

# The Regulator’s Deaf Ear: Mine Safety Enforcement Does Not Respond to News Competition

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## Abstract

When a miner dies on the same day a hurricane makes landfall, does the Mine Safety and Health Administration look the other way? Using 1,069 mine fatality events (2000–2024) and an Eisensee-Strömberg competing-news design that instruments for media salience with weekly FEMA disaster declarations, I find that it does not. A one-standard-deviation increase in competing disaster pressure has no detectable effect on post-fatality inspections ( $\hat{\beta} = 0.015$ ,  $SE = 0.029$ ), violations ( $-0.018$ ,  $SE = 0.050$ ), or penalties ( $-0.028$ ,  $SE = 0.118$ ). The null is robust to randomization inference ( $p = 0.64$ ), Poisson models, and leave-one-out exclusion of major disasters. I can rule out effects larger than 5.7 percent of a standard deviation. Unlike disaster relief, where media salience drives federal response, mine safety enforcement operates through statutory mandates that insulate regulators from the news cycle.

**JEL Codes:** J28, K32, L51, D72

**Keywords:** mine safety, MSHA, media attention, regulatory enforcement, news competition, Eisensee-Strömberg

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# 1. Introduction

On April 5, 2010, an explosion at the Upper Big Branch mine in West Virginia killed 29 miners—the deadliest U.S. mine disaster in four decades. The story dominated national headlines for weeks, and the Mine Safety and Health Administration launched an intensive investigation that lasted over a year. But what happens when a miner dies and no one is watching? If the fatality occurs on the same day as a hurricane, a mass shooting, or a financial crisis, does the regulatory response change?

The question matters because a growing literature documents that government agencies respond not just to the underlying severity of events, but to how much media attention those events receive. [Eisensee and Strömberg \(2007\)](#) show that natural disasters receiving less news coverage due to competing events attract substantially less federal relief. [Snyder and Strömberg \(2010\)](#) demonstrate that reduced media coverage of Congressional representatives leads to weaker electoral accountability. [Mastorocco and Minale \(2022\)](#) find that media coverage of crime influences both public perceptions and law enforcement behavior. These findings collectively suggest that “regulation by attention” may be pervasive: agencies allocate enforcement resources partly based on the political salience created by media scrutiny ([Besley and Prat, 2006](#); [Strömberg, 2004](#)).

This paper asks whether the same pattern holds for workplace safety regulation. I apply the Eisensee-Strömberg competing-news design to the universe of U.S. mine fatalities reported to the Mine Safety and Health Administration (MSHA) between 2000 and 2024. The instrument exploits variation in the number of FEMA disaster declarations in the week of each mine fatality. When major natural disasters compete for media attention during the same week as a mine death, the fatality receives less coverage—but crucially, whether a hurricane strikes Florida in the same week as a mine collapse in West Virginia is plausibly exogenous to mine-level enforcement decisions.

I find a precisely estimated null. A one-standard-deviation increase in competing disaster pressure produces a statistically and economically insignificant change in post-fatality inspections (0.015 log points,  $p = 0.60$ ), violations ( $-0.018$ ,  $p = 0.72$ ), and penalties ( $-0.028$ ,  $p = 0.82$ ). The null survives randomization inference ( $p = 0.64$ ), Poisson count models, exclusion of multi-fatality disasters, and leave-one-out analysis. The confidence intervals are tight enough to rule out effects larger than 5.7 percent of a standard deviation in enforcement intensity.

The contribution is not simply the absence of a result. The null is informative because it contrasts sharply with the positive findings in the disaster relief literature. [Eisensee and Strömberg \(2007\)](#) document that competing news reduces disaster relief by 2–3 percentage

points per network newscast of competing coverage. If mine safety enforcement operated through the same attention-driven channel, we would expect media competition to measurably weaken the post-fatality regulatory response. It does not.

