The Value of Names: Civil Society, Information, and Governing Multinationals*

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November 30th, 2024 [First Draft: December 2020]

Abstract

Does the human rights spotlight impact multinationals? We evaluate the effect of publicizing human rights violations on firm value, focusing on salient events at the center of international campaigns: the assassination of environmental activists opposing multinational mining operations in the global periphery. We employ 20 years of data on activist deaths and use a financial event study methodology to estimate the impact of a human rights spotlight on the stock price of firms associated with this violence. We find that the human rights spotlight has a substantial effect. Firms named in assassination coverage have large, negative abnormal returns after these events, with a median loss in market capitalization of USD 100 million. We show that the media plays a key role in generating these effects; the negative impact of assassinations is strongest when they coincide with calm rather than saturated news cycles, in which news is less likely to reach investors. Our study highlights economic over non-pecuniary mechanisms. An association with violence negatively impacts supply chain contracts and provokes a negative reaction from institutional investors. Lastly, we show that assassinations are positively related to the royalties paid by mining projects to domestic governments.

JEL: O16, O17, D74, G3, G14, L82

*We benefited greatly from conversations with Daron Acemoglu, Sam Bazzi, Sascha O. Becker, Roland Hodler, Eliana La Ferrara, Weijia Li, Nicola Limodio, Toby Nangle, Mark Schelker, Martin Schmalz, David Strömberg, and Christopher Woodruff. We benefited from participants in the Applied Young Economist Webinar (AYEW), Bocconi, CSAE-Oxford, NEUDC, PACDEV, Royal Economic Society, Stanford GSB, and the Univ. Rosario Applied Micro Seminar. We would like to thank Satya Borgohain, Rony Rodrigo Maximiliano Rodríguez Ramírez, Lucy Valentine, and Natasha Watkins for their outstanding research assistance. For the estimation package used in this paper, please refer to this study's **R** package, `synthReturn` at `github.com/davidkreitmeir/synthReturn`.

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I Introduction

Global corporations increasingly operate in weakly governed territories, engaging in conflict-prone activities like natural resource 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 by global organizations.[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), and recent work suggests that 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 and 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,

[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).

2015). Because the formal costs for human rights abuses are exceedingly rare, stock prices provide a gauge of the 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 use this rich data to evaluate the market-based penalties for firms embroiled in human rights controversies and consider the various channels driving these penalties.

We employ financial event study methodology to estimate the penalty from these 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 these penalties are not driven by contemporaneous events and, importantly, not anticipated by market reactions on the days prior to the event. Moreover, these

[3]No firm in our study has faced legal penalty. See Section V.4.A.

[4]See: github.com/davidkreitmeir/synthReturn

penalties are firm-specific and 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 imply that the median 10-day cumulative loss in market capitalization for companies named in assassination news is over USD 100 million. Thus, these effects may be severe enough to incentivize firm behavior. We show these effects are also on par with other studies of market penalties 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 driving the penalty: First, we show 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 dovetail with work on the role played by institutional investors in promoting social responsibility (Dyck et al., 2019), especially in emerging markets with weak institutions (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 and empirical finance to estimate the market penalty 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 indicate that informal actors and actions play potentially important roles 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 well impact downstream activity more than upstream activity. Nonetheless, we show that reputational risk may nonetheless be substantial for upstream firms.

Third, our findings contribute to the literature on the role of media and information in 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 II introduces the study's context. Section III details our data and coding process. Section IV outlines the empirical methodology and presents the main findings. In Section V, we investigate the mechanisms underlying the baseline results. Section VI explores reasons for the continued prevalence of assassinations. We conclude with a brief discussion of our results in Section VII.

II 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.

II.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 persons, assassinations aim to advance an agenda or goal.

Our study focuses on assassinations of civil society members mobilized around natural resource activities. The individuals considered were prominent in their communities and active in social groups; they include indigenous leaders, environmental and labor activists, clergy, educators, and others. 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) to reduce monitoring (Carey and Gohdes, 2021; Krain et al., 2023) and raise 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 repression (Butt et al., 2019; Albarracín and Wolff, 2024) and 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) show 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.

II.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:

i. 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 may 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]

ii. 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, assassination dates correspond closely with the dates when information is revealed to markets. Thus, unlike other events, there is a considerable overlap between the occurrence and initial reporting of assassinations.

iii. 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

[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).

information to markets. This approach allows us to avoid empirical issues related to event selection in financial event studies (Krüger, 2015; Carpentier and Suret, 2022), such as internal regulatory events that may not be publicized to markets.

iv. 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 confounding events.

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

III Data

III.1 Assassination Data

III.1.A Event Collection

Our data cover 354 assassinations (496 victims) over 20 years. 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 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 to the event. We detail this process below, and further technical detail is provided in 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, like LexisNexis, provide translations of international news coverage. Notwithstanding, we also perform multilingual searches, for example, in Spanish.

III.1.B Event Coding

Figure 2 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 2 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 it 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 2 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 IV). 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.

III.2 Firm Coding

III.2.A 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 III.1.B, 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 the mention of 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, according to public media reports.

Figure 1 summarizes the global distribution of assassinations and the headquarters of firms associated with these events. Colored panels on the left correspond to event countries, with their height representing the total number of event-company pairs in that 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 (Figure C.3 in the Appendix) and involve firms based in nations with the highest concentration of global mining companies (Figure C.4).

