THE VALUE OF NAMES: CIVIL SOCIETY, INFORMATION, AND GOVERNING MULTINATIONALS

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Abstract
Can markets discipline socially costly misbehavior abroad? We explore the market penalty associated with major human rights violations—specifically, the assassination of mining activists, a context where formal legal recourse is rare and events often do not involve counterparties. We show that firms featured in media coverage of these incidents experience significant, negative abnormal stock returns. Whereas reactions to related forms of corporate misconduct may be transitory or muted, we find that the market responses are both substantial and persistent, with a median 10-day loss exceeding USD 100 million. Since formal legal sanction is exceedingly rare, we consider the role of market penalties. We highlight three mechanisms that are consistent with reputational costs: (1) Media attention magnifies the market response. (2) Information-sensitive institutional investors systematically divest following assassination events. (3) Events reduce future trade, leading to a 32% decline in new contracts with counterparties. Despite these costs, events persist. We find that assassinations increase with dependence on mining royalties, suggesting that local rents sustain conflict despite market pressure. Thus, reputational sanctions may be significant—even in weakly institutionalized settings—yet may not fully deter misbehavior when local and global incentives diverge.

1. Introduction

Global corporations are increasingly operating in territories with weak governance, engaging in conflict-prone activities such as natural resource

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extraction. The mining sector has become a flashpoint between civil society and corporations involved in large-scale resource extraction (Ruggie 2013), especially amid the climate transition. Since 2020, attacks against human rights activists—many protesting mining activities—have occurred at a rate of one per day (Butt et al. 2019; Hearon et al. 2020; Business and Human Rights Resource Centre 2021). The green transition is likely to intensify the social tensions around resource extraction. Renewables and green technology are set to create unprecedented demand for critical minerals (e.g., lithium, cobalt, nickel) (IEA 2021) in the developing world (Owen et al. 2023). These trends have heightened international scrutiny of human rights practices in the mining sector.[1] With few formal laws, managing the social costs of extractive activity has become a critical issue in the global political economy (Ruggie 2013).

Can markets discipline human rights misconduct abroad? Market penalties, called "reputational" penalties, can be powerful disincentives for socially costly behavior (Becker 1968; Karpoff and Lott 1993; Alexander 1999).[2] Extensive research on firm misconduct shows that market-based reputational penalties can be substantial, yet not in all settings. Notoriously, market penalties are weakest for controversies that do not impact direct customers or suppliers (i.e., counterparties), as seen famously in the case of environmental violations (Jones and Rubin 2001; Karpoff et al. 2005; Cline et al. 2018). Perversely, some controversies may even enhance firm value (Armour et al. 2021), such as revelations of foreign bribery (Karpoff et al. 2017). Yet, evidence on reputational deterrence is particularly sparse for the developing world (Karpoff 2012). Recent work suggests that the reputational costs of firm conduct may be weaker for multinationals in foreign markets (Nardella et al. 2023).

We study how markets sanction corporations exposed to foreign human rights scandals. We do so by exploring how stock prices respond to significant human rights events, namely the assassination of environmental activists. We draw on a rich literature in law and economics on firm misconduct, and use these asset price responses to assess the market penalty. The stock market loss from events reflects both the formal costs and the market-based reputational costs borne by firms (Karpoff and Lott 1993; Krüger 2015). Because the formal costs for human rights abuses are exceedingly rare, stock prices provide a gauge of the

1. For example, high-profile United Nations initiatives (UN Secretary-General's Panel on Critical Energy Transition Minerals 2024), European Union initiatives to govern the mineral supply chain (e.g., the 2024 Critical Raw Materials Act), and increased monitoring by NGOs (e.g., the Business and Human Rights Resource Centre's (2024) Transition Mineral Tracker).

2. Market penalties, or reputational penalties, are defined as the decrease in a firm's market value as the market expects counterparties (investors, customers, and suppliers) to change the terms of business when revelations of firm conduct change the firm's reputation (Karpoff and Lott 1993; Sampath et al. 2018; Armour et al. 2021).

market penalty.[3] Nevertheless, the market penalty from foreign human rights revelations is not obvious, especially when they don't involve counterparties.

To study the market penalty from human rights scandals, we focus on well-publicized acts of violence against civil society. Specifically, we examine the assassination of activists mobilized around mining activity. These events reflect the social spillovers of large-scale extractive activity in developing economies, where legal institutions are weak and recourse is limited. As prominent public events, assassinations are among the best-documented human rights violations. Importantly, they provide an upper bound for detecting the reputational penalty from human rights scandals. We collect and code 20 years' worth of new granular event data on assassinations tied to global mining activity. Using the mining operations named in human rights reporting, we assign "treatment" to publicly traded mining companies. We utilize this rich data to assess the market-based penalties imposed on firms embroiled in human rights controversies and examine the various channels driving these penalties.

We employ financial event study methodology to estimate the penalty from these human rights events. Specifically, we use two related research designs to identify the impact of human rights events on firm value. First, we employ a traditional event study to estimate the abnormal returns for treated firms—that is, firms whose operations are implicated in targeted killings. We compare their realized returns to predicted returns over several event windows.

Second, we estimate the impact of assassinations by comparing the abnormal returns of treated firms to those of control firms. These control firms are firms operating in the same country and sector during the event but not associated with the assassination. We estimate these effects first using standard ordinary least squares (OLS) regression. Then, we use a synthetic matching estimator, in the spirit of Abadie et al. (2010) and Acemoglu et al. (2016), to account for unobserved group differences not fully captured by fixed effects and firm-level controls. In addition, we provide an open-source implementation of our synthetic matching estimator, the **R** package `synthReturn`.[4] Thus, we estimate the impact of controversies using different counterfactual assumptions and analyses.

We find that assassinations have a large, robust negative effect on the value of corporations caught in human rights controversy abroad. For these firms, we estimate significant negative cumulative abnormal returns following an event. Stock prices decline one day after an assassination and intensify over the subsequent 10-day window. We show that these penalties are not driven by contemporaneous events and, importantly, not anticipated by market reactions on the days preceding the event. Moreover, these penalties are firm-specific and

3. No firm in our study has faced legal penalty. See Section 5.4.1.

4. See: github.com/davidkreitmeir/synthReturn

are not polluted by broader spillovers (e.g., broader sectoral penalties), and the effects are not driven by local disruptions to operations.

Our estimates suggest that the market sanctions. The benchmark (short-run) estimates suggest that the median 10-day cumulative loss in market capitalization for companies named in assassination news exceeds USD 100 million. Thus, these effects may be severe enough to incentivize firm behavior. We demonstrate that these effects are comparable to those observed in other studies of market penalties resulting from misconduct.

Moreover, if market penalties are temporary, they may be insufficient to incentivize better behavior. We show that the effects are persistent by extending our short-run analysis and employing a long-run, buy-and-hold analysis. This contrasts with market overreactions to uninformative negative news events (e.g., Tetlock 2014) and evidence of transitory penalties from controversies (e.g., major environmental disasters (Carpentier and Suret 2015)) and noisy ESG news (Cui and Docherty 2020)).

We highlight three mechanisms that drive the penalty. First, we demonstrate that media attention amplifies the market penalty, potentially increasing the salience of events to financial decision-makers and relevant stakeholders. We employ exogenous variation in "news pressure" (Eisensee and Strömberg 2007) to demonstrate how media attention impacts the size of the penalty. We find that the penalty associated with human rights news disappears during more active news cycles but persists during less eventful news periods. Furthermore, we find that placebo firms operating near the event locations—and thus not named in media coverage—do not experience significant penalties compared to those explicitly named in coverage. Therefore, we contribute to research on misconduct that emphasizes the interaction between media coverage and reputational loss (Carberry et al. 2018; Sampath et al. 2018; Mariuzzo et al. 2020) by directly testing the role of media attention.

Second, we then provide evidence that information-sensitive institutional investors respond significantly to assassination events. We show that these investors, such as hedge funds, systematically divest from mining companies following assassination events. These results align with research on the role of institutional investors in promoting social responsibility (Dyck et al. 2019), particularly in emerging markets with weak institutional frameworks (Dyck et al. 2008). These effects are also compatible with work by Gantchev et al. (2021), who show evidence that ESG-sensitive players impose market penalties. Nevertheless, sophisticated investors may also expect firms to lose business, which we turn to next.

Third, we use rich supply chain data to show that downstream buyers respond negatively to these events. We find that being associated with an assassination leads to a 32% reduction in new contracts and a 39% decrease in new corporate customers from countries that emphasize human rights protection (i.e., countries in North America and Europe). Thus, the market penalty may partly reflect expectations of lost sales, even though assassinations do not represent misconduct toward counterparties. Importantly, this mechanism suggests that third-party controversies can still have demand-side repercussions, even for commodity producers. Thus, we highlight a novel mechanism driving reputational risk. Additionally, we do not find evidence that our effects are driven by (a) formal or legal sanctions, or (b) local contemporaneous costs to operations.

Despite high market-based reputational penalties, why do abuses persist? We conclude by exploring the political economy of this equilibrium. We collect new international data on mining royalties and show a significant relationship between assassinations and the importance of mining royalties paid to domestic governments. Although correlational, this relationship suggests that local rents may perpetuate conflict. Thus, even though market penalties may be high, they may fail to constrain local agents from engaging in socially deleterious behavior or "rational wrongdoing" (Shapira and Zingales 2017). For instance, market sanctions may not deter misconduct perpetrated in part by those outside the firm (e.g., paramilitaries).

This study makes the following contributions: First, we contribute to the literature on firm misconduct in an under-explored developing-country setting. We apply insights from law and economics, as well as empirical finance, to estimate the market penalty resulting from foreign scandals. These results are surprising because: (i) market penalties are typically weak or nonexistent for scandals impacting third parties (i.e., those not doing business with the firm) (Jones and Rubin 2001; Karpoff et al. 2005; Brady et al. 2019); and (ii) the stock market loss from third-party misconduct typically reflects legal fines. We study a setting where legal penalties are virtually nonexistent. Such settings are among the least studied: "[w]e do not know if reputational losses help to discipline related-party misconduct in other markets around the world" (Karpoff 2012, p. 376). Our results suggest they do. Nevertheless, the bulk of evidence comes from the developed world, specifically the U.S. Recent work demonstrates that reputational mechanisms can vary within the developed world, even between the U.S. and U.K. (Armour et al. 2017, 2021). By considering the role of reputation in a global setting, we join emerging empirical work from Nardella et al. (2023), Sampath et al. (2018), and others.

Second, our findings contribute to the growing literature on environmental, social, and governance (ESG) events and compliance. Globalization has led to the increasing role of (i) non-state actors—such as shareholders, media, and NGOs—and (ii) extra-legal mechanisms in monitoring firm conduct (Ruggie 2013; Carberry et al. 2018). Our results suggest that informal actors and actions may play a potentially important role in deterring misconduct. While work on economic crime shows that reputational penalties may be limited (e.g., for forms of foreign corruption and bribery (Karpoff et al. 2017; Sampath et al. 2018)), they can be substantial for other foreign scandals.

Importantly, our findings suggest that "naming and shaming"—and similar advocacy from civil society—may confer substantial penalties, and do so for less visible firms. Work on corporate responsibility literature emphasizes reputational risk for prominent brands exposed to consumer activism (Couttenier and Hatte 2016; Aouadi and Marsat 2018; Borelli-Kjaer et al. 2021). While existing studies show strong market reactions to misconduct by high-profile, consumer-facing firms (Harrison and Scorse 2010; Dyck et al. 2010; Krüger 2015; Capelle-Blancard and Petit 2019), less is known about reputational risks for less visible firms. Work by Klymak (2020) shows that naming campaigns may have a greater impact on downstream activity than on upstream activity. Nonetheless, we show that reputational risk may be substantial for upstream firms.

Third, our findings contribute to the existing literature on the role of media and information in shaping and disciplining conduct. First, we add to research demonstrating the role of media coverage in firm accountability (Fang and Peress 2009; Bushee et al. 2010; Griffin et al. 2011; Birz and Lott Jr 2011), showing that media and news coverage can impact the severity of the market penalty. Our findings are supported by studies indicating that the impact of scandals on stock performance is partly driven by the salience of media coverage (Palomino et al. 2009; Borelli-Kjaer et al. 2021). Second, our findings suggest that key—institutional investors and buyers—are responsive to news of mining misconduct and may contribute to the market penalty. Thus, media may impact reputation by disseminating information to key market participants. Our work highlights the interaction between institutional investors (hedge funds) and the media in penalizing firms in weak institutional environments (Dyck et al. 2008).

The remainder of the paper is organized as follows. Section 2 introduces the context of the study. Section 3 details our data and coding process. Section 4 outlines the empirical methodology and presents the main findings. In Section 5, we investigate the mechanisms underlying the baseline results. Section 6 explores reasons for the continued prevalence of assassinations. We conclude with a brief discussion of our results in Section 7.

2. Context: Assassinations and Civil Society

To study the market penalties from global human rights violations, we examine how assassinations in the natural resource sector—specifically mining—affect firms. We describe the context and also the empirical advantages of this setting.

2.1. Definition and Setting

Assassinations are defined as the intentional murder of prominent and influential figures (The Associated Press 2000). Throughout this study, we use the terms "targeted killing," "extrajudicial killing," and "assassination" interchangeably. We follow journalistic conventions (e.g., The Associated Press, British Broadcasting Corporation (BBC), National Public Radio) and the conflict studies literature, which treats assassinations as a unique form of political violence (Kalyvas 2019; Perliger 2015). Unlike spontaneous or opportunistic violence, assassinations are instrumental: they target "public figures for political reasons" (Kalyvas 2019, p. 23). By eliminating influential individuals, assassinations aim to advance a specific agenda or achieve a particular goal.

Our study focuses on the assassinations of civil society members who are mobilized around natural resource activities. The individuals considered were prominent in their communities and actively involved in various social groups, including indigenous leaders, environmental and labor activists, clergy, educators, and other community members. Scholars have emphasized the growing importance of targeted killings of high-profile civil society members (Bob and Nepstad 2007; Global Initiative Against Transnational Organized Crime 2020) in reducing monitoring (Carey and Gohdes 2021; Krain et al. 2023) and increasing the cost of collective action (Albarracín and Wolff 2024).

