Mandatory CSR Disclosures and Real-Asset Regulatory Leakage

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Abstract: How concerning is real-asset regulatory leakage after corporate social responsibility (CSR) disclosure mandates? To address this question, we examine changes in firms' real-asset investment decisions and safety outcomes following the 2010 Dodd-Frank Section 1503 (DF1503) requirement to disclose mine-safety records in SEC filings. Using data on publicly owned (regulated) and privately owned (unregulated) mines from 2000 to 2021, we find that, after DF1503, the share of the least-safe US mines owned by public firms decreases by 36%. We estimate that 74% of this reduction reflects genuine mine-level safety improvements, while the remaining 26% is attributable to real-asset regulatory leakage—that is, ownership transfers and the avoidance of new investments in high-safety-risk mines. Most of the reported safety gains attributable to leakage (90%) stem from transferring inherently high-risk mines to unregulated firms, rather than from subsequent declines in safety under private ownership, which are relatively minor (10%). Overall, our findings suggest that, while roughly one-quarter of the observed safety improvement following DF1503 results from the reallocation of high-CSR-risk assets, the modest declines in CSR performance of the leaked assets were more than offset by safety improvements in the retained assets, consistent with an economy-wide CSR improvement. Nonetheless, overlooking the nontrivial shift of high-risk assets to the private sector may overstate the true extent of CSR gains.

Keywords: Real effects, regulatory leakage, Dodd-Frank Act, mine safety, ESG, corporate social responsibility.

JEL Classification: D03, G14, G18, G38, I18, J28, K22, K32, L71, L72, M41, M48

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1. Introduction

Regulations mandating corporate social responsibility (CSR) disclosures in regulatory filings have become increasingly common, with the EU's 2022 Corporate Sustainability Reporting Directive and the SEC's 2024 Climate Disclosure Rule being prominent recent examples. Policymakers hope that the wide dissemination and intense scrutiny of firms' financial reports will pressure companies to reduce socially undesirable activities. However, a common concern is that, when applied to only a subset of firms, such as public companies, CSR disclosure mandates may prompt regulated firms to minimize the regulation's impact by selling high-CSR-risk assets or leaving undesirable investment opportunities to unregulated firms. We describe this phenomenon as *real-asset regulatory leakage* because, while the assets remain in operation, they fall outside the scope of the regulation. If the transferred assets perform worse on CSR metrics when operated by unregulated firms, regulatory leakage could create a misperception of true economy-wide CSR performance and ultimately reduce the effectiveness of CSR disclosure mandates. In this study, we decompose reported improvements in CSR performance following a mandatory disclosure regulation into two sources: genuine improvements in CSR and those stemming from real-asset regulatory leakage. While we expect both factors contribute to better reported safety, understanding their relative impact is crucial for assessing the overall effectiveness of CSR disclosure mandates in reducing undesirable corporate behavior.

As a setting, we use the 2010 introduction of mandatory mine-safety disclosures in SEC-registered firms' regulatory filings under Dodd-Frank Act Section 1503 (DF1503). This setting has three key features that allow us to comprehensively evaluate firm responses to a CSR disclosure mandate. First, the information required under DF1503 was already publicly available on a government website, helping us estimate the incremental effects of including this information in regulatory filings (see Section 2 for details). Second, while CSR disclosure mandates have recently gained in popularity, DF1503 has been in effect for over a decade, providing a relatively long post-

regulation period to assess potentially slow-to-materialize or transitory corporate responses. Third, we have high-quality, mine-level data on labor-safety outcomes (i.e., violations and injuries), labor inputs (i.e., hours worked), and ownership, covering both private and publicly owned mines. Such detailed information is rarely available in other settings, where estimates of CSR performance are often subjective (e.g., when based on carbon emissions). Additionally, in other contexts, analyses of CSR performance around ownership changes are often limited to financially material transactions, such as subsidiary sales (e.g., Ecker and Keeve 2023; Duchin et al. 2024). In contrast, we have comprehensive data on all ownership changes for U.S. mines.

Using data from the Mine Safety and Health Administration (MSHA) on safety outcomes and mine ownership from 2000 to 2021, we identify the least safe US mines based on MSHA safety citations and serious injuries. We classify a mine as *Unsafe* (*Safe*) if it experiences a serious injury in a given year or if it ranks (does not rank) in the top 10% of all mines each year based on the number of significant and substantial (S&S) violations.[1]

Figure 1 plots the percentage of all mines owned by firms subject to the DF1503 regulation ("DF1503 firms" or "public firms") alongside the percentage of *Unsafe* mines owned by both DF1503 and non-DF1503 ("private") firms from 2002 to 2021. After the enactment of Dodd-Frank in 2010, the share of *Unsafe* mines owned by DF1503 firms declines significantly—from around 12% in 2009 to 5% in 2021. In contrast, the share of *Unsafe* mines owned by non-DF1503 firms increases slightly, from about 10% to 12%. Notably, the overall share of mines owned by DF1503 firms remains relatively constant, indicating that the reduction in unsafe mines is not driven by a decline in public firms' total mine ownership of U.S. mines. Our objective in this paper is to estimate how much of the

[1] We use a relative measure (top 10%) for citations, but not injuries, because the criteria for issuing citations may shift over time due to changes in enforcement intensity or regulatory discretion, making comparisons over time less informative. Serious injuries, however, are defined based on consistent, observable outcomes and are therefore more comparable across time.

post-DF1503 decline in public firm ownership of unsafe mines is attributable to genuine safety improvements versus regulatory leakage.

We examine three related but distinct aspects of mine safety changes around DF1503—1) firm-level reported safety and mine-level safety under 2) fixed-2009 and 3) time-varying treatment assignments—and compare these estimates to assess the relative magnitudes of safety improvements and regulatory leakage. *Firm-level safety* refers to the safety measures reported in a firm's SEC filings for mines owned by DF1503 firms (as shown in Figure 1). Firm-level reported safety reflects all the possible actions a firm could take to improve its disclosed safety levels, including genuine safety improvements in the form of operational changes and shutdowns of unsafe mines and regulatory leakage in the form of selling unsafe mines or opening fewer new mines in high-safety-risk locations. Our difference-in-differences (DID) regression estimation indicates that, for public firms, reported firm-level safety increases by approximately 36% after DF1503. We find little evidence that public firms were more likely to shut down unsafe mines after DF1503, suggesting that nearly all of the estimated safety improvement is attributable to safety improvements of continuously operated mines.

To estimate *mine-level safety*, we incorporate mine fixed effects into our baseline regression framework. This approach excludes safety changes resulting from the transfer of inherently unsafe mines between DF1503 and non-DF1503 firms and any effects from new mine openings from the estimation.[2] As a result, this analysis reflects only actual safety improvements made by public firms and safety changes following ownership changes between public and private firms. We estimate two mine-level specifications: (1) a *fixed-2009* treatment specification, which keeps both the sample and a mine's classification as public (i.e., treated) constant as of 2009 (the year before DF1503); and (2) a *time-varying* treatment specification, which also holds the treatment sample constant as of 2009,

[2] This is because inherent safety is a mine-level fixed effect and because mine-level safety changes cannot be estimated for mines that do not exist in the pre-DF1503 period.

but allows mines to move between the treatment and control groups. Our DiD estimates indicate that, for treated firms, mine-level safety increases by 24% under the fixed-2009 treatment specification and 26% under the time-varying specification.

While the firm-level and two mine-level safety change estimates all provide insights into different aspects of the policy, none of them can individually isolate safety improvements from regulatory leakage. However, by comparing results from the three regression specifications, we can estimate the proportion of the total reported safety change attributable to each of the two components. First, by comparing the safety change estimates from the fixed-2009 and time-varying mine-level treatment specifications, we can isolate the safety changes that occur when a mine changes from public DF1503 to private non-DF1503 ownership and assess how safety evolves under private ownership (and vice versa). While transferring *Unsafe* mines to the private sector clearly improves reported safety for public firms, the broader impact on economy-wide safety—and how concerned we should be about regulatory leakage—depends on how safety changes after the mines are transferred.

We estimate that safety declines under private ownership account for 2.6% of the total firm-level reported safety change. This deterioration in safety could result from private firms operating mines less safely or from public firms strategically selling mines they anticipate will become less safe. The former explanation is more concerning from a policy perspective, as it suggests that regulatory leakage undermines the safety benefits intended by DF1503. To distinguish between these explanations, we examine the timing of safety changes relative to the sale of a mine from a public to a private firm in the post-DF1503 period. Our results provide little support for the idea that public firms strategically sell mines in anticipation of worsening safety conditions. Instead, safety declines are apparent several years after the ownership change, suggesting private firms operate the acquired mines less safely than public firms did.

Next, we estimate safety improvements made by public firms by subtracting the safety decline under private ownership from the fixed-2009 treatment estimate. Approximately 74% of the observed firm-level safety gains are due to actual improvements. Because safety tends to deteriorate under private ownership, the mine level *fixed-2009* treatment estimate underestimates the actual improvement in safety, but modestly so—about 3.7% for the combined safety measure.

Finally, using our estimate of genuine safety improvements among DF1503 firms, we can infer the extent of reported safety improvements attributable to regulatory leakage by subtracting actual mine-level improvements from the total reported change (which reflects both genuine improvements and regulatory leakage). This calculation suggests that approximately 26.3% of the reported safety improvement is due to regulatory leakage. While the magnitude of regulatory leakage is not negligible, the share attributed to deteriorating safety under private ownership is small—accounting for only 10% of the leakage effect—implying that the worsening of safety under private ownership is far exceeded by the safety improvements made by regulated firms. However, although the decline in safety for the leaked assets is modest, overlooking the reallocation of inherently high-risk assets to unregulated private firms could lead to the perception of a larger improvement in industry safety than is warranted. The underlying safety risks associated with these operations do not disappear; they are simply reallocated to firms that fall outside the scope of the regulation.

Next, we examine the frequency and timing of the asset transfers—either through ownership changes or forgone investments—to better assess the magnitude (in terms of the number of mines) and persistence of the safety changes as well as the broader implications of regulatory leakage for how safety risks are allocated between the public and private sectors.[3] Consistent with an increased

[3] Mines that are never opened because of DF1503 do not meet our definition of real-asset regulatory leakage. By "regulatory leakage," we refer specifically to the reallocation of high-risk assets from regulated (public) firms to unregulated (private) firms, such that these assets are *in operation* but outside the scope of the regulation. Mines that are never opened due to the deterrent effects of the regulation represent a true reduction in high-risk activity prompted by the mandate, but are unobservable, and so also are not included in our estimate of genuine safety improvements.

cost of owning unsafe mines for public firms, we find that after Dodd-Frank, DF1503 firms are 1.5 times more likely to transfer a mine classified as *Unsafe* in the previous year to the private sector—an increase of approximately 11.8 additional unsafe mines sold per year. Furthermore, DF1503 firms are also less likely to open mines in unsafe areas. We classify a mine as being in an unsafe area if the percentage of violations and/or serious injuries for all mines operating within the same 5-digit zip code exceeds the 90th percentile in the prior year. After DF1503, public firms are 37% less likely to open new mines in these high-risk areas, as measured by serious injuries—equivalent to approximately 3.5 fewer unsafe mines that are instead opened by unregulated (private) firms each year. Moreover, the observed pattern of the treatment effects in these analyses aligns more closely with a sustained response to the disclosure mandate and the persistent deterrence effects of disclosure rather than a temporary increase in public scrutiny.

Taken together, these analyses indicate that, on average, 15.3 fewer *Unsafe* mines are operated by public firms each year after Dodd-Frank—an effect which could have economically consequential implications for how safety risks are allocated between the public and private sectors. However, although the magnitude of real-asset regulatory leakage after DF1503 is not inconsequential, our estimates indicate that it is far exceeded by the safety improvements made by public firms (which lead to approximately 56 fewer unsafe mines per year).

Our study contributes most directly to two streams of literature on the effects of mandatory CSR disclosures: their impact on CSR performance and their role in real-asset regulatory leakage. The main takeaway from the former is that mandatory CSR disclosures improve CSR performance, including, for example, restaurant hygiene (Jin and Leslie 2003), labor safety (e.g., Christensen et al. 2017), tax payments (Rauter 2020), pollution (Bennear and Olmstead 2008; Tomar 2023; Bonetti et al. 2024), and broader CSR outcomes (Chen et al. 2018; Fiechter et al. 2022). The key takeaway from the latter is that CSR disclosure mandates lead to regulatory leakage of high-CSR-risk assets,

including forgoing investment opportunities (Rauter 2020), divestments (Ecker and Keeve 2023; Li et al. 2023), and shifting activity to non-reporting entities (Dai et al. 2021; Yang et al. 2023). While documenting these effects individually is informative, understanding the relative contributions of performance improvements versus regulatory leakage to the net CSR outcomes is critical for designing effective policies—particularly regarding the ex-post CSR performance of the assets transferred to the private sector. For instance, if the amount of asset leakage is large compared to CSR performance improvements, concerns about the application of CSR disclosure regulations only to a subset of firms may be justified. But even then, if unregulated firms have better CSR performance, asset leakage could enhance economy-wide CSR performance.

Regulatory leakage significantly reduces policy effectiveness only when asset leakage is substantial and the transferred assets perform worse than they would have if they remained in the public sector. Therefore, our primary contribution to the literature on the real effects of mandatory CSR disclosure is to document, in the context of a single disclosure regulation, the overall extent of asset leakage, the changes in CSR performance for assets transferred to the private sector, and the relative impact of each investment channel. Our results suggest that performance improvements in public firms' assets are the dominant response to the Dodd-Frank disclosure mandate. In the fewer instances where mandatory CSR disclosure leads to the regulatory leakage of high-CSR-risk assets, the resulting deterioration in CSR performance under private ownership is modest. Thus, in the context of DF1503, regulatory leakage does not appear to significantly undermine the overall positive CSR performance effects of the disclosure regulation, even over a relatively long horizon of more than 10 years.

Because CSR disclosure mandates likely incentivize corporate behavioral changes through more effective public pressure, our findings are also related to the literature examining how public pressure affects CSR performance and real-asset regulatory leakage in the absence of formal

disclosure mandates (e.g., Christensen et al. 2025; Duchin et al. 2024; Dyreng et al. 2016). Although central to the efficacy of mandatory CSR disclosure regimes, it is difficult for policymakers to control public pressure directly, and the public's focus on a particular issue may be short-lived, limiting firms' incentives to make costly, long-term investments to improve CSR performance (Duchin et al. 2024). In contrast, if firms anticipate that the mandatory disclosures could be used later for ex-post contextualization or in a confirmatory capacity when public pressure increases (Gigler and Hemmer 1998; Christensen 2022), disclosure mandates could, as our results suggest, serve as more effective long-term deterrents of socially undesirable CSR activities than public pressure alone.

2. Institutional Details and Conceptual Framework

In this section, we first discuss the origins and disclosure requirements of Dodd-Frank Section 1503 (DF1503). Second, we present a conceptual framework to illustrate how disclosure mandates can influence corporate investment policies and explain our approach to isolating the effect of the disclosure mandate from those arising purely from public scrutiny. Third, we discuss how disclosures in regulatory filings (Forms 8-K, 10-K, and 10-Q) can have real economic consequences, even when the underlying information is already publicly available elsewhere.

