M&A and Workplace Safety: Evidence from 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 changes of corporate control on workplace safety. Compared to mines that were never involved in any acquisition in the sample period, injury rates increase in mines that underwent changes of control. The potential mechanisms include increased working hours per employee, shifts in controllers' workplace safety practices, and increased employer labor market power. Overall, our evidence suggests that mergers and acquisitions in the mining sector have significant impacts on workplace safety. JEL Codes: G34, J24, J28.

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

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I. 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 of injury-related incidents 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.

[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.

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, the FTC and DOJ did not adopt the proposal at this time. However, they left the door open to revisiting the idea in the future if stronger evidence emerges to support the theory.[4] This paper helps fill this gap in the literature with relevant evidence on the average relationship between a new controller's past workplace safety record and the effect on workplace safety conditional on M&A.[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. Regarding other job-related risks, regulations exist to enforce minimum safety standards and to impose penalties on firms that violate these standards. However, 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

[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).

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 the takeover premia. For instance, a corporate raider might make short-term gains by firing employees once their investment in firm-specific human capital is sunk.[6] Building on this argument, we develop a simple model that predicts a reduction in workplace safety in firms undergoing a change of control. We proceed to test this hypothesis in the U.S. mining sector.

For three main reasons, the U.S. mining industry makes a compelling empirical laboratory to build new evidence on how changes in control affect workplace safety. 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 global and competitive. This implies that mergers are unlikely to affect producers' product-market power and thus any effects on workplace safety is driven by other factors than changes in product-market power.

To estimate the effects of changes of control on workplace safety, we use a 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

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

never involved in any acquisitions in the sample period (control mines) for 12 quarters around acquisition events. Our main finding is in full accord with the implicit contract theory. Specifically, changes of control lead to an estimated 15% increase in injury rates per labor hour of treated mines relative to control mines. The baseline result is robust to different types of injuries, an alternative set of control mines, and an alternative estimation method (Arkhangelsky et al., 2021). The estimated effect on the number of cited regulatory violations is similar: the number of reported violations significantly increases in the treated as compared with the control mines. As a placebo test, we examine the effect of management (or operator) changes on injury rates conditional on no change of control. The estimated effect is much smaller and not statistically different from zero, suggesting that the ultimate owner plays a critical role in workplace safety beyond management.

Five additional tests explore potential mechanisms and further strengthen the interpretation of breaching implicit contracts. First, keeping an (inefficient) mine open may make sense as part of an implicit contract with employees and we examine whether changes of control affect mine closures. We find that newly acquired mines are more likely to be closed immediately following the change of control. The probability of mine closure is 2.2% higher for treated mines during the quarter of and the first quarter after the control transfer; however, the difference in mine closure probability disappears after the second quarter from the change of control. Furthermore, the estimated effect on mine closure probability is larger when a treated mine is less efficient, measured as coal

production per labor hour, before an acquisition.

Second, favorable working hours may be part of an implicit contract with employees and we examine what happens to working intensity at a mine after being acquired. We define working intensity as quarterly working hours per employee and find that it increases in treated mines after the change of control relative to the change in control mines. Specifically, employees in treated mines on average experience a 5% increase in working hours (equivalent to 1.6 hours per week) after the change of control. Interestingly, the increase in injury rates is larger in treated mines where working intensity was lower before treatment. These first two pieces of evidence suggest that new controllers, as the corporate raiders in Shleifer and Summers (1988), seem to seek efficiency gains and increased working intensity at continuing mines after acquisitions.

Third, the estimated effect on injury rates is larger when the new controllers are characterized by worse workplace safety records than the previous controllers. We measure safety records with controller fixed effects estimated using pre-acquisition data. Controller identities seem to matter for workplace safety.

Fourth, the ability of a new controller to breach implicit contracts critically hinges on their bargaining power vis-a-vis employees in treated mines. In theory, the new controller should have more ability to renege on implicit contracts if it has more monopsony power in the local labor market (a commuting zone) after the change of control. For each controller that made acquisitions in a commuting zone, we follow Arnold (2021) and

calculate the predicted change in employment-based Herfindahl-Hirschman index (HHI) across mine controllers as a proxy for the predicted change in the local monopsony power of the controller. Consistent with this hypothesis, the estimated negative effect on injury rates is larger for acquisitions in which the merger is predicted to increase local employer monopsony power more.

Finally, more evidence supporting the implicit contract breach interpretation comes from the heterogeneity by whether a mine hires any contractors before acquisitions. If the new controllers breach the implicit contracts between employees and previous controller, then the estimated effects on injury rates are expected to increase more in mines that hire fewer contractors as workers hired by contractors are less bound by the implicit contracts. We split the sample into mines that hire no contractors and those hiring at least one contractor before acquisitions, and find that mines without contractors before treatment drive the effect.

This paper contributes to the literature on corporate finance and workplace safety. Using establishment-level data across the whole economy from the 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. Using the SOII data, 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 (M&As). Our evidence suggests that mergers and acquisitions and private equity buyouts have distinct implications for workplace safety in target firms.

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). On how M&As affect workers, most of the studies focus on the effects on employee compensation and employment. In the setting of hospital mergers, 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 studies how M&As affect workers. He 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 shareholders gain after M&As 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).

II. 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)). Pagano and Volpin (2005) rationalize why implicit contracts, or the alliance between management and workers, is incentive compatible in the context of a change of control.[7] The setup of the model is the following.

II. A. Setup

The game comprises four stages:

- At $t = 0$ the initial owner of the firm hires a worker. It sets the worker's wage to be $\hat{w}$ (to be paid at $t = 3$ conditional on output) and sets initial health and safety standards (H&S) $\hat{h} \geq 0$. We assume the worker signs a long-term contract such that

[7] In the Appendix, we present a model in the spirit of Pagano and Volpin (2005) where we add health and safety to efficiency wages and monitoring, and show that differences in monitoring costs between the new and initial owner yield similar predictions to the ones derived from the model presented in this section.

$\hat{w}$ cannot be altered in the event of a change of control. At this stage, the worker's outside option is worth $\hat{U} \geq 0$.

- At $t = 1$, with probability π there is a change of control (i.e., a new party in control); with probability $1 - \pi$, there is no change of control. P is the price paid in the case of a change of control to the initial owner.

- At $t = 2$, whoever is in control of the firm chooses the actual investment in health and safety $h \geq 0$. At this stage, the worker needs to invest in effort $e \in \{0, 1\}$. The worker is injured with probability $ep(h)$ where $p(h)$ is decreasing and convex ($p' < 0$ and $p'' > 0$ and $\lim_{h \to 0} p(h) = \overline{p} \in (0, 1]$ and $\lim_{h \to \infty} p(h) = 0$). As an example, $p(h) = \overline{p}/(1 + h)$. We assume that the worker's outside option at this stage is worth $\overline{U} \geq 0$.

- At $t = 3$, if the worker does not leave the firm, Y is produced with probability e and the wage $\hat{w}$ is paid.

The critical assumption is that the worker's outside option decreases over the employment time, $\overline{U} < \hat{U}$. An immediate justification for the assumption is that the worker must make a firm-specific investment that is not worth as much to another employer (Becker, 1993). Carnahan, Agarwal, and Campbell (2012) presents empirical evidence supporting this argument. Another justification is that once the worker is hired by the firm at $t = 0$, she will settle down in a geographical area and shape their family's patterns of life

around the present job. Many outside options become relatively less valuable once those place-specific investments are in place. They will prefer to search for jobs within the local labor market in the future. As a result, at $t = 2$, her set of outside options would become smaller compared to the one at $t = 0$ due to increased search and mobility costs. A further justification is that the worker's outside option declines as a result of the transfer of control because of an increase in labor market concentration. Though we will not be able to test the other explanations, we will provide direct evidence of this channel.

The worker is risk averse while the party in control is risk neutral. The worker's utility derived from wages is $U(w)$, which is increasing and concave in w. In case of injury, the worker's utility is decreased by L.

The essence of the implicit contract idea is that the initial owners face a reputational cost of reneging on their promise at $t = 2$ (i.e., the cost of choosing h when promising $\hat{h}$): We assume that this cost is quadratic, $\eta(h - \hat{h})^2/2$.[8]

The model is solved by backward induction starting at stage $t = 2$. There are two subgames to consider.

[8] As shown in the Appendix, this assumption can be removed if one adds to the model a monitoring choice by the controller (and differences in monitoring costs between the new and the initial owners).

II. B. Subgame I: No change of control

If there is no change of control at $t = 1$ (and assuming the worker exerted effort at $t = 0$), the initial owner chooses h at $t = 2$ to maximize:

$$Y - \hat{w} - h - \eta \frac{(h - \hat{h})^2}{2} \tag{II. .1}$$

subject to the incentive compatibility condition:

$$U(\hat{w}) - p(h)L \geq \overline{U}. \tag{II. .2}$$

We will later prove that the constraint (II. .2) is not binding when the initial owner remains in control. So, from the first order conditions (which are necessary and sufficient):

$$h = \hat{h} - \frac{1}{\eta} \tag{II. .3}$$

and thus

$$p = p\left(\hat{h} - \frac{1}{\eta}\right) \equiv p_{NC}. \tag{II. .4}$$

II. C. Subgame II: Change of control

If there is a change of control at $t = 1$ (again assuming that the worker exerted effort at $t = 0$), the new party in control chooses H&S standard $h \geq 0$ at $t = 2$ so as to maximize:

$$Y - \hat{w} - h \tag{II. .5}$$

subject to the incentive compatibility condistion (II. .2).

The key difference is that the new owner does not face the reputational costs of choosing a different h from $\hat{h}$. As (II. .5) is strictly decreasing in h, the optimal choice of h is such that the constraint (II. .2) is binding and thus:

$$p(h) = \frac{U(\hat{w}) - \overline{U}}{L} \equiv p_C. \tag{II. .6}$$

II. D. Impact of the change of control on the injury rate

We now proceed backwards to $t = 0$. The worker's expected utility is:

$$\mathcal{U} = (1 - \pi)[U(\hat{w}) - p_{NC}L] + \pi\overline{U}, \tag{II. .7}$$

where p_{NC} is defined in equation (II. .4).

The initial owner chooses $(\hat{h}, \hat{w})$ to maximize expected profits $\mathcal{P}$, where

$$\mathcal{P} = (1-\pi)\left(A - \hat{w} - \hat{h} + \frac{1}{2\eta}\right) + \pi P \tag{II. .8}$$

subject to the participation constraint:

$$(1-\pi)\left[U(\hat{w}) - p\left(\hat{h} - \frac{1}{\eta}\right)L\right] + \pi\overline{U} \geq \hat{U}. \tag{II. .9}$$

It is optimal for the initial owner to choose policies such that equation (II. .9) is binding. This observation implies that $U(\hat{w}) - P_{NC}L > \hat{U}$ and so the incentive compatibility condition (II. .2) is not binding in Section 2.2, as per our assumption.

Using the binding participation constraint (II. .9) and equation (II. .6), one obtains the following important result, which is the key prediction tested in the paper.

Proposition 1. *Following a change of control, the injury rate p increases as compared to the case without a change of control:*

$$\Delta p \equiv p_C - p_{NC} = \frac{\hat{U} - \overline{U}}{(1-\pi)L} > 0.$$

The increase in injury rate is increasing in the reduction in the worker's outside option value $\hat{U} - \overline{U}$ *and the probability of a change of control* π*; and it is decreasing in the cost of the injury L.*

As this result is independent of $\hat{h}$ and $\hat{w}$, we do not need to solve for the initial owner's

maximization problem to derive the key empirical prediction of the model.[9]

Section V. A. tests the prediction from this model that the injury rate per labor hour increases after a change of control. In Section V. B., we report further empirical results supporting the implicit contract theory. Sections V. B..1 and V. B..2 examine the effects of changes of control on two aspects of implicit contracts: mine closure and working intensity. Sections V. B..3 and V. B..4 examine the heterogeneity effects by the changes in controller workplace safety practices and predicted changes in employer market power, respectively. Finally, Section V. B..5 examines the heterogeneity effects by whether a mine hires any contractors before acquisitions to further support the interpretation of implicit contract breach after acquisitions.