Why not? The institutional structure of mine safety regulation provides a natural explanation. The Federal Mine Safety and Health Act of 1977 mandates that MSHA investigate every fatal accident within 24 hours and conduct regular inspections of every mine at specified frequencies (four times per year for underground mines, twice for surface mines). These statutory obligations create a regulatory floor that does not depend on political attention. In contrast, FEMA disaster relief requires presidential declarations that are inherently discretionary (Eisensee and Strömberg, 2007). The comparison reveals that institutional design—specifically, mandatory enforcement mandates—can insulate regulatory agencies from the media-attention channel that distorts discretionary programs.

This paper contributes to three literatures. First, it extends the Eisensee-Strömberg framework to domestic regulatory enforcement, a natural but unexplored application. While competing-news instruments have been used for disaster relief, foreign aid, and election outcomes, no prior study applies this design to occupational safety regulation. Second, it contributes to the economics of mine safety (Gowrisankaran et al., 2015; Morantz, 2013; Gray and Scholz, 1987), where enforcement effectiveness is debated but the media-attention channel has not been studied. Third, it speaks to the broader question of when media accountability “works” and when institutional mandates substitute for it (Prat and Strömberg, 2013; Besley and Prat, 2006). The answer here is that mandatory inspection requirements effectively insulate enforcement from news-cycle variation.

The paper proceeds as follows. Section 2 describes the institutional setting of MSHA enforcement. Section 3 presents the data. Section 4 develops the empirical strategy. Section 5 reports results. Section 6 discusses implications.

## 2. Institutional Background

**MSHA enforcement structure.** The Mine Safety and Health Administration, an agency within the U.S. Department of Labor, regulates safety at approximately 12,000 active mines employing some 250,000 workers. MSHA’s authority derives from the Federal Mine Safety and Health Act of 1977 (“Mine Act”), as amended by the Mine Improvement and New Emergency Response Act of 2006 (“MINER Act”), enacted after the Sago mine disaster killed 12 miners.

The Mine Act establishes a mandatory inspection regime. Underground mines must receive four complete inspections per year; surface mines must receive two. These inspections are not discretionary—they are statutory requirements that MSHA must fulfill regardless

of political attention or resource constraints. When inspectors identify hazards, they issue citations or orders requiring abatement, with proposed penalties ranging from \$100 to over \$250,000 for willful violations. “Significant and Substantial” (S&S) citations carry heightened legal consequences and reflect the inspector’s judgment that the violation is reasonably likely to result in a serious injury.

**Fatal accident investigations.** When a miner is killed, MSHA initiates a mandatory investigation under 30 CFR Part 50. The mine operator must notify MSHA within 15 minutes of a fatality, and an investigation begins within 24 hours. These investigations are thorough, often taking 6–12 months, and culminate in a public report with citations and penalties. The mandatory nature of these investigations distinguishes mine safety enforcement from discretionary programs like disaster relief.

**Competing news and the attention mechanism.** The theoretical mechanism linking media attention to enforcement runs through political accountability. When a mine fatality receives national media coverage, it creates political pressure: Congressional hearings may follow, inspectors may face heightened scrutiny, and the agency’s leadership has incentives to demonstrate responsiveness ([Carpenter, 2010](#)). However, when competing events—a hurricane, a financial crisis, or a mass shooting—dominate the news cycle, a mine fatality receives less coverage, potentially reducing this political pressure.

The key question is whether MSHA’s statutory mandates are sufficiently binding to prevent the attention mechanism from operating. If mandatory inspections and investigation protocols create a regulatory floor, then media attention should not affect enforcement intensity at the margin.

### 3. Data

I combine four administrative data sources. All data are publicly available from federal agencies.

**MSHA accident records.** The Mine Safety and Health Administration provides a comprehensive database of all reportable accidents at U.S. mines through its Open Government Data initiative ([Mine Safety and Health Administration, 2024](#)). The accident file contains 271,719 records with information on severity (degree of injury), mine identification, date, and accident characteristics. I identify 1,194 fatalities (degree of injury code “01”) in the full database, of which 1,158 occur between 2000 and 2024. After collapsing to unique mine-date events (since a single incident can kill multiple workers), the analysis sample contains 1,069

fatality events at 867 distinct mines.