III.2.B 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 event in question. 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, Security and Exchange Commission (and non-U.S.) filings, 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 2. 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 reveals that Corriente Resources Inc. was acquired in 2010 and, at the time of the event in 2014, was owned by two publicly traded companies: China Railway Construction Corporation and Tongling Nonferrous Metals Group Holdings. These two companies are coded as "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 2. If no identifying data (blue) are available, we do not match the assassination to a specific firm. For an illustrative example, refer to Appendix Figure B.1.

III.3 Finance and Geo-Location Data

III.3.A Data Sources And Construction

We collect daily stock return data for mining companies identified in Section III.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 III.1.B). 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 Appendix Figure B.3 Panel A for an example.

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 MSCI index for the country in which they are 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.3 and C.2.

III.3.B 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 mining 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.

IV Estimation: The Impact of Assassination on Firm Value

We show the impact of assassinations in two steps. First, in Section IV.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 IV.2, we use a contemporary regression-based event study to unpack 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 IV.3 uses a long-run analysis to show that negative abnormal returns are persistent.

IV.1 Traditional Event Study: Basic Patterns

IV.1.A 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]:

$$R_{ie\tau} = \alpha_{ie} + \beta_{ie} R^{M}_{ie\tau} + \epsilon_{ie\tau}, \tag{1}$$

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 III.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 $\overline{CAR}(\tau_1, \tau_2)$ follows a normal distribution with mean zero and variance $\sigma^2\left[\overline{CAR}(\tau_1, \tau_2)\right]$; *(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.

IV.1.B Results: Main Patterns

i. 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 IV.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) find 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 V.4.A).

ii. 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 its impact. Hence, we may observe a market under-reaction 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 IV.2) and in our long-run analysis (Section IV.3).

iii. 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 and not 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. 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, 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 there to be ambiguity surrounding 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 (IV.2) and also reject the possibility that other disruptions are confounding our estimates (Section IV.2.C).

iv. Robustness. The negative pattern described above is robust to alternative (stricter) estimation criteria. Appendix Figure C.7 shows that our results are unchanged when we require that companies are 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, hence, more vulnerable to disruption following an event than larger companies. Thus, stricter requirements on trading frequency may underestimate the true effects of assassination events.

IV.2 Regression-Based Event Study

IV.2.A Estimation

We build on the traditional event study analysis above (Section IV.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:

$$CAR_{ie}(\tau_1, \tau_2) = \alpha + \delta D_{ie} + \mathbf{X}_{ie}'\phi + \gamma_e + \epsilon_{ie}, \tag{5}$$

where $CAR_{ie}(\tau_1, \tau_2)$ 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 is equal to 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 choose control firms with mining operations in the same country in which 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 × 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 IV.2.D shows our OLS results are robust to this alternative method. We first turn to the baseline estimates from Equation (5).

IV.2.B Regression Event Study: Results

i. 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 shows our baseline specification, Equation (5), with event-fixed effects, while the bottom panel uses 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 IV.1.B).

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 1.4 percentage points for event- and company-fixed effects specifications, respectively. This negative trend continues, and CARs gradually decline over the next five days. By the tenth day following the assassination, the abnormal returns of exposed mining companies fall between 2.2 and 2.4 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.

ii. 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 they could benefit from firms that operate in the same country and industry being caught in the human rights spotlight, assassination events could also have negative industry-wide effects and spillovers (Bouzzine and Lueg, 2020; Bouzzine, 2021), by 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. 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.

iii. 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. 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 IV.3 and contrast these effects to reversals seen in the nascent reputation and deterrence literature.

iv. 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 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]

IV.2.C 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.

i. 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.

ii. Event Type. Our OLS results are also robust across different types of assassinations and similar events. 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 (Columns 6 to 10). While the point estimates follow a similar pattern to our baseline results, excluding these events (42 observations in the latter case) reduces the explanatory power and significance of the results.

iii. Functional Form. 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

[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)

specifications with different fixed effects, including less conservative sets like year (γ_y) and headquarter-country (γ_h) effects (Rows 2 and 3).

iv. Outliers. Our baseline results persist when we control for outliers. 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.

v. 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 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 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 and no pre-trend. Ten days after an event, CARs are 1.4 percentage points lower for our baseline specification with event effects. 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.

IV.2.D *Synthetic Matching Estimation*

We build on our OLS estimator and estimate the policy impact 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 (IV.2.A). 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. We determine a set of control firms for each event date and treatment company combination. The estimated average treatment effect, $\hat{\phi}(\tau_1, \tau_2)$, is averaged over the CARs for each treatment group company, weighted by the "goodness" of their synthetic match over the estimation window. The abnormal return is the difference between a treated firm's actual return and the return of its synthetic match. We construct confidence intervals by computing the average treatment effect for 3,425 placebo treatment groups. We randomly draw 25 placebo treatment groups of the same size as the actual treatment group ($N = 167$) for each of the 137 event dates in our sample. For more details on the modified synthetic matching method, see Appendix Section A.2.

Table 2 reports the synthetic event study estimates for the event day and the ten trading days thereafter. As in our OLS estimation, the impact of being associated with an assassination event is negative and increasing over the event window, except for Day 10, which is still significantly negative. These effects are highly significant: all estimates are significant at the 1% level and lie outside the $[0.005, 0.995]$ interval of the placebo treatment effects. The estimated treatment effects are slightly smaller than our OLS estimates but follow the same qualitative pattern. Appendix Figure C.12 depicts the size of the estimated treatment effect relative to the distribution of placebo treatment effects and illustrates the extent to which these returns are abnormal for the treated companies.