Over the last 20 years, civil society activists have increasingly become victims of lethal violence by state and non-state actors, reflecting rising constraints on civil society (Davenport 2014; Chaudhry and Heiss 2022). Since the 2000s, UN special rapporteurs have highlighted that targeted killings have become an important mode of repression against civil society and have markedly increased (Forst 2018; Lawlor 2021; Krain et al. 2023). These extreme events are emblematic of more diffuse violence and repression (Butt et al. 2019; Albarracín and Wolff 2024) as well as broader formal crackdowns on civil society (Davenport 2014; Bakke et al. 2020; Chaudhry and Heiss 2022). These trends have inspired new data collection efforts (Frontline Defenders 2023) and a nascent empirical literature on targeted violence against civil society (Butt et al. 2019; Le Billon and Lujala 2020; Carey and Gohdes 2021; Krain et al. 2023). We build on these empirical efforts by considering the role of private activity.

To study the impact of targeted violence, we focus on the mining sector. We do so for three reasons. First, lethal repression against civil society is predominantly observed in the natural resource sector. For human rights scholars and legal practitioners, this sector exemplifies how weak institutions hinder the prevention of human rights violations by multinationals (Ruggie 2013). Second, the mining sector is one of the deadliest for activists (Business and Human Rights Resource Centre 2021; Butt et al. 2019). Hearon et al. (2020) shows that nearly half of recent (and rare) human rights cases against multinationals involve extractive industries. Third, extractive industries are capital-intensive, and equity financing is common, enabling connections between publicly traded firms and human rights events.

2.2. Assassinations: Empirical Advantage and Scope

Activist assassinations are not only of substantive importance. They also offer empirical advantages in identifying the market penalty for severe human rights controversies:

Severity. Assassinations are unequivocal human rights violations and universally illegal. As clear instances of severe misconduct, they provide a means of assessing whether markets penalize such violations. They allow market participants to update their perceptions of a firm's reputation and social standards (Carpentier and Suret 2015). Studies often provide conflicting estimates of market penalties from negative ESG events. Such evidence, however, may simply reflect heterogeneous choices of events. These issues may be particularly important when studies use mass ESG datasets, which can include numerous insignificant events and uninformative events. Given the mixed estimates of reputational effects (Karpoff 2012), we follow (Carpentier and Suret 2015) and use a homogeneous class of unambiguous events—assassinations—to assess the scope of market penalty.[5]

Point-in-Time. Second, assassinations are discrete, well-defined events with concrete dates. This is especially true compared to other forms of misconduct or social conflict. Many negative events, such as regulatory episodes, often have complex and ambiguous timelines, which confound estimates of market losses (Armour et al. 2021).[6]. Other human rights-related events, such as complex labor violations, can suffer from the same issues of precision.

In contrast, assassination dates are well recorded and provide a consistent benchmark for information revelation. As noteworthy events, assassinations correspond closely with the dates when information is revealed to the public. Thus, unlike other events, there is a considerable overlap between the occurrence and initial reporting of assassinations.

Salience. Relatedly, assassination events are salient and public and among "the most visible and best documented" forms of targeted violence against social leaders (Albarracín and Wolff 2024, p. 8). They are closely monitored by international bodies and human rights press initiatives (Lawlor 2021; Global Initiative Against Transnational Organized Crime 2020; Frontline Defenders 2023), such as the Global Assassination Monitor. By focusing on these newsworthy events, we ensure they effectively reveal information to markets. This approach allows us to avoid empirical issues related to event selection

5. Given reputational penalties from environmental violations have been noisy and small, Carpentier and Suret (2015) uses major environmental accidents to test whether reputation could be significant enough to deter environmental misconduct.

6. There may also be a considerable gap between the event date (e.g., the violation of a regulation) and the public revelation of the event (e.g., by the press or government bodies). In studies of reputation shocks and fraud, abnormal activity may occur ahead of a press release (Gillet et al. 2010).

in financial event studies (Krüger 2015; Carpentier and Suret 2022), such as internal regulatory events that may not be publicized to markets.

Granularity. Our events permit high-resolution geographic and temporal analysis. Previous high-quality studies on targeted political killings have focused on national political events (Olken 2009), while recent research on the repression of civil society has often examined cross-country data (Bakke et al. 2020; Krain et al. 2023) and annual outcomes. In contrast, the availability of local information (Admin1 level) and daily event data allows for more granular analysis and greater statistical power. This temporal granularity also limits the occurrence of confounding events.

We now turn to the specifics of the data on assassination events.

3. Data

3.1. Assassination Data

3.1.1. Event Collection. Our data encompass 354 assassinations (496 victims) over a 20-year period. Table 1 provides an overview of these data. Our first observation is for 1998, and the sample covers 31 countries. Peru and the Philippines emerged as the most dangerous countries for mining activists. The geographic distribution of events is depicted in Online Appendix Figure C.2.

The data collection process for the events in Table 1 can be broadly summarized in four steps. First, we consider killings that are *publicly* reported by the media or human rights campaigns. Second, we examine events for which reporting connects a victim (or victims) to local mining and mineral extraction activity. Third, we code the location (e.g., the ADMIN1 unit) where the death occurred. Fourth, we code the mining companies or projects (if any) named in connection with the event. We detail this process below; further technical details are provided in Online Appendix B.1.

We collect data on activist assassinations through algorithmic and manual searches of international full-text media archives. These included databases such as the International Herald Tribune, the Associated Press wire archive, popular news APIs (e.g., The Guardian), and, importantly, global news databases (e.g., LexisNexis). Core databases, such as LexisNexis, offer translations of international news coverage. Notwithstanding, we also perform multilingual searches, for example, in Spanish.

3.1.2. Event Coding. Figure 1 depicts our coding process using an excerpt from the Guardian newspaper for an assassination event in our sample. Research assistants perform all coding manually, which principal investigators then cross-validate for certainty. The color highlights in Figure 1 denote the information we extract and record in our dataset (Watts and Collyns 2014). The article identifies the victim as Ecuadorian Indigenous leader José Isidro

Tendetza Antún (highlighted in green) and establishes his role as an activist working in opposition to mining activity (highlighted in purple). The section highlighted in yellow indicates that this incident resulted in the death of the victim.

Figure 1 also demonstrates how we define event dates (highlighted in pink). In most cases, media reports clearly state the time of death. However, some cases are ambiguous. In this example, although the crime occurred on November 28, 2014 (highlighted in pink), the news may have broken only after the discovery of the body. In such cases, financial markets are likely to react days after the actual date of death. We address this ambiguity in our estimation discussion (Section 4). Our event data maps assassinations to 15 of the 26 members of the International Council on Mining and Metals (ICMM), an industry network dedicated to corporate social responsibility (CSR) in the mining industry. This means that over half of the firms in the network have been associated with at least one assassination. But what exactly do we mean by "associated"? We turn to this question next.

3.2. Firm Coding

3.2.1. Treated Firms. Table 1 presents a list of publicly traded firms associated with at least one assassination event in our sample. In this study, we define an *association* as a company or its project being named in reporting on an assassination event. We use the terms "treatment," "connection," and "association" interchangeably to indicate statistical treatment.

We code the association between a public firm's operations and an assassination event, using the event material described in Section 3.1.2, including media reports and human rights releases. We consider a firm "treated" or connected to an event if the reporting mentions the firm or its operations. Importantly, our coding does not imply any stance on the relationship between a firm and an event beyond mentioning their operations in human rights reporting. An "associated" company may have no role in organizing or participating in violence. As we demonstrate below, it seems unlikely that multinationals will take an active role in these events. Our coding indicates only a firm's proximity to the event, as reported by public media.

Figure 2 summarizes the global distribution of assassinations and the headquarters of firms associated with these events. The colored panels on the left correspond to event countries, with their height representing the total number of event-company pairs in each country. The gray panels on the right correspond to countries in which publicly traded firms in our sample are headquartered, with their height representing the total number of events connected to firms in that country.

The figure shows that most assassinations in our dataset are linked to firms headquartered in Canada, the United Kingdom, and the United States. Despite these countries being advanced liberal democracies, legal actions against multinationals for human rights abuses remain exceedingly rare (Schrempf-Stirling and Wettstein 2017). More broadly, the assassination events central to our study occur in key mineral-producing countries (Online Appendix Figure C.3) and involve firms based in nations with the highest concentration of global mining companies (Online Appendix Figure C.4).

3.2.2. Coding and Matching Publicly Traded Firms. Our event study considers the closest publicly traded entity associated with an event, which may be the direct or indirect owner of a project where violence occurs. We hand-match the publicly traded firm associated with each assassination using the following process.

1. *Identifying Public Companies.* We first determine whether a company named in human rights reporting is publicly traded. If so, we match it to the corresponding event. A special case arises when another public mining company holds shares in the named company at the time of the event, such that the named company is not the ultimate owner of the project. As a convention, we only consider the "downstream" publicly treated companies, except where the global corporate owner is specifically named in reporting.
2. *Tracing Parent Companies.* If a company named in human rights reporting is not publicly listed, we conduct a manual search for its parent company. We determine whether the named company is a subsidiary or joint venture of a publicly traded company at the time of the event. We verify this information using historical ownership links from Bureau van Dijk's Orbis and SNL Metals & Mining database, as well as publicly available corporate information. The latter includes official corporate websites, corporate reports, Securities and Exchange Commission filings (and those of non-U.S. companies), public business registers, and more. We cross-validate all information.
3. *Identifying Projects.* Event reporting may refer to the colloquial or popular name for a mining project rather than the ultimate company. Mining operations are often referred to by their geographic or administrative unit names. For example, news reports may refer to the Rapu-Rapu Polymetallic Project (Philippines) rather than the firm that owns it, Lafayette Mining. In such cases, we attribute ownership of the project to the publicly traded company using the process described above.

As an example of our process, consider the event shown in Figure 1. In this case, the death involved the "Mirador copper and gold mine" (blue), owned by Corriente Resources Inc. through its subsidiary, EcuaCorriente S.A. We identified the ultimate owner using a public records search. Public data reveal that Corriente Resources Inc. was acquired in 2010, and at the time of the event in 2014, it was owned by two publicly traded companies: China Railway Construction Corporation and Tongling Nonferrous Metals Group Holdings. These two companies are coded as being associated with the event.

In rare cases, reporting does not name companies or projects. In these cases, we do not associate the assassination with a company. Consider the example in Figure 1. If no identifying data (blue) are available, we do not match the assassination to a specific firm. For an illustrative example, refer to Online Appendix Figure B.1.

3.3. Finance and Geo-Location Data

3.3.1. Data Sources and Construction. We collect daily stock return data for mining companies identified in Section 3.2, as well as returns for other companies operating in the same country during the year of the event for each event. Daily return data for 1998–2019 and additional firm-level covariates are drawn from the Datastream global financial database. We remove market holidays from our closing price time series and denominate all financial variables in USD to account for currency fluctuations.

We source location and company ownership information for the mining projects in our data from the SNL Metals & Mining database, which we then match to assassination events (Section 3.1.2). The SNL database also allows us to identify a robust set of control companies for each event year by identifying other mining companies with operations in the geographic vicinity of the event mine. For recent studies using the SNL Metals & Mining database, refer to Berman et al. (2017) and Knutsen et al. (2017).

We use annual project ownership information to track treated and control companies over time. For example, consider an assassination in Colombia in 2013. In this case, the control company set comprises all publicly traded companies that own mining projects in Colombia that year but are not associated with the assassination; see Online Appendix Figure B.3 Panel A for an example.

Online Appendix Table C.1 reports summary statistics for key financial variables, disaggregated by event country and treatment status. We construct market returns using the Morgan Stanley Capital International (MSCI) stock indices and match each mining company's security to the corresponding MSCI index for the country in which it is listed. Our sample includes mining companies listed in 23 markets and headquartered in 25 countries, specifically focusing on mining corporations active in event countries at the time of assassinations. We provide further details on this sample data in Tables C.2 and C.3.

3.3.2. Financial Factors and Methodological Considerations. We use the following daily, market-level Fama-French factors: small minus big (market capitalization), high minus low (book-to-price ratio), and momentum for the United States and developed markets, obtained from Kenneth French's website. For emerging markets, we construct the daily momentum factor for all emerging markets where at least one treated company is listed (i.e., Brazil, China, Mexico, Philippines, and South Africa). We follow Kenneth French's methodology to form winner-minus-loser (WML) portfolios using the intersection of two portfolios formed on size (market capitalization) and three portfolios formed on prior returns (month $t-12$ to month $t-2$).

Following the literature, we exclude thinly traded company securities from our baseline analysis. Specifically, we require that companies are traded for at least 200 days out of the 250 trading days in our estimation window and 8 out of the 11 days of the event window. We follow standard practice in dealing with events falling on non-trading days by assigning the event date to the first trading day after the actual event date.

4. Estimation: The Impact of Assassination on Firm Value

We show the impact of assassinations in two steps. First, in Section 4.1, we employ a classic financial event study to establish core empirical patterns. Assassinations have (i) a significant negative impact on treated firms, and (ii) these events are not anticipated by pre-event activity. Second, in Section 4.2, we employ a contemporary regression-based event study to further analyze these results. The negative impact of assassinations is (a) firm-specific and does not produce (negative or positive) spillovers to non-treated firms and (b) not driven by contemporaneous events or disruptions. Third, Section 4.3 uses a long-run analysis to show that negative abnormal returns are persistent.

4.1. Traditional Event Study: Basic Patterns

4.1.1. Estimation. We employ the traditional event study methodology (Campbell et al. 1997; MacKinlay 1997) to estimate the impact of events on treated firms. We study how assassination events influence the returns for treated firms relative to their counterfactual returns absent the event. We use the relationship between the treated firm's returns and the market during the estimation window prior to the event (Figure 3) to estimate the counterfactual "normal" returns. Specifically, we estimate the following linear market model over a standard 250-day estimation window ending 30 days before the event [-280,-30]:

$$R_{ie\tau} = \alpha_{ie} + \beta_{ie} R^M_{ie\tau} + \varepsilon_{ie\tau}, \tag{1}$$

where $R_{ie\tau}$ is the observed daily return for firm i, for event e at time τ, where τ denotes days relative to the event day ($\tau = 0$); $R^M_{ie\tau}$ is the return for the MSCI index for the country in which firm i's stock is listed. For robustness, we use a Fama-French four-factor model and construct factors for each international equity market (see Section 3.3).