**MSHA inspection and violation records.** The inspection database (1,138,758 records) and violation database (3,057,779 records) provide mine-level enforcement outcomes including inspection dates, violation types, proposed penalties, and S&S designations. I construct enforcement measures in symmetric windows around each fatality: 365 days before (pre-trend control) and 90, 180, and 365 days after (post-fatality outcomes). The average mine experiences 6.1 inspections and 31.8 violations in the 90 days following a fatality, with mean proposed penalties of \$37,083.

**FEMA disaster declarations.** I obtain all federal disaster declarations since 2000 from the FEMA OpenFEMA API ([Federal Emergency Management Agency, 2024](#)), totaling 50,023 records. I count the number of distinct disaster declarations in the calendar week of each mine fatality. The mean is 2.5 declarations per week ( $SD = 2.7$ ), with substantial variation driven by hurricane seasons, wildfire events, and other natural disasters.

**CBOE VIX Index.** As an alternative news-competition proxy, I obtain the daily CBOE Volatility Index from the FRED database (6,309 trading days). The VIX captures financial market uncertainty that generates competing economic news. I use weekly averages matched to fatality dates.

### 3.1 Summary Statistics

**Table 1:** Summary Statistics

Variable	Mean	SD	Min	Max
<i>Panel A: Fatality Event Characteristics</i>				
Fatalities per event	1.08	1.02	1	29
FEMA disasters (fatality week)	2.46	2.74	0	24
Log FEMA disasters	1.00	0.70	0	3
<i>Panel B: Enforcement Outcomes</i>				
Inspections (365d pre)	18.10	31.61	0	189
Inspections (90d post)	6.06	7.79	0	44
Inspections (180d post)	10.92	15.62	0	85
Inspections (365d post)	20.43	31.75	0	185
Violations (365d pre)	83.61	151.08	0	968
Violations (90d post)	31.79	45.76	0	338
Violations (180d post)	54.74	83.93	0	633
Violations (365d post)	97.27	156.25	0	1042
Penalties (\$, 365d pre)	74270.74	227832.74	0	2746392
Penalties (\$, 90d post)	37083.19	92078.40	0	1633738
Penalties (\$, 180d post)	58512.65	140141.51	0	2375706
Penalties (\$, 365d post)	101103.71	250576.98	0	3770177
S&S violations (365d pre)	24.47	48.82	0	372
S&S violations (90d post)	10.16	17.19	0	212

*Notes:* N = 1,069 fatality events at 867 unique mines, 2000–2024. FEMA disasters measure the number of distinct federal disaster declarations in the week of each mine fatality. Enforcement outcomes are counts (inspections, violations, S&S citations) and dollar amounts (proposed penalties) at the mine in the specified window around the fatality date. S&S = Significant and Substantial violations.

## 4. Empirical Strategy

### 4.1 The Eisensee-Strömberg Design

I adapt the competing-news instrumental variable design of [Eisensee and Strömberg \(2007\)](#). The core idea is that an exogenous shock to news competition—specifically, the occurrence of

natural disasters that dominate the media cycle—reduces coverage of mine fatalities that happen to coincide with those competing events. If enforcement responds to media attention, then fatalities occurring during high-competition weeks should trigger a weaker regulatory response.

The reduced-form estimating equation is:

$$Y_i = \alpha + \beta \cdot Z_i + \gamma' X_i + \delta_{q(i)} + \theta_{s(i)} + \varepsilon_i \quad (1)$$

where  $Y_i$  is the log of enforcement outcomes (inspections, violations, or penalties) at the mine of fatality  $i$  in the 90 days following the event;  $Z_i$  is the standardized log of FEMA disaster declarations in the calendar week of the fatality (the instrument for news competition);  $X_i$  is a vector of controls including the number of workers killed and mine characteristics (coal/metal indicator, log average employment);  $\delta_{q(i)}$  are year-quarter fixed effects absorbing seasonal patterns in both enforcement and news cycles; and  $\theta_{s(i)}$  are state fixed effects absorbing permanent differences in mining regulation across jurisdictions. Standard errors are heteroskedasticity-robust.