III. Data on Mines in the U.S.

We start with all 56,541 U.S. mines with strictly positive employment operating from 1983Q1 to 2023Q2. For each mine, we observe the evolution over time of its controllers, occupational injuries, employment, labor hours, and other characteristics using the fol-

[9]For completeness, we can show that the profit maximization problem of the initial owner at $t = 0$ (defined in equations (II. .8) and (II. .9)) has a unique solution. The optimal $(\hat{h}, \hat{w})$ are characterized by the following two conditions: (1) $\hat{w} = f\left(\frac{\hat{U}-\pi\overline{U}}{1-\pi} + p\left(\hat{h} - \frac{1}{\eta}\right)L\right)$ and (2) $\frac{\hat{U}-\pi\overline{U}}{1-\pi} + p\left(\hat{h} - \frac{1}{\eta}\right)L = g\left(\frac{-1}{L}\frac{1}{p'(\hat{h}-\frac{1}{\eta})}\right)$, where $f(.) = U^{-1}(.)$ and $g(.) = f'^{-1}(.)$. Given that the function $U(.)$ is increasing and concave, both functions $f(.)$ and $g(.)$ are increasing. As $p(.)$ in a decreasing function, the left-hand side of the second equation decreases in $\hat{h}$. Since $p(.)$ is a convex function, the right-hand side of second equation increases in $\hat{h}$. Therefore, there is a unique solution for $\hat{h}$ in that equation. Once that solution is inserted in the first equation , the corresponding unique value for $\hat{w}$ is found.

lowing five Mining Safety and Health Administration (MSHA) datasets:[10]

- **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, working hours, and coal production at the mine×subunit code×year×quarter level. The data is available since 1983. A subunit code refers to a location within a mine and it includes: (01) Underground operation; (02) Surface operation at underground mine; (03) Strip, quarry or open pit; (04) Auger (Coal only); (05) Culm bank or refuse pile (Coal only); (06) Dredge; (12) Other surface (Metal/Non-Metal only); (17) Independent shop or yard; (30) Mill operation/preparation plant; (99) Office workers at mine site. We aggregate data to the mine×year×quarter level by summing employment, working hours, and coal production across all subunits within a mine.

- **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 1983. 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 sum-

[10]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.

ming 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 further 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. 98.6% of mine-quarter observations have one inspection.

- **Data on Controllers and Operators of Mines**: To identify whether a mine is involved in a mergers and acquisition (M&A) event in a quarter, we use the data on the history of the controller and operator of a mine provided by MSHA. A con-

troller of a mine is the ultimate owner. An operator operates a mine on behalf of a controller. For each mine, the data provides the starting and ending dates of each controller and operator. A mine could be jointly owned by multiple controllers in a quarter and a controller could be a firm or a person.

In Section V. we will adopt an estimation window of [-12, +12] quarters around an acquisition. Therefore we focus on acquisitions made between 1986Q1 and 2020Q3. During this period, we can identify 33,719 mine-quarter observations that experienced changes of control, which involve 19,062 unique target mines and 11,520 unique acquirer controllers.[11] On average, around 4% of all mines (619 mines) experienced changes of control in a year. Cumulatively, around 28% of all mines experienced at least one change of control during this period. Most of the acquisitions in our sample are asset sales, that is, only a fraction of mines of a controller were acquired by an acquirer. There are 3,006 changes of control associated with mergers of two controllers, and it accounts for 9% 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 I.

IV. 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.

[11] There are 33,993 acquisitions. More than one acquirer can be associated with an acquisition.

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 and the new controller did not acquire other mines in the same 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 controllers 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 controllers do not make any other acquisitions within the estimation window. We further require that control mines do not experience any control change and their controllers 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. 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 Sant'Anna (2021).

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

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.[12] μ_{ki} is a set of cohort×mine fixed effects. $\eta_{kj(k)t}$ aims to control for average trends of injury rates before treatment. i's pre-treatment trend in injury rates, $\Delta InjuryRate$, is measured as the average difference in its injury rates from each of the 11 quarters leading up to k (from $t_k - 12$ to $t_k - 2$) and its injury rate in the immediate pre-treatment period $t_k - 1$. Within each cohort k, mines are grouped in deciles of $\Delta InjuryRate$ indexed by $j(k)$.

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 treat-

[12]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.

ment 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:

$$y_{kit} = \beta \times Treated_{ki} \times \mathbb{1}(t - t_k > 0) + \mu_{ki} + \eta_{kj(k)t} + \epsilon_{kit} \tag{IV. .2}$$

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.

Table I reports the means of the main outcome variables for treated and control mines in quarter $t - 1$ (for changes of control happening in quarter t) and the average changes in these variables in the pre-acquisition window relative to the levels in quarter $t - 1$.

Although the levels of injury rates and working intensity (working hours per employee) are statistically different between treated and control mines in quarter $t-1$, the average changes of these variables before acquisitions are not statistically significant between treated and control mines.

V. 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 are presented in section V. A.. Section V. B. explores the mechanisms at work. Additional analysis and robustness tests are discussed in section V. C..

V. A. Baseline Analysis

To examine the effects of changes of control on target mines' injury rates, our baseline analysis estimates Equation IV. .1. Estimated coefficients during the pre-treatment period are small and statistically insignificant, adding credibility to the maintained parallel trends assumption post-treatment (Figure II and Table II). 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 1.007 per 1,000 full-time workers 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 6.617 per 1,000 full-time workers, the estimated average treatment effect is economically meaningful and represents a 15.2% increase. This result is fully consistent with the prediction of our implicit contract model.

V. A..1 Traumatic vs. Non-Traumatic Injuries

A concern with using 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).[13] Both traumatic and non-traumatic injury rates increase in treated mines after changes of control relative to control mines. Figures A.1 and A.2 report estimated dynamic treatment effects on traumatic and non-traumatic injury rates, respectively. Columns (1) and (2) of Table III report their average treatment effects, respectively. In particular, traumatic injury rates increase 14.8% relative to the pre-treatment sample mean of treated mines (1.948 per 1,000 full-time employees). Non-traumatic injury rates

[13]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.

increase 15.4% relative to the sample mean of treated mines before treatment, 4.67 per 1,000 full-time employees.

V. A..2 Mergers vs. Asset Sales

The estimated effects may differ in the cases of mergers between two entities versus asset sales. In the sample, around 9% of changes of control are associated with mergers between two controllers (mergers), and the remaining cases are asset sales. Injury rates in target mines increase after control changes for both types of acquisitions (Figure A.3 and Table III: Columns 3 and 4). Injury rates increase 0.947 per 1,000 full-time employees after asset sales, a 9.96% increase relative to the pre-treatment sample mean of target mines (9.509 per 1,000 full-time employees). For mergers between two controllers, rates increase 1.259 per 1,000 full-time employees after control changes, a 19.8% increase relative to the pre-treatment sample mean of target mines (6.353 per 1,000 full-time employees). Although the difference in economic magnitudes is large, the p-value of the difference between the two estimates is 0.143.

V. A..3 Role of Operators

An operator is an entity that operates a mine on behalf of controllers, which are the ultimate owners of a mine. In addition to tracking history of the ultimate owner of each mine, MSHA also provides information on the history of operators. In the sample, 58%

of treated mines experienced changes in operators after control changes and 42% did not. We examine whether there are heterogeneous effects for the following two cases after an acquistion: (1) both the controller and operator of a mine change and (2) only the controller of a mine changes with the operator remaining the same. Given that operators are more directly involved with daily operations and may play a more important role in impacting workplace safety, we expect the estimated effect to be larger when both controllers and operators change. Figure A.4 reports dynamic treatment effects and columns 5 and 6 of Table III report average treatment effects.

When the operator of a mine remains the same after an acquisition, the injury rate increases 0.626 per 1,000 full-time employees after treatment (Column 6), an 8.13% increase relative to the pre-treatment sample mean of target mines (7.702 injuries per 1,000 full-time employees). In the case of changing an operator after an acquisition, the estimated effect is larger (Column 7), an increase of 1.292 per 1,000 full-time employees after treatment representing a 13.2% increase relative to the sample mean of target mines before treatment (9.823 injuries per 1,000 full-time employees). The p-value of the difference between the two estimates is 0.233.

V. B. Mechanisms

In this section, we explore potential mechanisms through which changes of control affect injury rates.

V. B..1 Effects on Closures of Target Mines

We first examine whether target mines are more likely to be closed after being acquired. In this analysis, we drop mines that were idle but reopened before acquisitions. We report the results in Figure III. The estimates show that, compared to control mines, target mines are 1.8% and 1.2% more likely to be closed in the acquisition quarter and the first quarter right after an acquisition, respectively. Column (1) of Table IV shows that treated mines are 2.2% more likely to be closed after being acquired in these two quarters. However, after the second post-acquisition quarter, the closure probability of target mines is either similar to or slightly smaller than the one in control mines.

We further examine the heterogeneous effects by the pre-acquisition operational efficiency of mines during the window $[t, t+1]$ for each cohort t. We measure the operational efficiency of a mine as production per hour of labor. This information is only available in coal mines. In column (2) of Table IV, we first show that the effect holds for coal mines. We then split the sample into quartiles based on the operation efficiency in the quarter before an acquisition. Columns (3)-(6) of Table IV reports the results. The least efficient target coal mines are 7.5% (statistically significant at the 1% level) more likely to be closed in the estimation window compared to control mines; the estimated effect among the most efficient target coal mines is 1.8% (not statistically significant), and the difference in these two estimates is statistically significant at the 10% level. Our evidence suggests that, com-

pared to control mines, target mines are more likely to be closed after being acquired and the effect is stronger for target mines with lower pre-acquisition operational efficiency.

The results suggest that new controllers, as corporate raiders in Shleifer and Summers (1988), seem to target efficiency. By closing inefficient mines, the new controllers may further consolidate operations and increase the utilization of mines that are kept open for operations. The increased utilization may contribute to increased injury rates, which is investigated in the next subsection.

V. B..2 Change in working intensity

One channel that acquisitions could impact injury rates in target mines is that the working intensity of employees in target mines would increase after acquisitions. In other words, new owners may prioritize improving target mines' performance over workplace safety. If an employee works longer hours after changes of control, then he or she is more likely to experience occupational injuries due to fatigue or inattention (Dembe et al., 2005). This could contribute to why acquisitions lead to increased injury rates in target mines. This subsection examines how acquisitions affect employees' working intensity in target mines.

We measure working intensity as working hours per employee and estimate Equation IV. .1 using the natural logarithm of working hours per employee as the dependent variable. We report the results in Figure IV and Panel A of Table V. 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 management transition. After the first post-treatment quarter, working intensity starts to increase. Working intensity 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 425 hours per quarter (32.7 hours per week), the estimated effect suggests that an employee, on average, works 21 hours more in a quarter (1.6 hours more per week) after control changes.

To further test this mechanism, we examine the heterogeneous effects on injury rates in target mines by working intensity before acquisitions. If increased injury rates after acquisition are partly driven by increased capacity utilization, the effect should concentrate in target mines that were under-utilized before acquisitions. To test this hypothesis, we sort mines into quartiles based on working intensity one quarter before acquisitions for each cohort of quarter t. We estimate equation (IV. .2) for each subsample.

We report estimated effects relative to the pre-treatment mean of the treated mines in each subsample in Panel B of Table V. Estimated effects on injury rates in target mines monotonically decrease in pre-treatment working intensity. Among mines with working intensity falling in the bottom quartile before acquisitions, the average treatment effect of

control changes on injury rates represents a 24.8% increase relative to the pre-treatment sample mean. Among mines with working intensity in the top quartile before acquisitions, the estimated increase in injury rates after control changes is much smaller, 8.7% relative to the sample mean. The difference in these two estimates is statistically significant at the 1% level.

V. B..3 Changes in Controller Workplace Safety Practices

Another possible explanation for the results in Figure II is that workplace safety practices change after the change of control. We follow Bertrand and Schoar (2003) and use the controller fixed effects to proxy for the unobserved controller workplace safety practices. Specifically, for each cohort of acquisitions, we estimate controller fixed effects using the pre-acquisition injury rate with the following regression:

$$y_{ifct} = X'_{it}\beta + \eta_f + \psi_{ct} + \epsilon_{ifct}, \tag{V. .1}$$

where i, f, t index for mine, controller, and time, respectively. η_f and ψ_{ct} are controller and commuting zone$\times$year$\times$quarter fixed effects, respectively. y_{ifct} is injury rate of mine i belonging to controller f in commuting zone c in time t. X_{it} 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. In each cohort of acquisitions, we extract the estimated controller fixed effects using pre-

acquisition data, $\widehat{\eta_f}$, expressing f's average residual contribution to injury rates above other factors. For each treated mine, we calculate the difference in the controller fixed effects of new and old controllers as $\Delta\eta = \widehat{\eta_{f,new}} - \widehat{\eta_{f,old}}$. We use $\Delta\eta$ as a proxy for the change in controller workplace safety practices after control changes.

For each cohort t, we sort treated mines into quartiles based on $\Delta\eta$. There are 515 mines with $\Delta\eta$ in the bottom quartile and their mean $\Delta\eta$ is -1.07, suggesting new controllers on average have better workplace safety records in this quartile. There are 312 mines with $\Delta\eta$ in the top quartile and their mean $\Delta\eta$ is 1.54, suggesting new controllers on average have worse workplace safety records. We estimate equation (IV. .2) for each subsample.

