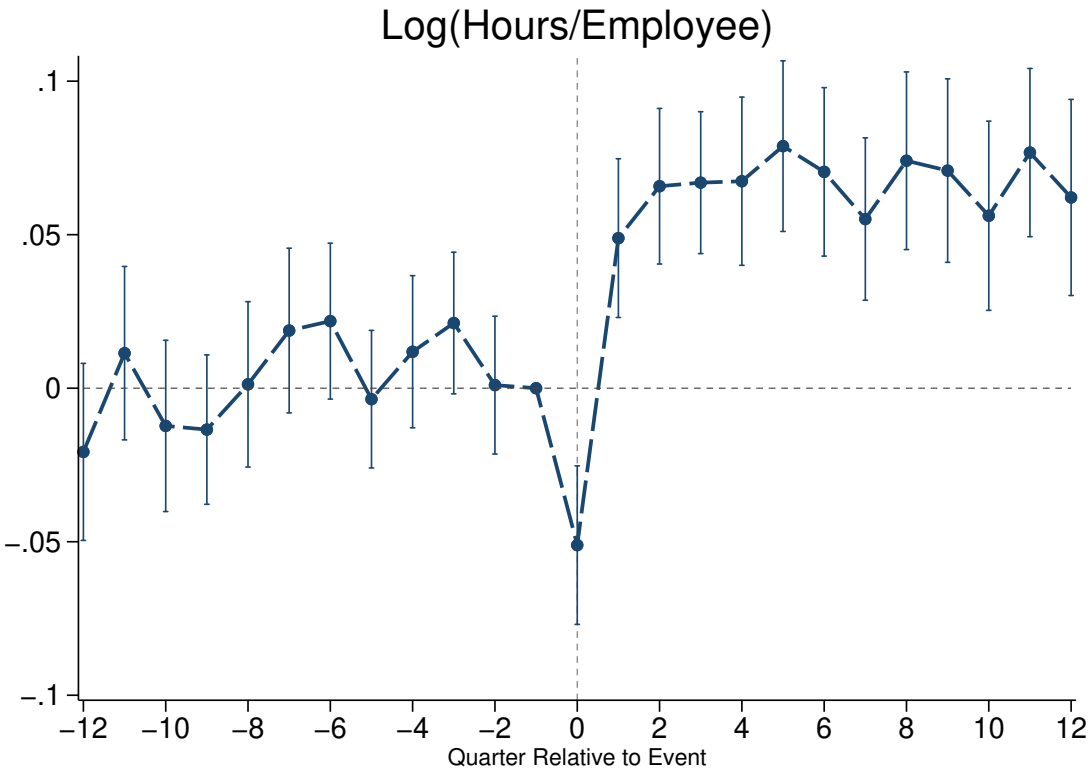


Table 1
Summary Statistics of Treated and Control Mines

The first two rows of this table report the means of injury rate (*Injury Rate*) and the natural logarithm of working intensity (*Log(Working Intensity)*) in treated and control mines before changes of control. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent workers. *Working Intensity* is defined as working hours per employee. The third and fourth rows report the means of Δ *Injury Rate* and Δ*Log(Working Intensity)*, conditional on Cohort$\times$Commodity and Cohort$\times$Change Decile fixed effects. For acquisitions in cohort t, Δ means the difference between the value of a variable in $t-1$ and the average value of a variable between $t-12$ and $t-2$. *Change Decile* is defined as the decile of Δ *Injury Rate* in each cohort of acquisitions. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Treated	**Control**	**p-value of (1)-(2)**
	(1)	(2)	(3)
Injury Rate	8.914	5.226	<0.01
Log(Working Intensity)	5.886	5.531	<0.01
Δ Injury Rate	0.032	0.049	0.944
ΔLog(Working Intensity)	-0.022	-0.027	0.649