The coefficient  $\beta$  captures the reduced-form effect of competing news on enforcement. Under the media-attention mechanism,  $\beta < 0$ : more competing disasters crowd out media coverage of the mine fatality, reducing political pressure and thereby enforcement intensity. The null hypothesis is  $\beta = 0$ : enforcement does not respond to news competition.

## 4.2 Identification Assumptions

The exclusion restriction requires that FEMA disaster declarations in the week of a mine fatality affect enforcement at that mine only through their effect on media attention—not through direct channels. Two potential concerns arise.

First, natural disasters could directly affect mine operations (e.g., a hurricane could close a mine, making inspection impossible). This concern is mitigated by the geographic separation between disaster locations and mine locations, and by the fact that FEMA disaster declarations are national events while mining is concentrated in specific regions (Appalachia, the West, the Mountain states). I address this directly with state fixed effects, which absorb any correlation between disaster frequency and mining activity within a state.

Second, disaster declarations could correlate with macroeconomic conditions that independently affect enforcement (e.g., recessions could trigger both fewer disasters and budget cuts at MSHA). Year-quarter fixed effects absorb common time shocks, and the mine-level variation in fatality timing within a quarter provides the identifying variation.

### 4.3 Balance Test

Table 2 presents balancing tests. I regress each mine characteristic on the instrument conditional on year-quarter fixed effects. The instrument is uncorrelated with the number of workers killed ( $p = 0.55$ ), coal/metal status ( $p = 0.12$ ), pre-fatality inspection rates ( $p = 0.13$ ), and pre-fatality violation rates ( $p = 0.17$ ). None of the six tests reject at the 10 percent level, supporting the exogeneity of the instrument conditional on time fixed effects.

**Table 2:** Balancing Test: News Competition and Mine Characteristics

Dependent Variable	Coefficient	SE	N
Fatalities per event	-0.0466	(0.0783)	1,069
Coal mine (=1)	0.0297	(0.0192)	1,069
Inspections (365d pre)	1.7307	(1.1383)	1,069
Violations (365d pre)	7.6126	(5.4876)	1,069

*Notes:* Each row reports the coefficient on the standardized log weekly FEMA disaster declarations from a separate regression of the dependent variable on the instrument, with year-quarter fixed effects. Heteroskedasticity-robust standard errors in parentheses. The instrument should be uncorrelated with mine characteristics conditional on time fixed effects. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 5. Results

### 5.1 Main Results

Table 3 reports the main estimates. Across all three enforcement outcomes—inspections, violations, and penalties—and across all four specifications from minimal to fully saturated, the coefficient on disaster pressure is small, statistically insignificant, and unstable in sign.

For inspections (Panel A), the preferred specification with all controls and fixed effects (column 4) yields  $\hat{\beta} = 0.015$  (SE = 0.029,  $p = 0.60$ ). This represents a 1.5 percent increase in inspections per standard deviation of disaster pressure—economically trivial against a base of 6.1 inspections in the 90-day post-fatality window. The 95 percent confidence interval of  $[-0.042, 0.073]$  rules out effects larger than 7.3 percent.

For violations (Panel B), the estimate is  $\hat{\beta} = -0.018$  (SE = 0.050,  $p = 0.72$ ). The sign is negative, consistent with a media-attention story, but the magnitude is negligible and

statistically indistinguishable from zero. For proposed penalties (Panel C), the estimate is similarly null ( $\hat{\beta} = -0.028$ ,  $SE = 0.118$ ,  $p = 0.82$ ).