We investigate the impact of assassinations by studying how observed stock returns deviate from normal returns on and around the event date or over the *event window*. Specifically, for each firm i and each day τ of the event window, we calculate the abnormal returns (ARs)—–the difference between the observed and normal (non-event) returns—–as follows:

$$\widehat{AR}_{ie\tau} = R_{ie\tau} - \left(\hat{\alpha}_{ie} + \hat{\beta}_{ie} R^{M}_{ie\tau}\right)., \tag{2}$$

where $\hat{\alpha}_{ie}$ and $\hat{\beta}_{ie}$ are estimates from the market model (1). Specifically, we study the cumulative abnormal returns (CARs), or the cumulative sum of $\widehat{AR}_{ie\tau}$ over the event window:

$$\widehat{CAR}_{ie}\left(\tau_1, \tau_2\right) = \sum_{\tau=\tau_1}^{\tau_2} \widehat{AR}_{ie\tau}, \tag{3}$$

where τ_1 and τ_2 represent the start and end points of each cumulative sum over the event window from $\tau = 0$ to $\tau = 10$: *i.e.*, $[0,0], [0,1], ...[0,10]$. Likewise, we consider the days leading up to the event to assess threats to the study design (contemporaneous leading events or market anticipation) and factors such as information leakage.

Our study considers the *average* impact of assassinations across treated firms. Thus, we compute the aggregate CARs across N company-event pairs:

$$\overline{CAR}\left(\tau_1, \tau_2\right) = \frac{1}{N} \sum_{j=1}^{N} \widehat{CAR}_{ie}\left(\tau_1, \tau_2\right). \tag{4}$$

We test whether the mean CAR for a given window is zero using a robust, non-parametric test statistic from Kolari and Pynnönen (2011).Kolari and Pynnönen (2011)'s generalized rank t-statistic, or GRANK relaxes parametric assumptions and introduces refinements suited to our setting. Specifically, GRANK performs well for longer event windows and when the exact pricing day–in contrast to the event (assassination) date–is uncertain (e.g., Luechinger and Moser 2014; Barrot and Sauvagnat 2016). The GRANK statistic is also robust to event-induced volatility, serial correlation, and event-day clustering.

We present our preferred test alongside three alternative parametric tests: *(i)* a simple (benchmark) t-test assuming that $\overline{CAR}\left(\tau_1, \tau_2\right)$ follows a normal distribution with mean zero and variance $\sigma^2\left[\overline{CAR}\left(\tau_1, \tau_2\right)\right]$; *(ii)* the BMP (Boehmer et al. 1991), which scales abnormal returns to prevent volatile securities from biasing estimates toward detecting average effects; and *(iii)* the modified BMP test of Kolari and Pynnönen (2010), which corrects for cross-correlation ("clustering") of abnormal returns.

4.1.2. Results: Main Patterns.

Negative Impact of Assassination Events. Figure 4 plots the traditional event study estimates for treated firms and the benchmark *market* model. The estimates show a steady negative abnormal reaction over the post-assassination period. We observe little market reaction on the day of the assassination, followed by a borderline significant effect of about -0.7 percentage points the next day. This initial reaction precedes a steady decline over the next four days and a steep (and robustly significant) decline from days 5 to 10. On average, the CAR is -2.0 percentage points 10 days following the event for the market model, and this result is significant at the 1% level when using our preferred non-parametric (GRANK) test statistic.

We report the alternative test statistics in Table C.4 and show that the results from the main market model are robust to using a Fama-French four-factor model (Figure C.5). Likewise, in (Section 4.2), we show similar effects using a regression-based exercise with control groups.

In other words, assassination events are significant and relevant to firm value—and negatively so. We can compare these effects to short-run estimates for instances of environmental crime (e.g., EPA and other regulatory violations) from Brady et al. (2019) (short-run estimates -1.6 to -1.7). In contrast to the environmental crime or environmental disasters studies in the reputation literature (Bouzzine and Lueg 2020), penalties are unlikely in our setting.

Would we expect a negative impact? Not necessarily, and at least not a strong one. Existing studies show reputational costs matter but vary widely depending on the type of event. Notably, the reputational penalties of events are small if those events are unlikely to change the value of future interactions with counterparties. The literature shows few reputational losses in cases where negative events do not impact the direct parties with whom the firm does business, as is the case, for example, with pollution and environmental disasters. The negative stock reactions to these thus tend to almost entirely reflect governmental and legal costs rather than market-based reputational losses; see reviews by (Karpoff 2012) and (Armour et al. 2021).

Importantly, controversial negative news has tended to be more impactful when it contains stronger legal and material (direct economic) language (Krüger 2015; Capelle-Blancard and Laguna 2010; Carberry et al. 2018). Whereas most studies focus on the United States and, to a lesser extent, the OECD countries, the negative impact of ESG events may, in part, be a reflection of the strength of institutions in those jurisdictions (Karpoff 2013). For instance, Lundgren and Olsson (2010) finds significant negative abnormal returns for European firms relative to the US. Yet, the imposition of legal penalties for human rights abuses abroad is notoriously rare (see discussion and analysis in Section 5.4.1).

Slow Diffusion and Market Under-Reaction. The short-run dynamics of Figure 4 are also notable. The market response in Figure 4 is not sharp and immediate; instead, the response to information is delayed. Among other things, this pattern suggests slower information diffusion through the market. For example, market participants may gather additional information on an event before pricing the expected costs for a treated company. Unlike more common ESG events, assassinations are relatively unique and complex. Political events of this nature may require further information and more time for investors to process their impact. Hence, we may observe a market underreaction to the initial event in Figure 4.

The dynamics of this market reaction differ from those identified in many of the studies of corporate controversies. Several empirical event studies document swift over-reactions (large short-run declines, followed by reversals) to corporate controversies (Cui and Docherty 2020; Chen and Yang 2020), which is consistent with work on negative media and over-reaction to corporate news (Tetlock 2007). We explore these dynamics in our regression analysis below (Section 4.2) and in our long-run analysis (Section 4.3).

No Pre-Event Abnormalities: Validating the pre-event window. We validate the estimation strategy above and show that stock returns did not move significantly over the days before the assassination dates. By doing so, we provide evidence that the market is indeed pricing new information, rather than merely detecting pre-existing trends or responding to other prior events. Furthermore, where (assassination) events are planned, the "authorization" date is unknown. A reasonable assumption is that if private information exists, it should be priced close to the actual event date when the likelihood of execution can be best assessed by insiders (Dube et al. 2011).

We study the pre-event period by aggregating abnormal returns *backward* starting on the day before the event and run a placebo exercise (see Dube et al. 2011; Luechinger and Moser 2014). We find no significant market responses before assassination events. Online Appendix Figure C.6 shows that the average abnormal return on the day before the event is positive, while the CAR over the 10 days before an event is close to zero or slightly negative and insignificant across test statistics.

In other words, Online Appendix Figure C.6 indicates two things. First, the market does not price prior knowledge of assassinations. For studies of malfeasance and ESG controversies, it is not uncommon for ambiguity to surround events and their timing (Krüger 2015), particularly in complex cases of firm misconduct (e.g., SEC enforcement activity (Karpoff et al. 2008)). Second, and importantly, our core event study results are not *merely* picking up a downward pre-trend in the asset prices for those companies associated with violence. Furthermore, if other non-assassination events drive these results, they do not occur prior to the event day. We test for pre-trends more thoroughly in Section (4.2) and also reject the possibility that other disruptions are confounding our estimates (Section 4.2.3).

Robustness. The negative pattern described above is robust to alternative (stricter) estimation criteria. Online Appendix Figure C.7 shows that our results remain unchanged when we require companies to be traded each day within the 11-day event window and for 225 out of the 250 days in the estimation window. This criterion drops seven company-event pairs and reduces the magnitude of our CARs to -1.5 percentage points 10 days after an event; the estimates remain highly significant and robust. The difference in point estimates may be driven by trading halts for highly affected securities.

Moreover, the securities of small mining companies may be less frequently traded than larger companies; we expect these firms to be dependent on a single project and, therefore, more vulnerable to disruption following an event than larger companies. Thus, stricter requirements on trading frequency may underestimate the true effects of assassination events.

4.2. Regression-Based Event Study

4.2.1. Estimation. We build on the traditional event study analysis above (Section 4.1) by estimating the impact of assassination publicity using a comparison between treated and non-treated firms. Specifically, we compare the CARs for companies with projects named in human rights reporting against those of control companies: firms operating in the same sector, country, and period in which the assassination occurred. Formally, consider the following regression equation:

$$CAR_{ie}\left(\tau_1, \tau_2\right) = \alpha + \delta D_{ie} + \boldsymbol{X}'_{ie}\varphi + \gamma_e + \varepsilon_{ie}, \tag{5}$$

where $CAR_{ie}\left(\tau_1, \tau_2\right)$ is the CAR for company i and event e from τ_1 to τ_2. CARs are calculated identically for both treatment and control firms. The indicator D_{ie} denotes the treatment and equals one if a company is associated with an event, and zero otherwise. The coefficient of interest, δ, captures the average difference in CARs between treated and control firms. Our empirical strategy relies on the assumption that, absent a company being linked to an assassination event in reporting, we would not observe systematic differences in the returns of treated and control firms during the event window.

The strength of this identifying assumption depends, in part, on our choice of controls and the control group. Our preferred specification includes event-fixed effects, γ_e, which control for common market reactions around dramatic events. These effects capture shifts in market sentiment toward the event country or increased excess volatility following political events. We also present an alternative version of Equation (5), which replaces event-specific effects, γ_e, with company-fixed effects (γ_i). In this case, we compare the impact of an assassination event on a company when it was merely active in the country during an event period to the impact when a company is directly tied to an assassination. Note that a company cannot be part of the control and treatment groups for the same event; we thus do not have a convolution of the control and treatment groups. The baseline specification (5) also includes a set of firm-level controls, $\boldsymbol{X}_{ie}$: total assets and leverage (total debt to capital). Small or highly leveraged firms may be more dependent on specific mining projects and thus differentially impacted by disruptions in the wake of an event. Hence, baseline estimates are conditioned on these key variables. We address issues of “bad controls” by using the value of each firm characteristic in the year before the event.

Importantly, we compute CARs for each event and set of firm controls for the company-specific effect of market movements on the firm's stock price over the event window. By re-estimating the market model for each event, we account for changes in the relationship between market and firm returns over time.

Our set of control companies has multiple advantages and is guided by the political economy and finance literature. First, we select control firms with mining operations in the same country where the event occurred. Doing so accounts for common exposure to political risk events—especially those impacting mining—in a given locale at the time of the event; for instance, incidents where violence against activists changes the national sentiment against the mining industry. Second, following Guidolin and La Ferrara (2007), we compare treated companies to those with a similar "comparative advantage" operating in high political risk environments (p.1987). Lastly, we control for commodity price fluctuations that impact mining companies operating in similar commodity markets and the same domestic market.

We estimate Equation (5) using a standard linear fixed effects regression. Because we use event $\times$ firm-specific cross-sectional variation, Equation (5) is not a two-way fixed-effects (TWFE) estimator. We are, therefore, able to avoid issues posed by TWFE regressions (Goodman-Bacon 2021, among others). Standard errors are robust to heteroscedasticity and clustered at the event level.

Further, we employ an alternative synthetic control-type estimator, where treated firms are matched with respect to their return patterns prior to each event. Section 4.2.4 shows our OLS results are robust to this alternative method. We first turn to the baseline estimates from Equation (5).

4.2.2. Regression Event Study: Results.

Main Patterns. Our OLS estimates of Equation (5) reveal clear patterns in the impact of assassinations on returns. Figure 5 displays these results, plotting 42 *individual* regression coefficients across time. The vertical axis shows the (τ) days after (before) the event. The top panel displays our baseline specification, Equation (5), with event-fixed effects, whereas the bottom panel employs company-fixed effects. Both panels present 90 and 95% confidence intervals.

Before assassination events, exposed and non-exposed firms show no significant differences in CARs; in fact, treated firms have slightly higher returns. This pre-event stability mirrors the lack of pre-period movement we observe in our traditional event study (Section 4.1.2).

Assassinations trigger a consistent decline in abnormal returns for treated companies relative to the control group. There is no change on the event day, yet significant changes emerge two days later. CARs drop by 1.0 and 1.3 percentage points for event- and company-fixed effects specifications, respectively. This negative trend persists, and CARs continue to decline over the next five days.

By the tenth day following the assassination, the abnormal returns of exposed mining companies fall between 2.0 and 2.8 percentage points.

The findings from our regression and traditional event study demonstrate that financial markets process information on assassination events gradually. This contrasts with the response to firm scandals and other ESG events. It is not uncommon for studies to find markets overreact with sharp initial declines, followed by reversions (Karpoff 2012; Borelli-Kjaer et al. 2021) or overreactions to negative ESG news (Chen and Yang 2020). For example, the negative effects of corporate crises (Wei et al. 2017) or major chemical disasters (Capelle-Blancard and Laguna 2010) are insignificant by Day 10.

Impact of assassinations on treated v. control firms. Are the effects in Figure 5 driven by negative returns among treated firms, or by benefits accruing to competitors in the control group? *A priori*, the impact on competitors is unclear. While competitors could benefit from firms operating in the same country and industry being embroiled in controversy, assassination events could also have negative industry-wide effects and spillovers (Bouzzine and Lueg 2020; Bouzzine 2021), prompting industry-wide scrutiny or regulation, especially among similar firms (Ouyang et al. 2020).