IV.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.[8]

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 3.1 percentage points. This pattern is robust to 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.

V 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

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

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.

V.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 shows that media attention matters for 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 misconduct research 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 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:

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

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.

i. 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 3.9 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.

ii. Alternative News Pressure. A potential concern is that perpetrators may strategically time their attacks to fall on high-pressure days to minimize 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.[9] 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

[9]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.

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.

iii. Robustness. In 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. In addition, we show that there is no statistically significant correlation between the daily level of news pressure and assassination events.

V.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 able to process 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).

i. Empirical Framework. We examine the role of institutional investors and analyze how they respond to the human rights controversies using a stacked difference-in-differences estimation framework developed by Wing et al. (2024), which accounts for staggered treatment and multiple events per company and non-continuous treatment in our setting.[10]

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

[10] Wing et al. (2024) demonstrate 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.

from $\kappa_{pre}, \ldots, \kappa_{post}$. Next, we stack all sub-experiments in event time such that at each event period, the stacked dataset comprises $N^D_{\Omega_\kappa} = \sum_{a \in \Omega_\kappa} N^D_a$ treatment-group units and $N^C_{\Omega_\kappa} = \sum_{a \in \Omega_\kappa} N^C_a$ 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:

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

where the bias-correcting sample weights are defined as

$$Q_{ia} = \begin{cases} 1 & \text{if} \quad D_{ia} = 1 \\ \frac{N^D_a / N^D_{\Omega_\kappa}}{N^C_a / N^C_{\Omega_\kappa}} & \text{if} \quad D_{ia} = 0, \end{cases} \tag{8}$$

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.

ii. 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.

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."

iii. 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 6.7% 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.[11]

This heterogeneity may be explained by a number of 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.[12]

iv. Robustness: Social Events Vs. ESG Scores. In 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 Business and Human Rights Clinic (2018), which highlight that ESG indicators often lack sufficient coverage of large companies operating in emerging markets.

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

[12]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.

V.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 employ rich supply chain data to explore whether downstream buyers adjust their behavior following such revelations.

i. 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 V.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.

ii. 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 17% 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 an even more pronounced negative effect of about 19% on supply-chain contracting (Column 3).[13] Our benchmark findings are robust to the use of alternative country-level indicators of human rights protection (see Appendix Figure C.16).

In contrast, 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.

V.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.

[13]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.

V.4.A Direct Legal And Financial Costs

We show that it is unlikely that our results are driven by investor expectations about legal penalties for human rights violations abroad. In environments where legal capacity is high, stock price movements reflect the expected formal and legal penalties following revelations of firm 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). Over our entire sample, we were unable to find evidence that any of the events resulted in convictions or meaningful legal fines for treated companies. A detailed qualitative discussion on the scarcity of court cases involving human rights conduct is provided in 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 V.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 using the quality of the judicial system in the event and headquarters country are presented in Figure C.18 in the 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 violations.

V.4.B 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.

i. 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.[14]

ii. Results. 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.[15]

The GDELT data (Panel B) shows a noisier and less consistent pattern, contrasting with anecdotal evidence. In Panels C and D of Figure C.19, we focus on MM protests

[14] Details on constructing our protest measures are available in Appendix Section B.4.

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

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 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.

VI 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.

i. 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 Appendix Table C.11, disaggregated by event country.[16]

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

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

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

iii. 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 × 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.[17] Estimates are qualitatively similar when we employ a *probit* instead of linear probability model (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 (Appendix Table C.13). These findings assuage concerns about reverse causality. Nevertheless, the estimates are indicative, not causal.

[16]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.

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

VII Conclusion

We study the power of human rights publicity and its impact on multinationals. We answer this question by turning to prominent, well-publicized events at the heart of current advocacy: the assassination of environmental activists in the sector at the center of this conflict, the global mining sector. Our study evaluates the impact of publicizing human rights violations on the value of the multinational companies connected with these. We compile a new database on 354 assassinations and extrajudicial killings of activists and link them to the publicly listed mining companies implicated in the events. We then combine this data with the companies' daily stock market returns and use event study methodology to estimate the effect of the killings on their abnormal daily returns.

Our results show that publicity around human rights abuse has substantial negative effects on multinationals. We estimate significant negative abnormal returns following assassinations for firms associated with these events. Negative effects appear on the (trading) day after an assassination occurs. These negative effects are amplified for the 10 days after the event and continue thereafter. We show these patterns using two types of event studies. All methods tell a consistent story. Standard event study estimates are robust to alternative test statistics, and regression methods are robust across OLS and synthetic estimators. Rather than benefiting firms, we show that eliminating activists leads to a statistically significant lowering of returns, with a cumulative median loss of over USD 100 million in the 10 days following the event. These effects seem to persist in the long run, as evidenced by consistent buy-and-hold abnormal returns over several months, suggesting durable market-based reputational penalties.

We highlight the critical role of the media and the importance of economic mechanisms in generating these effects. We first examine this channel by considering the likelihood that news of human rights events reaches financial decision-makers and ask how the impact of these events varies over the news cycle. Using daily "news pressure" data (Eisensee and Strömberg, 2007), we find that the penalty for human rights news is mediated by the strength of the new cycle. We further show that the visibility of the multinationals in this coverage—that is, their being named–also matters. We compare abnormal returns for companies named in connection to assassinations versus merely operating closely (geographically proximate) to events. We find that firms operating in the vicinity of events but *not* named in media coverage do not experience significant penalties relative to those named explicitly. The human rights tactics of "naming-and-shaming" may thus carry weight.