2.1 Dodd-Frank Section 1503

Catastrophic events often trigger new mine-safety regulations (Ruffennach 2002). One such event was the Upper Big Branch disaster on April 5, 2010, which claimed the lives of twenty-nine miners in West Virginia. In a move unprecedented at that time, policymakers turned to securities regulation for a solution. West Virginia Senator Jay Rockefeller IV introduced Section 1503 into the Dodd-Frank Act, with the explicit intent, not to aid investors in assessing financial performance, but to improve mine safety (Lynn 2011).

Dodd-Frank Section 1503(a) requires SEC-registered mine owners to disclose the safety records of their U.S. mines in their periodic financial reports (i.e., 10Qs and 10Ks for domestic issuers;

20Fs and 40Fs for foreign issuers).[4] Under the Federal Mine Safety and Health Act of 1977 (Mine Act), the MSHA must inspect surface mines at least twice a year and underground mines at least four times a year, with additional inspections triggered by complaints about hazardous conditions. When violations are found, inspectors issue citations or orders, which may include monetary penalties or, in severe cases, mine closures. DF1503 mandates that issuers report severe citations for Mine Act violations, proposed penalties, legal actions resulting from these inspections, and fatalities. Section 1503(b) also requires issuers to file a Form 8-K within four business days of receiving an Imminent Danger Order or a Pattern of Violations notice.[5]

Unlike most SEC reporting requirements, DF1503 does not include a materiality threshold, meaning issuers must disclose safety records regardless of whether their omission would influence users' decisions. Since 2000, the MSHA has disclosed all safety information covered by DF1503 on its public website, often providing updates shortly after incidents occur.[6] This prior public disclosure of safety records on the MSHA website allows us to isolate the effect of including this information in regulatory filings from those associated with its initial public disclosure.

2.2 Disclosure and Public Scrutiny

The mechanism most commonly suggested for why disclosure regulation affects real firm activities is stakeholder scrutiny. Stakeholders—such as investors, the media, and employees—scrutinize disclosures and impose costs on companies for disclosed misconduct, thereby incentivizing companies to avoid socially undesirable behaviors. Without public scrutiny, disclosure regulation alone is unlikely to have real effects. However, because disclosure regulation inherently relies on scrutiny to be effective, the ideal empirical setting for studying the effects of disclosure regulation is

[4] See Christensen et al. (2017, Appendix A) for a detailed description of the DF1503 reporting requirements.
[5] This requirement, however, does not apply to foreign issuers who are not obligated to file Form 8-K.
[6] MSHA provides ongoing updates and immediate notifications for certain types of accidents and injuries, which mine operators must report no later than 15 minutes from the time they know that an accident has occurred. In fact, he SEC estimates that DF1503 compliance costs are low because the required information is available on the MSHA website by the time firms file their reports (Release Nos. 33-9286; 34-66019; File No. S7-41-10).

not one without public scrutiny, but rather one where the incremental effects of disclosure can be separated from the effects of scrutiny alone.

At the same time, scrutiny can influence corporate behavior even in the absence of mandatory disclosure, making it a potentially correlated omitted variable in estimating the effects of disclosure regulation. In our study, two major shocks to public scrutiny present empirical concerns. First, DF1503 was enacted in response to public outcry following the Upper Big Branch disaster. Second, the post-DF1503 period (2010–2021) saw a significant increase in stakeholder attention to CSR and ESG-focused investing. Since investment policy changes in real asset markets take time to materialize, we need a long post-DF1503 sample period, but this also raises concerns about confounding effects from broader CSR trends. Moreover, increased scrutiny—whether triggered by major accidents or broader shifts in investor priorities—could be more salient for public firms, which, because of their broader stakeholder bases, are more exposed to fluctuations in public attention. This raises the possibility that any observed safety improvements following DF1503 might be solely attributable to public firms' heightened sensitivity to scrutiny.

Although public scrutiny and disclosure mandates are often intertwined, we argue that their effects can be distinguished empirically. Specifically, increases in public scrutiny are typically sporadic and inconsistent, particularly regarding CSR issues. For example, scrutiny may intensify when a firm becomes the target of a CSR activist campaign (e.g., Christensen et al., 2021), but public interest in corporate misconduct typically fades over time. Similarly, enthusiasm for ESG investing has fluctuated, as seen in the recent backlash against CSR-focused investment strategies.[7] While stakeholders may continue to value CSR initiatives, such as increasing workplace safety, their

[7] For instance, several states have taken measures against financial institutions that incorporate ESG factors into their investment decisions. These states have blacklisted money managers with public sustainability commitments and introduced legislation aimed at limiting the ability of financial institutions to include ESG considerations in investment strategies (https://www.thomsonreuters.com/en-us/posts/esg/state-laws).

engagement is sporadic, limiting the long-term influence of stakeholder pressure. Firms, anticipating that scrutiny will eventually subside, may adjust behavior only temporarily rather than committing to long-term improvements.

In contrast, mandatory disclosure regulations—although reliant on some expectation of stakeholder pressure—can create a more stable and enduring impact, even during periods of low public scrutiny. This is because managers anticipate that disclosures will remain public indefinitely and stakeholders can evaluate prior disclosures in future periods of heightened scrutiny (Gigler and Hemmer, 1998; Hail et al. 2021; Christensen 2022). For example, if journalists investigate a firm's safety record following a fatal accident, prior disclosures of poor safety performance could be used against the firm in media coverage, intensifying negative stakeholder reactions to the fatality. In other words, the costs of scrutiny (e.g., in the aftermath of a fatality) are higher when prior disclosures indicate a generally lower level of safety. If managers anticipate that such effects could occur ex post, disclosure mandates can serve as ex-ante long-term deterrents against socially undesirable behavior, even when public scrutiny is currently low.

To empirically distinguish the effects of mandatory disclosure from those of public scrutiny alone, we assume that public scrutiny has relatively short-term effects, whereas disclosure mandates should exert a more persistent influence. Specifically, we argue that the direct effect of scrutiny on safety can be inferred from safety changes occurring around fluctuations in scrutiny in the years before and after DF1503. In contrast, safety changes following DF1503 that persist over time—including periods of low scrutiny—are more likely attributable to the deterrence effect of disclosure regulation rather than temporary shifts in scrutiny.

To validate the assumption that scrutiny fluctuates and only lasts for a short period, we use Google search trends on mine safety and sustainability-focused investment flows as proxies for public scrutiny. We interpret spikes in Google searches as indicators of heightened public attention to safety

issues and sustainability-focused investment flows as a measure of investor interest in CSR-related issues, such as workplace safety.

Figure 2 Panel A tracks public interest in mine safety using U.S. Google search activity from 2005 to 2022. The largest spikes correspond to major disasters: the Sago and Darby incidents in 2006, the Crandall Canyon disaster in 2007, and the Upper Big Branch disaster in 2010. A spike in November 2019 aligns with a mining disaster in Shanxi Province, China. These patterns suggest public scrutiny around mining catastrophes is relatively short-lived.

Figure 2 Panel B shows that CSR-focused investment flows were virtually nonexistent before 2012, increased rapidly from 2013 to 2015, declined between 2016 and 2019, and peaked in 2021 before dropping again in 2022. Similar to patterns in Google search activity, this trend suggests that investors' sustainability preferences fluctuate over time.

Based on these observed patterns in public attention, in each of our subsequent analyses, we assess whether the dynamics of the observed treatment effects appear more consistent with public firms responding to a short-term increase in public scrutiny or with a sustained response to the disclosure mandate. The safety improvements that persist over time (including when scrutiny is low) are more likely deterrence effects of disclosure.

2.3 Disclosure in Regulatory Filings versus Other Disclosures

Financial statement disclosures, even of information that is already publicly available elsewhere, can influence firms' real-asset investment decisions and safety outcomes through several mechanisms (Christensen et al. 2017). First, financial statement disclosures can create a dissemination effect. SEC filings—such as Forms 8-K, 10-Q, and 10-K—are effectively the billboards of the financial community—serving as prominent sources of information that are widely distributed with low incremental acquisition cost. After DF1503, investors, financial analysts, and news media who monitor SEC filings are more likely to become aware of Mine Act violations, even if they are not

actively searching for them.

Second, financial statement disclosure reduces plausible deniability for sophisticated users and managers. Even if some sophisticated users are already aware of safety violations via other sources (e.g., the MSHA website), reducing the acquisition costs to near-zero could change their behavior. For instance, it would be difficult for an investment manager to claim they were unaware of safety issues if a portfolio firm had multiple Imminent Danger Orders filed in Form 8-Ks—and media coverage of major safety incidents, such as fatalities, may be more damaging if it is clear that the investment manager should have been aware of these issues. A similar consideration applies to managers of mining firms, who must sign off on the firm's financial reports, including, after DF1503, the firm's safety records. Consistent with this mechanism, Christensen et al. (2017) find that, while socially responsible investment funds reacted to website disclosures before DF1503, after DF1503, when the safety information was included in financial statements, investment flows became more sensitive to safety information.

Third, incorporating safety information into financial statements may raise concerns about increased securities litigation risk. A natural concern is that investors could pursue litigation if misreported safety information in regulatory filings leads to financial losses. However, because the regulation mandates disclosure of safety information that is already publicly available through other channels (e.g., MSHA databases), misreporting seems unlikely to be the basis for litigation-related losses in the context of DF1503. Supporting this argument, a 2014 indictment against Massey Energy (owner of the Upper Big Branch mine) alleging false mine safety disclosures prior to DF1503 was unsuccessful in court. Furthermore, to our knowledge, there have been no successful lawsuits specifically citing violations of DF1503 disclosure requirements.

In summary, financial statement disclosures can have real effects even when the information is available elsewhere, primarily due to increased dissemination and reduced plausible deniability.

Managers, rationally anticipating the implications of these disclosures for cash flows, discount rates, or reputational risks, may adjust investment decisions and implement safety improvements to mitigate these potential negative consequences.

3. Sample Selection, Defining Regulated and Unsafe Mines, and Descriptive Statistics

3.1 Sample Selection

We obtain mine-level data from the *U.S. Department of Labor MSHA Open Government Data* website, which compiles various datasets on health and safety for U.S. mining operations. We use the *Mines*, *Inspection*, *Violation*, *Accident/Injuries*, *Controller History*, *Production*, and *Address* databases. The sample spans 2000 to 2021 based on data from MSHA's *Mines* dataset (as of November 2022). After excluding mines that never exceed 10,000 employee working hours (at any point during the sample period) and mines never inspected in any year during the sample period (a sign that the mines are likely inactive), our sample includes 14,787 mines and 188,373 mine-year observations. Appendix C provides details on sample selection, where the samples for each analysis are indicated separately by table number.

3.2 DF1503 Mines

To compile our sample of public, regulated mines subject to DF1503, we first manually collect firm disclosures under SEC-filing Item 4, "Mine Safety Disclosures," from 2010 to 2021 using *SeekEdgar*. Since firms are required to disclose only mines with safety violations (e.g., citations and injuries), we cannot rely on the Item 4 disclosures alone to create a comprehensive list of all mines owned by each firm. Instead, we use the mine ID numbers and/or names disclosed under Item 4 to link public firms with controllers in the MSHA database. Once linked to a controller in the MSHA database we can observe the full population of the mines owned by each firm. To supplement this information, we (1) determine whether owners not flagged as public in the MSHA database were publicly listed before Dodd-Frank, (using Compustat), (2) conduct name-based searches to identify

firms that were public before Dodd-Frank but private during the post-period, and (3) match subsidiary names of public firms (from InfoGroup and WRDS Subsidiary data) with MSHA controller and operator records.

3.3 Unsafe Mines

We define unsafe mines in two ways: (1) based on significant safety violations and (2) based on serious injuries. These constructs complement each other since DF1503 focuses on disclosing safety violations, while reducing serious injuries is the regulation's ultimate policy objective (Rockefeller, 2010). *Unsafe (Violations)*$_{m,t}$ is an indicator set to one for mine *m* in year *t* if a mine ranks in the 90th percentile for violations per employee. *Violations* are the number of "Significant & Substantial" ("S&S") violations a mine receives in a year divided by its full-time equivalent employees.[8] *Unsafe (Serious Injury)*$_{m,t}$ is an indicator set to one for mine *m* in year *t* if the mine experiences one or more fatalities or permanent disabilities. *Unsafe(All)*$_{m,t,}$ is an indicator set to one if either *Unsafe(Violations)* or *Unsafe(Serious Injury)* equals one for mine *m* in year *t*. We use a relative measure for citations (top 10%), because the threshold for issuing a citation could change over time, whereas serious injuries are comparable across time.[9]

3.4 Descriptive Statistics

Table 1 Panel A shows the industry breakdown for issuers subject to DF1503. Mining is the most frequent primary industry sector, with coal-mining firms representing 12.4%, and non-coal firms 40.1% of the total sample. For 47.5% of DF1503 firms, mining is not their primary business. Among these, manufacturing and construction account for 19.9% and 9.2%, respectively.

[8] A violation is considered significant and substantial, or S&S, when it breaks a required health or safety rule and creates a hazardous situation. The hazard must be likely to cause a serious injury or illness.

[9] Using a relative safety measure prevents capturing spillover effects on unregulated entities, which should be considered when interpreting the results. Specifically, if safety improvements in public firms lead private firms to also improve safety—for example, because of competition for employees—these improvements would not be reflected in *Unsafe(Violations)*. Since this measure always identifies the 10% least safe mines, it does not account for general safety improvements across the sector.

Table 1 Panel B presents the distribution of public and private firms and mines from 2000 to 2021. The number of public firms declines over the sample period, from a maximum of 208 in 2001 to 122 in 2021 (a decline of 41%). The number of public mines also declines, but less so—reaching a maximum of 2,256 in 2005 and falling to 1,703 by 2021 (a decline of 25%). The trend for private firms and mines reveals a similar decline—private firms drop from 4,302 in 2002 to 3,212 in 2021 (a decline of 25%) and private mines fall from 8,091 in 2007 to 6,409 in 2021 (a decline of 21%). Although the table indicates a significant decline in the number of U.S. mines and mining firms during our sample period, this decline is similar in the public and private sectors—providing some reassurance that the two groups are exposed to similar macroeconomic trends. Moreover, despite the decline in U.S. mining activity, Figure 1 indicates that the proportion of all mines owned by public firms remains relatively constant throughout the sample period.

4. Estimating Safety Improvements and Regulatory Leakage

In this section, we outline the estimation of three distinct but related aspects of mine safety changes following DF1503: (1) firm-level reported safety changes, (2) mine-level safety changes under fixed-2009 treatment assignment, and (3) mine-level safety changes under time-varying treatment assignment. While each estimation approach provides valuable insights into different aspects of the policy's impact, none can individually isolate safety improvements from regulatory leakage. However, by comparing these estimates, we can indirectly infer the relative contribution of each channel. Below, we briefly describe each estimation approach, and in Appendix B, we provide a detailed example demonstrating how these estimates can be compared to distinguish between genuine safety improvements and regulatory leakage.