We assess the impact on control firms by conducting a placebo version of the traditional event study, restricted to the sample of treated firms. Online Appendix Figure C.8 shows that the effect of assassination events on control companies is small and insignificant. In absolute terms, placebo estimates are smaller than our principal event study estimates; the average CAR does not exceed 0.4 percentage points in the 10 days following the event.

These findings indicate that the negative market reactions in Figure 5 are not driven by spillovers to control firms (whether positive or negative). Instead, our results reflect firm-specific negative returns rather than broader sectoral or inter-firm effects. Practically, it is worth noting that our regression-based estimates align with traditional event study estimates, which do not rely on control group counterfactuals. Moreover, Figure 5 row 2 shows that differences in cumulative returns are particularly pronounced in our specification using only within-firm variation.

Widening the window: testing for pre-event trends and reversals. Next, we expand the event window to 20 days before and after the event to (a) investigate the potential of pre-trends and (b) test for reversals after the 10-day window. Online Appendix Figure C.11 displays the assassination coefficient estimates for our baseline specification, Equation (5). We adjust the minimum trading data criteria: companies must be traded on at least 15 of the 21 days after the event and 15 of the 20 days before the event. After the event date, returns decrease monotonically until Day 13, with this decline appearing permanent. Before the event date, estimates are positive but insignificant.

These findings suggest that our results are (a) not attributable to pre-existing trends and (b) are relatively persistent. We perform a longer-run band-and-hold analysis in Section 4.3 and contrast these effects to reversals seen in the nascent reputation and deterrence literature.

Loss from assassination events. The estimated effects are also economically meaningful. Figure 6 plots the estimated loss in market capitalization on each day for the median treated company. The dots correspond to baseline estimates; the error bars correspond to the minimum and maximum loss across specifications (see Online Appendix Figure C.9).

According to Figure 6, the median treated company is estimated to lose between USD 100 and 150 million in market capitalization over the 10 days following the event. The effect is also economically sizable in relative terms: for instance, Krüger (2015, p.313) finds a median loss of USD 76 million to news of negative ESG events in a well-known CSR database.[7]

4.2.3. Robustness: Regression Results. We demonstrate that our OLS event study results are robust to several extensions. Specifically, they are not driven by individual countries or firms, nor are they sensitive to specific events or specifications. We also rule out that the effects are driven by general contemporaneous conflict.

Sensitivity to single firms or countries. Figure 7 presents a "leave-one-out" analysis of our OLS results, showing that estimates are not driven by any single country or firm. Our baseline regression results appear in bold, with permutations in light gray. In Panel A, we re-estimate our baseline specification, sequentially dropping individual event countries from our sample. The negative effects remain visually similar across sample permutations, showing a clear and gradual decrease in abnormal returns over the 10 days following the event. Abnormal returns in the days leading up to the event remain slightly positive, though close to zero. Importantly, the core pattern persists even when excluding the two deadliest countries for mining activists: Peru and the Philippines (dashed red line).

This exercise is repeated in Panel B of Figure 7 with firms sequentially dropped. Our baseline results remain qualitatively similar, with the homogeneity across the light gray bands indicating that our findings are not driven by particular "bad actors". This suggests the broad applicability of our results to publicly traded mining firms.

Event type. Our OLS results are also robust across different types of assassinations and similar events. Online Appendix Table C.5 shows that post-event assassination estimates remain qualitatively unchanged when we exclude unsuccessful assassinations (Columns 1 to 5) or activist killings during protests

7. We use Krüger (2015)'s median CAR(-10,10) estimate of -1.11% and the median market capitalization of sample firms to compute the median loss, that is, 0.0111×6.86B.

(Columns 6 to 10). While the point estimates follow a similar pattern to our baseline results, excluding these events (26 events corresponding to 47 event-company pairs in the latter case) reduces the explanatory power and significance of the results.

Functional form. Online Appendix Figure C.9 demonstrates the robustness of our baseline results when accounting for potential non-linear effects of covariates. Including cubic polynomials in size, leverage, and profitability (return on equity) does not significantly alter our findings (Column 2). The core pattern remains consistent across specifications with different fixed effects, including less conservative sets like year (γ_y) and headquarter-country (γ_h) effects (Rows 2 and 3).

Outliers. Our baseline results persist when we control for outliers. Online Appendix Table C.5 shows that excluding CARs beyond the 1st and 99th percentiles (Columns 11 to 15) does not substantially change our findings. The assassination coefficients remain similar in magnitude and, in most cases, become more precise, with the exception of the company-fixed effects specification. This suggests that, for some firms, association with an assassination event is an extreme observation and supports the notion that assassinations can impact individual firms.

Assassinations or Contemporaneous Local Conflict? Are our estimates driven by assassination events or by contemporaneous disruptions? We address this question by comparing the returns of treated firms to geolocated control firms: mining companies operating within the same subnational administrative unit but not named in relation to the event (i.e., not treated). This approach helps distinguish between the market pricing of an association with activist murders and reactions to concurrent disruptions or unobserved regional violence.

We create a new set of control companies by matching assassination events to mining projects in the same subnational (*Admin1*) region. Using the SNL Metals & Mining database, we map the geolocation of assassinations to nearby properties, as illustrated in Online Appendix Figure B.3. This approach capitalizes on the geographically specific nature of most mining opposition, which often involves land claims, Indigenous territory, and environmental concerns. If there is other contemporaneous conflict, it is likely to be geospatially clustered. Although we face data constraints, we successfully match at least one publicly traded control company operating in the same region as an “exposed” company for 92 firms in our sample.

Using these granular control firms—those most susceptible to localized conflict—yields results qualitatively similar to our main findings. Panel B of Online Appendix Figure C.10 presents estimates of Equation 5 using the fine geographic controls; Panel A presents our benchmark estimates. Despite the reduced sample size following geomatching, Panel B shows a gradual relative decrease in CARs for treated firms, with no discernible pre-trend. Ten days after an event, CARs are 1.3 (2.8) percentage points lower for our baseline specification with event effects (company fixed effects). Online Appendix Table C.6 shows broadly negative estimates across specifications with similar magnitude. In the unrestricted specification (Column 1), coefficient estimates are precise and significant at the 5% level.

These findings suggest that the negative impact we observe is not driven by spatial disruptions from conflict. Rather, our estimates likely capture market responses to firms' associations with violence in reporting by the media and NGOs.

4.2.4. Synthetic Matching Estimation. We build on our OLS estimator and estimate the impact of events using a modified version of the synthetic matching estimator in the spirit of (Abadie et al. 2010) and Acemoglu et al. (2016). This approach accounts for potential unobserved differences between treated and control firms not captured by our regression Equation (4.2.1). This allows us to compare the returns for each treated firm to a synthetic control group with similar pre-event dynamics. Specifically, a synthetic firm's returns are a weighted, convex combination of control-company returns, where weights are chosen to optimally match pre-event returns for a given treated company. We provide an open-source **R** software package `synthReturn` that we used to implement this procedure.

Our synthetic matching estimator extends the approach in Acemoglu et al. (2016) to accommodate multiple event dates and two inference procedures. We select a set of control firms for each event date and treatment company combination. The estimated average treatment effect, $\hat{\varphi}(\tau_1, \tau_2)$, averages the CARs for each treatment group company, weighted by the "goodness" of its synthetic match over the estimation window. The abnormal return equals the difference between a treated firm's actual return and the return of its synthetic match. To ensure a sufficient number of control firms at each event date, we define the pool of potential control companies to include all mining companies traded in treatment markets. We use a simple nonparametric bootstrap and a permutation inference method, drawing placebo treatment groups from the set of control firms, to to obtain uncertainty estimates for the cumulative effect estimates.[8] For more details on the synthetic matching method, see Online Appendix Section A.2.

Table 2 reports the synthetic event study estimates for the event day and the ten subsequent trading days. As in our OLS estimation, the impact of being associated with an assassination event is negative and increasing over the event window. The estimated cumulative effect even exceeds our benchmark OLS estimates, reaching about 6.1 percentage points ten days after the event.

8. The distribution of the placebo treatment effects from randomly drawing 5,000 placebo treatment groups from the control group is presented in Online Appendix Figure C.12.

4.3. Long-Run Effects: Persistence and Testing for Reversals

We confirm that the market penalties are long-run. If negative returns are temporary, market sanctions may not provide sufficient incentives to improve firm behavior (Ambec and Lanoie 2008; Gantchev et al. 2021). Although work on reputation often establishes short-run impacts—typically $[-1, 1]$ or $[-2, 2]$ day windows—these penalties may be transitory. Indeed, numerous empirical studies find that stock market penalties for negative events are often transitory (Ambec and Lanoie 2008; Capelle-Blancard and Laguna 2010), such as in the case of major environmental disasters (Carpentier and Suret 2015). More generally, in the short run, equities may simply overreact to news events with negative valence and quickly mean-revert. Such reversals are seen in low-information negative news events (Tetlock 2014) or extreme events (Kwon and Tang 2020). However, the benchmark penalties thus far are not consistent with temporary market penalties or overreactions to noisy news.

We further confirm that the effects are not transitory. We show the long-run impact of human rights controversies by first (a) conducting descriptive analyses and then (b) using buy-and-hold abnormal returns (BHAR). This approach serves as an alternative to long-horizon event studies, which face econometric challenges and are sensitive to baseline specifications (Mitchell and Stafford 2000; Kothari and Warner 2007).

Panel A of Figure 8 presents the average CARs for treated companies in the 90 days following the event. Following the significant negative initial market reaction over the first 13 days, the long-run average of the CAR for the following 77 trading days remains at 2.4 percentage points. Given the challenges of measuring abnormal returns and statistical inference, these results are only indicative.

We follow the literature and use buy-and-hold abnormal returns to study the long-run effect on treated firms. The BHAR approach involves comparing the total returns of two portfolios of firms held for a pre-determined period and comparing the total returns of event firms to a benchmark portfolio held over the same long-run period.[9]

In other words, relative to a benchmark, how well would an investor perform if they bought and held treated stocks? According to Panel B (Figure 8), quite well over the long run. Although the difference between summing returns (CAR) and compounding (BHAR) returns is negligible in the short run, differences compound in the longer term. The short-run BHAR on Day 13 deviates only marginally from the CARs, whereas the long-run average of the BHAR surpasses the CAR by 3.2 percentage points. This pattern is robust to

9. The BHAR is defined as the geometric sum:
$\text{BHAR}(\tau_1, \tau_2) = \prod_{\tau=\tau_1}^{\tau_2} (1 + R_{ie\tau}) - \prod_{\tau=\tau_1}^{\tau_2} (1 + E(R_{ie\tau}|X_\tau))$

the use of a Fama-French normal-return model instead of the classic market model.

In sum, the long-run analyses above suggest a persistent impact of assassination events on firm returns. The extent and duration of these effects stand in contrast to the transitory shocks in studies of reputation and firm conduct. We next turn to the forces potentially driving the extent of the market penalties.

5. Mechanisms

We consider three channels driving the reputational penalties above. First, we examine the role of *(1)* media in disseminating information to stakeholders and show that key trading-partners (investors and buyers) respond: specifically, *(2)* sophisticated institutional investors, and *(3)* counterparties in the global supply chain. All three factors are compatible with reputational penalties. Accordingly, we do not find evidence that the decline in stock market value is driven by *(4)* other non-market forces, such as legal fines and local disruptions to operations.

5.1. Mechanisms: The Media

Media influences the informational environment of international financial markets, and the extent to which stakeholders act on revelations. We establish the role of media attention in disseminating information to other market participants, including stakeholders. Empirical behavioral finance demonstrates that media attention significantly impacts stock returns and the cost of capital (Tetlock 2014, 2015). Additionally, research on misconduct penalties indicates that media coverage amplifies the market penalty (Carberry et al. 2018; Mariuzzo et al. 2020). The reputational consequences of events for firms—distinct from formal legal costs—are likely mediated by the media environment. We build on earlier research into misconduct and directly test this channel using an empirical strategy from media economics (Eisensee and Strömberg 2007).

We show the impact of media coverage on market responses by considering exogenous variation in media attention. A principal challenge in isolating the role of reporting is that media attention may be endogenous to the event. For instance, simple proxies of media attention (e.g., counts of articles surrounding an assassination) may be driven by stock movements or other factors. Hence, we consider exogenous variation in media attention to our events, using the daily news pressure index of Eisensee and Strömberg (2007).

Our exogenous shifter of attention, *daily news pressure*, is defined as the median number of minutes the main US news broadcasters devoted to the top three news segments in a day Eisensee and Strömberg (2007) (see Section II.C and Online Appendix V.B). We expect the degree of attention to be lower if the assassination coincides with a "high news pressure" day—that is, a day on which an event is overshadowed by larger global events exogenous to the assassination.

We study the impact of media by expanding our baseline regression model:

$$CAR_{ie}\left(\tau_1, \tau_2\right) = \alpha + \alpha^N N_e + \delta D_{ie} + \delta^N \left(D_{ie} \times N_e\right) + \boldsymbol{X}'_{ie}\varphi + \gamma_e + \varepsilon_{ie}, \quad (6)$$

where we add an interaction term between the treatment indicator D_{ie} and a dummy variable, N_e that is equal to one if the event falls on a day where news pressure is high, and zero otherwise. We allow for different intercepts of high and low news pressure days to account for general differences in trading behavior on the days where news pressure is high. The coefficient δ^N conveys the difference in average CARs for events that fall on high versus low news pressure days.

Baseline Results. Figure 9 shows the differential impact of media and reports the estimates from Equation (6). In Panel A, high news pressure days are defined as those where the news pressure is above the median value of the news pressure index for the period 1998 to 2018. Estimates in Panel B use an alternative 75th percentile cutoff. Black lines (dots) correspond to the impact of assassination events that fall on days when news pressure is high ($\delta + \delta^N$), whereas red lines (dots) correspond to the impact of events that fall on low-pressure days (δ). Blue lines (squares) depict the absolute difference between the two ($\left|\delta^N\right|$) or the differential impact of assassinations depending on whether they fall on days with high or low news pressure. Each panel reports 95% confidence intervals.