Table VI reports estimated effects relative to the pre-treatment mean of the treated mines in each subsample. Among treated mines with top-quartile $\Delta\eta$, injury rates increase significantly after control changes. The average treatment effect of control changes on injury rates here represents a 75% increase relative to the pre-treatment sample mean. The estimate suggests that, if new controllers have worse workplace safety records than previous controllers, treated mines experience a deterioration in workplace safety after control changes.

However, when new controllers have better workplace safety records, we do not observe a decrease in injury rates in treated mines. The estimated treatment effect relative to the pre-treatment sample mean is 6%, but not statistically significant. The difference

between the estimates in the two subsamples is statistically significant at the 1% level. Overall, the results suggest that changes in controller workplace safety practices could play an important role in affecting injury rates.

V. B..4 Changes in Employer Market Power

Another alternative explanation for the increased injury rates among treated mines after control changes is that employers' market power increases in a local labor market. In this subsection, we investigate the validity of this mechanism.

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 controllers (Azar et al., 2020). To evaluate the importance of acquisitions made by a controller in a commuting zone, we follow Arnold (2021) and calculate the *predicted* change in commuting zone-level employment HHI ($\widehat{\Delta HHI}$) due to the acquisitions and then examine how the estimated effects on injury rates vary with it. Some mergers are between mines in different commuting zones; their $\widehat{\Delta HHI}$ will be zero. Others share a commuting zone and generate $\widehat{\Delta HHI} > 0$.

For each controller that made acquisitions in a commuting zone in quarter t, to measure $\widehat{\Delta HHI}$, we assume the event happened in quarter $t-1$ and there were no other control changes, and then calculate the pseudo employment HHI in the commuting zone level in quarter $t-1$. $\widehat{\Delta HHI}$ is the difference between the pseudo employment HHI if the

mines had been counterfactually merged in $t-1$ and the actual employment HHI in the commuting zone in quarter $t-1$.

We split the treated mines in each cohort into quartiles of $\widehat{\Delta HHI}$ and estimate the equation (IV. .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 430 point increase in HHI is far above the 100 point increase considered significant under FTC and DOJ Merger Guidelines. We report the estimated effects relative to the pre-treatment mean of treated mines in each subsample in Table VII.

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 22% 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 smaller (a 6% increase relative to the sample mean) and is statistically significant at the 10% level. The difference between the two estimates is statistically significant at the 1% level.

While this suggests that the increase in employer market power explains part of the increase in injury rates, that injury rates increase non-trivially in target mines with low $\widehat{\Delta HHI}$ suggests that increased employer market power is not the only channel that mat-

ters. These acquisitions may affect workplace safety through other channels, for example, changes in production decisions as shown in Table V or changes in controller workplace safety practices in Table VI.

V. B..5 Employees vs. Workers Hired by Contractors

Finally, to further support the implicit contract breach interpretation, we examine the heterogeneous estimated effects on injury rates by whether a mine hires any contractors one quarter before acquisitions. If the new controllers breach the implicit contracts between previous management and employees, then the estimated effects are expected to be larger for mines that hire fewer contractors. The assumption behind this test is that workers hired by contractors are not bound by implicit contrasts.[14]

MSHA does not track the number of workers hired by contractors in each mine, but it provides information on the starting and ending dates of a contractor in a mine. We use this data to split the sample into mines with no contractors and with at least one contractor one quarter before an acquisition. Around 35% of treated mines hired at least one contractor before acquisitions. We report estimated effects relative to the pre-treatment mean of the treated mines in each subsample in Table VIII. Dynamic treatment effects are reported in Figure A.5. The results show that our results are entirely driven by mines that did not hire any contractors before acquisitions. Among these mines, compared to

[14]The Mine Act defines an independent contractor as any person, partnership, corporation, subsidiary of a corporation, firm, association or other organization that contracts to perform services or construction at a mine, https://arlweb.msha.gov/regs/complian/ppm/pmvol3a.htm.

control mines, the injury rates of treated mines increase 20% relative to the pre-treatment sample mean after acquisitions and the estimate is statistically significant at the 1% level. In contrast, among mines that hired at least one contractor before acquisitions, the estimate is close to zero and is not statistically significant. The difference between these two estimates is statistically significant at the 1% level.

V. C. Additional Results

V. C..1 Synthetic Difference-in-Differences

To assess the robustness of the empirical strategy, we estimate the baseline effects using an alternative estimator, the synthetic difference-in-differences (synthetic DiD) estimator of Arkhangelsky et al. (2021). This estimator requires balanced panel data. Specifically, for each acquisition event quarter, we estimate using the synthetic DiD estimator and then aggregate estimates across event quarters weighted by the number of target mines in each event quarter (Callaway and Sant'Anna, 2021). In each event quarter, the reported estimates are relative to the pre-treatment average. We compute standard errors based on bootstraps with 300 re-samplings. We report the results in Figure A.6 and in column (1) of Table IX. The estimated average treatment effect is 0.897 with the 99% CI is [0.256,1.538]. The magnitude of the estimated effect is similar to the baseline result.

V. C..2 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 local labor market. 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. Relative to control mines, the injury rates at target mines on average increase by 1.409 per 1,000 full-time employees (Figure A.7 and column (2) of Table IX). The estimate is similar to but larger than the baseline estimate, suggesting that there could be spillovers from target mines to control mines in the same local labor market, such as through increased employer market power. Our baseline estimate potentially understates the true treatment effect.

V. C..3 Control for Demand Shocks

One concern with the baseline results is that positive demand shocks for commodities produced by mines could drive both an acquirer's propensity to make acquisitions and the injury rate at a mine (Charles et al., 2022). To mitigate this concern, we further control for demand shocks by including the cohort$\times$commodity$\times$year$\times$quarter fixed effects in the baseline analysis. We report the average treatment effect in column (3) of Table IX and

the dynamic treatment effects in Figure A.8. Our baseline results are robust.

V. C..4 A Placebo Test

To further demonstrate that control matters for injury rates, we perform a placebo test. Specifically, we examine the effect of operator changes on injury rates conditional on no control changes. In this analysis, we focus on mines that were not involved in any acquisitions between 1986Q2 and 2020Q3. Among these mines, we identify changes in operators. For each cohort of operator change events, we define treated mines as those that experienced changes in operators and control mines as those that never experienced changes in operators in our sample period. Similar to the baseline analysis, we further require that treated mines did not experience changes in operators in other years within the estimation window, and their operators did not acquire other mines within the estimation window. We report the dynamic treatment effects in Figure A.9 and report the average treatment effect in column (4) of Table IX.

The estimate on $Treated \times Post$ is -0.217 and is not statistically significant, with a p-value of 0.769. The evidence suggests that, conditional on no changes of control, injury rates in treated mines do not change relative to control mines after operators change. This result suggests that control changes, rather than just operator changes, drive the baseline results.

V. C..5 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 estimate equation (IV. .1) using Poisson regression and use the number of violations during an inspection in a quarter as the outcome variable. The estimated coefficients during the pre-treatment period are small and statistically insignificant (Figure A.10). The trajectories of the number of violation citations between treated and control mines only start to diverge after treatment. The number of violations during an inspection of treated mines on average increases 10% relative to control mines after control changes (column (5) of Table IX), and it is statistically significant at the 1% level.

V. C..6 Compensating Differentials

We have presented evidence showing that a change of control in a mine leads to higher injury rates. In this section, we study whether acquisitions affect hourly wages, examining whether compensating wage differentials arise in response to the deterioration in workplace safety.

One limitation of the MSHA dataset is that it does not provide data on payroll and we therefore are not able to examine the changes in the mine-level hourly wages after acquisitions. We instead examine the effects of acquisitions on hourly wages at the commuting zone (CZ) level. For each quarter, the CZ-level payroll data is measured by aggregating the mining sector (3-digit NAICS=212) payroll data at the county level from the Quarterly Census of Employment and Wages (QCEW). The CZ-level working hours data is obtained by aggregating the mine-level data on working hours. The CZ-level hourly wages are calculated as payroll divided by working hours.[15]

For each cohort of acquisition events, we define a CZ to be treated if at least one mine in the CZ experiences the change of control. We further require treated CZs not to experience shocks in other quarters within the estimation window (12 quarters before and after an event). We define control CZs are those that do not experience any acquisition events within the estimation window. To examine the effects of acquisitions on CZ-level hourly wages, we perform a standard event-study analysis by including the cohort$\times$CZ and cohort$\times$year$\times$quarter fixed effects. The estimated average treatment effect is 1% with a p-value of 0.853 as reported in column (6) of Table IX, and the dynamic treatment effects are reported in Figure A.11. Our estimate overall provides evidence that CZ-level hourly wages do not change after mines in a CZ experience changes of control.

[15]Our sample for this analysis starts in 1990. The reason is that around 60% of pre-1990 county-level data in the mining sector were suppressed due to confidentiality. In contrast, only around 20% of post-1990 data were suppressed. We winsorize the CZ-level hourly wages at the 10 and 90 percentiles because of outliers. The average CZ-level hourly wages are 25 dollars in 2023 dollars.

VI. 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 perform further tests and our results support the interpretation that increased injury rates in target mines after acquisitions could be driven by the breach of implicit contracts by the new controllers. First, newly acquired mines are more likely to be closed immediately following the change of control, and the estimated effect on mine closure probability is larger when a treated mine is less efficient, measured as coal production per labor hour, before an acquisition. Second, we find that working intensity, defined as working hours per employee, increases in target mines after acquisitions and the estimated effect on injury rates is stronger for mines in which pre-treatment working intensity is lower, suggesting that new controller increase the utilization of target mines. These first two pieces of evidence suggest that new controllers, as the corporate raiders in Shleifer and Summers (1988), seem to seek efficiency gains.

Third, we find that the estimated effects on injury rates are larger when new controllers have worse workplace safety records than previous controllers. The evidence suggests that changes of control are associated with changes in controller workplace safety practices, which plays an important role in workplace safety. Fourth, we find that the esti-

mated effects on injury rates are larger when new controllers are expected to enjoy greater monopsony power in a local labor market. This evidence is consistent with the implicit contract theory in the sense that employers are more likely to renege on implicit contracts when they have larger labor market power. Finally, we find that the estimated effect concentrates on mines that do not hire contractors, which further supports the interpretation of breaching implicit contracts.

Our paper has important policy implications for evaluating the potential labor market effects of mergers and acquisitions. 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 control changes towards a controller with a worse safety record have much more negative effects on workplace health and safety than M&As where the new controller has a similar or better record. While this highlights an important dimension of the employment relationship, it is only one aspect among many that warrant consideration.

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APPENDIX: EXAMPLE OF MICROFOUNDATION OF THE IMPLICIT CONTRACTS

In this section, we provide a simple model that can be viewed as a microfoundation of the implicit contract setting analyzed in Section 2. Our model adds an investment in health and safety to the setting studied by Pagano and Volpin (2005), who rationalize why breaching the implicit contracts is incentive compatible in the context of a change of control.

Setup: The game comprises five stages:

- At $t = 0$, the initial owner hires a worker at a fixed wage $\hat{w}$ (that cannot be conditional on performance). At this stage, the worker's outside option is worth $\hat{U} \geq 0$.
- At $t = 1$, control can be transferred between the initial owners of the firm (the initial owners will be denoted with a subscript O) and new owners (the new owners will be denoted with a subscript N) provided there is a gain from trade.
- At $t = 2$, the controller chooses how much of the firm's resources to invest in health and safety standards (H&S) $h \geq 0$ and how much effort to put in the monitoring technology. At a private cost qK_i, where $i \in (O, N)$ indicates whether the controller is new or old, monitoring enables the controller to find out with probability $q \in [0, 1]$ whether the worker has exerted effort or not in a way that is verifiable by the court.

If the firm can prove that the worker has not exerted effort, it can withhold the wage payment $\hat{w}$. The parameter q is observable to the worker.

- At $t = 3$, the worker chooses effort $e \in \{0, 1\}$. Effort increases the chance of workplace injury with associated disutility L per worker. The probability of a workplace injury also depends on the safety standards chosen by the firm: $ep(h)$, where $p(h) = \frac{\overline{p}}{1+h}$ and so $p(h)' < 0$ and $p(h)'' > 0$.

- At $t = 4$, the worker produces Y with probability e. The controller finds out the worker's effort with probability q and pays wages accordingly.

The model is solved by backward induction starting from stage $t = 3$.

Effort decision by the worker: The worker's effort decision consists in the comparison between the expected utility if they exert effort ($U(\hat{w}) - p(h)L$) and the utility if they do not exert effort ($(1 - q)U(\hat{w})$). The incentive compatibility condition is thus:

$$qU(\hat{w}) \geq p(h)L. \tag{VI. .1}$$

This condition indicates that there are three ways for the firm to induce the worker to exert effort: they can monitor the worker more (by choosing a higher q and thus making shirking more expensive), they can pay them more (by increasing the efficiency wage $\hat{w}$), or they can invest more in H&S (by increasing h and thus reducing the injury-related cost of exerting effort).