4.1. Estimating Firm-level Reported Safety Changes

Our estimate of *firm-level safety* changes is based on the safety *reported* in a firm's SEC filings for mines owned by public firms in a given year. It encompasses all the possible actions a firm

might take to improve its *disclosed* safety levels, including safety improvements (operational improvements or shutdowns of unsafe mines) and leakage (selling unsafe mines or opening fewer new mines in high-safety-risk locations). Figure 1, which illustrates the net change in the number of unsafe mines owned by public and private firms before and after Dodd-Frank, is based on this measure, and thus provides a comprehensive view of firm responses to the regulation across each of the possible margins for adjustment.

To calculate changes in firm-level reported safety, we estimate the change in unsafe mines operated by DF1503 firms in a DiD framework, with unregulated private firms as the benchmark. This estimation is based on the following OLS regression estimated at the mine-year level:

$$Unsafe_{m,t} = \beta_1 Public_{m,t} \times Post2009 + \beta_2 Public_{m,t} + \theta Controls + \Sigma \beta_i YearFE + \varepsilon_{m,t} \quad (1)$$

The dependent variable, $Unsafe_{m,t}$, is an indicator coded as one if a mine is classified as unsafe, based on *Unsafe(Violations)*, *Unsafe(Serious Injury)*, and *Unsafe(All)* as defined in Section 3.3. The variable of interest, $Public \times Post2009$, is the interaction between *Post2009* (equal to one after 2009) and $Public_{m,t}$ (equal to one for mines with at least one public owner).[10] By including a time-varying *Public* indicator in this specification, we allow mines to change ownership status over the sample period (i.e., from the treatment to the control group and vice versa). We do *not* include individual mine fixed effects in this specification because we want the $Public \times Post2009$ coefficient to reflect safety changes related to both time-invariant and time-varying mine characteristics (i.e., the sale of an inherently unsafe mine from a public to a private firm as well as changes in safety). We include year fixed effects to control for economy-wide changes in safety technology and other factors over time and the log of each mine's annual hours worked to account

[10] A mine can have multiple owners. We classify a mine as *Public* if any of those owners is an SEC-registered DF1503 filer because the safety records of that mine will be reported in at least one set of financial reports. In theory, we would expect stronger effects for firms with a larger proportion of public ownership. However, we lack data on each owner's stake in the mine.

for differences in mine size. We cluster standard errors at the mine-owner level to adjust for the lack of independence among mines owned by the same firm.

Table 2 Panel A (Panel B) presents descriptive statistics for the variables used in the analysis for publicly (privately) owned mines. *Public* firms control 20.6% of the mine-year sample. For the sample of mines owned by *Public* (*Private*) firms, 52.9% (57.4%) of observations come from *Post2009*, 6.1% (10.3%) of mines are identified as unsafe based on *Unsafe(Violations)*, and 2.9% (1.1%) of mines are categorized as unsafe based on *Unsafe(Serious Injury)*. On average, public mines are about three times larger than private mines based on average *Annual Hours(Thousands)* worked.

Table 2 Panel C Column (1), presents estimates from Eq. (1), the firm-level reported safety changes specification, using *Unsafe(Violations)* as the dependent variable. $Public \times Post2009$ is negative and statistically significant, indicating a 3.1 percentage point (pp) reduction in unsafe mines, approximately 41.7% compared to the pre-DF1503 average among DF1503 mine owners (7.4 pp). In Column (4) where *Unsafe* is defined based on serious injuries (*Unsafe(Serious Injury*)), we observe a 0.9 pp reduction, representing 24.6% of the pre-DF1503 average. Column (7) shows similar results when defining *Unsafe* based on both violations and injuries (*Unsafe(All)*)—a decrease of 3.8 pp, equivalent to 36.0% of the pre-DF1503 average.

Figure 3 Panel A maps out treatment effects over two-year periods for firm-level reported safety changes, using *Unsafe(All)* as the dependent variable.[11] The figure shows that the treatment effects emerge soon after DF1503's implementation and remain relatively stable throughout the post-treatment period. In contrast, the counterfactual treatment effects in the pre-regulation periods are

[11] We use two-year periods in the figures mapping out the treatment effects and 3-year rolling windows in the univariate figures to reduce the influence of short-term fluctuations in mine-level safety outcomes, which can be driven by idiosyncratic or transitory factors such as weather conditions, temporary operational changes, or inspection timing. The relatively low frequency of mine incidents makes it difficult to detect meaningful patterns over shorter periods. Aggregating over two-year intervals helps smooth this noise and allows us to better identify persistent effects associated with the disclosure mandate.

small and close to zero, supporting the parallel-trends assumption. The sustained post-DF1503 treatment effect aligns more closely with the long-term influence of mandatory disclosure on firm behavior rather than the temporary effects of heightened public scrutiny following mining disasters (see Section 2.2 and Figure 2).

4.2. Estimating Mine-level Safety Changes – Fixed-2009 Treatment Estimate

We estimate *mine-level* safety changes by including mine fixed effects in Eq. (1). The mine fixed effects ensure that we isolate within-mine safety changes, removing any effects of selling inherently unsafe mines or opening new mines during the post-DF1503 period. In this specification, we hold each mine's public or private ownership status constant as of 2009, the year before DF1503.[12] This ensures that safety changes for mines changing ownership after DF1503 are still attributed to their original 2009 classification, regardless of subsequent ownership changes. Consequently, the mine-level fixed-2009 treatment specification more closely approximates the overall real impact of DF1503 on mine-safety (i.e., akin to an intention-to-treat effect), compared to the firm-level safety estimate, which captures both genuine safety changes and changes in ownership.[13]

We estimate mine-level safety with fixed-2009 treatment using the following regression:

$$Unsafe_{m,t} = \beta_1 Public_{m,2009} \times Post2009 + \beta_2 Public_{m,2009} + \theta Controls + \Sigma\beta_i YearFE + \Sigma\beta_i MineFE + \varepsilon_{m,t} \quad (2)$$

This specification is the same as Eq. (1), but with two key differences: (1) we include mine-level fixed effects to control for inherent differences in mine safety, and (2) the *Public* indicator is fixed based on ownership as of 2009, regardless of subsequent ownership changes after DF1503.

[12] The first time DF1503 is included in publicly available draft of Dodd-Frank is in June 2010, approximately two months after the Upper Big Branch disaster that motivated it. Hence, it is unlikely that anyone in 2009 anticipated DF1503 being enacted in 2010.

[13] This specification is similar to the specification in Christensen et al. (2017). The only differences are the dependent variables (continuous safety measures versus indicators for unsafe mines) and that Christensen et al. (2017) only infer DF1503 mines based on disclosures after DF1503 rather than on name-based searches in the pre-DF1503 period. In the Internet Appendix, we confirm that inferences are similar using similar continuous safety measures.

In Table 2 Panel C Columns (2), (5), and (8), we report the results from estimating Eq. (2), for the dependent variables *Unsafe(Violations)*, *Unsafe(Serious Injury)*, and *Unsafe(All)*, respectively. The $Public_{2009} \times Post2009$ coefficients are all negative and statistically significant and imply safety changes for mines owned by public firms in 2009 of 26.6%, 20.6%, and 24.3% compared to the pre-DF1503 averages.

Figure 3 Panel B maps out the treatment effects, which, in the pre-regulation periods, are close to zero and provide no indication of a differential pre-treatment safety trends between *Public* and *Private* firms (with the exception of a marginally significant positive coefficient in 2006/2007 that indicates private firms may have temporarily *increased safety more* than public firms following the Sago and Darby mining disasters). Similar to reported safety, the treatment effects occur relatively quickly after the inception of DF1503, but like the coefficients in Table 2, are somewhat smaller in magnitude than for the firm-level analysis. Overall, the pattern of the post-DF1503 treatment effects is, once again, more consistent with a sustained response to the disclosure mandate and the deterrence effects of disclosure than trends in public scrutiny (see Figure 2).

4.3. Estimating Mine-level Safety Changes – Time-varying Treatment Estimate

While the safety change for the mine-level safety with *fixed-2009* treatment specification comes close to capturing the actual safety improvements made by public firms after DF1503, it still conflates these improvements with one component of regulatory leakage—changes in safety when a mine switches ownership from a public to a private firm (and vice versa). To assess how much regulatory leakage contributes to reported firm-level safety changes, we need an estimate of the change in safety attributable to such ownership changes.

To accomplish this, we estimate the mine-level *time-varying* treatment effect where we also include mine fixed effects but allows a mine's public or private ownership status to vary over time. In this specification, all time-invariant mine-level safety differences are still eliminated by the

inclusion of mine fixed effects. However, if, for example, a public firm sells a mine to a private firm, any subsequent changes in that mine's safety would be reflected in the safety changes attributed to the private control group (as opposed to the treatment group as in the *fixed-2009* treatment specification)—this allows us to compare the two mine-level specifications to infer the post-sale safety changes (as discussed below).

We estimate mine-level safety with time-varying treatment using the following regression:

$$Unsafe_{m,t} = \beta_1 Public_{m,t} \times Post2009 + \beta_2 Public_{m,t} + \theta Controls + \Sigma\beta_i Year\ FE + \Sigma\beta_i Mine\ FE + \varepsilon_{m,t} \quad (3)$$

As in Eq. (1), we allow mines to change ownership status over the sample period (i.e., *Public* is time-varying), and as in Eq. (2) we include mine fixed effects to control for inherent safety differences.

In Table 2 Panel C Columns (3), (6), and (9), we estimate Eq. (3) for the mine-level time-varying treatment specification for *Unsafe(Violations)*, *Unsafe(Accidents)* and *Unsafe(All)*, respectively. The $Public_t \times Post2009$ coefficients are all negative and statistically significant and imply reductions in unsafe mines by public firms of 26.6%, 23.2%, and 26.1% compared to the pre-DF1503 averages.

Figure 3 Panel C maps out the treatment effect based on the specification in Table 2 Panel C Column (9). The pre-DF1503 trend in unsafe mines between public and private mines is reasonably flat and, consistent with the regression results, shows evidence of a significant and persistent decline in the post-DF1503 period that is, as in the previous specifications, more consistent with a sustained response to the disclosure mandate and the deterrence effects of disclosure than trends in scrutiny.

4.4 Estimating Reported Safety Changes due to Regulatory Leakage

Although informative about different aspects of the policy, neither the firm-level nor mine-level safety estimates directly capture the extent of regulatory leakage. For example, the mine-level estimate excludes safety changes arising from new mine openings and does not distinguish whether

safety changes result from public firms' improving safety or from private firms operating mines acquired from public firms differently. The estimate of firm-level reported safety changes captures all possible actions a firm might take to improve its disclosed safety levels but conflates safety improvements with regulatory leakage. However, as detailed in Appendix B, the aggregate treatment effect can be decomposed into the components attributable to safety improvements and regulatory leakage by comparing results from the three regressions.

To summarize, the difference between the fixed-2009 and time-varying treatment effect estimates is attributable to safety changes in mines that change status between public and private ownership in the post-DF1503 period. To calculate the actual safety change under new ownership, the difference between the two estimates must be halved because the difference-in-differences (DiD) estimate double counts the effect when ownership changes shift entities from the treatment to the control group. Based on this calculation, we estimate that changes in mine-level safety following ownership transfers between public and private firms are zero for violations (i.e., the fixed-2009 and time-varying estimates are equal), 0.05pp for injuries, and 0.10pp for the combined estimate—representing about 0.0%, 1.3%, and 2.6% of the total firm-level reported safety change, respectively. These findings suggest that there is only a small safety deterioration (improvement) under private (public) ownership.

These findings also imply that the mine-level safety improvements estimated in the fixed-2009 treatment specification are smaller than the safety improvements observed in mines that remain continuously owned by public regulated firms. This is because the estimated safety change includes the deterioration in safety that occurs after mines are transferred to private unregulated firms. However, given that we have an estimate of the changes in safety following ownership changes, we can calculate the safety improvements made by public firms by subtracting the safety decline under private ownership from the fixed-2009 treatment estimate. Comparing the estimates across columns,

safety improvements are 2.1pp for violations, 0.7pp for injuries, and 2.6pp for the combined measure. This implies that because safety tends to deteriorate under private ownership the mine level *fixed-2009* treatment estimate underestimates the actual improvement in safety by about 0.0%, 12.5%, and 3.7% for violations, injuries, and the combined measure, respectively.

Finally, given that we have an estimate for the actual safety improvement, and we know that the firm-level reported safety change is a combination of that safety improvement and regulatory leakage, we can now estimate the extent of regulatory leakage by subtracting the actual safety improvement from the reported safety change. Regulatory leakage is 1.0pp (32.3%) for violations, 0.2pp (22.2%) for injuries, and 1.2pp (31.6%) for the combined measure.

4.5 Timing of Safety Changes for Mines Sold to Unregulated Firms

The deterioration in safety under private ownership documented above could result either from private firms operating mines less safely or public firms strategically selling mines with worsening safety performance. From a policy perspective, the former is more concerning as it suggests that regulatory leakage undermines DF1503's aggregate safety benefits. Distinguishing between these explanations empirically is challenging, and prior research provides mixed evidence. Some studies find that public firms have *better* CSR-performance (Fowlie 2010), others that public firms have *worse* CSR-performance (Hart and Zingales 2017, Shive and Forster 2020), and still others that ownership structure has *no impact* on CSR-performance (Duchin et al. 2022, Im 2023).

To assess whether the deterioration in safety after mines are sold to private firms in the post-DF1503 period is more likely attributable to public firms strategically selling unsafe mines or to private firms operating the newly acquired mines less safely, we examine the timing of safety changes relative to the date of sale. We use a stacked OLS DiD estimation of Eq. (2), defining the event of interest as an ownership transfer from public to private firms between 2010 and 2021.The control group consists of mines that remained private throughout the sample period. To mitigate potential

confounding effects from overlapping ownership transfers, we exclude mines that were sold multiple times during the sample period.

Figure 4 shows a relatively stable safety level in the years immediately preceding the sale, followed by a deterioration in safety beginning the first full year after. Although this decline occurs relatively soon after the ownership transfer, the change in performance around the sale and deteriorating performance over the subsequent four years aligns more closely with the interpretation that private owners operate mines less safely, rather than public firms engaging in strategic sales. However, while Figure 4 suggests *Public* firms do not systematically sell mines with deteriorating safety, that conclusion relies on the assumption that mine owners do not anticipate *future* safety declines when deciding to sell.

4.6 Mine Shutdowns' Contribution to Safety Improvements

From the analysis in Section 4.4, we know that safety improvements appear to dominate asset leakage in explaining firm-level reported safety increases after DF1503. However, safety improvements can take the form of either operational improvements or shutdowns of unsafe mines, which have different implications for non-safety outcomes such as employment. In this section, we estimate the change in shutdowns around DF1503 to assess the importance of the different sources of safety improvements. Specifically, we use a DiD framework, where private mines are the control group, and estimate the following regression:

$$Shutdown_{m,t} = \beta_1 Public_{m,t} \times Post2009_t + Public_{m,t} + \theta Controls + \Sigma\beta_i YearFEs + \varepsilon_{m,t} \quad (4)$$

In Eq. (4), the dependent variable is *Shutdown*, an indicator equal to one if mine *m* is classified as inactive for the first time in year *t*, and zero otherwise. *Public* and *Post* are defined as in Eq. (1). The model includes year fixed effects and controls for mine type (i.e., *Underground* and *Coal* indicators) and size (*Log(Annual Hours)*). We cluster standard errors at the mine-owner level.