Accordingly, informationally sensitive institutional investors may play an important role in our effects. We find institutional investors that follow event-based trading strategies—such as hedge funds—divest from mining companies after assassination events. These results dovetail with work on the role of institutional investors in promoting social responsibility (Dyck et al., 2019), especially in emerging markets with weak institutions (Dyck et al., 2008). Additionally, we highlight the importance of supply chain linkages as a potential mechanism for the negative reaction of the stock market. Following an assassination event, corporate customers headquartered in countries with high levels of human rights protection systematically reduce their contractual relationships with mining companies named by the reporting.

We believe that our results contribute to emerging work on the political economy of multinationals and the political economy of human rights more broadly. Our findings show that informational campaigns by civil society do, in fact, impact multinational corporations by making ongoing societal conflict more visible and salient. Being linked to human rights abuses can significantly influence a company's market value and may do so through economic (rather than non-pecuniary) mechanisms.

Our results highlight the potential of human rights reporting, advocacy, and journalism and their interaction with financial markets. They indicate that institutional investors incorporate ESG events into their portfolios. Some point to the lack of quantitative evidence of human rights violations having financial implications as the main barrier to elevating human rights concerns in actual 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. This can help investment managers to better incorporate human rights impacts in their long-term investment strategies and might ultimately ensure stock prices better reflect that there are human rights violations associated with the company's operations.

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Tables

Table 1: Assassination Summary Data

	Events		Victims	Assassination Attempts	Company-Event Pairs		Distinct Company Entities	
Country	Total	w/o 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
Papua New Guinea	1	0	4	0	1	1	1	1
Peru	57	5	87	4	79	65	29	19
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**	**18**	**306**	**224**	**147**	**87**

Notes: Events "w/o Ties" refer to events for which reports established a connection between the assassination (attempt) and the victim's opposition to mining, but no specific mining project or company was mentioned. The "Distinct Company Entities" entries correspond to the number of unique companies associated with assassination events in the respective country or on a world-wide scale.

Table 2: Synthetic Matching Method Acemoglu et al. (2016)

	Estimate	Confidence Interval (0.5%)	Confidence Interval (99.5%)
$\widehat{\phi}\,(0,0)$	-0.0007***	0.000000	0.000000
$\widehat{\phi}\,(0,1)$	-0.0014***	-0.000002	0.000001
$\widehat{\phi}\,(0,2)$	-0.0046***	-0.000008	0.000027
$\widehat{\phi}\,(0,3)$	-0.0049***	-0.000001	0.000001
$\widehat{\phi}\,(0,4)$	-0.0034***	-0.000002	0.000001
$\widehat{\phi}\,(0,5)$	-0.0047***	-0.000001	0.000002
$\widehat{\phi}\,(0,6)$	-0.0068***	-0.000006	0.000003
$\widehat{\phi}\,(0,7)$	-0.0103***	-0.000025	0.000009
$\widehat{\phi}\,(0,8)$	-0.0091***	-0.000131	0.000117
$\widehat{\phi}\,(0,9)$	-0.0126***	-0.000219	0.000483
$\widehat{\phi}\,(0,10)$	-0.0055***	-0.000135	0.000183

Notes: The table reports the estimated effect of assassination events on corporate stock returns and the 99% confidence interval using the modified synthetic matching method of Acemoglu et al. (2016) (For more details, please see Section A.2 in the Appendix). Confidence intervals for hypothesis testing are constructed as the interval that contains the $[5, 95]$, $[2.5, 97.5]$, respectively $[0.5, 99.5]$ percentiles of the effect of 3425 *placebo* treatment groups; *, **, *** denote significance at the 10%, 5%, and 1%.

Table 3: Tax Revenue Shares and the Likelihood to observe Assassinations

	Dependent Variable: 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: The *Tax Share* is defined as the taxes and royalties paid by a corporation to the host country government divided by the total tax and royalty revenues received from the mining industry. Robust standard errors clustered on the company-country level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Figures