Proceeding backwards to the hiring decision at $t = 2$, we can start by deriving the worker's participation constraint:

$$U(\hat{w}) \geq p(h)L + \overline{U}. \tag{VI. .2}$$

As we set $\overline{U} = 0$ and $q \in [0, 1]$, the participation constraint (VI. .2) is redundant. So, the only constraint that the controller is facing at $t = 2$ is the IC condition (VI. .1).

Health & Safety chosen by the controller: The controllers in charge at $t = 2$ have a cost of monitoring K_i, where $i = O$ if there is no change of control and $i = N$, if there is a change of control at $t = 1$. Their problem is to set (h, q) in such a way to maximize:

$$\max_{(h,q)} Y - \hat{w} - h - qK_i. \tag{VI. .3}$$

subject to the incentive compatibility condition (VI. .1).

It is immediate to notice that the IC constraint will be strictly binding in equilibrium and thus the problem becomes:

$$\max_{h} Y - \hat{w} - h - \frac{\overline{p}L}{(1+h)U(\hat{w})}K_i. \tag{VI. .4}$$

The trade-off in (VI. .4) is as follows: investing more in H&S is expensive but reduces the need for monitoring activity from the controller.

From the first order condition, we obtain:

$$(h_i^*, q_i^*) = \left(\sqrt{\frac{\overline{p} L K_i}{U(\hat{w})}} - 1, \sqrt{\frac{\overline{p} L}{U(\hat{w}) K_i}} \right). \tag{VI. .5}$$

Equation (VI. .5) implies that a controller with a higher cost of monitoring K will invest more in health and safety, and monitor less.

As the probability of injury $p(h)$ is strictly decreasing in h, (VI. .5) also implies that the probability of workplace injury increases with K_i:

$$p_i^* = \sqrt{\frac{\overline{p} U(\hat{w})}{L K_i}}. \tag{VI. .6}$$

Change of control: Using equations (VI. .5) and (VI. .6), the value of the firm is:

$$V_i = Y + 1 - \hat{w} - 2\sqrt{\frac{\overline{p} L K_i}{U(\hat{w})}} \tag{VI. .7}$$

which is strictly decreasing in the monitoring cost K_i. A change of control happens only if $V_N > V_O$. This observation implies the following result:

Proposition 2. *There is a change of control iff $K_N < K_0$, i.e. the monitoring cost of the new controllers is lower than the monitoring cost of the old owner.*

As in the model presented in Section 2, this result implies that following a change of control, the injury rate increases relative to the case without a change of control. This is

due to the fact that the new controller has incentives to monitor more after the change of control. As a result, the new controller lowers the investment in health and safety standards, resulting in higher injury rates.[16]

The key difference with the model presented in Section 2 is that the reputational costs faced by the initial owner are replaced by differences in monitoring costs between the initial and the new owners. Because the new owners have lower monitoring costs than the initial owners, they have ex-post incentives to lower health and safety standards (compared with the initial owners).

[16] Although immaterial to our analysis, the model closes with the choice of $\hat{w}$: $\hat{w}$ is determined by the initial participation constraint: $U(\hat{w}) - \left(\pi\sqrt{1/K_N} + (1-\pi)\sqrt{1/K_O}\right)\sqrt{\overline{p}U(\hat{w})L} = \hat{U}$, where π is the probability of the change of control (which happens when the new owner has monitoring costs $K_N < K_O$).

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MAIN FIGURES AND TABLES

Figure I: 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.

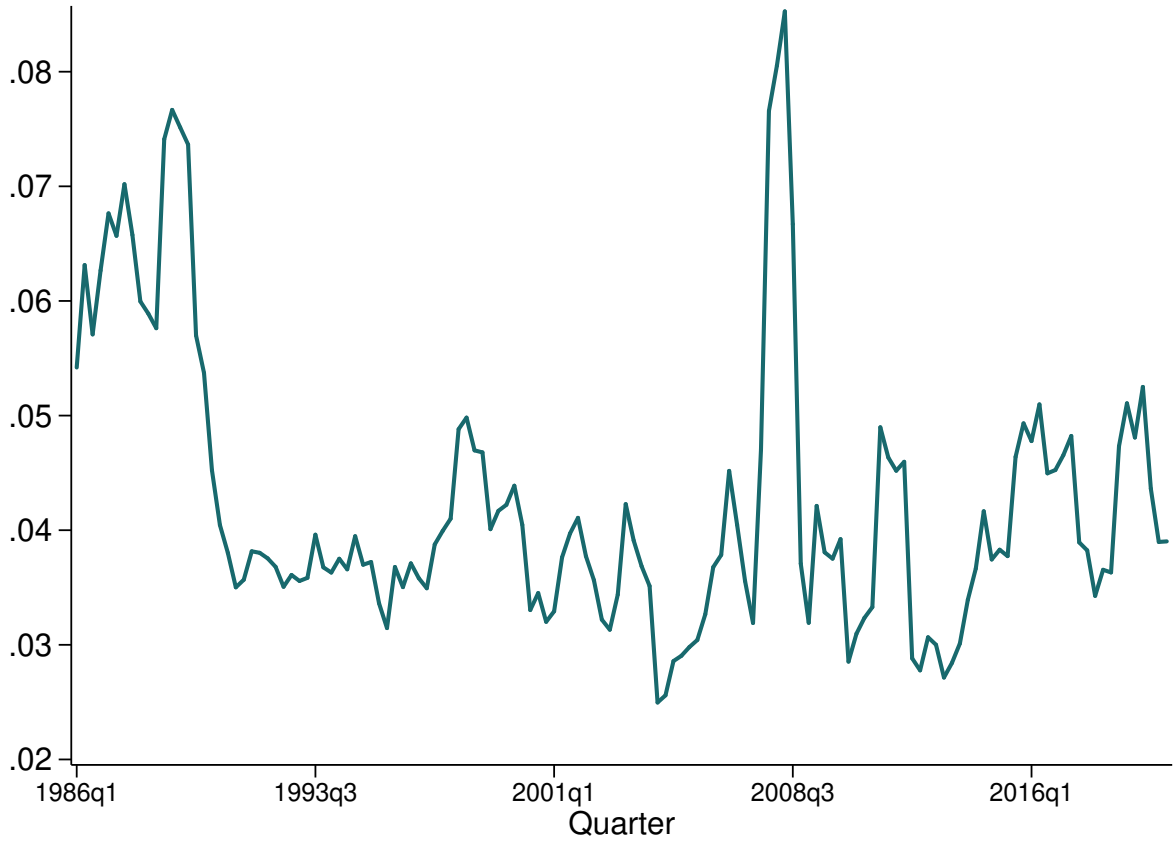


Figure II: Effects of Changes of Control on Injury Rates of Target Mines

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.

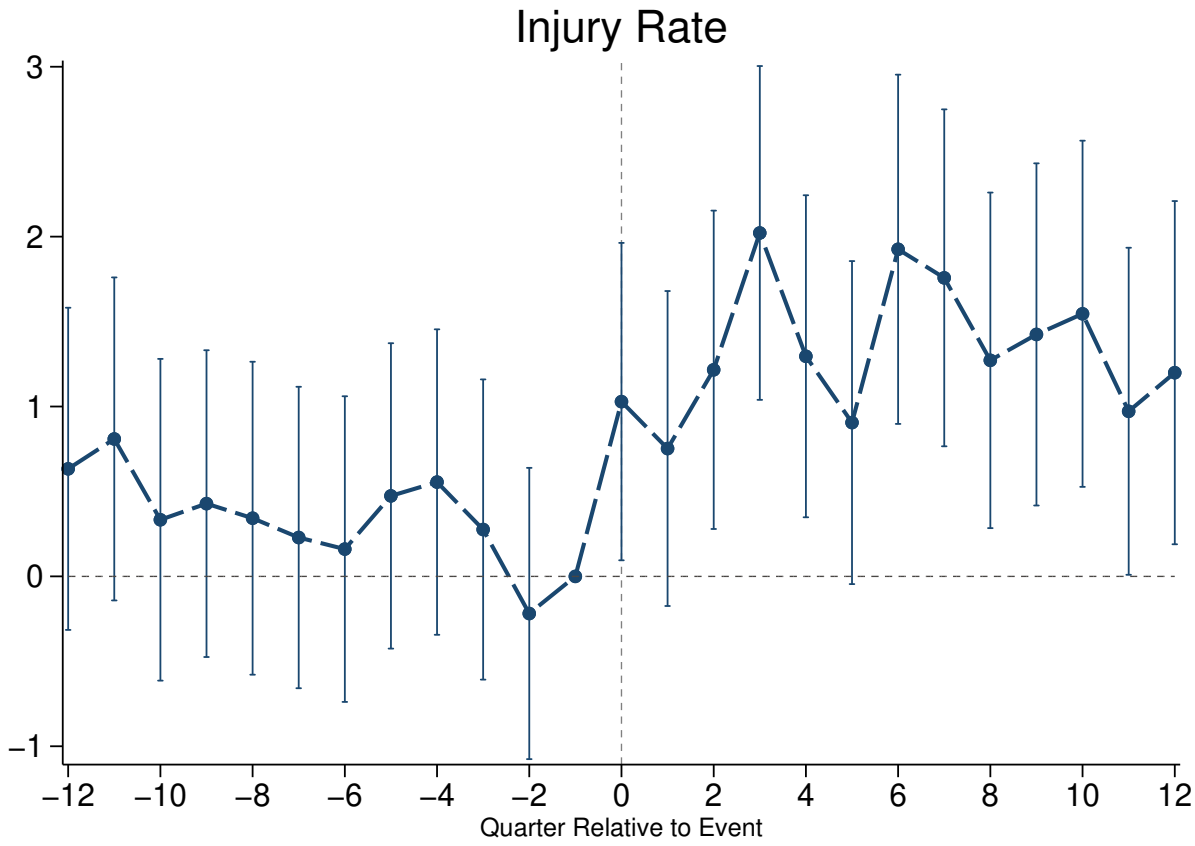


Figure III: Effects of Changes of Control on Closure Probability of Target Mines

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

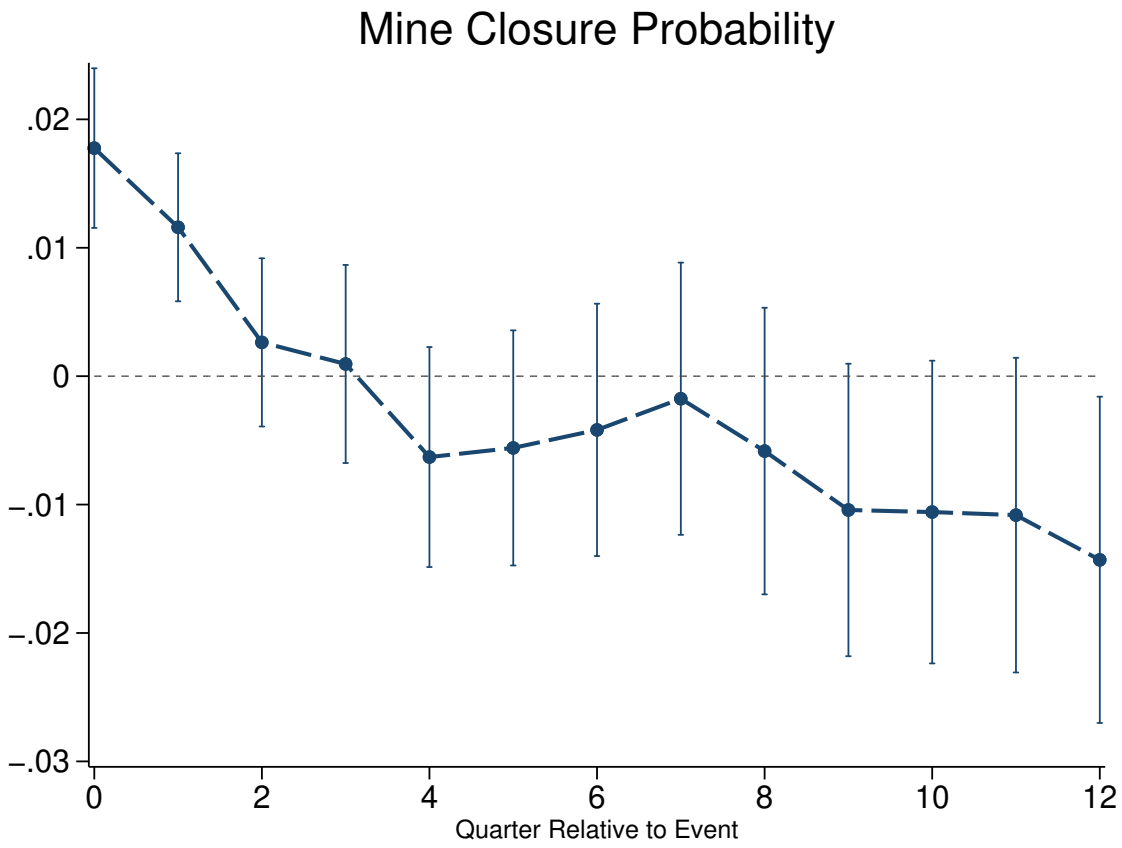


Figure IV: Effects of Changes of Control on Working Intensity of Target Mines

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.