Figure 5 Panel A illustrates the proportion of unsafe mines shut down by public and private firms over the sample period. Before Dodd-Frank, private firms (solid blue line) were about twice as likely as public firms to shut down an unsafe mine (4% versus 2%). After Dodd-Frank, this likelihood declined significantly for private firms falling from 4% to below 2%, while for public firms, there was a modest increase, to around 3% from 2011 to 2016 before a decline to around 1%, indicating that compared to private firms, public firms were more likely to shut down an unsafe mine after DF1503. Figure 5 Panel B shows that *Public* firms reduced shutdowns of safe mines at a faster rate than unsafe mines. This evidence suggests that, compared to private firms and safe mines, public firms were relatively more likely to shut down unsafe mines in the post-Dodd-Frank period.

Table 3 Panel A presents descriptive statistics. On average, 4.4% of all mines are shut down each year. public firms account for 22.8% of mine-year observations, with 9.9% of mine-years deemed unsafe based on *Unsafe(Violations)* and 1.9% based on *Unsafe(Serious Injury)*. Underground mines comprise 9.4% of the sample and coal mines 21.0%.

Table 3 Panel B presents results from two specifications—one examining shutdowns of mines classified as safe in the prior year and the other focusing on unsafe mines. In Columns (1) and (2), we find no evidence of significant increases in shutdowns of either *Unsafe* or *Safe* mines based on violations. In Column (3), we observe a statistically significant increase in the likelihood that public firms shut down *Unsafe* mines based on serious injuries. In Column (4), the increase for *Safe* mines is positive but insignificant. A test of the difference in the *Public*×*Post* coefficients across Columns (3) and (4) indicates that the difference is statistically significant at the 5% level (p-value 0.02). In Columns (5) and (6), we find no significant effects when examining shutdowns based on *Unsafe(All)*.

Figure 6 maps out treatment effects for shutdowns based on the specification in Table 3 Panel B Column (3), the only specification in Table 3 where we find a significant effect. A comparison of pre-Dodd–Frank trends in unsafe mine ownership across public and private mines shows that the

counterfactual treatment effects in the pre-regulation periods are small and statistically indistinguishable from the benchmark period. Consistent with the regression analysis, there is evidence of only a small increase in the number of shutdowns of unsafe mines for public firms after DF1503. Overall, based on the modest increase in shutdowns that only occur when unsafe is defined based on *Unsafe(SeriousInjury)*, we conclude that most of the safety improvement after DF1503 estimated in Section 4.4. is attributable to safety improvements of continuously operated mines.

5. Timing and Magnitude of Real Asset Leakage

In this section, we examine the frequency and timing of asset transfers to better assess the persistence of safety changes and the broader implications of regulatory leakage. We examine two primary channels for leakage: (1) ownership changes, where existing unsafe mines are sold from public to private firms, and (2) forgone investments, where public firms refrain from opening new mines that are instead opened by private firms. While our aggregate estimate captures the combined effect of these channels, it does not reveal how many mines are affected, the relative contribution of each mechanism, or whether the leakage represents a short-term response or a more persistent shift in asset ownership. Importantly, this reallocation of high-risk assets from regulated to unregulated firms may have broader social and economic consequences beyond mine safety. For example, it could reduce overall asset diversification within the regulated sector and weaken risk-sharing.

5.1. Selling Unsafe Mines

We estimate the average effect of DF1503 on the changes in mine ownership between the public and private sectors in the following OLS regression estimated at the mine-year level:

$$Sell_{m,t} = \beta_1 Public_{m,t} \times Post2009 + Public_{m,t} + \theta Controls + \Sigma\beta_i Year\ FEs + \varepsilon_{m,t} \quad (5)$$

The dependent variable, *Sell*, is the absolute value of the change in a mine's public ownership ($Public_{t+1} - Public_t$), capturing transfers of mine ownership between the public and private sectors. $Public \times Post2009$ indicates whether the mine is owned by a public firm in the post-Dodd-Frank

period. As in the previous specifications, we include mine-year control variables, year fixed effects, and cluster standard errors at the mine-owner level.

Figure 7 Panel A plots the percentage of *Unsafe* mines sold each year for *Public* and *Private* firms. Before Dodd-Frank, *Private* firms (solid blue line) and *Public* firms (dashed red line) exhibit relatively similar ownership changes, though *Public* firms experience a notable spike between 2006 and 2008. After Dodd-Frank, starting around 2012, the percentage of *Unsafe* mines sold by *Public* firms increases significantly, peaking near 18% in 2014 before returning to near pre-Dodd-Frank levels around 2018. In contrast, the sales of *Unsafe* mines by *Private* firms remains largely unchanged after Dodd-Frank.

Figure 7 Panel B compares sales of *Unsafe* and *Safe* mines by *Public* firms. Notably, in the pre-DF1503 period, the spike in the percentage of *Unsafe* mines is closely followed (and even slightly exceeded) by an increase in *Safe* mines sold. This pattern suggests that whatever macroeconomic factor drove the increase in mine sales during this period, *Public* firms sold *Safe* and *Unsafe* mines at similar rates. In contrast, during the post-DF1503 period, a 2014 to 2016 spike in *Safe* mine sales is evident, but is significantly smaller than the corresponding spike in *Unsafe* mine sales.

Table 4 Panel A presents descriptive statistics. On average, 1.9% of all mines experience a change in ownership status (*Sell*) in a given year. The descriptive statistics for the remaining variables are similar to those in the previous analyses.

Table 4 Panel B presents results from Eq. (5). Odd columns examine changes in the likelihood of selling *Unsafe* mines and even columns examine changes in the likelihood of selling *Safe* mines. We expect an increase in the probability that DF1503 firms sell *Unsafe* mines in the post-Dodd-Frank period, whereas no similar increase is anticipated for *Safe* mines.

In Column (1), $Public \times Post2009$ is positive and statistically significant at the 1% level, indicating that public firms are more likely to sell *Unsafe* mines after Dodd-Frank. In contrast, in

Column (2), the coefficient estimate is statistically insignificant for *Safe* mines. The two coefficient estimates are statistically different at the 1% level. Looking at *Serious Injuries*, results in Columns (3) and (4) are similar, but less precisely estimated. Columns (5) and (6) again suggest firms are significantly more likely to sell *Unsafe* mines when considering both *Violations* and *Serious Injuries*.

The magnitude of the estimated coefficients in Columns (5) and (6) suggests that, after Dodd-Frank, public firms sell 6.3% *more* of their *Unsafe* mines. This result translates to a 1.6× higher likelihood of public firms selling *Unsafe* mines compared to the pre-DF1503 period, where the probability of such sales was 3.9%. While this represents a substantial relative effect, the absolute impact is modest. On average, only nine *Unsafe* mines are sold by public and private firms annually before DF1503, this result corresponds to an increase of 13 additional *Unsafe* mines sold per year.

Figure 8 maps out the treatment effects for ownership changes of *Unsafe* mines, based on the specification in Table 4 Panel B Column (5). The pre-DF1503 trend in *Unsafe* mine ownership between *Public* and *Private* firms is relatively flat. However, in the post-DF1503 period, particularly in 2014-2015, there is a substantial increase in ownership transfers of *Unsafe* mines. Overall, the observed pattern of the treatment effects in Figure 8 does not align with either the trends in public scrutiny of mine safety (Figure 2 Panel A) or investor preferences for ESG (Figure 2 Panel B). Instead, the pattern is more indicative of a sustained response to the disclosure mandate and the deterrence effects of disclosure.

5.1.2 Investment Opportunities Forgone by Regulated Firms

The change in the relative cost of owning unsafe mines for public versus private firms after DF1503 is also likely to influence public firms' propensity to open mines in high-safety-risk areas. Even when holding constant a mine's type (e.g., coal versus metal or above- versus below-ground),

geographic location can significantly impact operational safety due to geological factors.[14] In this section, we examine whether public firms became less likely to open new mines in areas that have historically been prone to citations and serious accidents.

We define a mine opening as the initial appearance of a mine in the MSHA database and estimate the average effect of DF1503 on changes in mine openings in the following OLS regression estimated at the mine-year level:

$$Open\ Unsafe\ \ ZIP_{m,t} \ = \ \beta_1 Public_{m,t} \times Post2010 \ + Public_{m,t} + \theta Controls$$

$$+ \Sigma \beta_i YearFEs \ + \ \varepsilon_{m,t} \quad (6)$$

The dependent variable, *Open Unsafe ZIP*, is an indicator equal to one if a new mine, *m*, is opened in an area classified as an *Unsafe ZIP* in year *t-1*, and zero otherwise. We classify an area (5-digit zip code) as an *Unsafe ZIP* if it falls above the 90th percentile in the ratio of *Unsafe* mines to total mines. *Public* and *Post2010* are defined as in Eq. (1). As in previous specifications, we include year fixed effects, control for mine type (*Underground* and *Coal* indicators) and size (*Log(Annual Hours)*), and cluster standard errors at the mine-owner level.

Figure 9 plots the proportion of new mines opened in *Unsafe* areas for public and private firms over the sample period. Before Dodd-Frank, *Public* firms (dashed red line) opened approximately twice as many mines in *Unsafe* areas as *Private* firms. After Dodd-Frank, the percentage of mines opened in high-risk areas declines by 45%, to around 15% of mine openings. For *Private* firms, mine openings in unsafe areas decrease modestly, by about 1%.

Table 5 Panel A presents descriptive statistics. On average, 20.1% (10.6%) of all mines are opened in an *Unsafe ZIP* each year based on *Violations* (*Serious Injuries*). *Public* firms account for 13.7% of the mine-year observations, while 28.9% of the observations fall in the *Post* period,

[14] For example, the quality of the rock forming the roof of an underground coal mine, from crystallized sandstone (most stable) to clay shale (least stable), is a major determinant of the likelihood of a roof collapse—one of the most common causes of mining accidents and fatalities in the US (CDC 2003).

reflecting a general decline in new mine openings over time, particularly among public firms. Other variables are consistent with previous analyses.

Table 5 Panel B presents regression results for Eq. (6). In Column (1), $Public \times Post2010$ is positive and statistically insignificant. In Column (2), the coefficient is negative and statistically significant at the 5% level, suggesting that public firms are less likely to open new mines in *Unsafe* areas based on the prevalence of serious injuries, though no such effect is observed for violations. In Column (3), *Unsafe(All)* is negative but statistically insignificant.

Figure 10 maps out treatment effects for new *Unsafe* mine openings, based on the specification in Table 5 Panel B Column (2). The pre-DF1503 trend in *Unsafe* mine openings between public and private firms is relatively stable. Consistent with the regression results, we observe a significant decline in the post-DF1503 period, beginning around 2014-2015 period and generally increasing over time. This delayed effect seems reasonable given the operational adjustments firms likely require to alter their geographic footprint. Overall, the observed pattern of the treatment effects in Figure 10 aligns more closely with a sustained—albeit delayed—response to the disclosure mandate and the persistent deterrence effects of disclosure rather than more temporary increases in public scrutiny.

6. Conclusion

We examine how Dodd-Frank Section 1503 (DF1503) influenced firms' real-asset investment decisions and mine safety outcomes through mandatory disclosure requirements. We estimate that after DF1503, the proportion of unsafe mines owned by public firms decreases by 36%, with 74% of this reduction attributable to mine-level safety improvements and 26% due to regulatory leakage—i.e., the transfer of high-risk assets to private firms and the avoidance of new investments in potentially unsafe assets. While safety outcomes deteriorate slightly after mines are transferred to private firms, this accounts for only around 10% of the leakage effect, whereas the remainder reflects the reallocation of inherently high-risk assets.

Taken together, our findings suggest that DF1503 achieved meaningful safety improvements among regulated firms, reinforcing the effectiveness of mandatory CSR disclosures in reducing socially undesirable corporate behavior. The long-term deterrent effect of safety disclosures in financial reports likely contributed to firms adopting safer practices, even in periods of lower public scrutiny. However, a nontrivial portion of the firm-reported safety improvement stems from regulatory leakage, as some high-risk mines were transferred to private entities. While these transfers improved reported safety among publicly traded firms, they also shifted risks to the unregulated sector, where safety performance is less transparent and deteriorates slightly after the transfer.

Our findings highlight an important trade-off in disclosure-based regulation: while transparency requirements can improve corporate behavior, they also incentivize firms to reallocate high-risk assets to unregulated sectors. This suggests that policymakers considering similar CSR disclosure mandates—such as climate risk reporting—should anticipate potential regulatory leakage. Future regulation may need to consider extending disclosure requirements to private firms or implementing complementary enforcement mechanisms to prevent leakage from undermining overall policy effectiveness.

One limitation of our analysis is that we cannot directly observe how DF1503 influenced decisions about mines that were never opened by regulated or unregulated firms. However, not opening a mine at all is a very costly way to avoid the regulation, and thus we expect this effect to be relatively small. Another limitation is that our analysis will understate safety improvements if there is a positive spillover effect on the safety of unregulated private firms. Such a spillover effect could happen through new safety technology demanded by regulated firms but ultimately adopted by all firms. Finally, our evidence is naturally limited to a single sector of the economy—mining—that is quite different from other sectors in that safety issues are heavily regulated tend to get a lot of public attention, even though mining is no longer among the least safe sectors.

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Appendix A - Variable Definitions

Variable	Description
Public	An indicator equal to one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F in the year specified. Data Source: SEC Edgar.
*Post*2009	An indicator equal to one if the year is between 2010 and 2021, and zero if the year is between 2000 and 2009.
Unsafe(Violations)	An indicator equal to one if the number of Significant & Substantial (S&S) violations, scaled by the number of employees, is higher than the 90th percentile of all mines in the year specified, and zero otherwise. Data Source: MSHA Mine Data Retrieval System.
Unsafe(Serious Injury)	An indicator equal to one if there are any fatalities or permanent disabilities in the year specified, and zero otherwise. Data Source: MSHA Mine Data Retrieval System.
Unsafe(All)	An indicator equal to one if the number of S&S violations, scaled by the number of employees, is higher than the 90th percentile of all mines, or if there are any fatalities or permanent disabilities in the year specified, and zero otherwise. Data Source: MSHA Mine Data Retrieval System.
Shutdown	An indicator equal to one if the mine transitions from active to inactive for the first time, and zero in active years before. A mine-year is active if the mine is inspected that year and inactive if it is not inspected that year. Data Source: MSHA Mine Data Retrieval System.
Annual Hours (Thousands)	The annual summation of employee working hours. Data Source: MSHA Mine Data Retrieval System.
Log(Annual Hours)	The log of *Annual Hours*. Data Source: MSHA Mine Data Retrieval System.
Underground	An indicator equal to one if the mine is an underground mine, and zero if it is a facility mine or a surface mine. Data Source: MSHA Mine Data Retrieval System.
Coal	An indicator equal to one if the mine is a coal mine, and zero if the mine is a metal mine. Data Source: MSHA Mine Data Retrieval System.
Sell	The absolute value of ($Public_{t+1}$ - $Public_t$). Data Source: SEC Edgar, MSHA Mine Data Retrieval System.
Open Unsafe (Violations) ZIP	An indicator variable equal to one if, between 2000 and 2009, the percentage of *Unsafe (Violations)* mine-year observations in the area measured by the 5-digit zip code is above the 90th percentile of all areas, and zero if the percentage is below the 90th percentile. Data Source: MSHA Mine Data Retrieval System.
Open Unsafe (Serious Injury) ZIP	An indicator equal to one if, between 2000 and 2009, the percentage of *Unsafe (Serious Injury)* mine-year observations in the area measured by the 5-digit zip code is above the 90th percentile of all areas, and zero if the percentage is below the 90th percentile. Data Source: MSHA Mine Data Retrieval System.
Open Unsafe (All) ZIP	An indicator equal to one if, between 2000 and 2009, the percentage of *Unsafe (Violations)* or *Unsafe (Serious Injury)* mine-year observations in the area measured by the 5-digit zip code is above the 90th percentile of all areas, and zero otherwise. Data Source: MSHA Mine Data Retrieval System.