**Table 3:** The Spotlight Effect: Disaster Pressure and Post-Fatality Enforcement

	(1)	(2)	(3)	(4)
<i>Panel A: Log inspections (90 days post-fatality)</i>				
Disaster pressure (std.)	0.0436 (0.0307)	0.0443 (0.0307)	0.0248 (0.0288)	0.0153 (0.0293)
Observations	1,069	1,069	1,069	1,069
R <sup>2</sup>	0.133	0.134	0.259	0.310
<i>Panel B: Log violations (90 days post-fatality)</i>				
Disaster pressure (std.)	0.0063 (0.0514)	0.0091 (0.0513)	-0.0178 (0.0491)	-0.0180 (0.0504)
Observations	1,069	1,069	1,069	1,069
R <sup>2</sup>	0.136	0.137	0.221	0.279
<i>Panel C: Log penalties (90 days post-fatality)</i>				
Disaster pressure (std.)	0.0129 (0.1157)	0.0177 (0.1156)	-0.0178 (0.1153)	-0.0275 (0.1183)
Observations	1,069	1,069	1,069	1,069
R <sup>2</sup>	0.149	0.150	0.176	0.240
Year-quarter FE	Yes	Yes	Yes	Yes
Fatality severity		Yes	Yes	Yes
Mine characteristics			Yes	Yes
State FE				Yes

*Notes:* Each cell reports the OLS coefficient on the standardized log of weekly FEMA disaster declarations in the week of the mine fatality. The dependent variable is the log of enforcement outcomes in the 90 days following the fatality. Fatality severity is the number of workers killed. Mine characteristics include coal/metal indicator and log average employment. Heteroskedasticity-robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Minimum detectable effect.** With 1,069 observations and standard errors around 0.029–0.050, the minimum detectable effect at 80 percent power and 5 percent significance is approximately  $2.8 \times 0.029 = 0.081$  log points for inspections, equivalent to roughly 8.4 percent.

Converting to standardized units (dividing by the standard deviation of the outcome), I can rule out effects larger than 0.057 standard deviations—well within the range that [Eisensee and Strömberg \(2007\)](#) document for disaster relief.

## 5.2 Heterogeneity: Coal vs. Metal/Nonmetal Mines

Coal mines and metal/nonmetal mines differ substantially in regulatory regime, public salience, and political economy. Coal mining carries a stronger cultural narrative of danger that may make fatalities more newsworthy regardless of competing events. I split the sample by mine type.

For coal mines ( $N = 470$ ), the coefficient on inspections is 0.024 ( $SE = 0.059$ ). For metal/nonmetal mines ( $N = 599$ ), it is  $-0.038$  ( $SE = 0.029$ ). The difference is suggestive but not statistically significant, and neither subsample individually rejects zero. The violation results show a similar but more pronounced asymmetry: coal  $+0.091$  ( $SE = 0.088$ ) versus metal/nonmetal  $-0.121$  ( $SE = 0.064$ ,  $p = 0.06$ ). The marginally significant negative coefficient for metal/nonmetal mines is consistent with a weak media-attention channel operating specifically for lower-profile mining fatalities—but the finding does not survive multiple-testing correction and should be interpreted cautiously.

## 5.3 Dynamic Effects

If competing news temporarily delays enforcement that eventually catches up, we would expect stronger effects in short windows that dissipate over longer windows. I estimate the reduced form at 90-day, 180-day, and 365-day horizons. The coefficients are 0.015, 0.032, and 0.042, respectively—all insignificant and, if anything, growing over time rather than decaying. This pattern is inconsistent with temporary suppression and provides no evidence of delayed enforcement responses.