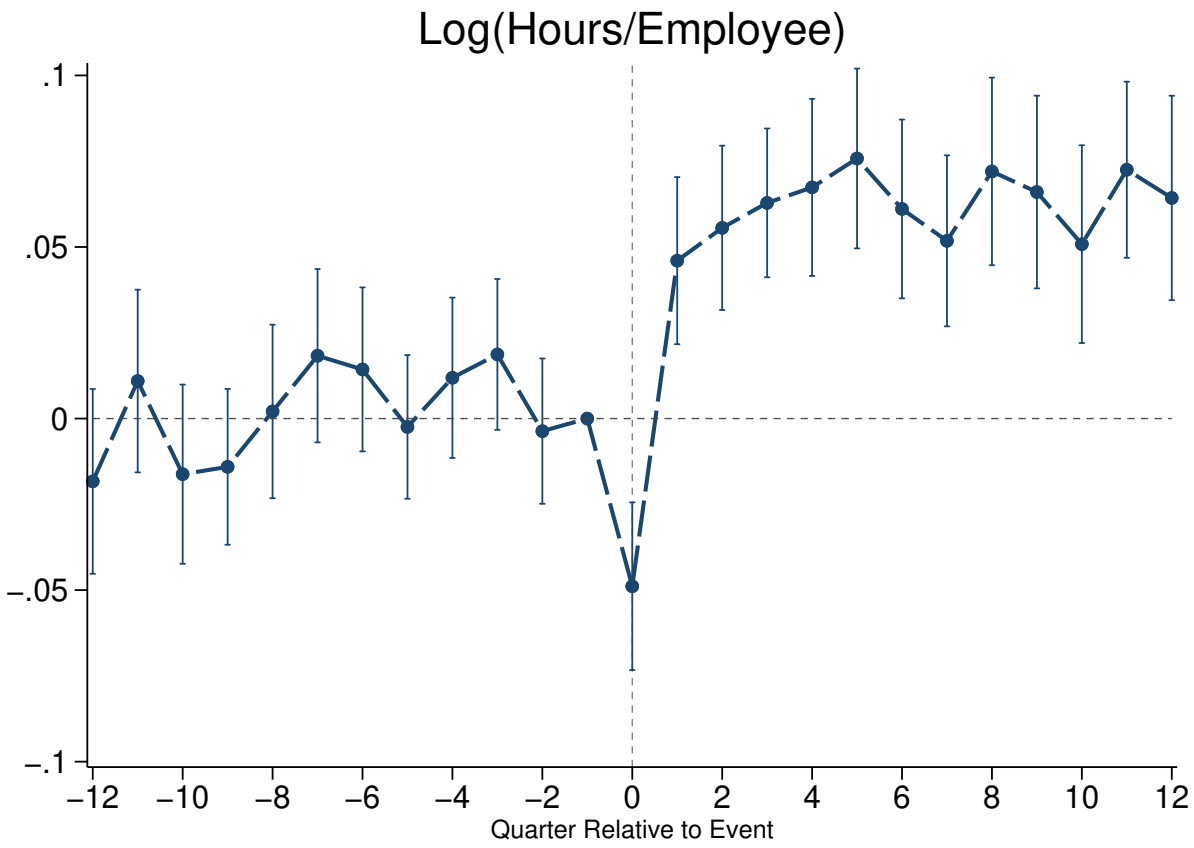


Table I
Summary Statistics of Treated and Control Mines

This table reports the means of main outcome variables in treated and control mines in quarter $t-1$ for acquisitions in quarter t and the average changes in these variables in the pre-acquisition window relative to the levels in quarter $t-1$. Control mines are mines that did not experience any change of control and their controllers did not make any acquisitions during the whole sample period between 1983Q1 and 2023Q2 and are in the same decile of the average trend of injury rate as treated mines before an acquisition. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. *Traumatic (Non-Traumatic) Injury Rate* is defined as the number of traumatic (non-traumatic) injuries per 1,000 full-time equivalent employees. *Working Intensity* is defined as working hours per employee. There are 4,836 treated mines and 17,497 control mines in the sample. Δ means the difference between the average value of a variable between $t-12$ and $t-2$ and the value of a variable in $t-1$. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Treated	**Control**	**p-value of (1)-(2)**
	(1)	(2)	(3)
Injury Rate	6.119	4.765	<0.01
Traumatic Injury Rate	1.786	1.277	<0.01
Non-Traumatic Injury Rate	4.333	3.488	<0.01
Working Intensity	411.046	359.309	<0.01
Δ Injury Rate	0.075	-0.048	0.591
ΔTraumatic Injury Rate	0.094	-0.005	0.353
ΔNon-Traumatic Injury Rate	-0.020	-0.043	0.908
ΔLog(Working Intensity)	0.017	0.027	0.394

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

This table reports the baseline results on the estimated effects of control transfers on the injury rates of target mines. Columns (1) and (2) report the average and dynamic treatment effect, respectively. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. For acquisitions in cohort t, *Trend Decile* is measured as decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Treated×Post	1.007***	
	[0.191]	
Treated×Post(-12)		0.633
		[0.484]
Treated×Post(-11)		0.809*
		[0.485]
Treated×Post(-10)		0.333
		[0.483]
Treated×Post(-9)		0.428
		[0.461]
Treated×Post(-8)		0.342
		[0.470]
Treated×Post(-7)		0.229
		[0.453]
Treated×Post(-6)		0.161
		[0.459]
Treated×Post(-5)		0.474
		[0.459]
Treated×Post(-4)		0.555
		[0.459]
Treated×Post(-3)		0.276
		[0.451]
Treated×Post(-2)		-0.219
		[0.437]
Treated×Post(0)		1.029**
		[0.477]
Treated×Post(+1)		0.753
		[0.473]
Treated×Post(+2)		1.216**
		[0.478]
Treated×Post(+3)		2.022***
		[0.501]
Treated×Post(+4)		1.296***
		[0.484]
Treated×Post(+5)		0.905*
		[0.485]
Treated×Post(+6)		1.925***
		[0.524]
Treated×Post(+7)		1.757***
		[0.506]
Treated×Post(+8)		1.272**
		[0.504]
Treated×Post(+9)		1.424***
		[0.514]
Treated×Post(+10)		1.546***
		[0.520]
Treated×Post(+11)		0.972**
		[0.491]
Treated×Post(+12)		1.199**
		[0.515]
Cohort×Mine FEs	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓
Adjusted R^2	0.163	0.163
N	10,906,650	10,906,650

Table III
Heterogeneous Effects by Types of Injury and Changes of Control

This table reports the heterogeneous treatment 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. Columns (3) and (4) report the estimated effects on injury rates for asset sales and mergers, respectively. Columns (5) and (6) report the estimated effects on injury rates for acquisitions in which mine operators do not change and operators change, respectively. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. *Traumatic (Non-Traumatic) Injury Rate* is defined as the number of traumatic (non-traumatic) injuries per 1,000 full-time equivalent employees. For acquisitions in cohort t, *Trend Decile* is measured as decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Traumatic Injury Rate (1)	Non-Traumatic Injury Rate (2)	Asset Sales (3)	Mergers (4)	Not Change Operators (5)	Change Operators (6)
Treated×Post	0.288***	0.719***	0.947***	1.259***	0.626**	1.292***
	[0.071]	[0.155]	[0.214]	[0.406]	[0.243]	[0.279]
Cohort×Mine FEs	✓	✓	✓	✓	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.096	0.137	0.163	0.163	0.163	0.163
N	10,906,650	10,906,650	10,883,886	10,809,973	10,838,501	10,855,326

Table IV
Effects of Changes of Control on Closure Probability of Target Mines

This table reports the estimated effects of changes of control on the closure probability of target mines. For acquisitions in quarter t, the estimation window is $[t,t+1]$. Column (1) reports the result using all mines in the sample. Column (2) reports results using coal mines. Columns (3)-(6) report heterogeneous results by the operational efficiency of a mine, which is defined as the coal production per hour of labor in quarter $t-1$. For acquisitions in cohort t, *Trend Decile* is measured as decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	All Mines	Coal Mines	Efficiency Q1	Efficiency Q2	Efficiency Q3	Efficiency Q4
	(1)	(2)	(3)	(4)	(5)	(6)
Treated×Post	0.022***	0.053***	0.075***	0.058**	0.049**	0.018
	[0.004]	[0.011]	[0.024]	[0.023]	[0.024]	[0.022]
Cohort×Trend Decile FE	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.013	0.005	-0.008	-0.003	-0.003	-0.009
N	376,651	36,737	9,360	8,327	9,648	9,229

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

Panel A of this table reports the results on the estimated effects of control transfers 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. Panel B of this table reports the heterogeneity results by working intensity, defined as working hours per employee, one quarter before an acquisition. *Working Intensity* is defined as working hours per employee. For acquisitions in cohort t, we sort mines into quartiles (Q1, Q2, Q3, and Q4) based on working intensity in quarter $t-1$. In each subsample, we report the point estimation relative to the pre-treatment mean of the treated mines. *Trend Decile* is measured as the decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Panel A: Baseline	
	(1)	(2)
Treated×Post	0.050***	
	[0.007]	
Treated×Post(-12)		-0.018
		[0.014]
Treated×Post(-11)		0.011
		[0.014]
Treated×Post(-10)		-0.016
		[0.013]
Treated×Post(-9)		-0.014
		[0.012]
Treated×Post(-8)		0.002
		[0.013]
Treated×Post(-7)		0.018
		[0.013]
Treated×Post(-6)		0.014
		[0.012]
Treated×Post(-5)		-0.002
		[0.011]
Treated×Post(-4)		0.012
		[0.012]
Treated×Post(-3)		0.019*
		[0.011]
Treated×Post(-2)		-0.004
		[0.011]
Treated×Post(0)		-0.049***
		[0.012]
Treated×Post(+1)		0.046***
		[0.012]
Treated×Post(+2)		0.056***
		[0.012]
Treated×Post(+3)		0.063***
		[0.011]
Treated×Post(+4)		0.067***
		[0.013]
Treated×Post(+5)		0.076***
		[0.013]
Treated×Post(+6)		0.061***
		[0.013]
Treated×Post(+7)		0.052***
		[0.013]
Treated×Post(+8)		0.072***
		[0.014]
Treated×Post(+9)		0.066***
		[0.014]
Treated×Post(+10)		0.051***
		[0.015]
Treated×Post(+11)		0.073***
		[0.013]
Treated×Post(+12)		0.064***
		[0.015]
Cohort×Mine FEs	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓
Adjusted R^2	0.677	0.677
N	10,906,650	10,906,650

	Panel B: Heterogeneity by Working Intensity			
	Q1	Q2	Q3	Q4
	(1)	(2)	(3)	(4)
Treated×Post	0.248***	0.097**	0.079**	0.087**
	[0.085]	[0.042]	[0.035]	[0.037]
Cohort×Mine FEs	✓	✓	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓	✓	✓
Adjusted R^2	0.152	0.172	0.172	0.151
N	3,803,745	3,008,295	2,258,575	1,926,721

Table VI
Heterogeneous Effects by Changes in Controller Workplace Safety Practices

This table reports the heterogeneous estimated effects on injury rates by the changes in controller workplace safety practices, which is proxied by changes in the controller fixed effects. All estimates are relative to the pre-treatment mean of injury rates of treated mines. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. For acquisitions in cohort t, we sort treated mines into quartiles (Q1, Q2, Q3, and Q4) based on the changes in the controller fixed effects. In each subsample, we report the point estimation relative to the pre-treatment mean of the treated mines. *Trend Decile* is measured as decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. 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	0.063	0.116	0.260***	0.745***
	[0.047]	[0.074]	[0.090]	[0.146]
Cohort×Mine FEs	✓	✓	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓	✓	✓
Adjusted R^2	0.163	0.163	0.163	0.163
N	10,887,792	10,885,501	10,884,652	10,883,390