Notes: This table provides the descriptions and sources of variables used in the paper.

Appendix B – Comparing Safety Estimates

In this Appendix, we discuss how our estimates of firm-level reported safety and mine-level safety with fixed-2009 and time-varying treatment assignment can be compared to isolate the proportion of the firm-level reported safety change that is attributable to safety improvements from that attributable to regulatory leakage. We briefly describe each of the estimation approaches below and provide a detailed example of how they can be compared to separate safety improvements from regulatory leakage.

1. Safety Change – Firm-Level Reported Improvements

We define *firm-level safety* as the safety *reported* in a firm's SEC filings for mines owned by regulated (*DF1503*) firms in a given year. Improvements in firm-level safety include two components: (1) safety improvements at existing mines, including shutting down unsafe mines, and (2) changes in ownership, such as the sale of unsafe mines from public (regulated) firms to private (unregulated) firms or decisions by public firms to avoid opening new mine in high-safety-risk areas—leaving these assets outside of the scope of the regulation. Consequently, the firm-level safety estimate conflates the effects of genuine safety improvements with regulatory leakage.

We estimate firm-level safety using the following regression model (Eq. 1) as described in Section 3 of the manuscript:

$$Unsafe_{m,t} = \beta_1 Public_{m,t} \times Post2009 + \beta_2 Public_{m,t} + \theta Controls + \Sigma\beta_i Year\ FE + \varepsilon_{m,t} \quad (1)$$

Importantly, by including a time-varying $Public_{m.t}$ indicator in this specification, we allow mines to change ownership status from the pre- to the post-period (i.e., from the treatment to the control group). We *exclude* individual mine fixed effects in this specification because we want the $Public \times Post2009$ coefficient to reflect safety changes related to both inherent and time-varying mine characteristics.

The example below illustrates how the estimated treatment effect in this specification encompasses the possible actions a firm could take to improve its disclosed safety levels. These actions include direct safety improvements at existing mines, shutdowns of unsafe mines, the sale of unsafe mines, and the decision to open fewer new mines in high-safety-risk locations.

Assume there is a single regulated public firm and a single unregulated private firm. Before the introduction of DF1503, the public firm owns four mines—*Mines A*, *B*, *C*, and *D*—all classified as *Unsafe* ($Unsafe_{Total}$ = 4). The private firm owns two mines: *Mine F*, classified as *Safe*, and *Mine G*, classified as *Unsafe* ($Unsafe_{Total}$ = 1). After DF1503, the following changes occur:

- Public Firm—The total number of *Unsafe* mines declines from 4 to 1 due to the following actions:
    - *Mine A*: safety improves, it is no longer classified as *Unsafe*.
    - *Mine B*: shut down, reducing the total count of *Unsafe* mines.
    - *Mine C*: sold to the private firm, reducing the total count of *Unsafe* mines.
    - *Mine D*: no change; it remains classified as *Unsafe*.

- *Mine E*: newly opened and classified as *Safe*.

- Private Firm—The total number of *Unsafe* mines increases from 1 to 4 due to the following actions:
  - *Mine F*: no change, it remains classified as *Safe*.
  - *Mine G*: no change, it remains classified as *Unsafe*.
  - *Mine C*: purchased from the public firm, and its safety worsens further under private ownership (*Unsafe* = 2, 1 from inherent mine characteristics and 1 from the safety change).
  - *Mine H*: newly opened and classified as *Unsafe*.

Table A1 Panel A presents the number of *Unsafe* mines for the public and private firms before and after DF1503 following this example. The rows represent individual mines and their safety classifications over time, while the columns distinguish the pre- and post-DF1503 periods. Safety changes and regulatory leakage effects are visually distinguished using color coding: green shading indicates safety improvements (i.e.., *Mine A*), yellow shading indicates regulatory leakage resulting from the transfer of inherently unsafe mines from public to private ownership (i.e., *Mine C*), and red shading indicates leakage related to worsening safety outcomes under private ownership (i.e., *Mine C* after the transfer).

Table A1 Panel A – Pre-Post DF1503 Safety Changes for Firm-level Reported Safety

Firm-level Reported Safety Change

	Safety Improvements
	Leakage - Inherent High-Risk Transfer
	Leakage - Worse Safety Under Private Ownership

Public - Regulated

	Pre	Post	
Mine A	1	0	Improve Safety
Mine B	1	-	Shutdown
Mine C	1	-	Sell to Private
Mine D	1	1	No Change
Mine E	-	0	Open New
Unsafe Mines	**4**	**1**	

Private - Unregulated

	Pre	Post	
Mine F	1	1	No Change
Mine G	0	0	No Change
Mine C	-	2	Buy from Public, Safety Declines
Mine H	-	1	Open New
-	-	-	
Unsafe Mines	1	4	

Based on the assumptions outlined above, the estimated DiD treatment effect is a net decrease in *Unsafe* mines of -6, as shown in Table A1 Panel B. This decrease reflects: (1) safety improvements (green), which contribute -2 to the overall effect (e.g., public firms improving safety at *Mine A* and shutting down *Mine B*), (2) leakage due to the transfer of inherently unsafe mines to private ownership (yellow), which also contributes -2 (e.g., the sale of *Mine C,* which moves from the treatment to the control group), and (3) leakage from worsening safety under private ownership (red) of -2 (e.g., *Mine C* safety deteriorating and *Mine H* being newly opened under private ownership).

Table A1 Panel B – Breakdown of the DiD Effect in the Firm-level Reported Safety Specification

	Pre	Post	Post - Pre
Public	4	1	-3
Private	1	4	3
		DiD	**-6**

Breakdown of Treatment Effect	
Public Improves Safety (Mine A)	-1
Public Shuts down Unsafe (Mine B)	-1
Public Sells Unsafe to Private (Mine C)	-1
Private Buys Unsafe form Public (Mine C)	-1
Operate that Unsafe Worse (Mine C)	-1
Private Opens New Unsafe (Mine H)	-1
DiD Estimate	**-6**
Leakage Effect	**-4**

As shown, the firm-level reported safety change DiD estimate reflects all changes contributing to the pre-post safety differences between public and private firms. However, as a single regression coefficient in Eq. (1), this estimate conflates safety improvements and regulatory leakage effects. To disentangle these components, we demonstrate below how the leakage effect of -4 and the safety effect of -2 can be isolated by comparing the firm-level estimate with the mine-level fixed-2009 and time-varying treatment effect estimates.

2. Mine-level Safety Change - Fixed Treatment Estimate

Mine-level safety measures changes in the safety levels of mines that existed at the time of regulation, regardless of subsequent ownership changes. For the fixed-2009 treatment estimate, a mine's public or private ownership status is held constant as of 2009, before the implementation of DF1503. This approach ensures that any changes in safety for mines that change ownership after DF1503 are still attributed to their original 2009 ownership classification, as described below.

We estimate mine-level safety with fixed-2009 treatment using the following regression model (Eq. 2).

$$Unsafe_{m,t} = \beta_1 Public_{m,2009} \times Post2009 + \beta_2 Public_{m,2009} + \theta Controls + \Sigma\beta_i Year\ FE + \Sigma\beta_i Mine\ FE + \varepsilon_{m,t} \quad (2)$$

This specification is similar to Eq. (1), but with two key differences: (1) mine-level fixed effects are included to control for time-invariant differences in mine-level safety (approximating inherent mine safety), and (2) the *Public* indicator is fixed at the mine level as of 2009, before DF1503, regardless of subsequent ownership changes. By including mine-level fixed effects and holding public/private status fixed as of 2009, the estimate isolates within-mine safety changes, eliminates any effects from sales of inherently unsafe mines, and assigns any impact of safety changes following mine sales to the owner as of 2009. For example, if a public firm sells a mine to a private firm, any subsequent changes in the safety of that mine are still included in the treatment effect for public firms because the mine remains classified as public based on its 2009 ownership status.

Building on the previous example Table A2 illustrates the mine-level fixed-2009 treatment specification. Mines with safety changes between the pre- and post-DF1503 periods contribute to the estimate, while mines with no changes or those introduced in the post-period are excluded. Notable differences compared to the firm-level reported safety specification include:

- Excluded Mines: *Mines D*, *F*, and *G* have no changes in safety between the pre- and post-periods, and *Mines E* and *H* only exist in the post-period. As such, these mines do not contribute to the estimated treatment effect.
- *Mine C* Treatment: Although *Mine C* is sold to a private firm in the post-DF1503 period, it remains classified as public based on its 2009 ownership. Its safety deterioration in the post-period is attributed to the treatment group.

Table A2 Panel A summarizes the pre- and post-DF1503 safety changes for the mine-level fixed treatment specification. The public firm shows a reduction in *Unsafe* mines by −1, while the private firm has no change.

Table A2 Panel A – Pre-Post DF1503 Safety Changes for the Mine-level Safety Fixed-2009 Treatment Specification

Fixed Treatment Estimate

Public - Regulated				**Private - Unregulated**		
Including Mine Fixed Effects	**Post - Pre**			Including Mine Fixed Effects	**Post - Pre**	
Mine A	-1	Improve Safety		Mine F	Excluded from Estimate	No Change
Mine B	-1	Shutdown		Mine G	Excluded from Estimate	No Change
Mine C	1	Sell to Private, Safety Declines		Mine C	Excluded from Estimate	Treatment Status Fixed
Mine D	Excluded from Estimate	No Change		Mine H	Excluded from Estimate	Open New
Mine E	Excluded from Estimate	Open New				
Unsafe Mines	**-1**			**Unsafe Mines**	**0**	

Table A2 Panel B provides a detailed breakdown of the estimated treatment effect, which is decomposed as follows:

- Safety Improvements (green): −2, resulting from:
  - *Mine A* improving safety (−1).
  - *Mine B* being shut down (−1).
- Leakage Effect (red): +1, resulting from *Mine C*'s safety worsening after being sold to the private firm, though it remains classified as public due to the fixed treatment definition.
- The estimated DiD treatment effect is −1, reflecting the combination of these components.

Table A2 Panel B – Breakdown of the DiD Effect in the Mine-level Safety Fixed Treatment Specification

	Pre	**Post**	**Post - Pre**
Public	3	2	-1
Private	0	0	0
		DiD	**-1**

Breakdown of Treatment Effect	
Public Improves Safety (Mine A)	-1
Public Shuts down Unsafe (Mine B)	-1
Operate that Unsafe Worse (Mine C)	1
DiD Estimate	**-1**

As shown, the mine-level fixed-2009 treatment DiD estimate captures only the changes in mine safety from the pre- to the post-DF1503 period. It excludes the effects of safety improvements resulting from the transfer of inherently unsafe mines and the opening of new mines. While the safety change for the treatment group in this specification comes close to capturing the actual safety improvements made by public firms after DF1503, it still conflates these improvements with one component of regulatory leakage—changes in safety when a mine is sold to a private firm. In the following section,

we will show how the mine-level time-varying treatment specification allows us to isolate this component of regulatory leakage and estimate the actual firm-level safety improvements.

3. Safety Change – Time-Varying Treatment Estimate

The mine-level time-varying treatment estimate of safety is similar to the fixed treatment estimate (i.e., it includes mine-level fixed effects) except that it allows a mine's public status to change between the pre- and post-DF1503 periods. That is, as in Eq. (1), the firm-level reported safety changes specification, we define the public firm indicator, $Public_{m.t}$, to allow mines to change ownership status from the pre- to the post-period (i.e., from the treatment to the control group). By shifting any changes in safety when mines are transferred to the control group, we can compare this specification to the fixed treatment estimate to capture the deterioration of mine safety after a transfer to private ownership.

Table A3 Panel A provides an illustration of the mine-level time-varying treatment specification. Compared to the fixed-2009 treatment estimate, this approach attributes safety changes following ownership transfers to the subsequent owner. Specifically, as shown in Table A3 Panel A, the public firm achieves a reduction in *Unsafe* mines of −2, reflecting its safety improvements (e.g., *Mines A* and *B*). The private firm experiences an increase of *Unsafe* mines by + 1 driven by the worsening safety of *Mine C* after transfer. The key difference here is that unlike in the fixed treatment estimate, *Mine C*'s post-sale safety deterioration (+1) is attributed to the private firm, not the public firm.

Table A3 Panel A – Pre-Post DF1503 Safety Changes for the Mine-level Safety Variable Treatment Specification

Variable Treatement Estimate

Public - Regulated				**Private - Unregulated**			
Including Mine Fixed Effects		**Post - Pre**		Including Mine Fixed Effects		**Post - Pre**	
Mine A		-1	Improve Safety	Mine F	Excluded from Estimate		No Change
Mine B		-1	Shutdown	Mine G	Excluded from Estimate		No Change
Mine C		-	Sell to Private, Safety Declines	Mine C		1	Buy from Public, Safety Declines
Mine D	Excluded from Estimate		No Change	Mine H	Excluded from Estimate		Open New
Mine E	Excluded from Estimate		Open New				
Change in Unsafe Mines		**-2**		**Change in Unsafe Mines**		**1**	

Table A3 Panel B breaks down the estimated treatment effect as follows:

- Safety Improvements (green): −2, resulting from *Mine A*'s safety improvement and *Mine B*'s shutdown.
- Leakage Effect (red): +1, representing the worsening safety of *Mine C* after it is transferred to private ownership.
- The DiD treatment effect is calculated as −2 – 1= −3, capturing both public safety improvements and the regulatory leakage effect.

Table A3 Panel B – Breakdown of the DiD Effect in the Mine-level Safety Fixed Treatment Specification

	Pre	Post	Post - Pre
Public	2	0	-2
Private	0	1	1
		DiD	-3

Breakdown of Treatment Effect	
Public Improves Safety (Mine A)	-1
Public Shuts down Unsafe (Mine B)	-1
Operate that Unsafe Worse (Mine C)	-1
DiD Estimate	-3

Next, we will discuss how shifting the safety changes associated with mines transferred to private firms into the control group allows us to (1) capture the deterioration in mine safety after transfer to private ownership, (2) isolate the safety improvements made by public firms, and (3) construct separate estimates of genuine safety improvements and regulatory leakage.

4. Comparing the Estimates to Calculate Regulatory Leakage

Estimates from the three regressions can be compared to decompose the total treatment effect into the component attributable to safety improvements and the componed attributable to regulatory leakage.

First, the difference between the time-varying treatment estimate (−3) and the fixed-2009 treatment estimate (−1) is 2. This difference reflects the safety decline under private ownership for *Mine C*, which shifts from the treatment group in the fixed-2009 specification to the control group in the time-varying specification. As shown in Table A4, the decline in mine safety under private ownership can be calculated as 1 by dividing this difference in half (i.e., because the DiD estimate double counts the effect when it shifts from the treatment to the control group).