The estimates in Panels A and B show a significant, continuous decline in CARs for treated companies when assassinations fall on low-pressure days. However, estimates are indistinguishable from zero when the event coincides with a high-news-pressure day. Moreover, we observe a gradual divergence in CARs between the two types of events. By Day 10, the difference in CARs between above and below median days is 4.1 percentage points and significant at the 5% level (Panel A). A qualitatively similar pattern appears if the event falls on a day where the news pressure is above the 75th percentile in Panel B. Quantitatively, the divergence is less precisely estimated and slightly attenuated in this more demanding specification.

Alternative News Pressure. A potential concern is that perpetrators may strategically time their attacks to coincide with high-pressure days, thereby minimizing public scrutiny. We address this concern by employing the empirical strategy of Jetter and Walker (2022) and consider an alternative measure: "disaster predicted news pressure." This measure isolates the exogenous component of news pressure. Specifically, we regress the daily news pressure index on *(i)* the day-to-day count of unpredictable disasters (earthquakes, epidemics, and volcanic eruptions) in countries hosting at least 50,000 U.S. emigrants (plus Iraq and Afghanistan), *(ii)* linear and squared time trends, and *(iii)* a set of day-of-the-week, month-, and year-fixed effects.[10] The disaster coefficient estimate is 0.247 (significant at the 1% level using Newey–West standard errors, with a lag of one day). We then use these parameter estimates to predict the news pressure on any day t for the period 1998 to 2018 and create an indicator of high levels of news pressure using only statistically unpredictable variations in news pressure.

We rerun the analyses in Panels A and B using this alternative pressure variable, as shown in Panel C. These estimates are quantitatively and qualitatively similar when the event occurs on a day on which the *predicted* news pressure is above the 75th percentile, substantiating the baseline findings in Panel A.

Robustness. In Online Appendix Section C.3, we show that the findings above are robust to additional sensitivity checks and that more transparency surrounding the extractive industry magnifies the human rights penalty. Additionally, we demonstrate that there is no statistically significant correlation between the daily level of news pressure and assassination events.

5.2. Mechanisms: Institutional Investors

Having shown the negative impact of assassination events and the role of the media, we now consider the role of institutional investors. Institutional investors have the power to move markets in response to revelations. These investors are large, sensitive to new information, and capable of processing complex social events (Puckett and Yan 2011; Hendershott et al. 2015, e.g.,). Importantly, large investors are increasingly attentive to and act on reputational events (Brady et al. 2019). Sizeable market players, such as sovereign wealth funds, may have explicit ESG objectives. Likewise, a growing literature documents that sophisticated investors react to revelations of misconduct. “Irresponsible” corporate behavior is an important trigger for shareholder activism for 72% of institutional investors (Mccahery et al. 2016). Importantly, large investors may actively trade on revelations and transmit ESG preferences through stock prices, as recently shown by Gantchev et al. (2021).

Empirical Framework. We examine the role of institutional investors and analyze how they respond to human rights controversies using a stacked difference-in-differences estimation framework developed by Wing et al. (2024).

10. We require that, to be included in the analysis, disaster events in the EM-DAT database (CRED / UCLouvain, 2024, www.emdat.be, (Delforge, Damien and Wathelet, Valentin and Below, Regina and Sofia, Cinzia and Tonnelier, Margo, and van Loenhout, Joris and Speybroeck, Nico 2024)) need to have information on their start and end dates and must meet one of the following three conditions: *(i)* 10 or more deaths; *(ii)* 100 or more people affected/injured/homeless; *(iii)* declaration by the country of a state of emergency and/or an appeal for international assistance. See Jetter and Walker (2022) for details.

This framework accounts for staggered treatment and multiple events per company, as well as non-continuous treatment in our setting.[11]

To employ this estimator, we use a separate dataset balanced in event-time for each *sub-experiment*, that is, each event calendar period. Each of these sub-experiments $a \in \Omega_\kappa$ comprises N_a^D treated and N_a^C clean control units for the entire event window from $\kappa_{pre}, ..., \kappa_{post}$. Next, we stack all sub-experiments in event time such that at each event period, the stacked dataset comprises $N_{\Omega_\kappa}^D = \sum_{a \in \Omega_\kappa} N_a^D$ treatment-group units and $N_{\Omega_\kappa}^C = \sum_{a \in \Omega_\kappa} N_a^C$ control-group units. Therefore, each observation in the stacked data refers to a company $\times$ sub-experiment $\times$ event-time (i, a, τ) observation.

We then estimate the following weighted event-study regression:

$$Y_{ia\tau} = \alpha_0 + \alpha_1 D_{ia} + \sum_{j=\kappa_{pre},...,-2}^{\kappa_{post}} \left(\lambda_\tau \mathbf{1}\left[\tau = j\right] + \delta_\tau D_{ia} \times \mathbf{1}\left[\tau = j\right]\right) + e_{ia\tau}, \quad (7)$$

where the bias-correcting sample weights are defined as

$$Q_{ia} = \begin{cases} 1 & \text{if} \quad D_{ia} = 1 \\ \frac{N_a^D / N_{\Omega_\kappa}^D}{N_a^C / N_{\Omega_\kappa}^C} & \text{if} \quad D_{ia} = 0, \end{cases} \quad (8)$$

and where D_{ia} equals 1 if company i is treated in sub-experiment a, and 0 otherwise. The dependent variable $Y_{ia\tau}$ denotes the total value of shares of company i held by institutional owners in quarter τ divided by the total market capitalization of company i in quarter τ. By expressing the holding position in *relative* rather than *absolute* terms, the outcome is unaffected by price changes, and we can isolate the effect on the institutional owners' trading behavior.

The parameters of interest are the event-study coefficients $\delta\tau$, with $\tau = -1$ serving as the reference period. Following Wing et al. (2024), we allow all observations from the same unit to be dependent, even if they appear in multiple sub-experiments and cluster standard errors at the unit level.

Data. Data on institutional ownership come from the FactSet Ownership database, which reports institutional investors' equity holdings on a quarterly basis from 2000 to 2017. FactSet is widely used in the empirical finance literature (e.g. Aggarwal et al. 2011; Dyck et al. 2019) due to its global coverage of institutional investors, with data collected from fund reports, regulatory authorities, fund associations, and fund management companies. We rely on "Institutional Ownership Summary Statistics by Firm" as developed by Ferreira and Matos (2008) and provided by WRDS. Data on annual firm characteristics are from the FactSet Fundamentals database.

11. Wing et al. (2024) demonstrates that existing stacked DID estimators (e.g. Cengiz et al. 2019; Deshpande and Li 2019; Butters et al. 2022) are unable to recover causal effects because of implicitly assigning different weights for treatment and control groups across sub-experiments.

Our final stacked panel comprises 67 public mining companies associated with assassination events and 144 event-company pairs. The set of possible control companies includes firms active in the "extractive" sector; we define corporations as operating in the extractive sector if their TRBC Business Sector classification is "Energy - Fossil Fuels," "Uranium," or "Mineral Resources."

Results. Our baseline results are presented in Figure 10. Overall, the total share of institutional ownership decreases in the event quarter and the following year. However, these total effects are imprecise. Importantly, institutional investors differ in their objectives and investment strategies (e.g., pensions versus hedge funds) and how responsive they are to ESG events.

Thus, Figure 10 depicts separate estimates for Equation (7) by type of institutional investor. We find that, except for banks and insurance companies, institutional investors appear to reduce their holdings after assassination events. This is the case for hedge funds, in particular; these investors reduce their holdings by about 5.2% relative to their average position. This swift liquidation relative to other institutional investors aligns with findings that hedge funds have shorter investment horizons (Cella et al. 2013), are more inclined to monitor corporate behavior and respond rapidly to costly information disclosures (Gargano et al. 2017), particularly following revelatory news (Huang et al. 2020). In contrast, institutional investors with a long-term, strategic view of their portfolios, such as pension funds, do not seem to systematically change their holdings in the aftermath of an assassination event.[12]

This heterogeneity may be attributed to several factors. Despite their ESG commitments, even investors who actively engage companies on human rights issues may be reluctant to set clear divestment timelines, even when investee companies have contributed to severe human rights abuses over extended periods (UN Working Group on Business and Human Rights 2021). The cases where pension or sovereign wealth funds have divested from companies accused of human rights violations are the exception; divestment is often the last resort among strategies to engage with investee companies on human rights issues.[13]

Robustness: Social Events vs. ESG Scores. In Online Appendix Section C.5, we show that assassination events have no significant impact on the overall ESG performance scores of mining corporations, nor on their human rights or community scores. These findings help explain why institutional investors, who primarily rely on external ESG indicators for portfolio decisions rather than sourcing information from NGOs or media outlets, show no reaction—or react slowly. Our results align with survey responses from institutional investors in

12. Results are quantitatively unchanged if we include time-varying firm characteristics as additional controls (Online Appendix Figure C.15).

13. For example, in December 2019, the largest Danish pension fund, ATP, divested from Grupo Mexico after eight months of failed attempts to engage with the mining company over the environmental and human rights risks associated with a new dam project.

Business and Human Rights Clinic (2018), which highlight that ESG indicators often lack sufficient coverage of large companies operating in emerging markets.

5.3. Mechanisms: Supply Chain

Does the market penalty above reflect expectations of future lost business in the supply chain? We explore this effect by considering whether the market penalty could stem from changes in supply chain activity.

Why might investors have negative expectations about sales after a human rights controversy? In classic reputation theories, the size of the reputational loss reflects the value of lost activity from counterparties, such as buyers. Yet, ex ante, we may not anticipate a reputational penalty in our setting. First, assassinations target community members, not downstream buyers, as civil society tends not to purchase unrefined cobalt concentrate. Conceptually and empirically, news of misconduct against consumers tends to produce significant reputational losses (Karpoff and Lott 1993; Alexander 1999; Murphy et al. 2009). Second, it may be relatively harder to boycott undifferentiated commodities. However, downstream buyers may still change their behavior following revelations. We contribute to the literature on reputation by examining how third-party controversies can impact reputation when they alter the behavior of second parties (those trading with the firm).

We use rich supply chain data to investigate whether downstream buyers adjust their behavior following these events.

Supply Chain Data and Empirical Framework. Specifically, we employ data at the corporate-customer-supplier level from FactSet Revere, matched with our event dataset and firm fundamentals from Worldscope. Revere provides a unique database of global supply chain relationships that extends back to 2003 and has become the research gold standard (Dai et al. 2021; Darendeli et al. 2022, among others). This data covers both major and minor private and publicly listed customers. The database includes approximately 23,400 companies worldwide, including 38 mining companies linked to at least one of our assassination events and 143 untreated control mining companies.

For each of the 181 mining firms in our sample, we construct two main outcome variables at the calendar-year level to capture transactions with their corporate customers: *(i)* the number of contracts signed in year τ, and *(ii)* the number of unique customers initiating those contracts. We analyze data at the calendar-year level because the average contract duration for mining firms in our sample is 308 days. This duration makes it unlikely that corporate customers would respond to a CSR shock by canceling existing contracts, which would expire within the next year (Darendeli et al. 2022). Instead, we expect customers to reduce new or follow-up contracts. Additionally, we disaggregate contracts by customer location to test whether customers from countries with strong human rights records are more likely to sever ties with mining companies linked to severe human rights violations.

We estimate the effect of assassinations on supply chain contracting by employing the stacked-DID framework introduced in Section 5.2. Our baseline specification includes the number of contracts expiring in a given year as an additional control. This accounts for the independent effect of re-contracting on our outcome variables (Darendeli et al. 2022). We consider an event window that extends from three years before to one year following the event.

Results. Figure 11 presents our baseline estimates. We find that, overall, assassinations have a negative effect on both the number of new contracts (first row) and the number of new customers (second row), although these estimates are insignificant. Column 2 shows that these negative estimates become stronger and more significant when we focus on customers from countries with strong human rights protections, defined as an above-median V-Dem Civil Liberties index score (Coppedge et al. 2022). Mining companies in the human rights spotlight experience a 19% decrease in the number of contracts signed and the number of unique customers relative to the mean.

Likewise, the results in Column 3 underscore the importance of civil society. For customers from countries with a strong civil society, we find a quantitatively and qualitatively comparable effect on supply chain contracting (Column 3).[14] Our benchmark findings are robust to the use of alternative country-level indicators of human rights protection (see Online Appendix Figure C.16).

In contrast, Online Appendix Figure C.17 shows that being linked to an assassination event attracts customers from countries with weak institutions. That is, customers based in authoritarian regimes or countries with weak civil societies are unlikely to be deterred by reports of human rights violations. Instead, they may strengthen their ties with the implicated supplier if the loss of customers from countries with strong human rights protection results in more favorable contract conditions.

5.4. Mechanisms: Alternative Channels

Thus far, we have highlighted market-based penalties for human rights controversies. In this section, we offer additional analysis and explore whether our results are driven by non-market costs born by firms following assassinations. First, we consider the extent to which the decline in stock prices reflects expectations about formal penalties. Next, we consider whether the events directly impact firm operations by seeding unrest. In other words, we verify the extent to which our main results reflect market-based losses born by firms.

14. Note that V-Dem codes all indexes such that a higher score represents a more positive outcome. Hence, an above-median score for "Civil Society Repression" indicates a below-median level of civil society repression.

5.4.1. Direct Legal and Financial Costs. We demonstrate that our results are unlikely to be driven by investor expectations regarding legal penalties for human rights violations abroad. In environments where legal capacity is high, stock price movements reflect the expected formal and legal penalties that follow revelations of a firm's conduct. In key cases, the price response to negative events entirely reflects the formal legal penalty for misconduct, implying a small reputational penalty (Brady et al. 2019). Studies on responses to social controversies are almost entirely based on controversies in the United States and, to a lesser extent, Western Europe (Karpoff 2012). Nevertheless, weakly governed areas may lack the legal and enforcement capacity for firms to internalize the social costs of misconduct.

Our setting is no different. When it comes to corporate human rights violations abroad, there are notoriously few legal tools available (Ruggie 2013). Across our entire sample, we were unable to find evidence that any of the events resulted in convictions or significant legal fines for the treated companies. A detailed qualitative discussion on the scarcity of court cases involving human rights conduct is provided in Online Appendix Section C.7.1.