Table VII
Heterogeneous Effects by Predicted Changes in Local Monopsony Power

This table reports the heterogeneous estimated effects on injury rates by predicted changes in local monopsony power, proxied by employment-based HHI driven by acquisitions made by controllers in a commuting zone ($\widehat{\Delta HHI}$). All estimates are relative to the pre-treatment mean of injury rates of treated mines. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. For acquisitions in cohort t, we split the treated mines into quartiles (Q1, Q2, Q3, and Q4) of $\widehat{\Delta HHI}$. In each subsample, we report the point estimation relative to the pre-treatment mean of the treated mines. *Trend Decile* is measured as the decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. 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	0.062*	0.082	0.102**	0.220***
	[0.032]	[0.116]	[0.049]	[0.038]
Cohort×Mine FEs	✓	✓	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓	✓	✓
Adjusted R^2	0.163	0.163	0.163	0.163
N	10,948,680	10,883,283	10,891,783	10,907,071

Table VIII
Heterogeneous Effects by Employees vs. Contractors

This table reports the heterogeneous estimated effects on injury rates by whether a mine hired contractors one quarter before acquisitions. All estimates are relative to the pre-treatment mean of injury rates of treated mines. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. In each subsample, we report the point estimation relative to the pre-treatment mean of the treated mines. For acquisitions in cohort t, *Trend Decile* is measured as the decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	No Contractors	Has Contractors
	(1)	(2)
Treated×Post	0.199***	-0.006
	[0.033]	[0.027]
Cohort×Mine FEs	✓	✓
Cohort×Trend Decile×Time FEs	✓	✓
Adjusted R^2	0.164	0.164
N	8,915,678	1,990,972

Table IX
Additional Results

This table reports additional results on the estimated effects of changes of control on the injury rates of target mines. Column (1) reports the estimated effect using the synthetic difference-in-differences (Synthetic DiD) estimator in Arkhangelsky et al. (2021) and the 99% confidence interval. The Synthetic DiD estimator requires balanced panel data. Column (2) 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 (3) reports the results after controlling for Cohort×Commodity×Time fixed effects. Column (4) reports the estimated effect of change in mine operators on injury rates conditional on no changes of control. Column (5) reports the estimated effect on the number of violations of safety regulations, estimated using a Poission regression. Column (6) reports the estimated effects on hourly wages at the commuting zone (CZ)×time level. *Injury Rate* is defined as the number of injuries per 1,000 full-time equivalent employees. For acquisitions in cohort t, *Trend Decile* is measured as decile of the average difference in a mine's injury rates from each of the 11 quarters leading up to t and the injury rate in the immediate pre-treatment period $t-1$. Standard errors in brackets are clustered at the mine level. ***, **, and * represent significance at 1%, 5%, and 10% levels, respectively.

	Synthetic DiD (1)	Cleaner Controls (2)	Control for Demand Shocks (3)	Placebo Test (4)	Number of Violations (5)	CZ-Level Hourly Wages (6)
Treated×Post	0.897***	1.409***	0.994***	-0.022	0.095***	0.010
	[0.025]	[0.280]	[0.198]	[0.074]	[0.021]	[0.055]
Cohort×Mine FEs		✓	✓	✓	✓	✓
Cohort×Trend Decile×Time FEs		✓	✓	✓	✓	
Cohort×Commodity×Time FEs			✓			
Cohort×Time FEs						✓
Adjusted R^2		0.196	0.166	0.144		0.814
N	5,225,375	632,561	10,906,650	10,165,055	3,830,426	39,020

APPENDIX FIGURES AND TABLES

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.
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.

Figure A.1: Effects of Changes of Control on Traumatic Injury Rates of Target Mines

This figure reports the dynamic treatment effects of changes of control on traumatic 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.

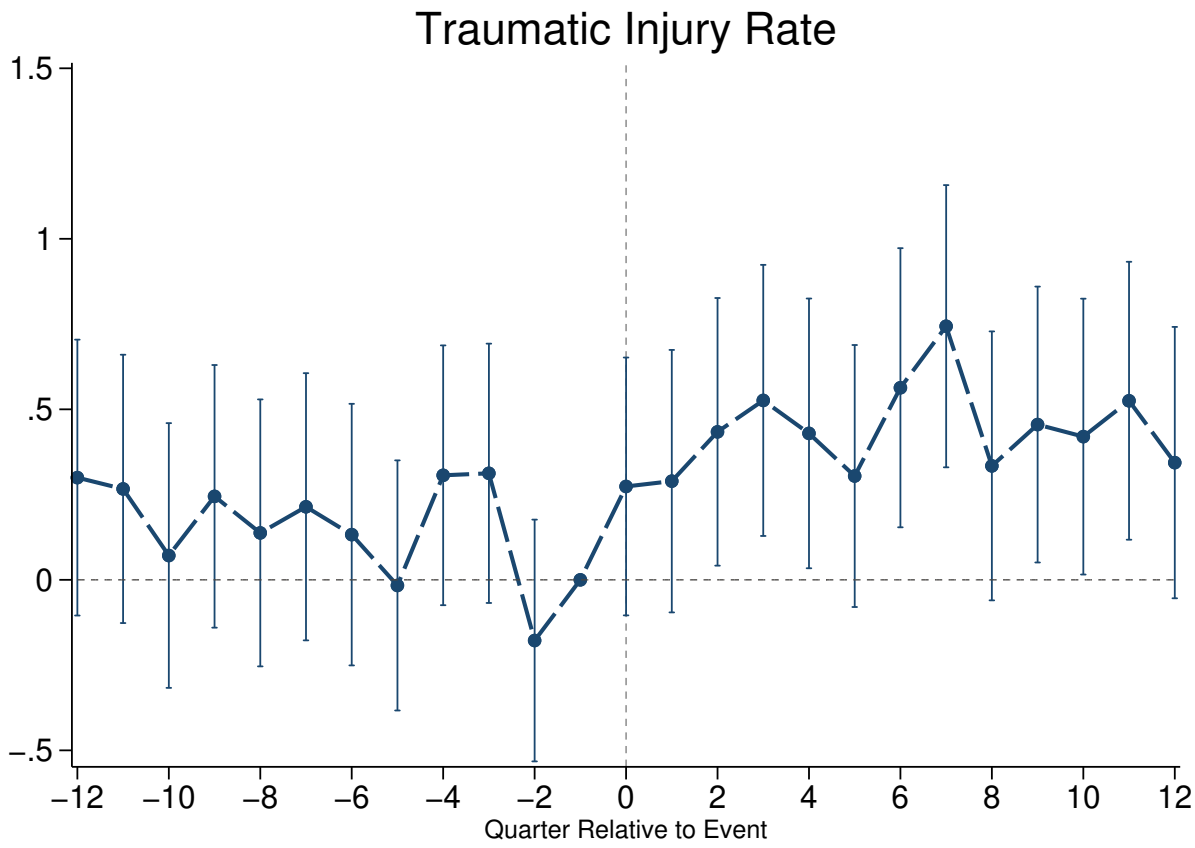


Figure A.2: Effects of Changes of Control on Non-Traumatic Injury Rates of Target Mines

This figure reports the dynamic treatment effects of changes of control on non-traumatic 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.

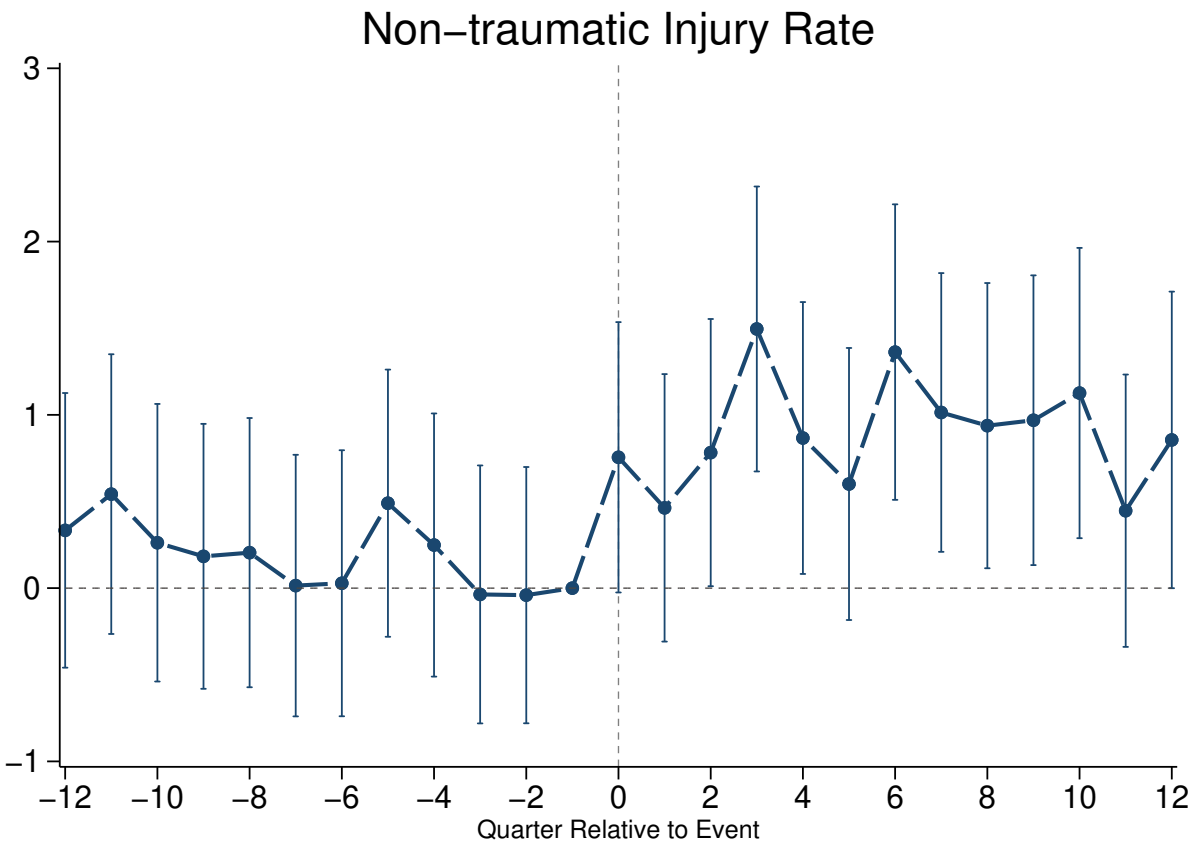


Figure A.3: Heterogeneous Effects by Types of Acquisitions

This figure reports the dynamic treatment effects of changes of control on the injury rates of target mines by mergers vs. asset sales acquisitions. In each subsample, the point estimates are relative to the pre-treatment mean of treated mines. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

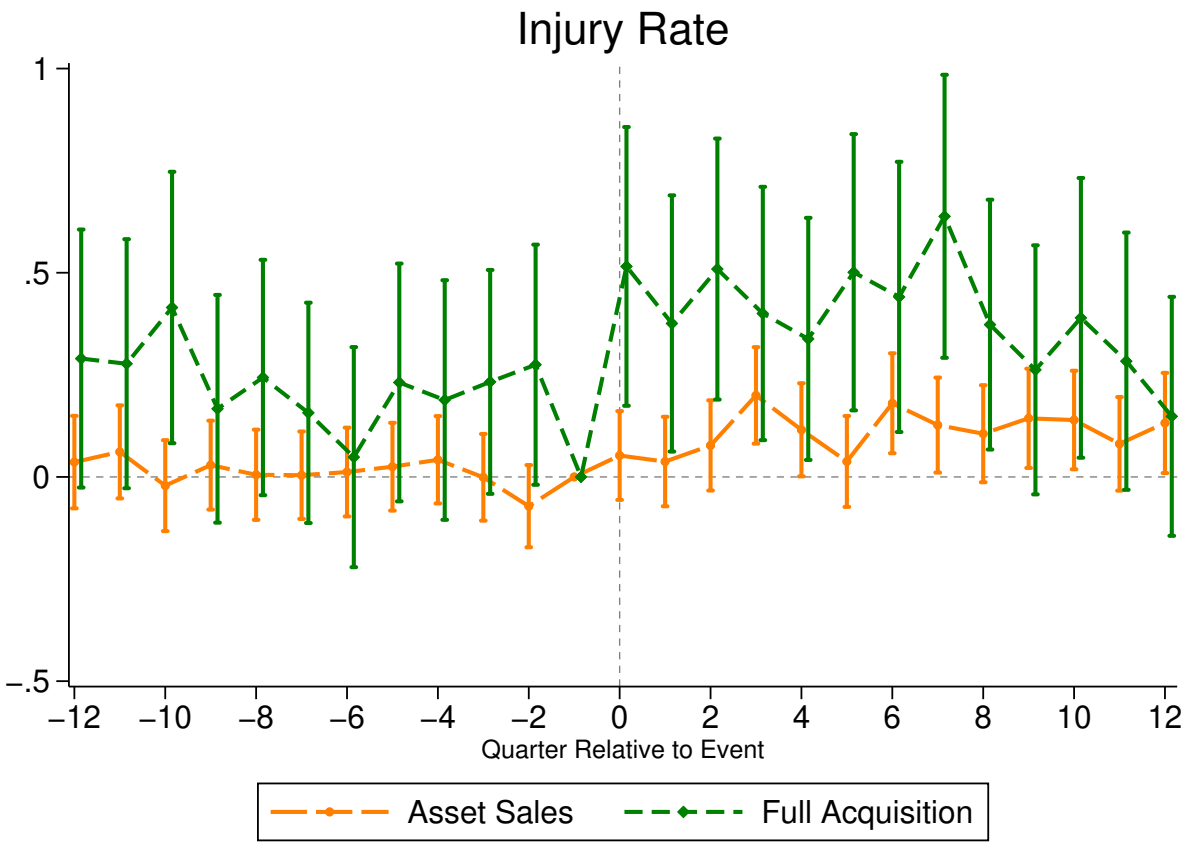


Figure A.4: Heterogeneous Effects by whether Operators Change

This figure reports the dynamic treatment effects of changes of control on the injury rates of target mines by whether operators change after acquisitions. In each subsample, the point estimates are relative to the pre-treatment mean of treated mines. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