Table 2
Effects of Changes of Control on Injury Rates of Target Mines

This table reports the baseline results on the estimated effects of changes of control on the injury rates of target mines. Columns (1) and (2) report the average and dynamic treatment effect, respectively. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Treated×Post	0.994***	
	[0.198]	
Treated×Post(-12)		0.481
		[0.500]
Treated×Post(-11)		0.632
		[0.498]
Treated×Post(-10)		0.301
		[0.499]
Treated×Post(-9)		0.272
		[0.476]
Treated×Post(-8)		0.167
		[0.489]
Treated×Post(-7)		0.081
		[0.465]
Treated×Post(-6)		-0.016
		[0.475]
Treated×Post(-5)		0.481
		[0.476]
Treated×Post(-4)		0.505
		[0.471]
Treated×Post(-3)		0.017
		[0.463]
Treated×Post(-2)		-0.168
		[0.454]
Treated×Post(0)		0.881*
		[0.488]
Treated×Post(+1)		0.736
		[0.492]
Treated×Post(+2)		1.079**
		[0.486]
Treated×Post(+3)		2.083***
		[0.517]
Treated×Post(+4)		1.112**
		[0.494]
Treated×Post(+5)		0.633
		[0.504]
Treated×Post(+6)		1.898***
		[0.542]
Treated×Post(+7)		1.516***
		[0.521]
Treated×Post(+8)		1.146**
		[0.518]
Treated×Post(+9)		1.512***
		[0.533]
Treated×Post(+10)		1.405***
		[0.537]
Treated×Post(+11)		0.763
		[0.504]
Treated×Post(+12)		1.030*
		[0.536]
Cohort×Mine FEs	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓
Adjusted R^2	0.166	0.166
N	10,906,650	10,906,650

Table 3
Effects of Changes of Control on Working Intensity of Target Mines

This table reports the estimated effects of changes of control on the natural logarithm of working intensity of target mines. Working intensity is defined as working hours per employee. Columns (1) and (2) report the average and dynamic treatment effect, respectively. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Treated×Post	0.052***	
	[0.008]	
Treated×Post(-12)		-0.021
		[0.015]
Treated×Post(-11)		0.011
		[0.014]
Treated×Post(-10)		-0.012
		[0.014]
Treated×Post(-9)		-0.013
		[0.012]
Treated×Post(-8)		0.001
		[0.014]
Treated×Post(-7)		0.019
		[0.014]
Treated×Post(-6)		0.022*
		[0.013]
Treated×Post(-5)		-0.004
		[0.011]
Treated×Post(-4)		0.012
		[0.013]
Treated×Post(-3)		0.021*
		[0.012]
Treated×Post(-2)		0.001
		[0.011]
Treated×Post(0)		-0.051***
		[0.013]
Treated×Post(+1)		0.049***
		[0.013]
Treated×Post(+2)		0.066***
		[0.013]
Treated×Post(+3)		0.067***
		[0.012]
Treated×Post(+4)		0.067***
		[0.014]
Treated×Post(+5)		0.079***
		[0.014]
Treated×Post(+6)		0.070***
		[0.014]
Treated×Post(+7)		0.055***
		[0.013]
Treated×Post(+8)		0.074***
		[0.015]
Treated×Post(+9)		0.071***
		[0.015]
Treated×Post(+10)		0.056***
		[0.016]
Treated×Post(+11)		0.077***
		[0.014]
Treated×Post(+12)		0.062***
		[0.016]
Cohort×Mine FEs	✓	✓
Cohort×Commodity×Time FEs	✓	✓
Cohort×Change Decile×Time FEs	✓	✓
Adjusted R^2	0.680	0.680
N	10,906,650	10,906,650