Figure 1: Event Country Activity and Company Headquarter Locations

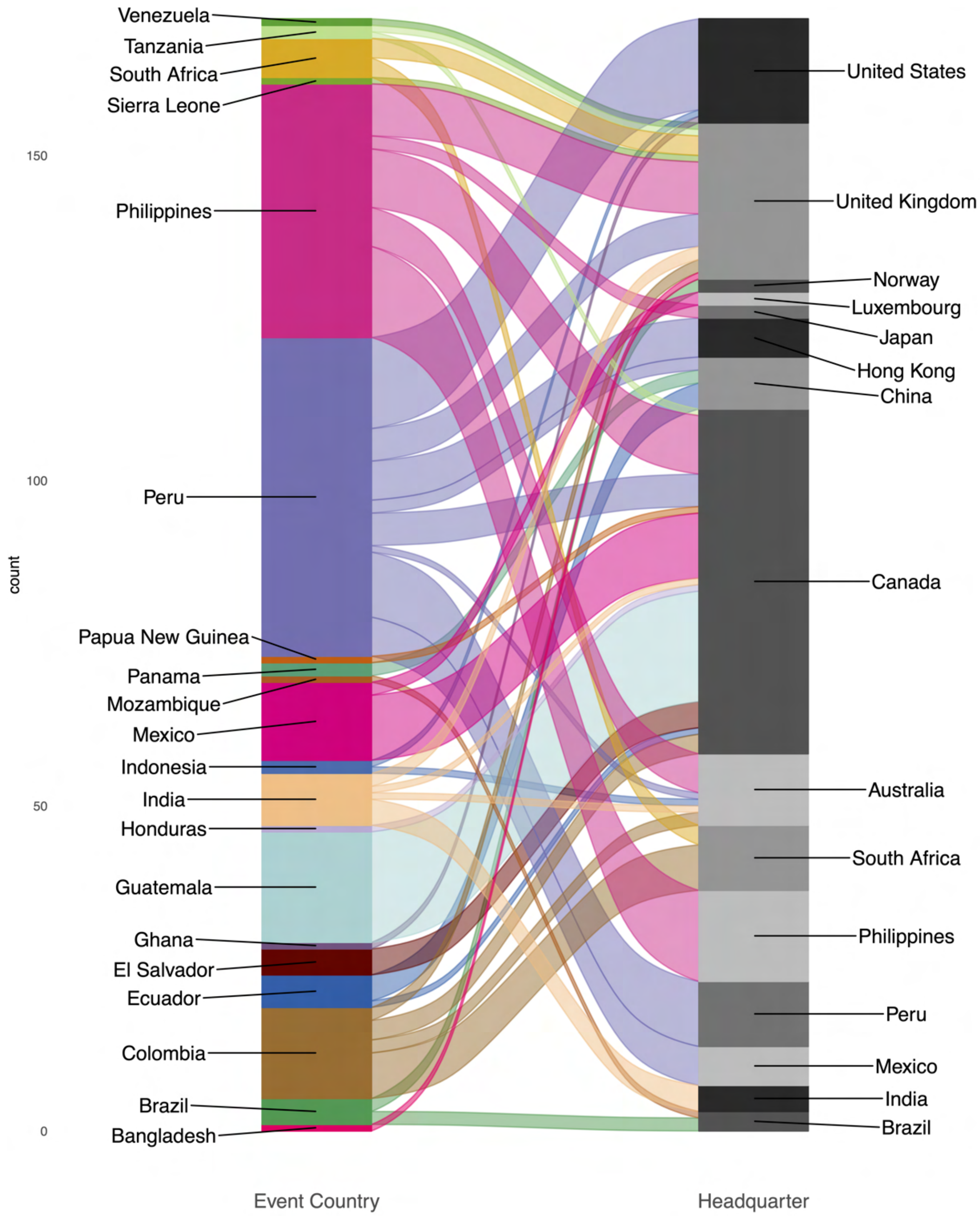
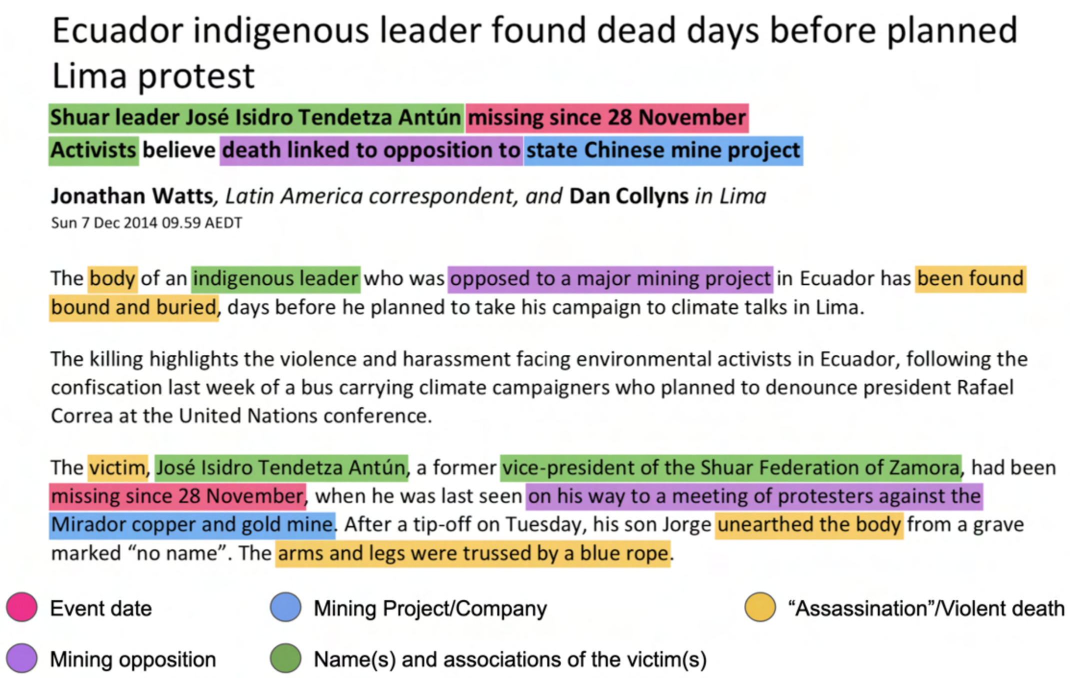


Figure 2: Extracting Events and Company Associations from NGO and Media Reports - Example Case