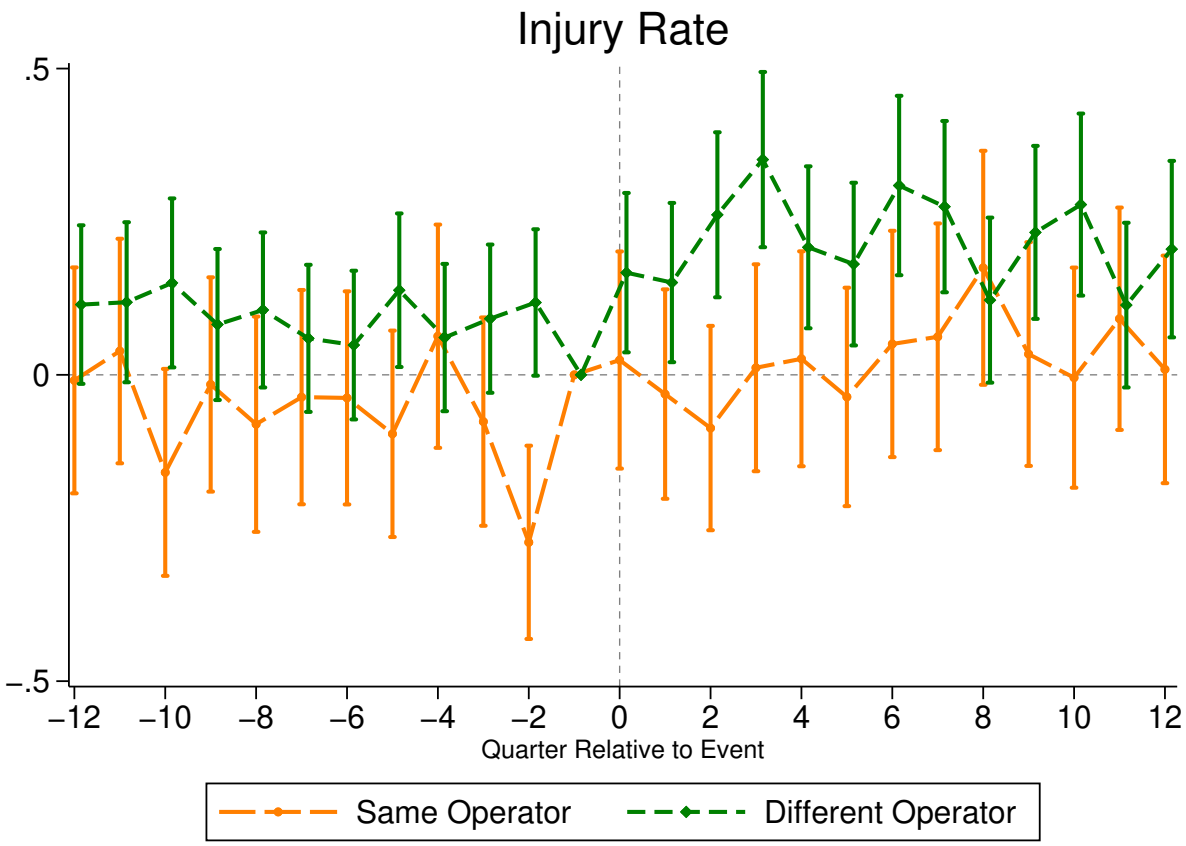


Figure A.5: Heterogeneous Effects by Employees vs. Contractors

This figure reports the dynamic treatment effects of changes of control on the injury rates of target mines by whether a mine hires any contractors in the quarter before an acquisition. In each subsample, the point estimates are relative to the pre-treatment mean of treated mines. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

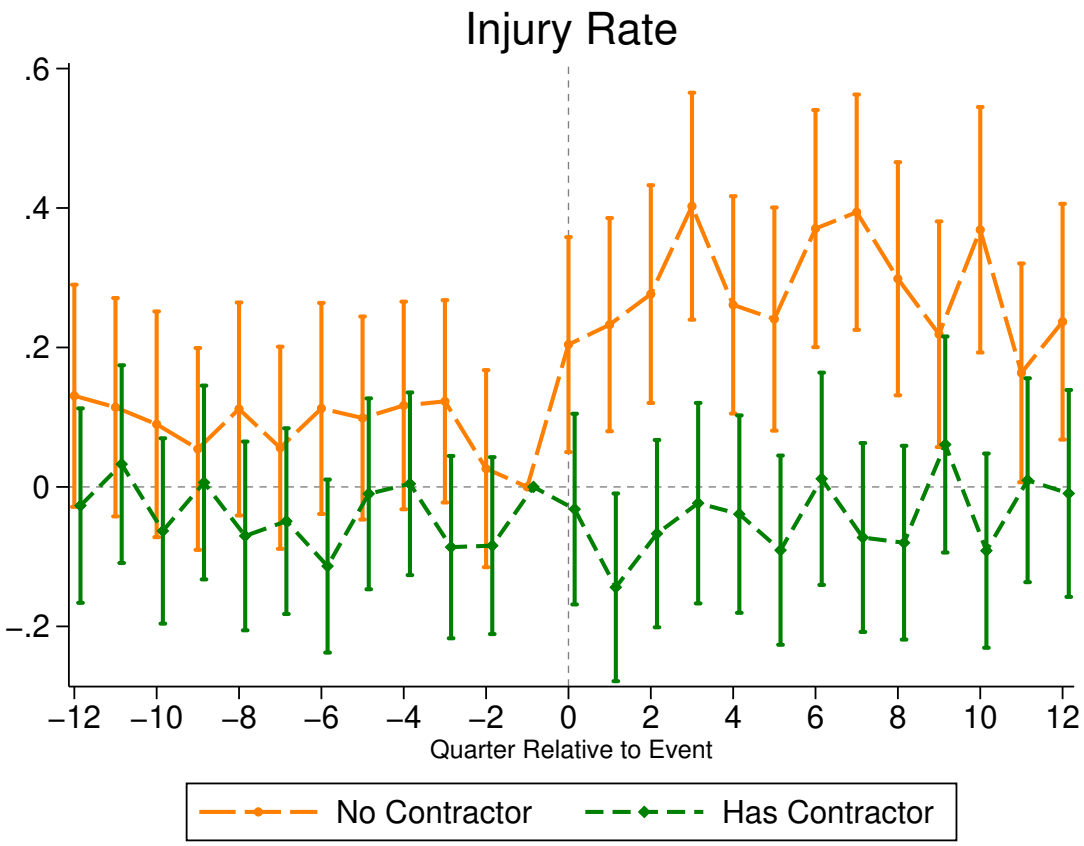


Figure A.6: Synthetic Difference-in-Differences

This figure reports the dynamic treatment effects of changes of control on the injury rates of target mines using the synthetic difference-in-differences estimator in Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021). The bars around point estimates represent 95% confidence intervals, and they are based on bootstrapped standard errors with 300 resamplings.

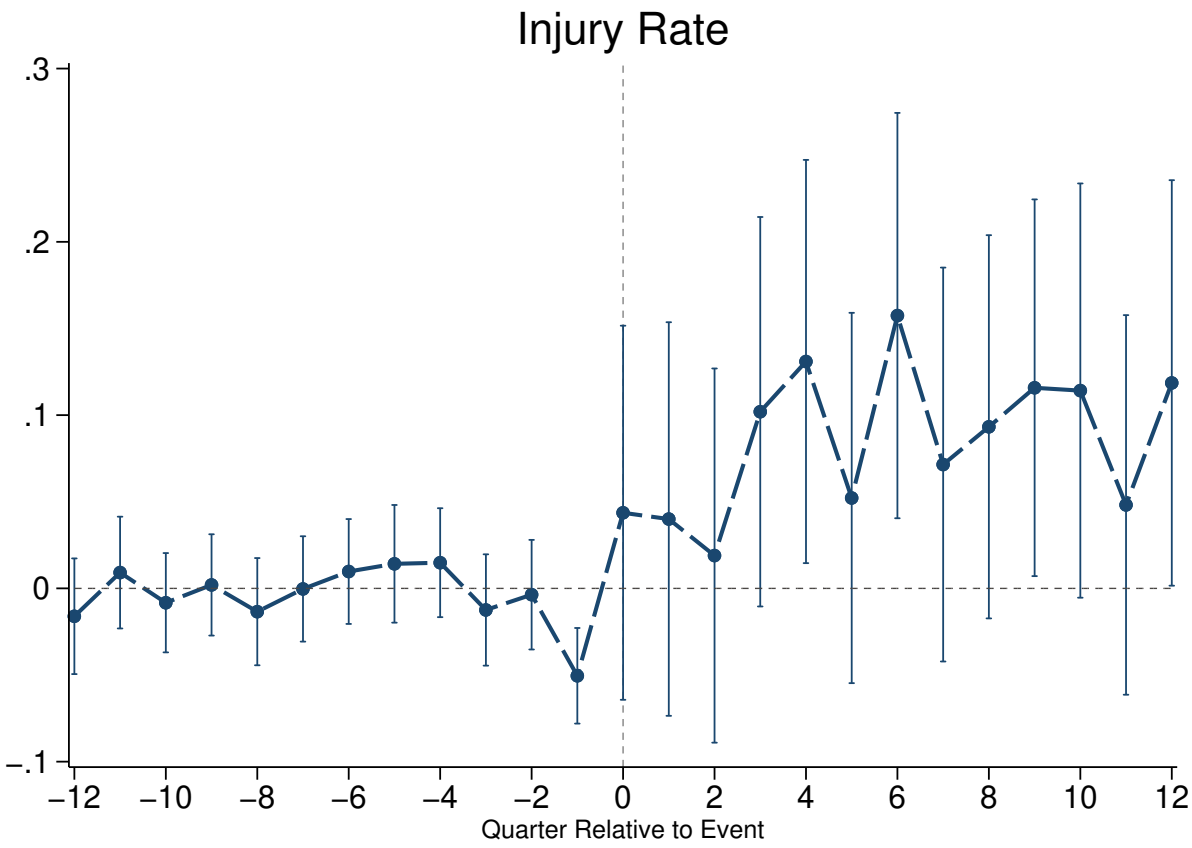


Figure A.7: Use Cleaner Control Mines

This figure reports the spillover effects of changes of control. Cleaner control mines are those in commuting zones where there were no mines involved in any acquisition within the estimation window. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