Formally, we conduct an empirical analysis to determine if the effect of being associated with an activist's assassination varies with the likelihood of facing legal consequences. We combine our event-level data with the "Law and Order" index from The International Country Risk Guide (ICRG) by the PRS Group, which measures the quality of a country's judicial system. Specifically, we obtain information about the judicial system's quality for both the country in which the mining company is headquartered and the country where the assassination occurred.

If investors expect that association with an activist's assassinations will result in legal indictments and subsequent financial losses due to fines, then the effect on the company's CAR should increase with the quality of the judicial systems in both the headquarters and event countries. Our empirical analysis follows a similar approach to specification (6) in Section 5.1. We analyze whether the effect of assassination varies by the quality of the judicial system; we include an interaction term between the treatment indicator D_{ie} and a dummy variable equal to one if the judicial-system quality is high (defined as the respective country's Law and Order index at the 25th, 50th, or 75th percentile) and zero otherwise.

The results, which examine the quality of the judicial system in the event and headquarters country, are presented in Figure C.18 of the Online Appendix. We find no evidence that the magnitude of the assassination effect differs by the likelihood of facing legal challenges and fines in either the headquarters or event country, as proxied by the quality of the country's judicial system. Given the lack of evidence of legal indictments resulting in substantial penalties for the associated mining companies, we suspect that investors' reactions are not driven by expectations about future legal actions for this type of human rights violation.

5.4.2. Protest and Local Disruptions. Do assassinations and the resulting media attention provoke protests and cause costly disruptions to firm activity? A priori, the effect of assassinations on protests is ambiguous. On the one hand, eliminating a key opposition figure might deter future protests and hinder coordination. On the other hand, this violence could provoke a backlash from the local community and increase opposition to a mining project. Protests—whether nonviolent or violent—against a company's operations can lead to temporary disruptions in resource extraction, interruptions in logistics, damage to physical assets, or even a permanent shutdown of operations.

Data and Empirical Framework. We employ a stacked-DID estimation framework to examine how assassinations influence local protest movements in a two-month window surrounding the event. Since protests are expected to intensify in the weeks leading up to an assassination, we select a reference in the saturated event-study regression as the start of the event window. Our sample includes all 25 countries in which at least one assassination linked to a mining company occurred. The analysis is conducted at the ADMIN1-calendar-week level.

We source protest data from two datasets. First, the Mass Mobilization (MM) Data Project (Clark, David and Regan, Patrick 2016) provides reliable, granular information on global protests, including their type and cause, making it more dependable than other sources. Second, we use the Global Database of Events, Language, and Tone (GDELT), which categorizes over 300 activities using CAMEO event codes (Schrodt et al. 2009) and records roughly 60 attributes for each event based on print, broadcast, and web news media in over 100 languages.[15]

Results. Online Appendix Figure C.19 displays the stacked DID event-study estimates of the impact of assassinations on protest incidence probabilities during the four weeks before and after the event. For protests recorded in MM, we observe a modest, imprecisely estimated increase in incidence probability during the event week, with no significant effect in subsequent weeks (Panel A). The fact that several extrajudicial killings in our dataset occur during and around protests is consistent with these protests being tied to mining opposition and related violence.[16]

The GDELT data (Panel B) exhibit a noisier and less consistent pattern, which contrasts with anecdotal evidence. In Panels C and D of Online Appendix Figure C.19, we focus on MM protests using detailed start and end dates to analyze whether assassinations trigger or suppress opposition. The probability of protest onset peaks within the three weeks surrounding the event but

15. Details on constructing our protest measures are available in Online Appendix Section B.4.

16. In unreported results, we obtain similar estimates when protests are limited to those categorized as addressing "land tenure or farm issues."

drops sharply after the second week. In contrast, the probability of protest termination peaks during the event week and remains elevated thereafter.

Overall, our findings suggest that protest activity peaks in the week before and during the week in which the event occurs. Activity then declines. The absence of market reaction in the weeks leading up to assassinations, combined with the drop in protest activity after the event, reinforces the conclusion that expectations of escalating costs from intensified protests are unlikely to drive investor behavior.

6. Persistence: Assassinations and the Political Economy of Local Rents

Given the economic losses associated with these assassinations, why do they persist? Multinationals are sprawling and complex, with blurry organizational boundaries. Their upstream operations may be controlled by actors whose incentives diverge from those of their owners. Local stakeholders benefiting from mining projects may have incentives to suppress or eliminate opposition while avoiding the financial losses borne by shareholders. Domestic governments and state-aligned paramilitaries exemplify such agents, as uninterrupted production generates higher royalties and tax revenues. The relative gains from malfeasance should, therefore, increase with an increase in the mining company's relative importance as a revenue source. Put differently, the larger a company's tax payments relative to its competitors, the greater the incentive for local authorities to suppress opposition leaders who challenge its projects.

Data. We construct new data on local public financing of mining companies to examine the relationship between violence and government rents. We collect this data manually from reports published by the Extractive Industries Transparency Initiative (EITI), an international civil society organization. Nations participating in EITI commit to disclosing payments received from extractive companies, typically including payments from subsidiaries and joint ventures.

We aggregate tax payments at the company level by hand coding yearly ownership shares using historic ownership data from Bureau van Dijk's Orbis database. When such data are unavailable, we obtain supplementary data from annual reports. Tax revenues from subsidiaries and joint ventures are allocated to owners according to their ownership shares. For instance, in 2014, Anglo American owned 81.90% of the Peruvian mining company Anglo American Quellaveco S.A., while Mitsubishi owned the remaining 18.10%. In this case, USD $202,232$ of the USD $246,925$ in tax and royalty payments to the Peruvian government is attributed to Anglo American, with the remainder being attributed to Mitsubishi.

For each country-year pair (i.e., report), revenues are aggregated at the corporate-owner level and divided by the total tax revenues from the mining industry to calculate the annual tax share of firm i. Summary statistics are presented in Online Appendix Table C.11, disaggregated by event country.[17]

Empirical Framework. To analyse how government revenues influence the occurrence of assassinations. Specifically, we estimate the following equation:

$$\mathrm{K}_{ict} = \beta_1 \mathrm{T}_{ict} + \gamma_{ct} + \varepsilon_{ict}, \tag{9}$$

where K_{ict} is a dummy variable that takes the value of one if an assassination event in country c in year y is associated with company i and T_{icT} corresponds to the tax share of company i in country c in year t.

In our preferred specification, we include country $\times$ year fixed effects (γ_{ct}) to account for country-specific economic and political developments. By relying exclusively on within-country $\times$ year (i.e., EITI report) variation, we minimize concerns about potential sample bias. Standard errors are clustered at the company $\times$ country level.

Results. Table 3 reports the results. In Column 1, we find a significant and positive unconditional correlation coefficient of 13.8 percentage points. Including country-fixed effects (Column 2) and year-fixed effects (Column 3) increases the estimated effect to 17.4 percentage points. In our preferred specification, which includes country $\times$ year-fixed effects (Column 4), we estimate that a hypothetical mining company (as the sole taxpayer) is associated with an 18-percentage-point increase in the likelihood of an assassination event occurring in connection with one of its projects. For the average firm in our sample, this represents a 26% increase relative to the mean.[18] Estimates are qualitatively similar when we employ a *probit* instead of a linear probability model (Online Appendix Table C.12).

Reassuringly, we find no significant impact of an assassination event on the company's relative importance as a source of government revenue (Online Appendix Table C.13). These findings assuage concerns about reverse causality. Nevertheless, the estimates are indicative, not causal.

7. Conclusion

Can markets discipline human rights misconduct abroad? We study the significance of market or reputational penalties resulting from significant corporate events abroad. Extensive research on firm misconduct shows that

17. It is worth noting that this analysis is not limited to public companies; private companies are also matched to assassination events in our dataset. For completeness, we retain potentially relevant cross-country variation and do not exclude countries where an assassination falls outside the EITI coverage period.

18. The average probability of an assassination event in the sample is 4.16%, and the average tax share is 5.9%.

market-based reputational penalties can be substantial, yet not in all settings. Notoriously, market penalties are weakest for controversies that do not impact direct customers or suppliers (i.e., counterparties), as seen famously in the case of environmental violations (Jones and Rubin 2001; Karpoff et al. 2005; Cline et al. 2018). Yet, evidence on reputational deterrence is particularly sparse for the developing world (Karpoff 2012). Recent work suggests that the reputational costs of firm conduct may be weaker for multinationals in foreign markets (Nardella et al. 2023).

We study how markets sanction corporations exposed to foreign human rights scandals by examining how stock prices respond to the assassination of environmental activists. We employ financial event study methodology using 20 years' worth of new granular event data on assassinations tied to global mining activity. We find that assassinations have a large, robust negative effect on the value of corporations caught in human rights controversy abroad. For these firms, we estimate significant negative cumulative abnormal returns following an event, with the median 10-day cumulative loss in market capitalization exceeding USD 100 million. We show that the effects are persistent by extending our short-run analysis and employing a long-run, buy-and-hold analysis. This contrasts with market overreactions to uninformative negative news events and evidence of transitory penalties from controversies.

We highlight three mechanisms, all of which are compatible with reputational channels. First, we demonstrate that media attention amplifies the market penalty, potentially increasing the salience of events to financial decision-makers and relevant stakeholders. We find that the penalty associated with human rights news disappears during more active news cycles but persists during less eventful news periods. Second, we provide evidence that information-sensitive institutional investors respond significantly to assassination events. Third, we use rich supply chain data to show that downstream buyers respond negatively to these events. We find that being associated with an assassination leads to a 32% reduction in new contracts and a 39% decrease in new corporate customers from countries that emphasize human rights protection. Despite high market-based reputational penalties, we demonstrate that assassinations increase with dependence on mining royalties, suggesting that local rents may perpetuate conflict even when multinationals face severe reputational costs. All three mechanisms are compatible with prior theoretical and empirical work on market penalties.

This work contributes to the emerging work on the political economy of multinationals and the political economy of human rights. Human rights advocates identify the lack of quantitative evidence on the financial impacts of human rights violations as the primary barrier to integrating these concerns into investment decisions (Business and Human Rights Clinic 2018). Our study provides a better understanding of the material consequences of human rights violations and quantifies their effect on the valuation of associated companies.

Thus, our findings highlight the potential of human rights reporting, advocacy, and journalism, especially through their reputational (market) mechanisms. Our results indicate that informational campaigns by civil society can indeed influence multinational corporations, specifically through reputational (market) mechanisms. However, our results illustrate that, while significant, market mechanisms alone may be insufficient to fully constrain misconduct abroad.

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Table 1. Assassination Events

	Events		Victims	Unsuccessful Attempts	Company-event Pairs		Distinct Mining Firms	
Country	Total	No Ties	Total	Total	Total	Public	Total	Public
Bangladesh	1	0	3	0	1	1	1	1
Bolivia	1	0	1	0	1	1	1	1
Brazil	11	7	11	0	4	4	2	2
Chile	1	1	1	0	0	0	0	0
China	1	0	4	0	1	0	1	0
Colombia	40	21	46	1	28	18	17	8
DR Congo	2	2	6	0	0	0	0	0
Ecuador	4	0	4	1	6	5	4	3
El Salvador	6	0	7	0	6	6	1	1
Gambia	1	0	2	0	1	0	1	0
Ghana	1	0	1	0	1	1	1	1
Guatemala	28	3	48	6	28	19	10	6
Honduras	9	4	12	1	6	2	6	2
India	25	15	57	0	12	9	10	7
Indonesia	4	1	5	0	5	3	5	3
Liberia	1	1	1	0	0	0	0	0
Mexico	21	4	25	0	20	17	12	9
Mozambique	1	0	1	0	1	1	1	1
Myanmar	4	1	4	0	4	0	4	0
Panama	1	0	2	0	2	2	2	2
PNG	1	0	4	0	1	1	1	1
Peru	57	5	87	3	72	58	28	18
Philippines	116	57	145	1	85	57	43	27
Sierra Leone	1	0	1	0	1	1	1	1
South Africa	7	0	8	3	7	7	4	4
Sri Lanka	1	1	1	0	0	0	0	0
Tanzania	1	0	1	1	2	2	2	2
Thailand	3	2	3	0	1	0	1	0
Turkey	1	0	2	0	1	0	1	0
Ukraine	1	1	1	0	0	0	0	0
Venezuela	2	1	2	0	2	2	2	2
World	**354**	**127**	**496**	**17**	**3**	**1**	**584**	**344**

Notes: Events where a connection between the assassination (attempt) and the victim's opposition to mining was established, but no specific mining project or company was mentioned, are referred to as *no ties*. *Unsuccessful attempts* are assassination attempts that resulted in non-fatal injuries. Entries in the *distinct company entities* column report how many unique mining firms were associated with at least one assassination event in the country.