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 3: Event Study Timeline

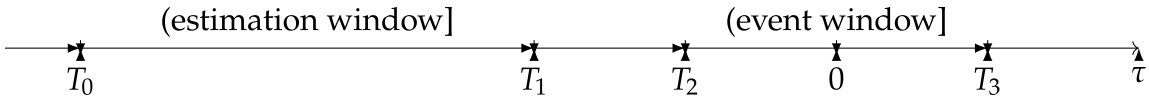
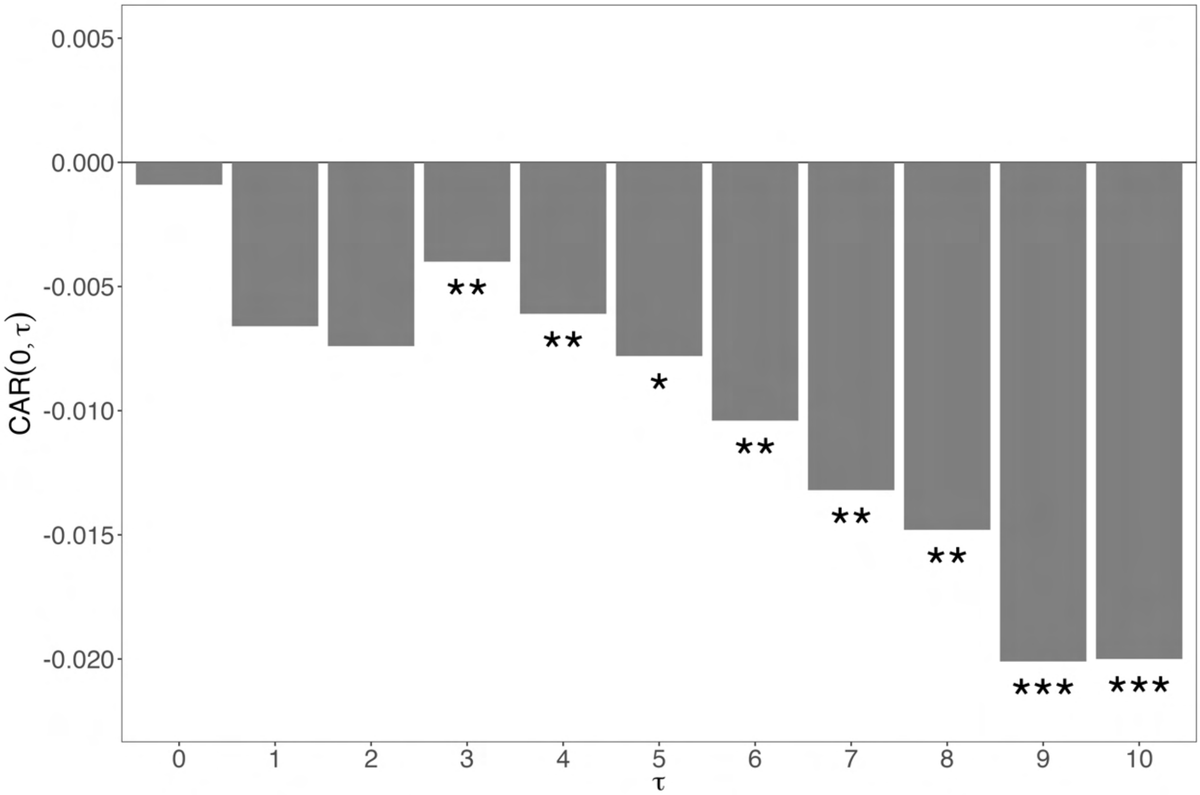
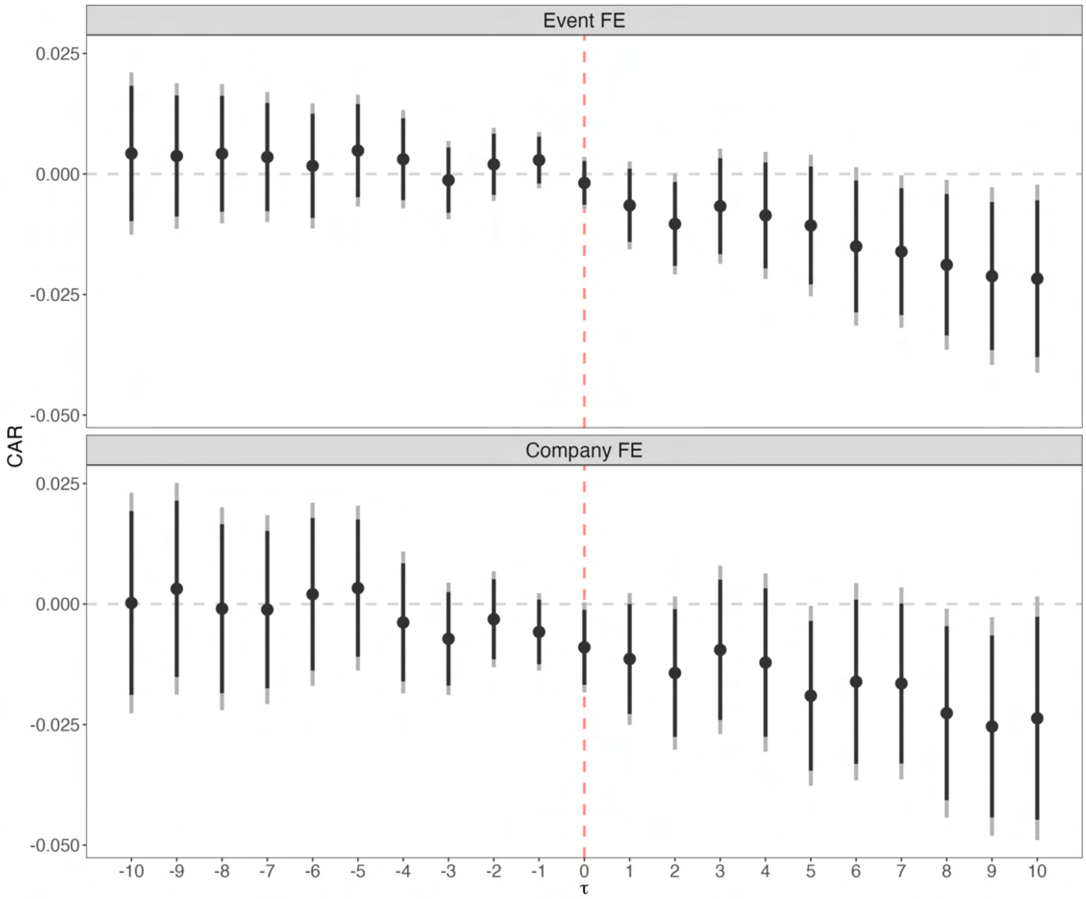