## 5.4 Robustness

**Table 4:** Robustness and Placebo Tests

Specification	Coefficient	SE	N
Placebo: pre-365d inspections	0.0451	(0.0362)	1,069
Placebo: pre-365d violations	0.0831	(0.0564)	1,069
Levels: inspections (90d)	0.1624	(0.2590)	1,069
Levels: violations (90d)	0.6463	(1.7330)	1,069
Single fatality: inspections	0.0147	(0.0299)	1,038

*Notes:* Each row reports the coefficient on the standardized disaster pressure instrument from a separate regression. All specifications include year-quarter and state fixed effects, fatality severity, and mine characteristics. Placebo regressions use pre-fatality enforcement (365 days before) as the dependent variable. The single-fatality sample excludes events with multiple deaths. Heteroskedasticity-robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Placebo test.** I regress pre-fatality enforcement (365 days before the event) on the disaster pressure instrument. If the instrument is valid, it should have no predictive power for enforcement that occurred before the fatality. The placebo coefficients are insignificant for inspections ( $p = 0.21$ ) and violations ( $p = 0.14$ ). Pre-fatality penalties show a marginal association ( $p = 0.07$ ), which I note as a caveat—though the main outcome (inspections) has a clean placebo.

**Alternative specifications.** The results are robust to using level outcomes instead of logs (ruling out transformation artifacts), Poisson count models (appropriate for enforcement counts), and restricting to single-fatality events (excluding the 31 multi-death disasters that may trigger exceptional responses regardless of media attention).

**Randomization inference.** I permute the instrument within year-quarters 500 times, preserving the temporal fixed-effect structure. The randomization inference  $p$ -value is 0.64, confirming the parametric null.

**Leave-one-out.** I sequentially drop each of the 8 major disasters (3+ deaths) from the sample. The coefficient ranges from 0.014 to 0.017 against a baseline of 0.015—the result is

not driven by any single event.

## 6. Discussion

The central finding—that mine safety enforcement does not respond to news competition—contrasts sharply with the disaster relief context where [Eisensee and Strömberg \(2007\)](#) find large effects. This contrast is informative about the institutional conditions under which media accountability operates.

Three features of mine safety regulation may explain the null. First, mandatory inspection mandates create a regulatory floor. MSHA must inspect underground mines four times per year and surface mines twice, regardless of political attention. This statutory requirement removes the discretionary margin through which media pressure could operate. Second, fatal accident investigations are triggered automatically by notification requirements (15-minute reporting mandates), not by political judgment. Third, MSHA’s career inspectorate—federal employees with specialized mining expertise—may be more insulated from political pressure than FEMA’s disaster relief apparatus, which depends on presidential declarations.

The comparison suggests a design principle for regulatory institutions: mandatory enforcement mandates can substitute for media accountability. When enforcement is discretionary—as with disaster relief, environmental enforcement, or financial regulation—media attention may be a necessary complement to ensure adequate government response ([Besley and Prat, 2006](#); [Johnson, 2020](#)). But when statute requires enforcement regardless of political salience, the media-attention channel becomes redundant, and the regulatory response is insulated from the noise of the news cycle.

This insulation has welfare implications that cut both ways. On one hand, media-insensitive enforcement is more equitable: a miner’s death receives the same regulatory response whether or not it happens to coincide with a hurricane. On the other hand, media-insensitive enforcement may also be less responsive to genuine public concern—a criticism leveled at MSHA after the Upper Big Branch disaster, when critics argued the agency had been slow to act despite accumulating violations ([Morantz, 2013](#)).

The null result should be interpreted with appropriate caveats. First, the instrument measures disaster pressure at the weekly level, which may be too coarse to capture within-day news competition effects. Second, I observe the reduced-form effect of news competition on enforcement, not the structural effect of media coverage itself; if the first stage is weak (competing disasters do not substantially crowd out mine fatality coverage), the reduced form may understate the true media effect. Third, the analysis focuses on the mine where the fatality occurred, where enforcement is largely mandatory; effects on discretionary enforcement

at peer mines remain an open question.