Table 2. Synthetic Matching Estimation

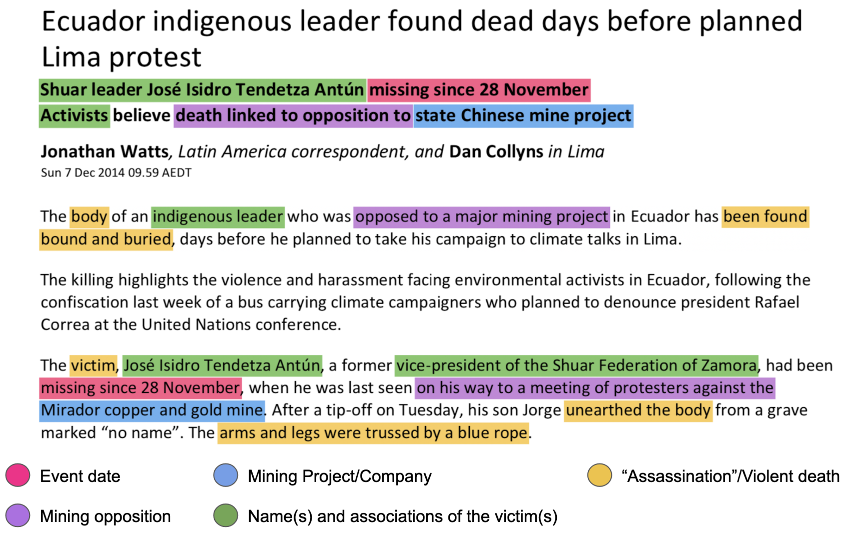
		Inference Procedure		
		Bootstrap		Permuatation
	CAR	SE	p-value	p-value
$\hat{\varphi}(0,0)$	-0.0209	0.0114	0.067	0.019
$\hat{\varphi}(0,1)$	-0.0441	0.0270	0.102	0.009
$\hat{\varphi}(0,2)$	-0.0471	0.0278	0.090	0.013
$\hat{\varphi}(0,3)$	-0.0487	0.0258	0.059	0.015
$\hat{\varphi}(0,4)$	-0.0552	0.0286	0.054	0.014
$\hat{\varphi}(0,5)$	-0.0655	0.0317	0.039	0.012
$\hat{\varphi}(0,6)$	-0.0705	0.0360	0.050	0.012
$\hat{\varphi}(0,7)$	-0.0739	0.0357	0.038	0.010
$\hat{\varphi}(0,8)$	-0.0727	0.0366	0.047	0.013
$\hat{\varphi}(0,9)$	-0.0624	0.0352	0.076	0.020
$\hat{\varphi}(0,10)$	-0.0613	0.0349	0.079	0.024

Notes: The estimated effect of assassination events on corporate stock returns using the synthetic matching method are reported. P-values for two inference procedures are presented: (1) nonparametric bootstraps (clustered at the unit level) of $5,000$ times; (2) drawing $5,000$ placebo treatment groups from the set of control firms (permutation inference). For more details, please see Section A.2 in the Online Appendix.

TABLE 3. Assassination Events

	Dep. Var.: Assassination			
	(1)	(2)	(3)	(4)
Tax Share	0.138*	0.174**	0.174**	0.181**
	(0.073)	(0.073)	(0.074)	(0.075)
Country FE		✓	✓	
Year FE			✓	
Country × Year FE				✓
Observations	1081	1081	1081	1081

Notes: *Assassination* equals 1 if a mining firm was associated with an assassination event in a given year, and 0 otherwise. The *Tax Share* is defined as the amount of taxes and royalties paid by a corporation to a country's government divided by the total government revenue the mining industry. Robust standard errors clustered at the company-country level are in parentheses: * p<0.1, ** p<0.05, *** p<0.01.



Ecuador indigenous leader found dead days before planned Lima protest

Shuar leader José Isidro Tendetza Antún missing since 28 November
Activists believe death linked to opposition to state Chinese mine project

Jonathan Watts, *Latin America correspondent, and* **Dan Collyns** *in Lima*
Sun 7 Dec 2014 09.59 AEDT

The body of an indigenous leader who was opposed to a major mining project in Ecuador has been found bound and buried, days before he planned to take his campaign to climate talks in Lima.

The killing highlights the violence and harassment facing environmental activists in Ecuador, following the confiscation last week of a bus carrying climate campaigners who planned to denounce president Rafael Correa at the United Nations conference.

The victim, José Isidro Tendetza Antún, a former vice-president of the Shuar Federation of Zamora, had been missing since 28 November, when he was last seen on his way to a meeting of protesters against the Mirador copper and gold mine. After a tip-off on Tuesday, his son Jorge unearthed the body from a grave marked "no name". The arms and legs were trussed by a blue rope.

FIGURE 1. Extracting events and company associations from NGO and media reports. The source of the article is: https://www.theguardian.com/world/2014/dec/06/ecuador-indigenous-leader-found-dead-lima-climate-talks (Last accessed: *24 May 2025*).

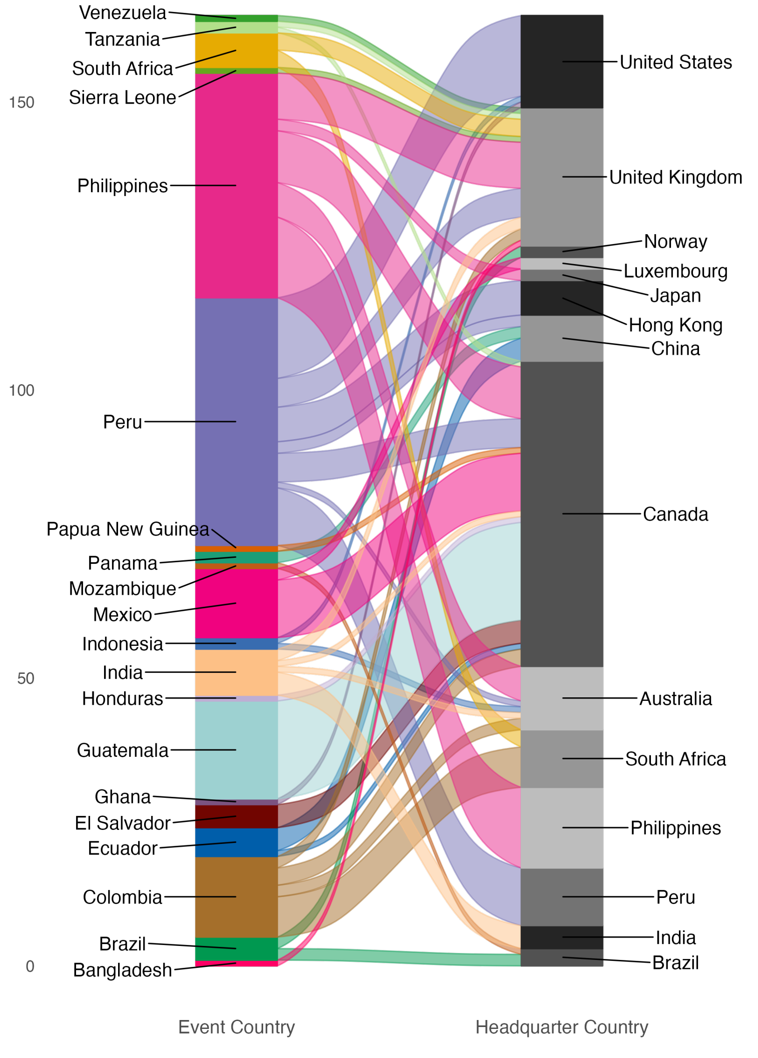


FIGURE 2. Event country activity and company headquarter locations.

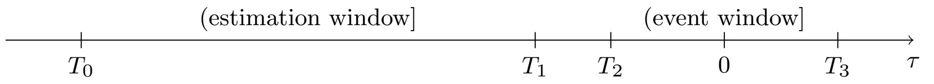


Figure 3. Event Study Timeline

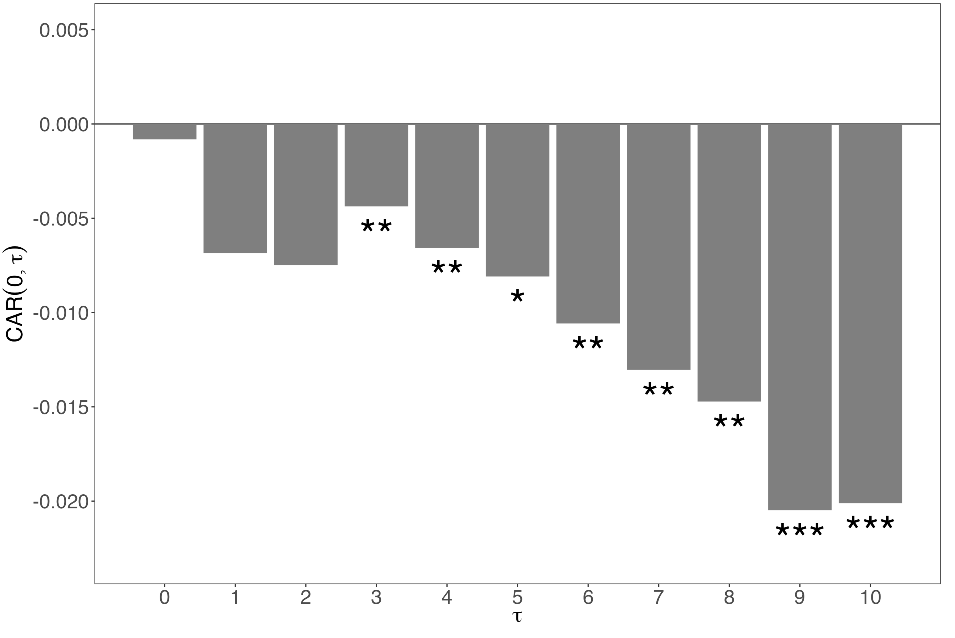


FIGURE 4. The effect of assassinations on stock returns. The average cumulative abnormal return (CAR) for the baseline market model. The number of company-event pairs, N, is 167. A minimum of eight trading days is required during the event window from Days 0 to 10. The estimation window spans from Day -280 to Day -30, with a minimum of 200 trading days during this period. Stars depict significance levels for the GRANK (Kolari and Pynnönen 2011) test-statistic: *p<0.1, ** p<0.05, *** p<0.01. The graphed results and additional test statistics are presented in Online Appendix Table C.4.

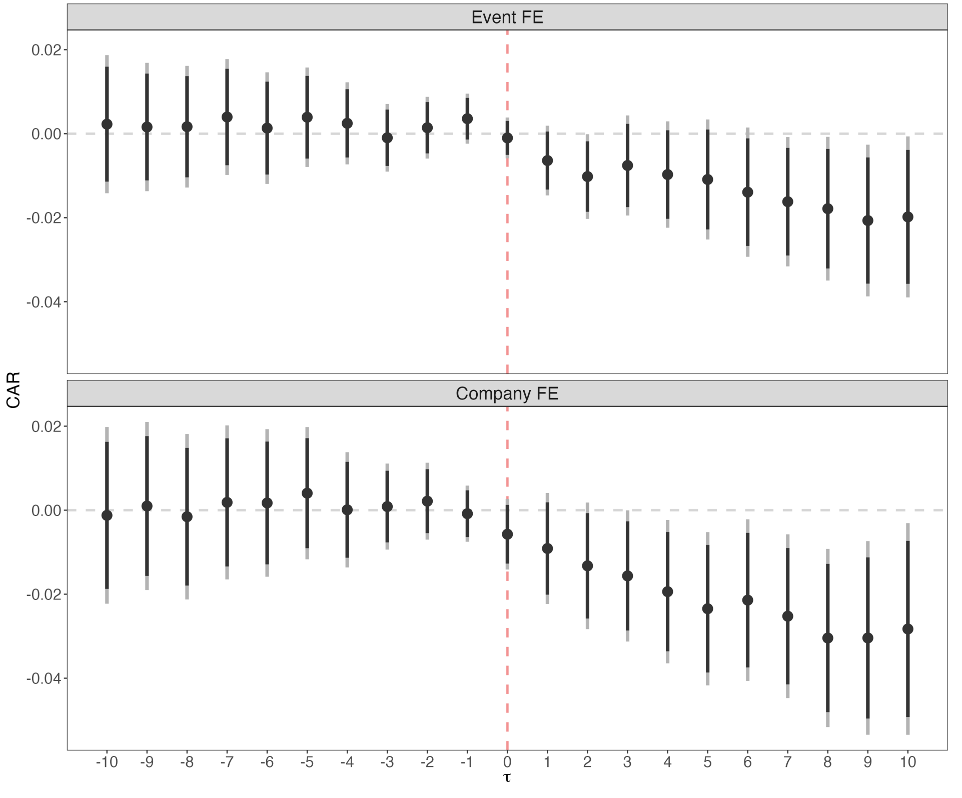


FIGURE 5. The treatment effect of assassination events on mining companies. The coefficients when regressing the respective cumulative abnormal return (CAR) on an indicator for being tied to an assassination event are represented by black dots. The horizontal axis represents the trading days before and after the event on $\tau = 0$. CARs are aggregated backward before the event date and forward starting with the event date; for example, -5 refers to the CAR between -1 and -5 while 5 refers to the CAR between 0 and 5. The top panel displays the point estimates for $\hat{\delta}$ when event-fixed effects are included, and the bottom panel displays estimates for the specification with company-fixed effects. In total, the coefficients of 42 regressions are displayed; 90% and 95% confidence intervals using robust standard errors clustered on the event level are depicted in black and gray, respectively.

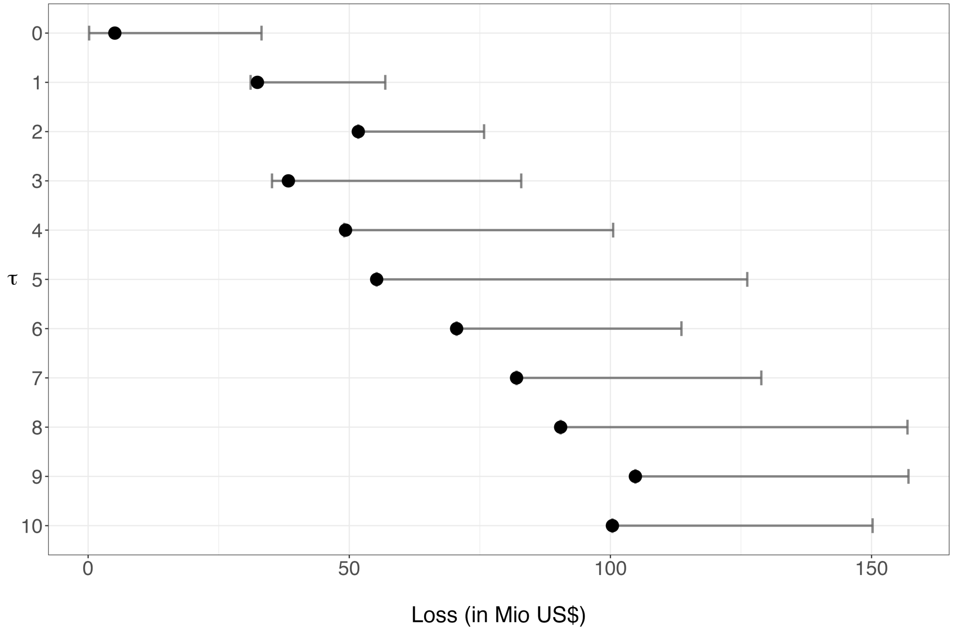


FIGURE 6. The estimated economic value of assassination events. Dots correspond to the estimated loss in market capitalization of the median company for the baseline event fixed effects specification. Gray bars depict the estimated minimum and maximum loss in market capitalization for the median company across all specifications reported in Online Appendix Figure C.9.