Figure 4: The Effect of Assassinations on Stock Returns



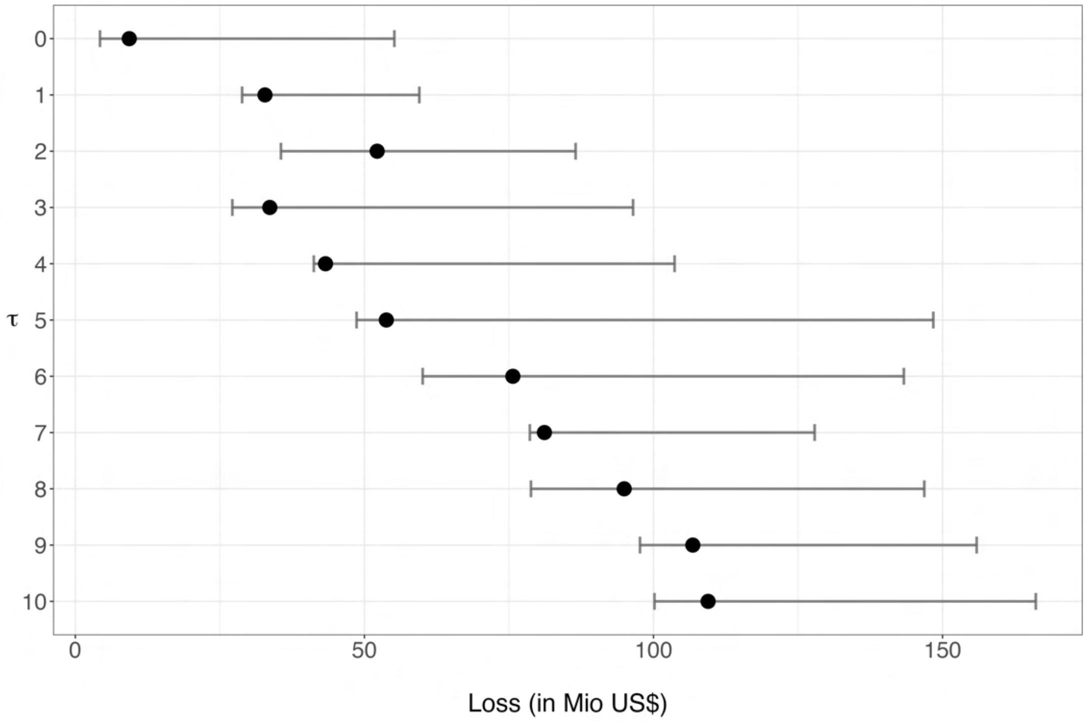
Notes: The average cumulative abnormal return (CAR) for the baseline market model. The number of company-event pairs, N, is 167. A minimum of eight trading days is required during the event window from Days 0 to 10. The estimation window spans from Day -280 to Day -30, with a minimum of 200 trading days 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 table format in Appendix Table C.4.

Figure 5: The Treatment Effect of Assassination Events on Mining Companies



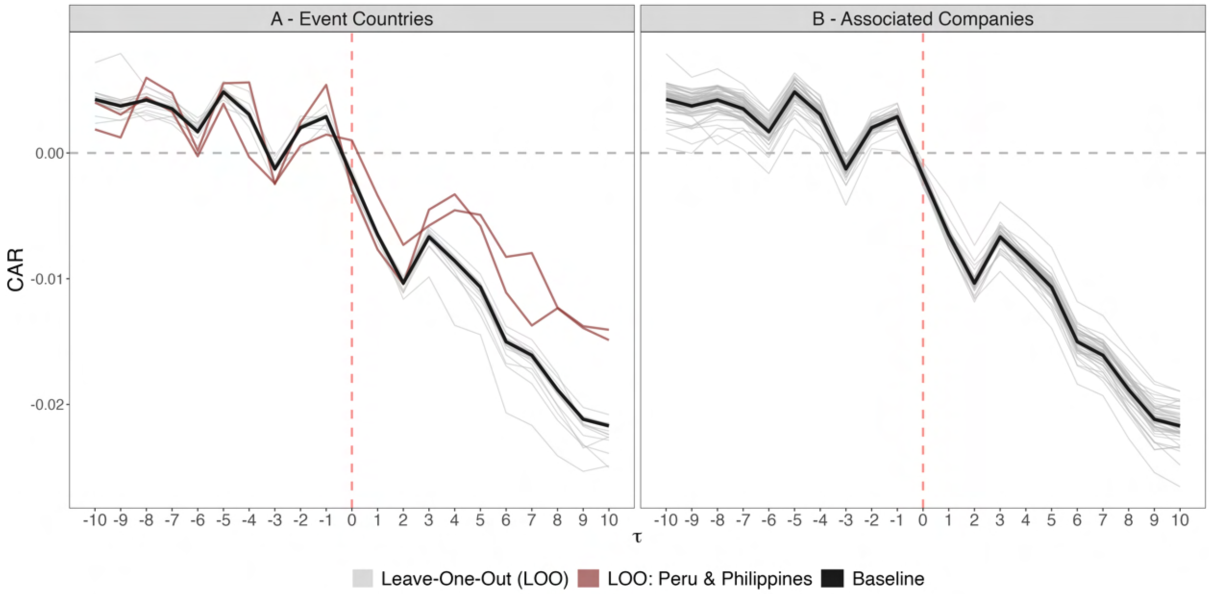
Notes: The coefficients when regressing the respective cumulative abnormal return (CAR) on an indicator for being tied to an assassination event are represented by black dots. The horizontal axis represents the trading days before and after the event on $\tau = 0$. CARs are aggregated backward before 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