Second, the safety improvements made by public firms can be estimated by subtracting the safety decline under private ownership (1) from the fixed-2009 treatment estimate (−1). This calculation yields a total safety improvement of −2, as reflected in Table A4. These improvements correspond to *Mines A* (safety enhancement) and *B* (shutdown).

Finally, the regulatory leakage effect (which we know from the example is -4), can be calculated by comparing the safety improvement effect (−2) to the total reported safety change in the firm-level specification (−6). The difference (−4) represents the regulatory leakage effect, which arises from selling inherently unsafe mines to private firms, a deterioration in safety under private ownership, and private firms being more likely to open new mines in high-safety-risk locations.

The table below illustrates this calculation, both for the example above and for our estimated regression coefficients in Table 2 of the manuscript. As summarized in the table:

- Safety Improvements: the total effect of safety improvements is −2 in the example and 3.8% based on the regression estimates in Table 2.
- Regulatory Leakage: the total leakage effect is −4 in the example and 1.0% based on the regression estimates in Table 2.

Based on this breakdown, we can say that 73.7% (-2.8%/-3.8%) of the reported safety improvement is attributable to actual safety improvements, while 26.3% (-1.0%/-3.8%) is due to regulatory leakage.

Table A4 – Comparing the Estimates to Calculate Regulatory Leakage

1. Compare Variable Treatment Estimate to Fixed Treatement Estimate

	Example	Table 2
Variable Treatment Estimate (Col. 9)	-3	-2.9%
Fixed Treatment Estimate (Col. 8)	-1	-2.7%
Difference	2	0.2%
Safety Decline under Private	**1**	**0.1%**

2. Subtract Safety Decline under Private from Fixed Treatment Estimate

	Example	Table 2
Fixed Treatment Estimate (Col. 8)	-1	-2.7%
Safety Decline Under Private	1	0.1%
Safety Improvements	**-2**	**2.8%**

3. Subtract the Safety Improvements from the Reported Safety Change Estimate

	Example	Table 2
Firm-Level Treatment Estimate (Col. 7)	-6	3.8%
Safety Improvements	-2	2.8%
Regulatory Leakage	**-4**	**-1.0%**

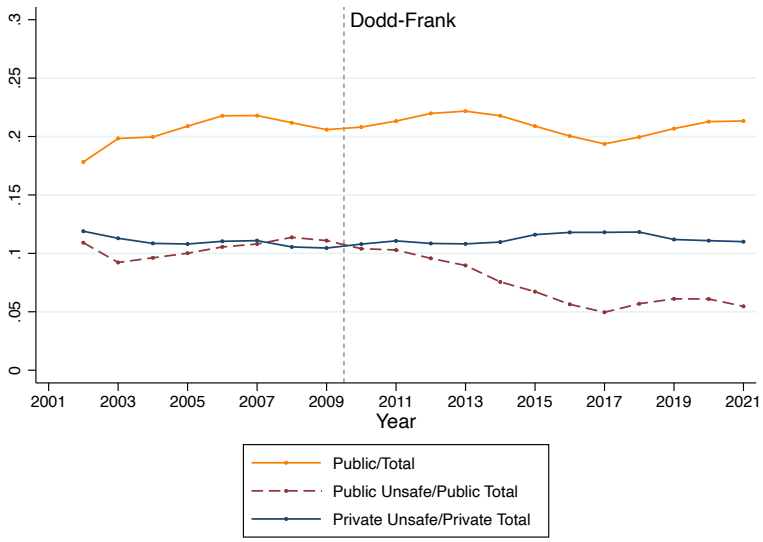
Percentage of Reported Safety Improvement Attributable to:	
Safety improvements (2.80%/3.80%)	**73.7%**
Regulatory Leakage (1.00%/3.80%)	**26.3%**

Appendix C - Sample Selection Criteria

Sample Selection:	Unique Mines	Mine-Year Observations
MSHA Mines - Downloaded Date 11/25/2022 - From 2000 to 2021	35,117	374,550
Excluding mines that never exceed 10,000 employee working hours	14,825	208,890
Excluding mines never inspected	14,787	208,755
Table 2 - Firm- and Mine-level Changes in Unsafe Mines	14,787	188,373
Table 3 - Shutdowns of Unsafe Mines	14,543	137,861
Table 4 - Mine Ownership Changes	14,045	164,359
Table 5 - Opening Mines in High-Safety-Risk Areas	4,860	4,860

Notes: This table describes the sample selection process for our analyses. The sample is from 2000 to 2021 for Table 2, and from 2001 to 2021 for Table 3, Table 4, and Table 5.

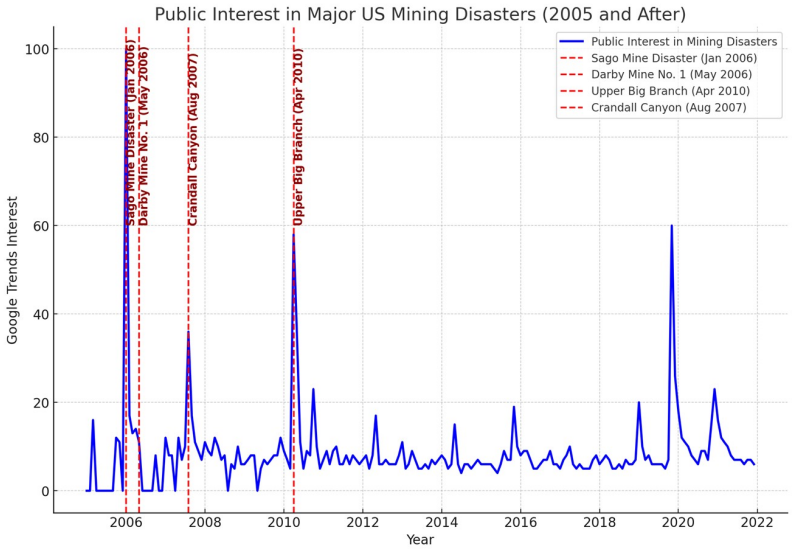
Figure 1: Percentage of Public Mines and Unsafe-to-Total Mines Owned by Public and Private Firms



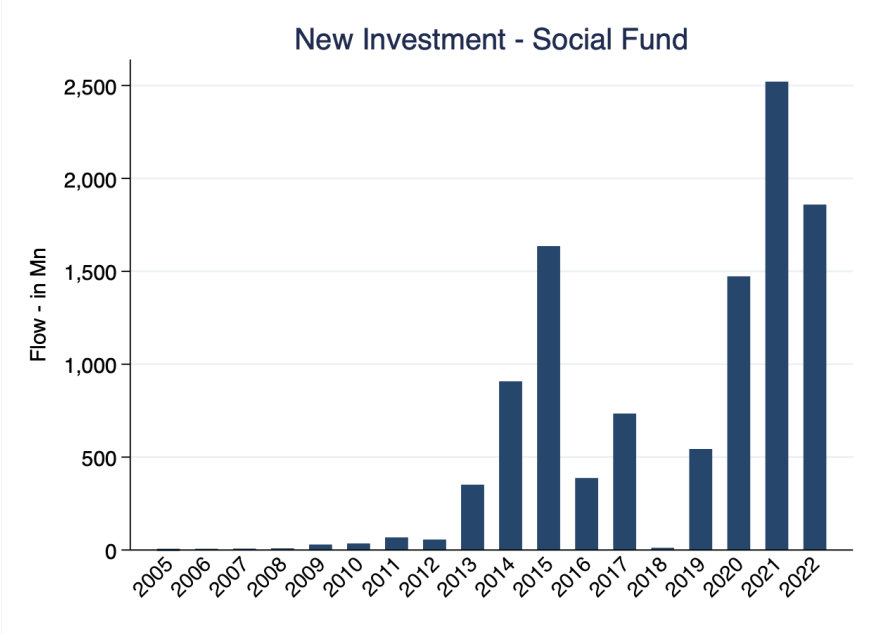
Notes: The figure shows the proportion of *Public* mines and the univariate changes in the ratio of $Unsafe$ mines to total mines for *Public* and *Private* firms. The orange line shows the 3-year rolling average (years t, t-1, t-2) of the ratio of *Public* mines to all mines. The red line shows the 3-year rolling average (years t, t-1, t-2) of the ratio of $Unsafe$ *Public* mines to all *Public* mines. The blue line shows the 3-year rolling average (years t, t-1, t-2) of the ratio of $Unsafe$ *Private* mines to all *Private* mines. Since calculating the 3-year rolling average requires data from the two preceding years, the graph starts in 2002.

Figure 2: Public Scrutiny and Investor Preferences

Panel A: Public Scrutiny of Mine Safety based on Google Trends



Panel B: New Subscription to Social Funds



Notes: Panel A illustrates public interest in mining disasters within the United States from 2005 onwards (due to the finite availability of Google Trends data), based on Google Trends data from the search term "mine safety." The blue line represents the level of public interest over time, with the percentage scale on the y-axis indicating the relative search volume. Panel B shows the new subscription to social-oriented US investment funds (2005-2022), based on Survivor-Bias-Free US Mutual Fund Database from CRSP.

Table 1: Characteristics of Public Firms

Panel A: Public Owners by Industry

Industry	Percentage of Issuers
Mining	
Coal	12.4%
Non-coal	40.1%
Construction	8.9%
Manufacturing	19.9%
Transportation, Communications, Electric, Gas, and Sanitary Services	9.2%
Wholesale Trade	2.4%
Retail Trade	4.3%
Finance, Insurance, and Real Estate	1.2%
Services	1.4%
Public Administration	0.2%
Number of issuers subject to MSD	100.0%

Panel B: Mines and Firms by Year

Year	Number of Public Firms	Number of Public Mines	Number of Private Firms	Number of Private Mines
2000	176	1281	4203	8100
2001	208	1941	4273	7870
2002	200	1984	4302	7974
2003	186	1939	4276	7978
2004	183	2019	4288	7974
2005	189	2256	4286	7811
2006	184	2234	4246	7917
2007	191	2055	4179	8091
2008	186	2107	4106	8071
2009	183	2045	4058	8000
2010	183	2095	3980	7815
2011	166	2196	3903	7587
2012	170	2191	3863	7545
2013	165	2075	3778	7470
2014	160	2018	3735	7385
2015	157	1790	3627	7391
2016	148	1677	3553	7295
2017	144	1671	3493	7176
2018	147	1820	3438	6891
2019	135	1769	3374	6776
2020	126	1700	3304	6663
2021	122	1703	3212	6409

Notes: This table presents the industry distribution (Panel A) and the sample distribution by year (Panel B). MSHA provides ownership information by pairing a controller and an operator for each mine during a given period. We classify a mine as public if either the controller or the operator is public. To avoid duplication, each pair is counted only once in this table.

Table 2: Firm- and Mine-level Changes in Unsafe Mines

Panel A: Descriptive Statistics – Public Mines

	N	Mean	SD	P1	P25	P50	P75	P99
*Post*2009	38,747	0.529	0.499	0.000	0.000	1.000	1.000	1.000
Unsafe(Violations)$_t$	38,747	0.061	0.240	0.000	0.000	0.000	0.000	1.000
Unsafe(Serious Injury)$_t$	38,747	0.029	0.168	0.000	0.000	0.000	0.000	1.000
Unsafe(All)$_t$	38,747	0.087	0.282	0.000	0.000	0.000	0.000	1.000
Annual Hours(Thousands)	38,747	119.311	286.988	0.317	15.390	34.154	89.370	1,390.708

Panel B: Descriptive Statistics – Private Mines

	N	Mean	SD	P1	P25	P50	P75	P99
*Post*2009	149,626	0.514	0.500	0.000	0.000	1.000	1.000	1.000
Unsafe(Violations)$_t$	149,626	0.103	0.304	0.000	0.000	0.000	0.000	1.000
Unsafe(Serious Injury)$_t$	149,626	0.011	0.105	0.000	0.000	0.000	0.000	1.000
Unsafe(All)$_t$	149,626	0.112	0.315	0.000	0.000	0.000	0.000	1.000
Annual Hours(Thousands)	149,626	38.962	88.484	0.207	8.421	16.278	35.258	376.137

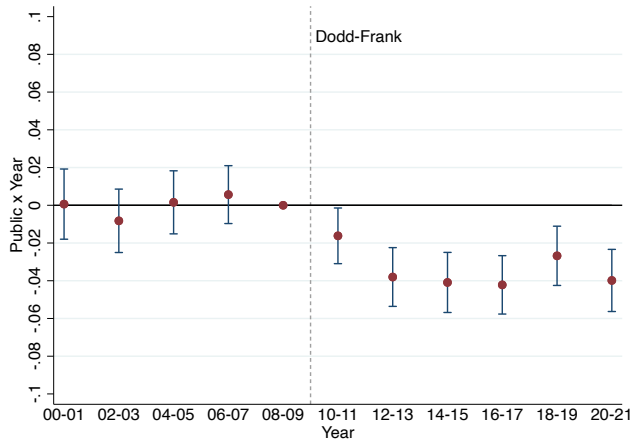
Panel C: Analyses of Changes in Unsafe Mines

	Violations			Serious Injury			All		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable: Unsafe$_t$									
$Public_t \times Post2009$	-0.031***		-0.021***	-0.009***		-0.009***	-0.038***		-0.029***
	(-5.394)		(-6.755)	(-3.789)		(-4.265)	(-5.750)		(-7.851)
$Public_{2009} \times Post2009$		-0.021***			-0.008***			-0.027***	
		(-6.715)			(-3.698)			(-7.339)	
$Public_t$	-0.018***		-0.015***	0.012***		0.005**	-0.008		-0.009**
	(-3.234)		(-4.198)	(5.816)		(2.524)	(-1.242)		(-2.317)
Log(Annual Hours)	-0.008***	0.003**	0.003***	0.013***	0.006***	0.006***	0.004***	0.007***	0.007***
	(-11.107)	(2.560)	(2.753)	(30.288)	(16.114)	(16.198)	(4.804)	(7.123)	(7.311)
Fixed Effects:									
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mine	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Cluster	Owner	Owner	Owner	Owner	Owner	Owner	Owner	Owner	Owner
Mine-Year Observations	188,373	164,932	164,932	188,373	164,932	164,932	188,373	164,932	164,932
Adjusted R-Squared	0.006	0.215	0.216	0.030	0.083	0.083	0.002	0.194	0.195