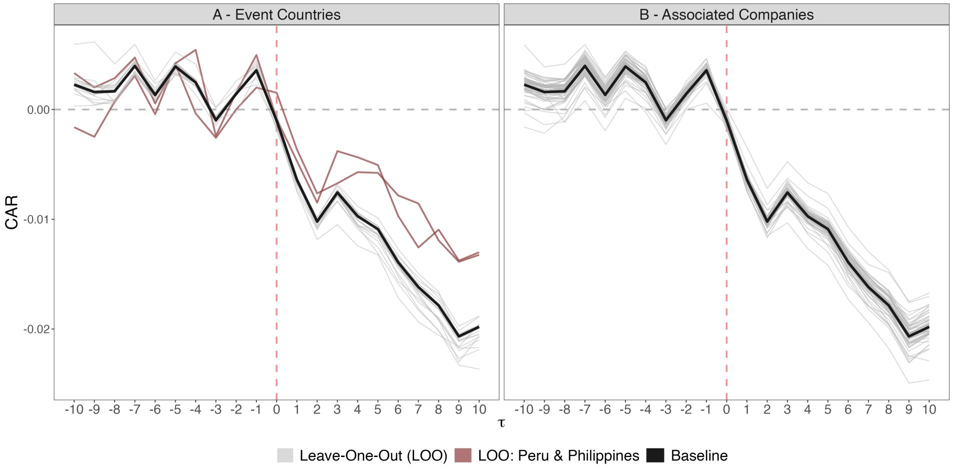


FIGURE 7. Leave-one-out robustness check. The thick black line in Panels A and B depicts the coefficient estimates for our baseline specification with event fixed effects (Figure 5). Panel A presents CAR estimates when we consecutively drop one country at a time from the sample. The red lines depict the estimates for excluding all events in the Philippines and Peru from the sample. Panel B displays the estimated coefficients when treated companies are dropped from the sample one at a time.

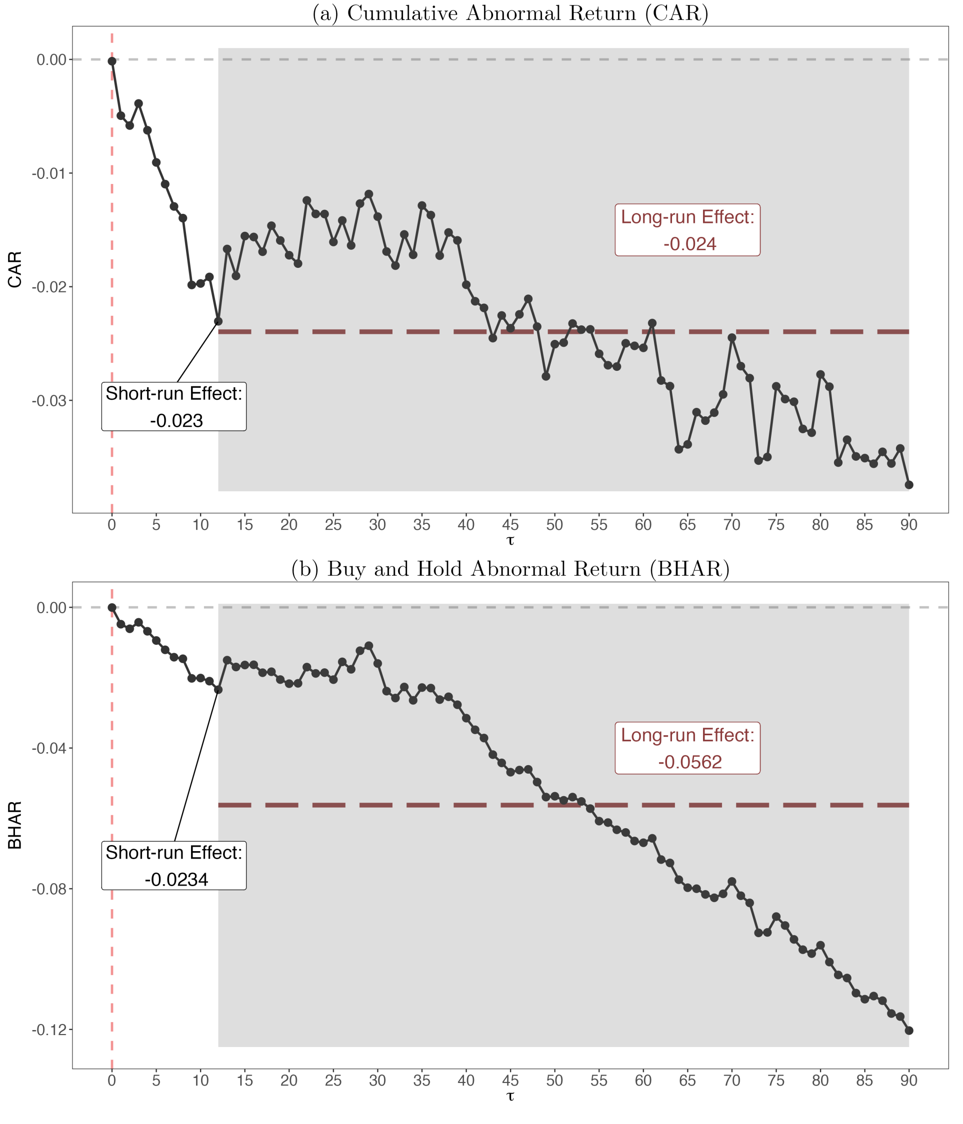


FIGURE 8. The long-run effect of assassination events. Panel A displays CAR estimates for the market model. Panel B presents the buy-and-hold abnormal return (BHAR). Companies have to have been traded on 70 out of the 91 days following the event at $\tau = 0$ to be included in the sample.

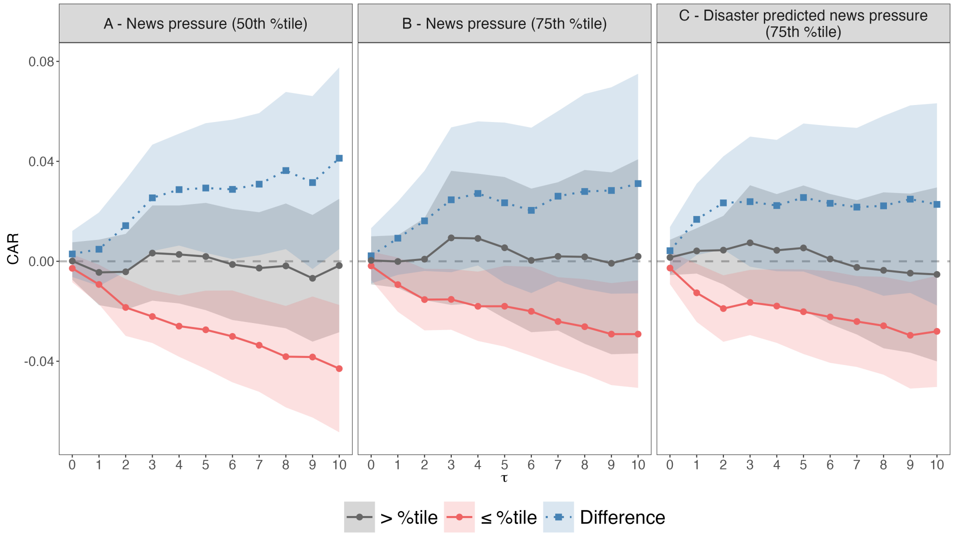


FIGURE 9. The influence of news pressure on the event day. Estimates of equation (6) for the marginal treatment effect of assassination events on high versus low news pressure days are depicted by the black solid line and red dots, respectively. The estimated (absolute) difference in treatment effects $|\hat{\delta}^N|$ is represented by the dotted line and squares. In each panel, the treatment indicator D is interacted with a different binary indicator for high news pressure (Eisensee and Strömberg 2007) on the event day: *(a)* an above median news pressure day; *(b)* an above the 75th percentile news pressure day; and *(c)* an above the 75th percentile disaster-predicted news pressure day. 95% confidence bands using robust standard errors clustered at the event level are displayed.

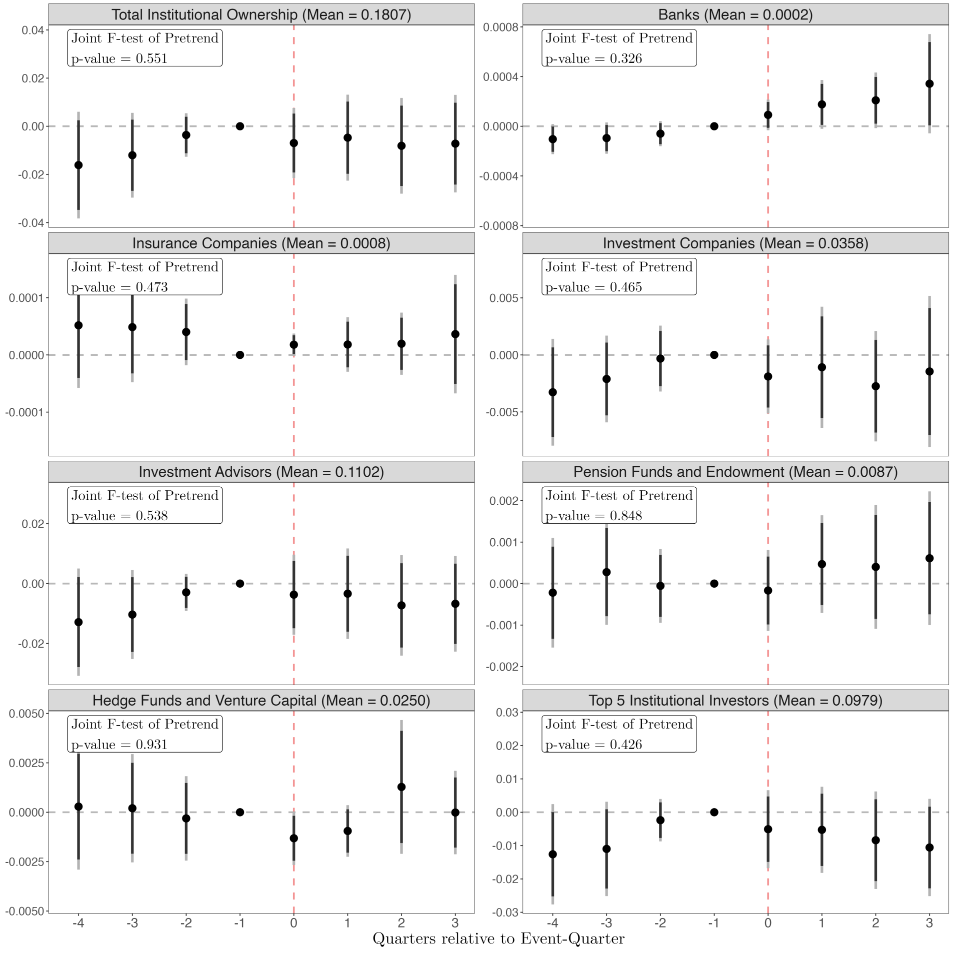


FIGURE 10. The effect of assassination events on institutional investor holdings. Stacked DiD estimates of equation (7) are presented. The control group comprises mining firms that were not treated during the event period. The dependent variable mean is presented in parentheses in the panel header; 90% and 95% confidence intervals using robust standard errors clustered on the company level are displayed in black and gray, respectively.

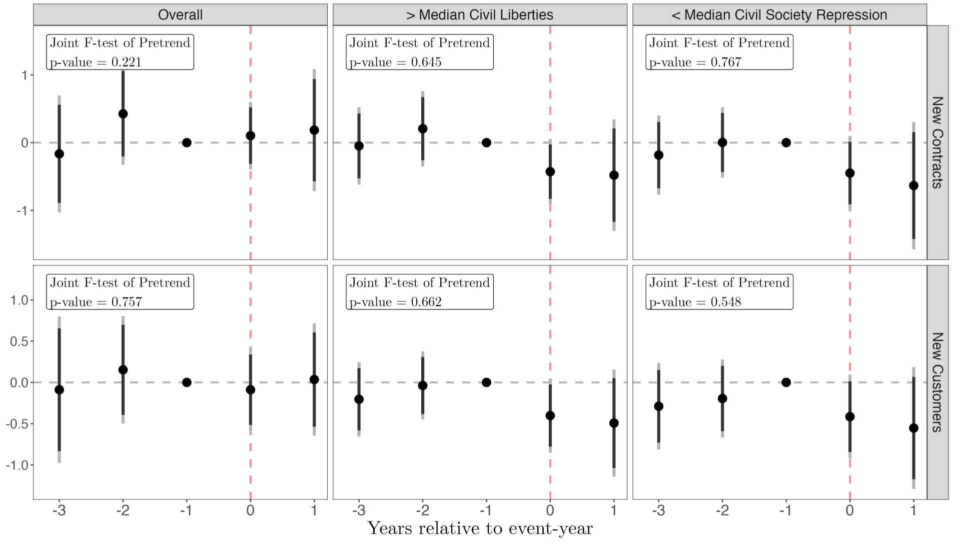


FIGURE 11. The impact of assassinations on supply chain contracts Stacked DiD estimates are presented. The first row presents estimates for the log of the number of new contracts, and the second row displays estimates for the log of the number of new customers. The estimated effect on overall contracting is presented in the first column, while the second and third column displays how contracting with customers headquartered in countries with *(2)* above median civil liberties and *(ii)* below median civil society repression was affected, respectively. The control group comprises mining firms that were not treated during the event period. 90% and 95% confidence intervals using robust standard errors clustered on the company level are displayed in black and gray, respectively.