## 7. Conclusion

Not all government agencies are hostage to the news cycle. This paper shows that MSHA mine safety enforcement—bound by statutory inspection mandates and automatic investigation protocols—does not respond to the ebb and flow of competing media attention. The contrast with disaster relief, where [Eisensee and Strömberg \(2007\)](#) find that competing news substantially reduces government response, reveals that institutional design can insulate regulatory agencies from the distortions of media-driven accountability. The practical lesson is straightforward: when policymakers want enforcement to be consistent regardless of what else is in the headlines, they should mandate it by statute rather than leave it to discretion.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

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## A. Data Appendix

**MSHA data.** All MSHA data are obtained from the Open Government Data portal.<sup>1</sup> The Accidents file (271,719 records) contains pipe-delimited records for all reportable mine accidents with fields for mine ID, accident date, degree of injury, and accident characteristics. Fatalities are identified by `DEGREE_INJURY_CD = 01`. The Inspections file (1,138,758 records) contains inspection begin/end dates, activity codes, and mine IDs. The Violations file (3,057,779 records) contains violation dates, proposed penalties, and Significant and Substantial designations. The Mines file (91,603 records) contains mine characteristics including type (coal/metal), employment, location, and status.

**FEMA data.** Disaster declarations are obtained from the FEMA OpenFEMA API v2.<sup>2</sup> I paginate through all 50,023 records from 2000 onward, retaining disaster number, declaration date, declaration type (DR = major disaster, EM = emergency, FM = fire management), incident type, and state.

**Sample construction.** I restrict to fatalities between 2000 and 2024 ( $N = 1,158$ ) and collapse to unique mine-date events ( $N = 1,069$ , as some mines experience multiple fatalities on the same date). For each fatality event, I count MSHA inspections and violations at the same mine in symmetric windows of 90, 180, and 365 days before and after the event. I merge FEMA weekly disaster counts and FRED weekly average VIX values by fatality date.

## B. Identification Appendix

The balancing test (Table 2) confirms that the instrument is uncorrelated with mine characteristics conditional on year-quarter fixed effects. The placebo test using pre-fatality enforcement shows null effects for inspections and violations, with a marginal association for penalties ( $p = 0.07$ ) that suggests some caution in interpreting the penalty results.

**Randomization inference.** I implement a permutation test by randomly reassigning the instrument within year-quarters (preserving the FE structure) 500 times. The observed coefficient of 0.015 falls at the 36th percentile of the permutation distribution (two-sided  $p = 0.64$ ), confirming the null.

**Leave-one-out.** Dropping each of the 8 multi-fatality disasters (3+ killed) produces coefficients ranging from 0.014 to 0.017, indicating that no single catastrophic event drives the

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<sup>1</sup><https://arlweb.msha.gov/OpenGovernmentData/OGIMSHA.asp>

<sup>2</sup><https://www.fema.gov/api/open/v2/DisasterDeclarationsSummaries>

result.

## C. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD( $X$ )	SD( $Y$ )	SDE	SE(SDE)	Classification
Inspections (90d post)	0.0153	0.0293	—	0.838	0.0183	0.0350	Small positive
Violations (90d post)	-0.0180	0.0504	—	1.421	-0.0126	0.0355	Small negative
Penalties (90d post)	-0.0275	0.1183	—	3.301	-0.0083	0.0358	Small negative

*Notes:* This table reports standardized effect sizes (SDE) to facilitate cross-study comparison of treatment effect magnitudes. The treatment variable (news competition) is the standardized log of weekly FEMA disaster declarations, so  $SD(X) = 1$  by construction (marked “—”).  $SDE = \hat{\beta}/SD(Y)$ , measuring the effect of a one-standard-deviation increase in news competition on the outcome in standard deviation units.

**Research question:** Does news competition reduce post-fatality mine safety enforcement? **Treatment:** Continuous (standardized log weekly FEMA disaster declarations). **Data:** MSHA accidents, inspections, violations; FEMA disaster declarations; 1,069 fatality events, 2000–2024. **Method:** OLS reduced form with year-quarter and state fixed effects, heteroskedasticity-robust standard errors. **Sample:** All mine fatalities with matching FEMA weekly disaster data. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. “Null” denotes a near-zero effect size ( $|SDE| < 0.005$ ), not a failure to reject a null hypothesis.