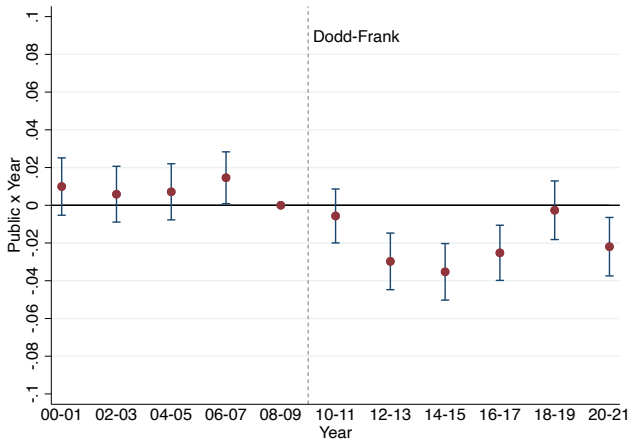
Notes: This table presents results on firm- and mine-level changes in unsafe mines by *Public* firms compared to *Private* firms around DF1503. Panel A shows descriptive statistics for *Public* firms. Panel B shows descriptive statistics for *Private* firms. Panel C shows regression results on changes in $Unsafe$ mines by *Public* firms. $Public_t$ is an indicator that equals one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F for that fiscal year. $Public_{2009}$ is an indicator that equals one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F for the fiscal year of 2009. $Post2009$ is an indicator that equals one if the year is between 2010 and 2021 and zero if the year is between 2000 and 2009. Columns (1)—(3) present the results when the dependent variable, $Unsafe_t$, is defined based on violations. $Unsafe_t$*(Violations)* is an indicator that equals one if the number of S&S violations scaled by the number of employees is higher than the 90th percentile of all mines in that year, and zero otherwise. Columns (4)—(6) present the results when the dependent variable, $Unsafe_t$, is defined based on serious injuries in that year. $Unsafe_t$*(Serious Injury)* is an indicator that equals one if there are any fatalities or permanent disabilities in that year, and zero otherwise. Columns (7)—(9) present the results when the dependent variable, $Unsafe_t$, is defined based on either violations or serious injuries. $Unsafe_t$*(All)* is an indicator that equals one if the number of S&S violations scaled by the number of employees is higher than the 90th percentile of all mines or if there are any fatalities or permanent disabilities in that year, and zero otherwise. All columns include year fixed effects. Columns (1), (4), and (7) do not include mine fixed effects, whereas all other columns do. We provide a detailed description of the variables in Appendix A. Panel C reports ordinary least squares (OLS) coefficient estimates with t-statistics (in parentheses) based on robust standard errors clustered by mine owner. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 3: Treatment Effects in Event-time Table 2

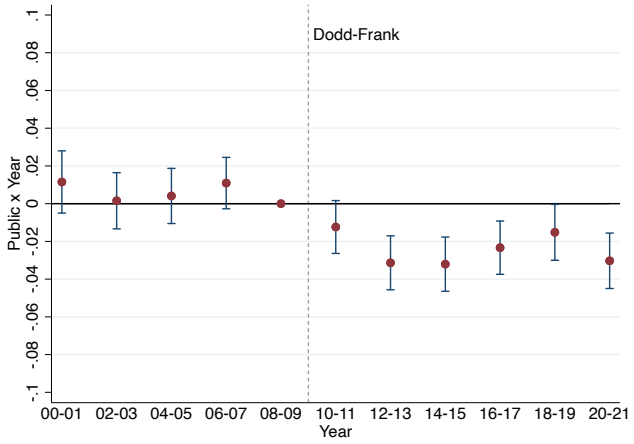
Panel A: Column (7)



Panel B: Column (8)

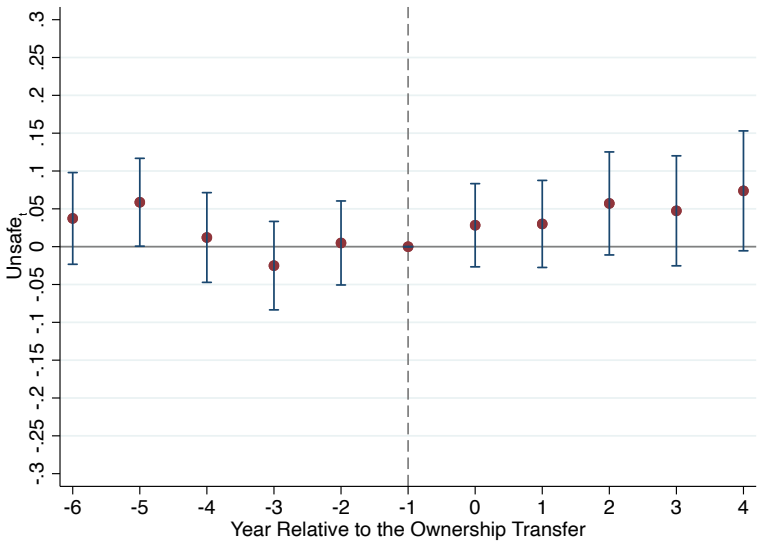


Panel C: Column (9)



Notes: This figure displays OLS regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at mine-owner level from Table 2 Panel C Column (7), Column (8), and Column (9) in Panels A, B, and C, respectively. To map out the pattern in the counterfactual treatment effects, we include indicators for every two-year period in the sample except for 2008-2009, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We map out the estimates over two-year periods because violations and serious injuries are infrequent events.

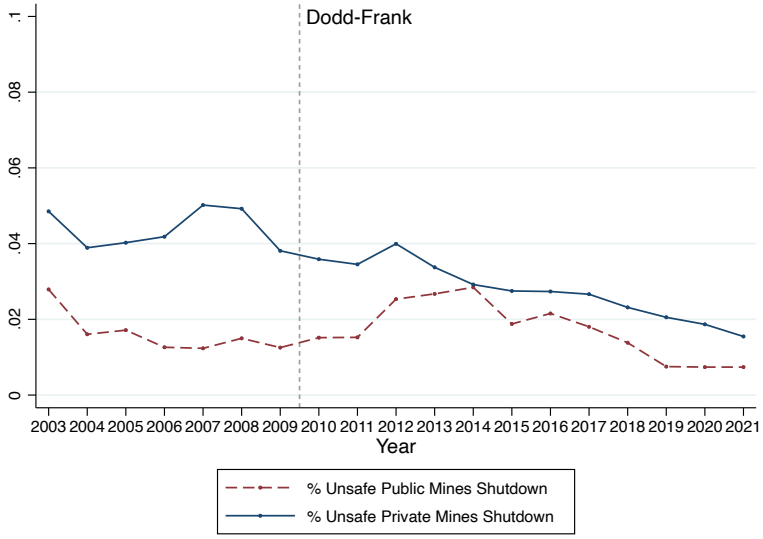
Figure 4: Safety Change Around Ownership Changes from Public to Private



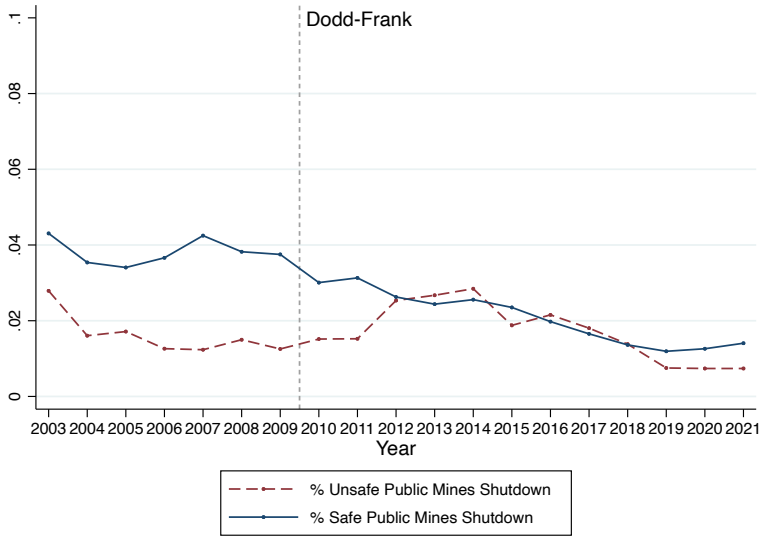
Notes: This figure reports the OLS regression coefficient estimates and two-tailed 95% confidence intervals in a stacked difference-in-differences framework. The specification uses the same control variables, fixed effects, and clustering of standard errors as in Table 2 Panel C Column (8). The graph shows mine safety changes around mine ownership changes from *Public* to *Private* firms between 2010 and 2021, with the control group comprising mines that remain *Private* throughout the sample period. To address potential confounding effects from overlapping ownership transfers, mines experiencing multiple transfers during the sample period were excluded from the analysis.

Figure 5: Univariate Changes in the Percentage of Mine Shutdowns

Panel A: Shutdowns of Unsafe Mines by Public and Private Firms



Panel B: Shutdowns of Safe and Unsafe Mines by Public Firms



Notes: The figure shows univariate changes in the percentage of mine shutdowns. Panel A presents the figure for shutdowns of $Unsafe$ mines by $Public$ and $Private$ firms. Panel B presents the figure for shutdowns of $Safe$ and $Unsafe$ mines by $Public$ firms. In Panel A, the red line represents the 3-year rolling average (years t, t-1, t-2) percentage of $Unsafe\ Public$ mines being shut down. The blue line shows the 3-year rolling average (years t, t-1, t-2) percentage of $Unsafe\ Private$ mines being shut down. In Panel B, the red line represents the 3-year rolling average (years t, t-1, t-2) percentage of $Unsafe\ Public$ mines being shut down. The blue line shows the 3-year rolling average (years t, t-1, t-2) percentage of $Safe\ Public$ mines being shut down. Since calculating the 3-year rolling average requires data from the two preceding years, the graph starts in 2003.

Table 3: Mine Shutdowns

Panel A: Descriptive Statistics

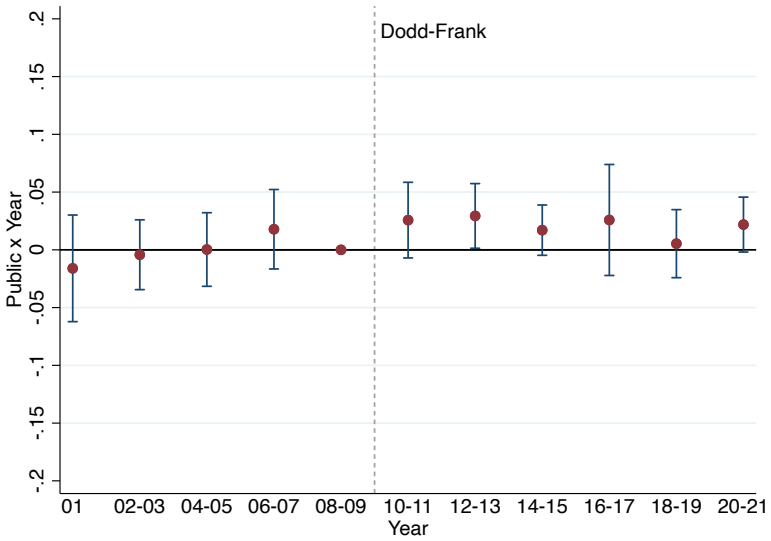
	N	Mean	SD	P1	P25	P50	P75	P99
$Public_t$	133,563	0.228	0.419	0.000	0.000	0.000	0.000	1.000
$Post2009$	133,563	0.480	0.500	0.000	0.000	0.000	1.000	1.000
$Unsafe(Violations)_{t-1}$	133,563	0.099	0.298	0.000	0.000	0.000	0.000	1.000
$Unsafe(Serious\ Injury)_{t-1}$	133,563	0.019	0.135	0.000	0.000	0.000	0.000	1.000
$Unsafe(All)_{t-1}$	133,563	0.114	0.318	0.000	0.000	0.000	0.000	1.000
Shutdown	133,563	0.044	0.206	0.000	0.000	0.000	0.000	1.000
Annual Hours(Thousands)	133,563	66.577	176.225	0.000	10.668	22.809	53.333	841.571
Underground	133,563	0.094	0.292	0.000	0.000	0.000	0.000	1.000
Coal	133,563	0.210	0.407	0.000	0.000	0.000	0.000	1.000

Panel B: Analyses of Shutdowns of Unsafe and Safe Mines

	Violations		Serious Injury		All	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:						
Shutdown	Unsafe_{t-1}	Safe_{t-1}	Unsafe_{t-1}	Safe_{t-1}	Unsafe_{t-1}	Safe_{t-1}
$Public_t \times Post2009$	0.000	0.003	0.021***	0.002	0.009	0.002
	(0.067)	(0.823)	(2.643)	(0.566)	(1.540)	(0.628)
$Public_t$	0.016***	0.015***	0.002	0.015***	0.015***	0.014***
	(3.470)	(6.524)	(0.348)	(6.820)	(3.708)	(6.336)
Log(Annual Hours)	-0.021***	-0.032***	-0.013***	-0.031***	-0.020***	-0.033***
	(-16.531)	(-51.264)	(-4.345)	(-52.329)	(-17.870)	(-51.192)
Underground	0.005*	0.014***	-0.003	0.007***	0.004	0.012***
	(1.831)	(6.494)	(-1.420)	(3.914)	(1.632)	(5.545)
Coal	-0.055***	-0.034***	-0.018***	-0.038***	-0.050***	-0.035***
	(-15.011)	(-20.552)	(-5.132)	(-25.497)	(-16.520)	(-21.239)
Difference in Coefficients (p-value):	0.777		0.022		0.283	
Fixed Effects:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Owner	Owner	Owner	Owner	Owner	Owner
Mine-Year Observations	13,182	120,381	2,496	131,067	15,271	118,292
Adjusted R-Squared	0.093	0.116	0.068	0.113	0.093	0.117

Notes: This table presents results for the analysis of mine shutdowns. Panel A shows descriptive statistics. Panel B shows regression results for the analysis of shutdowns of $Unsafe$ and $Safe$ mines. The dependent variable, *Shutdown*, is an indicator that equals one if a mine transitions from active to inactive for the first time in year t, and zero in active years before. *Public* is an indicator that equals one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F for that year. $Post2009$ is an indicator that equals one if the year is between 2010 and 2021 and zero if the year is between 2001 and 2009. Columns (1) and (3) present the results for the $Unsafe$ subsample, where $Unsafe$ is defined based on violations and serious injuries, respectively. Column (5) presents the results for the $Unsafe$ subsample, defined by either violations or serious injuries. Column (2), (4), and (6) show the results for the $Safe$ subsample, where the mine is not classified as $Unsafe$ based on these criteria. All columns include year fixed effects. We provide a detailed description of the variables in Appendix A. Panel B reports ordinary least squares (OLS) coefficient estimates with t-statistics (in parentheses) based on robust standard errors clustered by mine owner. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

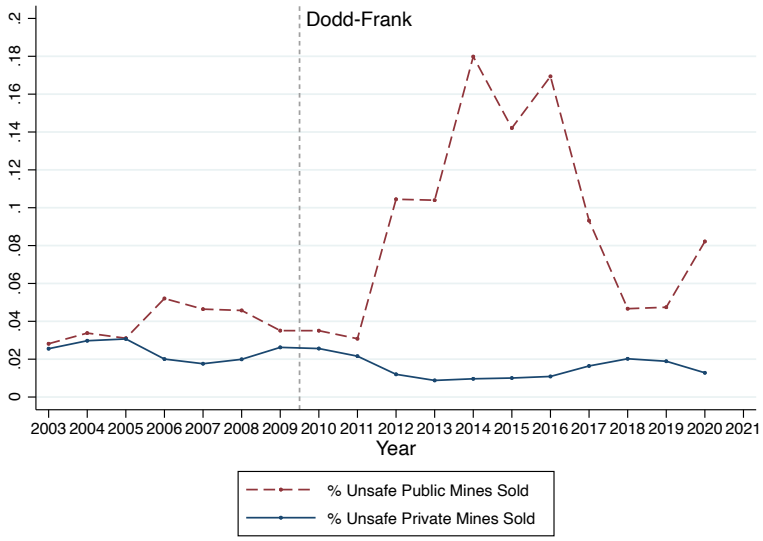
Figure 6: Treatment Effects in Event-time Table 3 Panel B Column (3)



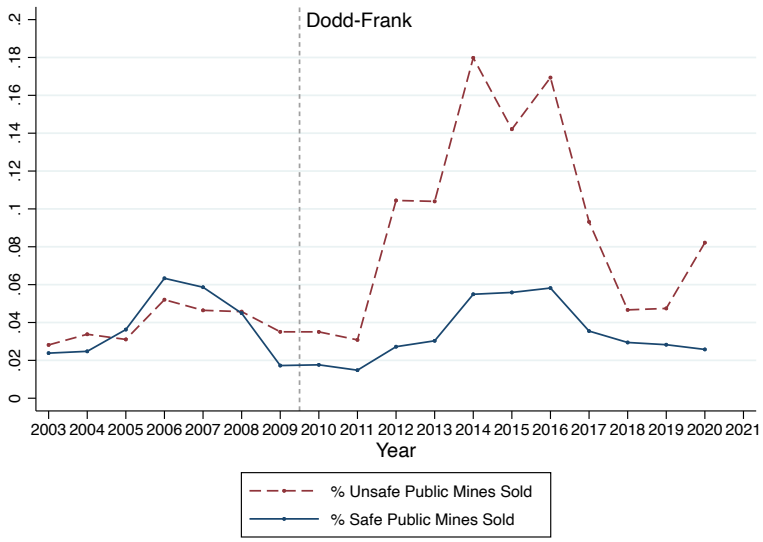
Notes: This figure displays OLS regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at mine-owner level from Table 3 Column (3). To map out the pattern in the counterfactual treatment effects, we include indicators for every two-year period in the sample except for 2008-2009, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We map out the estimates over two-year periods because violations and serious injuries are infrequent events.