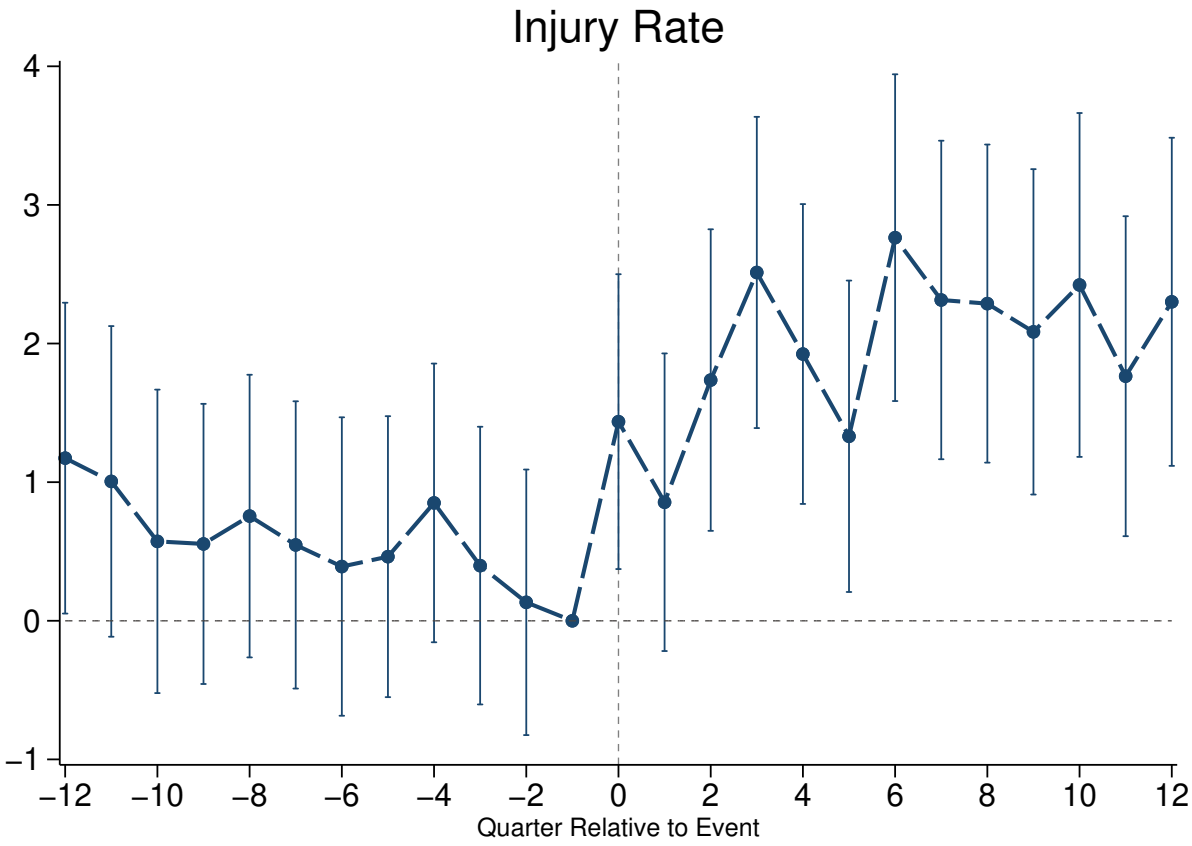


Figure A.8: Control for Demand Shocks

This figure reports the dynamic treatment effects of changes of control on injury rates after controlling for demand shocks. We include the cohort×commodity×year×quarter fixed effects in regressions. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

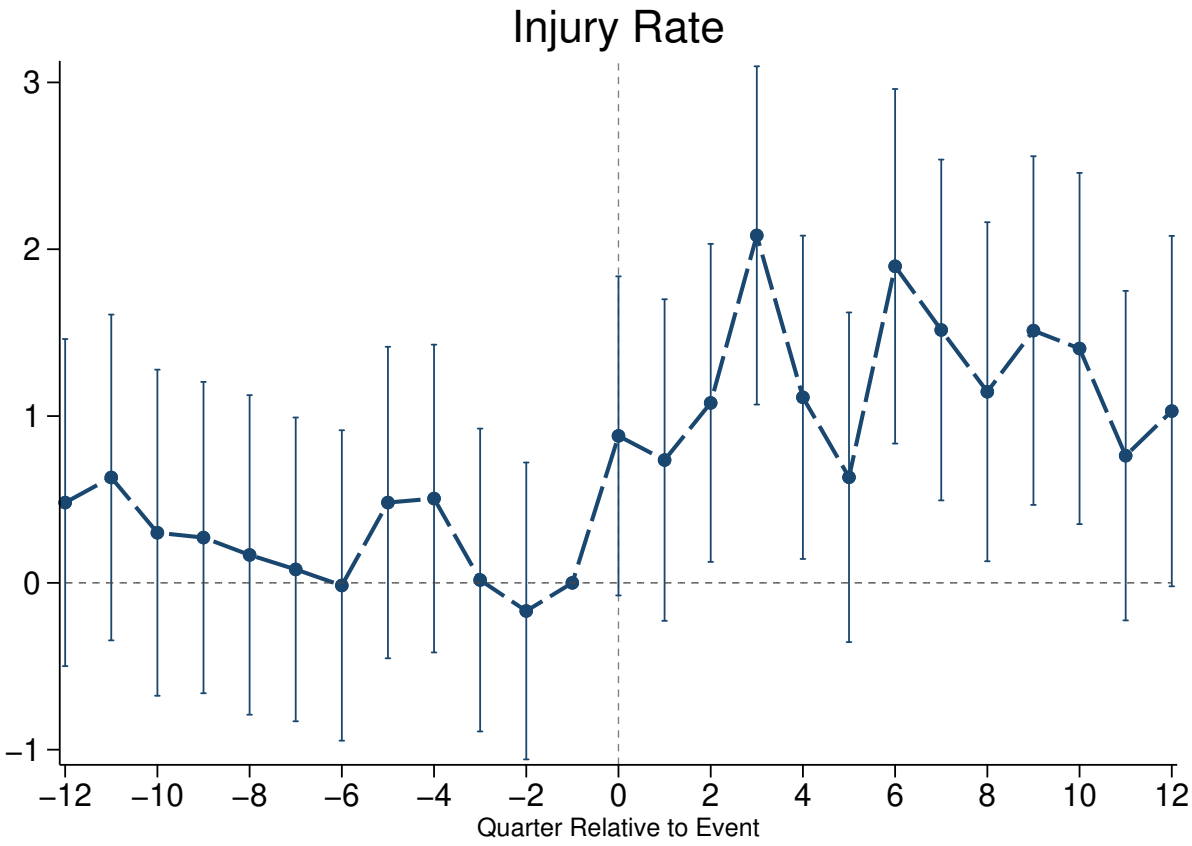


Figure A.9: A Placebo Test

This figure reports the dynamic treatment effects of operator changes on the injury rates of mines conditional on no changes of control. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

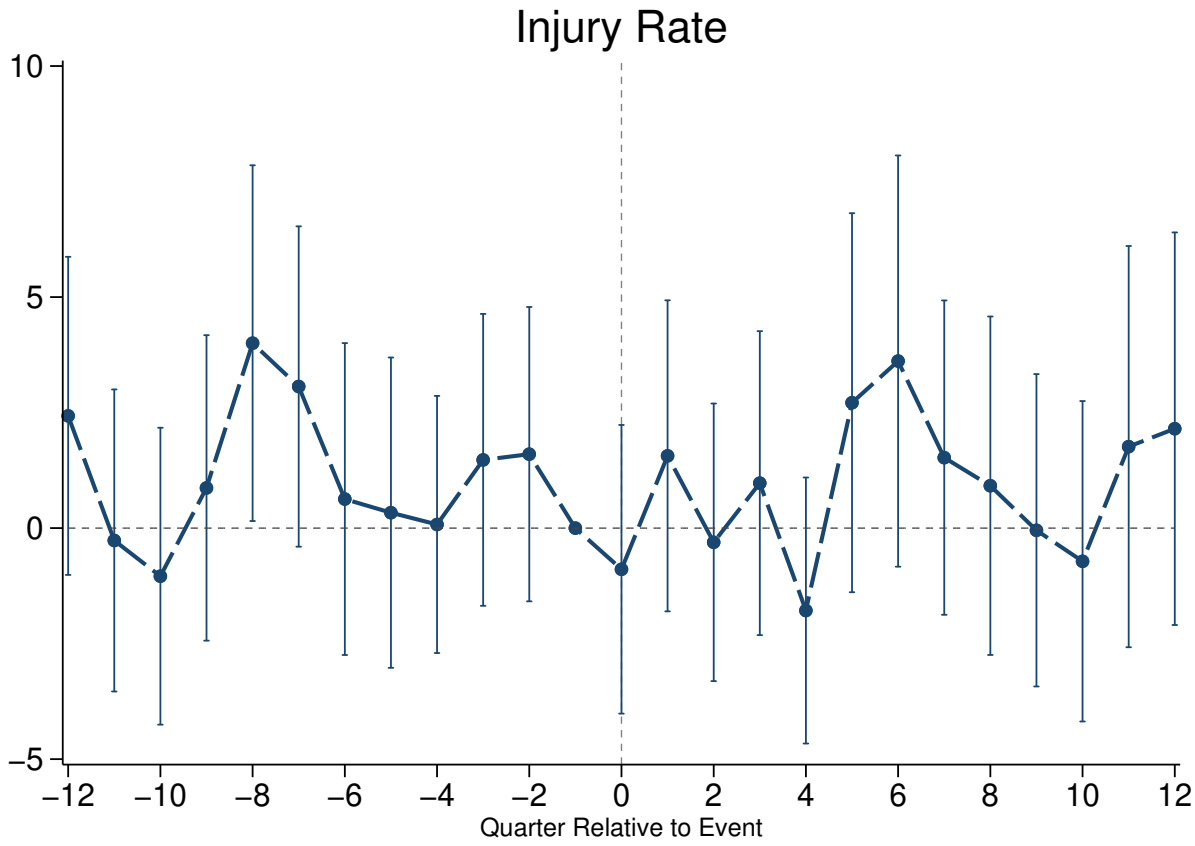


Figure A.10: Effects of Changes of Control on the Number of Violations

This figure reports the effects of changes of control on the number of violations. We estimate the effects using Poisson regression. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

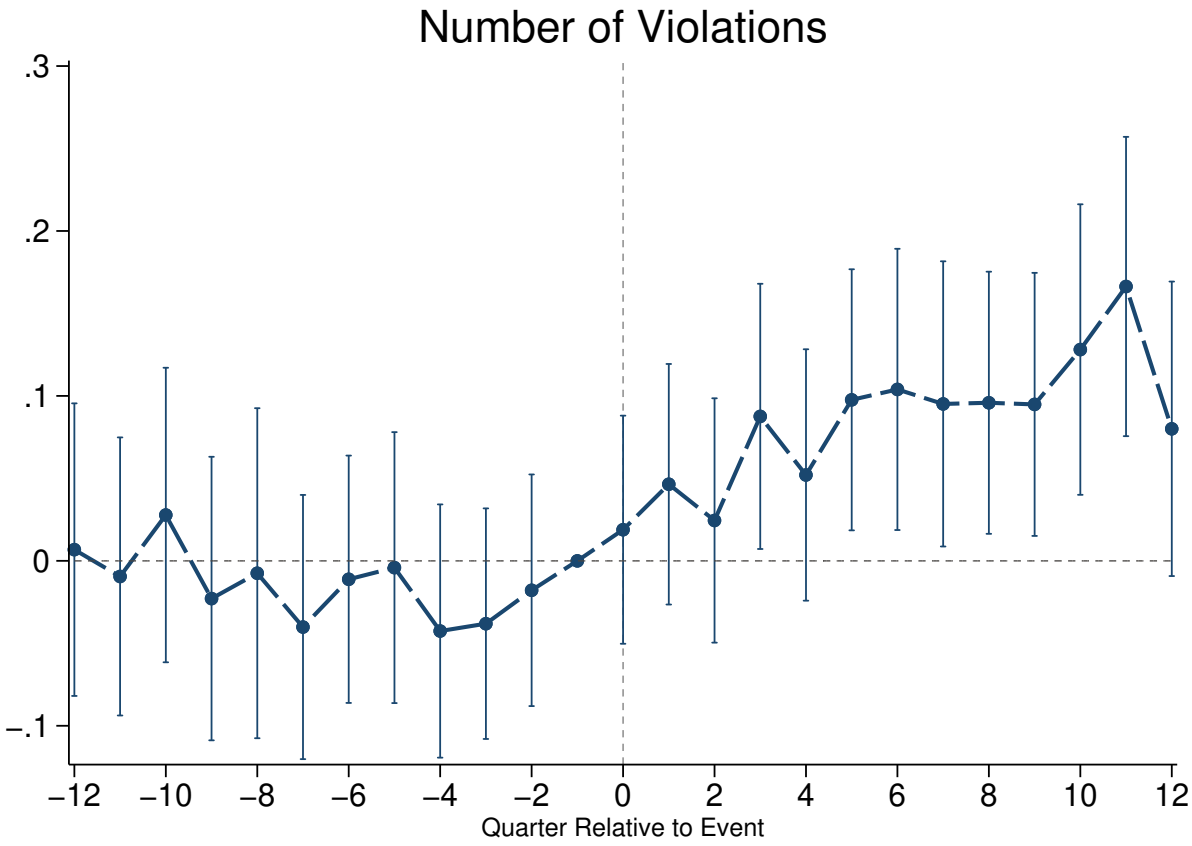


Figure A.11: Effects of Changes of Control on Commuting Zone-Level Hourly Wages

This figure reports the effects of changes of control on the commuting zone-level hourly wages. The bars around point estimations represent 95% confidence intervals, and they are based on standard errors clustered at the mine level.