Table 4
Changes in Owners vs. Changes in Managers

This table reports the estimated effects of changes in owners vs. changes in managers in target mines on the injury rates of target mines. In column (1), the estimation sample includes treated mines that experience changes in both owners and managers and control mines that do not experience changes in managers during the acquisition quarters. In column (2), the estimation sample includes treated mines in which managers do not change and control mines that do not experience changes in managers during the acquisition quarters. In column (3), the estimation sample only includes control mines. The *Treated* dummy variable in column (3) is equal to one if control mines experience changes in managers. In each subsample, the dependent variable is injury rate scaled by the pre-treatment sample average of mines with *Treated* dummy equal to one. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Both Owners and Managers Change	Only Owners Change	Only Managers Change
	(1)	(2)	(3)
Treated×Post	0.129***	0.078**	-0.035
	[0.029]	[0.032]	[0.116]
Cohort×Mine FEs	✓	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓	✓
Adjusted R^2	0.166	0.165	0.165
N	10,846,267	10,830,360	10,786,100

Table 5
Changes in Owner-Specific Occupational Injury Record

This table reports the heterogeneous estimated effects of changes of control on the injury rates of target mines by the changes in the owner-specific occupational injury record. For an owner, the owner-specific occupational injury record is the owner fixed effect estimated from Equation 5.1. Within each cohort of acquisitions, we sort treated mines into quartiles (Q1, Q2, Q3, and Q4) based on the changes in the owner-specific occupational injury record. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Treated×Post	0.051	0.106	0.246***	0.773***
	[0.049]	[0.071]	[0.091]	[0.148]
Cohort×Mine FEs	✓	✓	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓	✓	✓
Adjusted R^2	0.165	0.165	0.165	0.165
N	10,797,157	10,794,866	10,794,017	10,792,706

Table 6
Changes in Local Monopsony Power

This table reports the heterogeneous estimated effects of changes of control on the injury rates of target mines by the predicted changes in employment-based HHI in a commuting zone due to acquisitions ($\widehat{\Delta HHI}$). Within each cohort of acquisitions, we split the treated mines into quartiles (Q1, Q2, Q3, and Q4) of $\widehat{\Delta HHI}$. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Treated×Post	0.049	0.079	0.118**	0.233***
	[0.031]	[0.116]	[0.051]	[0.040]
Cohort×Mine FEs	✓	✓	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓	✓	✓
Adjusted R^2	0.165	0.165	0.165	0.165
N	10,858,045	10,792,648	10,801,148	10,816,436

Table 7
PE vs. non-PE Acquirers

This table reports the heterogeneous estimated effects of changes of control on the injury rates of target mines for PE and non-PE acquisitions in columns (1) and (2), respectively. In each subsample, the estimate is relative to the pre-treatment mean of injury rates of treated mines. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mines injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	PE Acquisitions	Non-PE Acquisitions
	(1)	(2)
Treated×Post	-0.163	0.116***
	[0.124]	[0.023]
Cohort×Mine FEs	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓
Adjusted R^2	0.165	0.166
N	10,788,869	10,904,990