Figure 7: Univariate Mine Ownership Changes

Panel A: Ownership Changes of Unsafe Mines by Public and Private Firms



Panel B: Ownership Changes of Safe and Unsafe Mines by Public Firms



Notes: The figure shows univariate mine ownership changes around DF1503. Panel A illustrates ownership changes of $Unsafe$ mines by $Public$ and $Private$ firms. The figure shows the proportion of $Unsafe$ mines owned by $Public$ and $Private$ firms. The red line represents the 3-year rolling average (years t, t-1, t-2) of the ratio of $Unsafe$ $Public$ mines to all $Unsafe$ mines. The blue line shows the 3-year rolling average (years t, t-1, t-2) of the ratio of $Unsafe$ $Private$ mines to $Unsafe$ mines. Panel B displays ownership changes of $Safe$ and $Unsafe$ mines by $Public$ firms. The red line represents the 3-year rolling average (years t, t-1, t-2) of the ratio of $Unsafe$ $Public$ mines to all $Public$ mines. The blue line shows the 3-year rolling average (years t, t-1, t-2) of the ratio of $Safe$ $Public$ mines to $Public$ mines. As calculating the 3-year rolling average requires data from the two preceding years, the graph starts in 2003.

Table 4: Mine Ownership Changes

Panel A: Descriptive Statistics

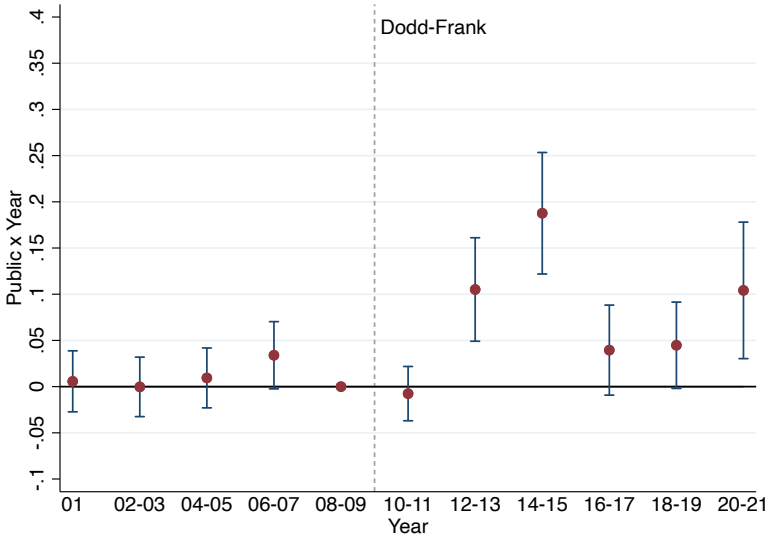
	N	Mean	SD	P1	P25	P50	P75	P99
$Public_t$	164,359	0.211	0.408	0.000	0.000	0.000	0.000	1.000
$Post2009$	164,359	0.530	0.499	0.000	0.000	1.000	1.000	1.000
$Unsafe(Violations)_{t-1}$	164,359	0.093	0.290	0.000	0.000	0.000	0.000	1.000
$Unsafe(Serious\ Injury)_{t-1}$	164,359	0.016	0.124	0.000	0.000	0.000	0.000	1.000
$Unsafe(All)_{t-1}$	164,359	0.106	0.308	0.000	0.000	0.000	0.000	1.000
Sell	164,359	0.019	0.138	0.000	0.000	0.000	0.000	1.000
Annual Hours(Thousands)	164,359	56.828	158.242	0.000	9.248	19.064	44.012	746.880
Underground	164,359	0.074	0.261	0.000	0.000	0.000	0.000	1.000
Coal	164,359	0.167	0.373	0.000	0.000	0.000	0.000	1.000

Panel B: Analyses of Ownership Changes in Unsafe Mines

	Violations		Serious Injury		All	
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable:						
Sell	$Unsafe_{t-1}$	$Safe_{t-1}$	$Unsafe_{t-1}$	$Safe_{t-1}$	$Unsafe_{t-1}$	$Safe_{t-1}$
$Public_t \times Post2009$	0.075***	0.001	0.036**	0.004	0.063***	-0.000
	(5.212)	(0.168)	(2.187)	(0.685)	(5.733)	(-0.064)
$Public_t$	0.008	0.019***	-0.021**	0.020***	0.002	0.020***
	(1.031)	(4.312)	(-2.365)	(4.601)	(0.328)	(4.478)
Log(Annual Hours)	0.001	0.001***	-0.002	0.001***	0.000	0.001***
	(1.529)	(3.490)	(-0.739)	(3.898)	(0.627)	(3.677)
Underground	0.019***	0.014***	0.022**	0.016***	0.019***	0.014***
	(3.594)	(5.178)	(2.225)	(6.315)	(4.049)	(5.005)
Coal	0.026***	0.022***	0.030***	0.024***	0.027***	0.023***
	(6.582)	(9.470)	(3.533)	(10.345)	(7.392)	(9.489)
Difference in Coefficients (p-value):	0.000		0.052		0.000	
Fixed Effects:						
Year	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	Owner	Owner	Owner	Owner	Owner	Owner
Mine-Year Observations	15,229	149,130	2,566	161,793	17,392	146,967
Adjusted R-Squared	0.039	0.014	0.029	0.016	0.034	0.014

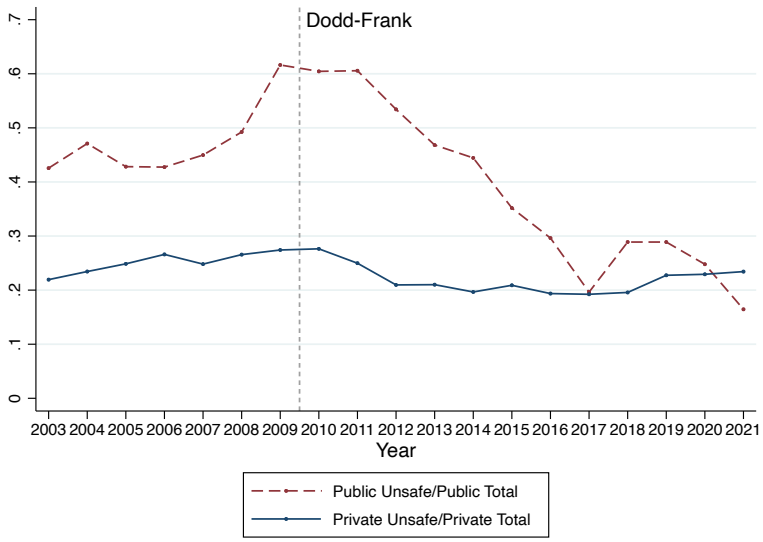
Notes: This table presents results on the analysis for mine ownership changes. Panels A shows descriptive statistics. Panel B shows regression results. The dependent variable, *Sell*, is an indicator that equals one if $Public_{t+1}$ is different from $Public_t$. $Public_t$ is an indicator that equals one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F for that year. $Post2009$ is an indicator that equals one if the year is between 2010 and 2021 and zero if the year is between 2001 and 2009. Columns (1) and (3) present the results for the $Unsafe$ subsample, where $Unsafe$ is defined based on violations and serious injuries, respectively. Column (5) presents the results for the $Unsafe$ subsample, defined by either violations or serious injuries. Column (2), (4), and (6) show the results for the $Safe$ subsample, where the mine is not classified as $Unsafe$ based on these criteria. All columns include year fixed effects. We provide a detailed description of the variables in Appendix A. Panel B reports ordinary least squares (OLS) coefficient estimates with t-statistics (in parentheses) based on robust standard errors clustered by mine owner. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 8: Treatment Effects in Event-time Table 4 Panel B Column (5)



Notes: This figure displays OLS regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at mine-owner level from Column (5) of Table 4 Panel B. To map out the pattern in the counterfactual treatment effects, we include indicators for every two-year period in the sample except for 2008-2009, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We map out the estimates over two-year periods because violations and serious injuries are infrequent events.

Figure 9: Univariate Changes in Mines Opened in High-Safety-Risk Areas for Public and Private Firms



Notes: The figure shows univariate changes in mines opened in high-safety-risk areas for *Public* and *Private* firms. The red line represents the 3-year rolling average (years t, t-1, t-2) of the ratio of *Open Unsafe(All) ZIP Public* mines to all *Public* mines. The blue line represents the 3-year rolling average (years t, t-1, t-2) of the ratio of *Open Unsafe(All) Private* mines to all *Private* mines. Since calculating the 3-year rolling average requires data from the two preceding years, the graph starts in 2003.

Table 5: Opening Mines in High-Safety-Risk Areas

Panel A: Descriptive Statistics

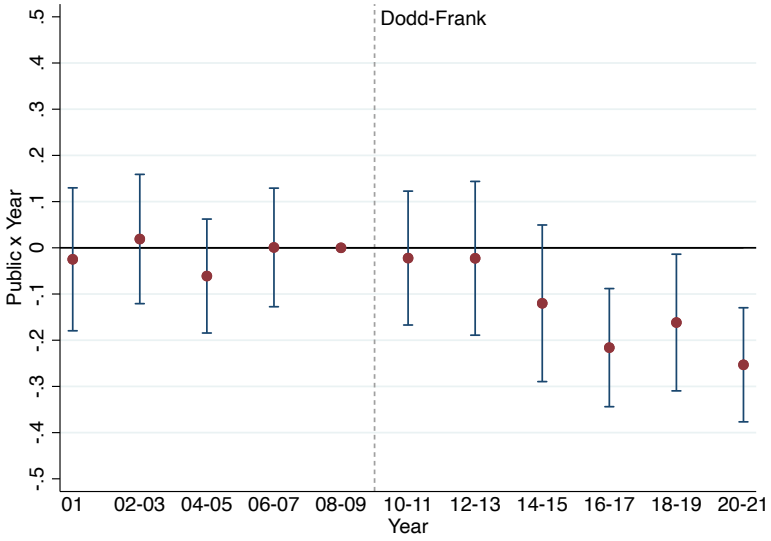
	N	Mean	SD	P1	P25	P50	P75	P99
$Public_t$	4,860	0.137	0.344	0.000	0.000	0.000	0.000	1.000
$Post2009$	4,860	0.289	0.453	0.000	0.000	0.000	1.000	1.000
Open Unsafe(All) ZIP	4,860	0.271	0.444	0.000	0.000	0.000	1.000	1.000
Open Unsafe(Violations) ZIP	4,860	0.201	0.401	0.000	0.000	0.000	0.000	1.000
Open Unsafe(Serious Injury) ZIP	4,860	0.106	0.308	0.000	0.000	0.000	0.000	1.000
Annual Hours(Thousands)	4,860	11.241	27.098	0.000	0.606	4.860	12.256	92.187
Coal	4,860	0.367	0.482	0.000	0.000	0.000	1.000	1.000
Underground	4,860	0.150	0.358	0.000	0.000	0.000	0.000	1.000

Panel B: Analyses of Mine Opening

	(1)	(2)	(3)
Dependent Variable:			
Open Unsafe ZIP	Violations	Serious Injury	All
$Public_t \times Post2009$	0.026	-0.088**	-0.015
	(0.582)	(-2.312)	(-0.323)
$Public_t$	-0.046*	0.155***	0.070***
	(-1.879)	(6.096)	(2.853)
Log(Annual Hours)	0.002	0.000	0.001
	(0.954)	(0.028)	(0.290)
Coal	0.352***	0.087***	0.381***
	(18.801)	(6.026)	(19.021)
Underground	0.184***	0.104***	0.210***
	(7.558)	(5.049)	(8.448)
Fixed Effects:			
Year	Yes	Yes	Yes
Cluster	Owner	Owner	Owner
Mine Opening-Year Observations	4,860	4,860	4,860
Adjusted R-Squared	0.257	0.087	0.277

Notes: This table presents the results from the analysis of opening mines in high-safety-risk areas. Panel A shows descriptive statistics. Panel B shows regression results. The dependent variable, *Open Unsafe ZIP*, is an indicator equal to one if, between 2000 to 2009, the percentage of $Unsafe$ mine-year observations in the area measured by the 5-digit ZIP code is above the 90th percentile of all areas, and zero if the percentage is below the 90th percentile. *Public* is an indicator that equals one if at least one of a mine's owner(s) files a 10-K, 20-F, or 40-F for that year. $Post2009$ is an indicator that equals one if the year is between 2010 and 2021 and zero if the year is between 2001 and 2009. Column (1) presents results when an $Unsafe$ mine is defined based on violations. Column (2) presents results when an $Unsafe$ mine is defined based on serious injuries. Column (3) presents results when an $Unsafe$ mine is defined based on either violations or serious injuries. All columns include year fixed effects. We provide a detailed description of the variables in Appendix A. Panel B reports OLS coefficient estimates with t-statistics (in parentheses) based on robust standard errors clustered by the mine owner. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Figure 10: Treatment Effects in Event-time Table 5 Panel B Column (2)



Notes: This figure displays OLS regression coefficient estimates and two-tailed 95% confidence intervals based on standard errors clustered at mine-owner level from Table 5 Panel B Column (2). To map out the pattern in the counterfactual treatment effects, we include indicators for every two-year period in the sample except for 2008-2009, which serves as the benchmark period (i.e., the coefficient is constrained to equal zero). We map out the estimates over two-year periods because violations and serious injuries are infrequent events.