Table 8
Robustness Checks

This table reports additional robustness checks on the estimated effects of changes of control on the injury rates of target mines. Columns (1) and (2) report the estimated effects on traumatic and non-traumatic injury rates, respectively. *Traumatic (Non-Traumatic) Injury Rate* is defined as the number of traumatic (non-traumatic) injuries per 1,000 full-time equivalent employees. Column (3) reports the estimated effect on the number of violations of safety regulations during regular inspections, estimated using a Poisson regression. Column (4) reports the estimated effect using the synthetic difference-in-differences (Synthetic DiD) estimator in Arkhangelsky et al. (2021). Column (5) reports the estimated effect based on propensity score matching. Column (6) controls for demand shocks by including Cohort×Commuting Zone×Year×Quarter fixed effects. Column (7) reports the estimated effect using cleaner control mines in commuting zones where there were no mines involved in any acquisition within the estimation window. Column (8) excludes mines that produce commodities that compete in local product markets. For acquisitions in cohort t, *Change Decile* is defined as the decile of the difference between a mine's injury rate in quarter $t-1$ and its average injury rate over the 11 preceding quarters. Standard errors in brackets are clustered at the mine level except for column (4). In column (4), the standard error is calculated based on bootstraps. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Traumatic Injury (1)	Non-Traumatic Injury (2)	Number of Violations (3)	Synthetic DiD (4)	Propensity Score Matching (5)	Control Local Shocks (6)	Cleaner Controls (7)	Exclude Local Product Market (8)
Treated×Post	0.299***	0.695***	0.086***	0.897***	0.982**	1.047***	1.225***	1.202***
	[0.074]	[0.160]	[0.022]	[0.025]	[0.425]	[0.229]	[0.286]	[0.465]
Cohort×Mine FEs	✓	✓	✓			✓	✓	✓
Cohort×Commodity×Year×Quarter FEs	✓	✓	✓			✓	✓	✓
Cohort×Change Decile×Year×Quarter FEs	✓	✓	✓			✓	✓	✓
Cohort×Matched Pair×Year×Quarter FEs					✓			
Cohort×Matched Pair×Year×Quarter FEs					✓			
Cohort×CZ×Year×Quarter FEs						✓		
Adjusted R^2	0.101	0.139			0.101	0.166	0.212	0.209
N	10,906,650	10,906,650	3,830,426	5,225,375	225,285	10,906,650	632,561	1,662,685

Appendix Figures and Tables

Table A.1 Variable Definition

Variable	Definition
Injury Rate	The number of injuries per 1,000 full-time equivalent employees. Following Charles et al. (2022), the number of full-time equivalent workers in a mine in a quarter is calculated as the number of hours worked divided by 500, which is the number of hours a full-time worker (40 hours per week) works in a quarter.
Traumatic Injury Rate	The number of traumatic injuries per 1,000 full-time equivalent employees.
Working Intensity	Working hours per employee.
Log(Employment)	Natural logarithm of employment.
Age	Age of a mine.
Change in Log(Employment)	For acquisitions in cohort t, it is defined as the difference between a mines natural logarithm of employment in quarter $t-1$ and its average natural logarithm of employment over the 11 preceding quarters.
Owner-Specific Occupational Injury Record	Owner fixed effects estimated based on Equation 5.1.
Owner Controls Multiple Mines	A dummy variable indicating whether a mine belongs to an owner that controls multiple mines.
Employ At Least One Contractor	A dummy variable indicating whether a mine employs at least one independent contractor.
Treated	A dummy variable equal to one if a mine is acquired in a quarter, and equal to zero if a mine did not experience any change of control and their controllers did not make any acquisitions during the whole sample period between 1983Q1 and 2023Q2.
Post	For acquisition in time t, it is a dummy variable that equals one for the quarters including and after time t, and zero otherwise.
$Post(-\tau)$	For acquisition in time t, it is a dummy variable equal to one for the τth quarter before time t.
$Post(+\tau)$	For acquisition in time t, it is a dummy variable equal to one for the τth quarter after time t.

Table A.2
Estimating Propensity Scores

This table reports the regression results that predict propensity scores. The definitions of all variables are in the Appendix Table A.1. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	(1)
Log(Employment)	-0.001197
	[0.000740]
Square of Log(Employment)	0.001118***
	[0.000242]
Log(Working Intensity)	-0.000154
	[0.000469]
Square of Log(Working Intensity)	0.000125**
	[0.000059]
Log(Age)	0.000240
	[0.000641]
Square of Log(Age)	-0.000004
	[0.000096]
Change in Log(Employment)	-0.003704***
	[0.000532]
Owner-Specific Occupational Injury Record	0.001958***
	[0.000298]
Owner Controls Multiple Mines	0.001467***
	[0.000449]
Employ At Least One Contractor	0.005034***
	[0.000710]
Injury Rate	-0.000009
	[0.000007]
Cohort×CZ FEs	✓
Cohort×Commodity FEs	✓
Adjusted R^2	0.148
N	519,871