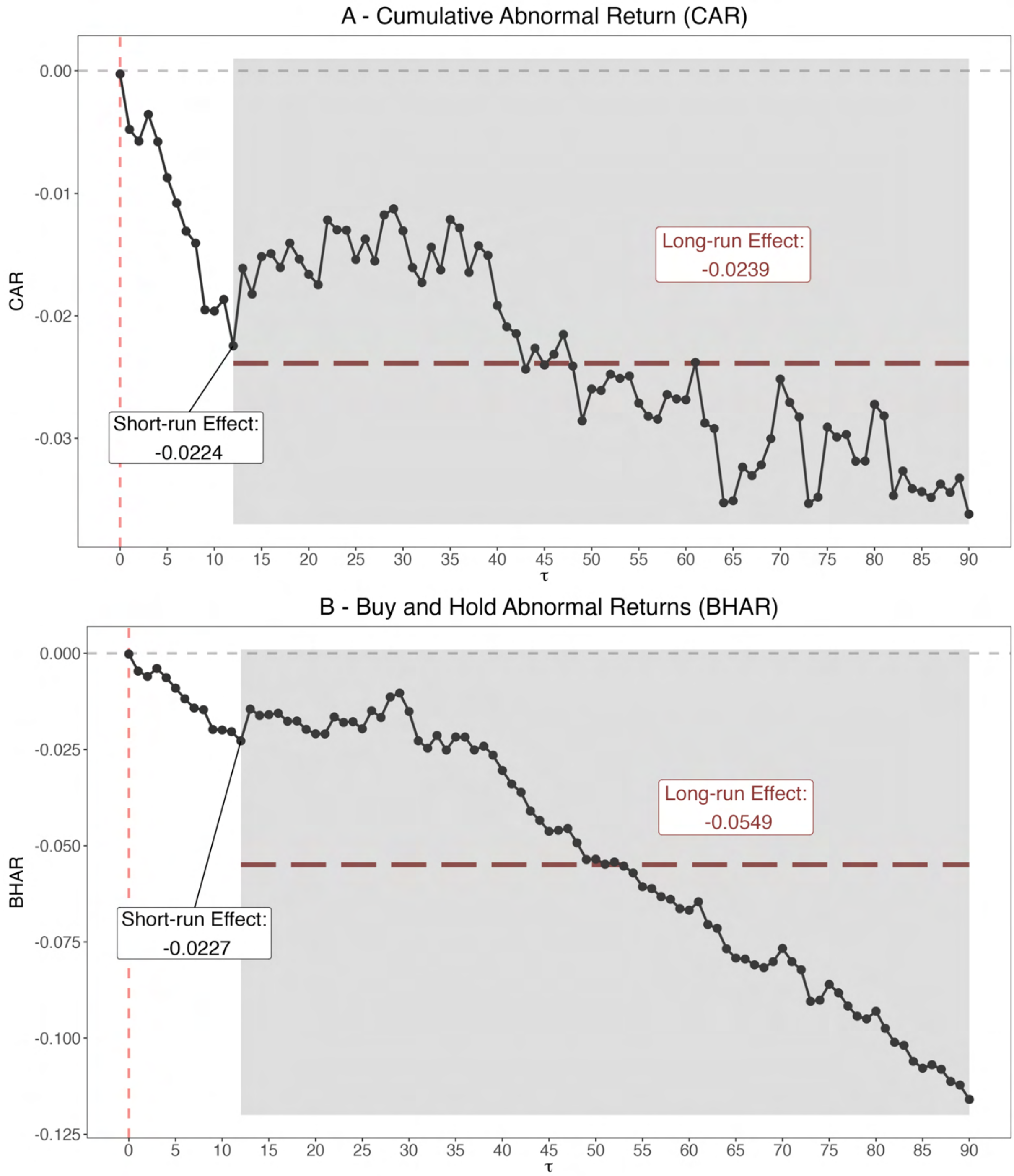
Notes: Dots correspond to the estimated loss in market capitalization of the median company for the baseline event-fixed effects specification. The gray bars depict the estimated minimum and maximum loss in market capitalization for the median company across the specifications presented in Figure C.9.

Figure 7: Robustness of Baseline Results - Leave-One-Out



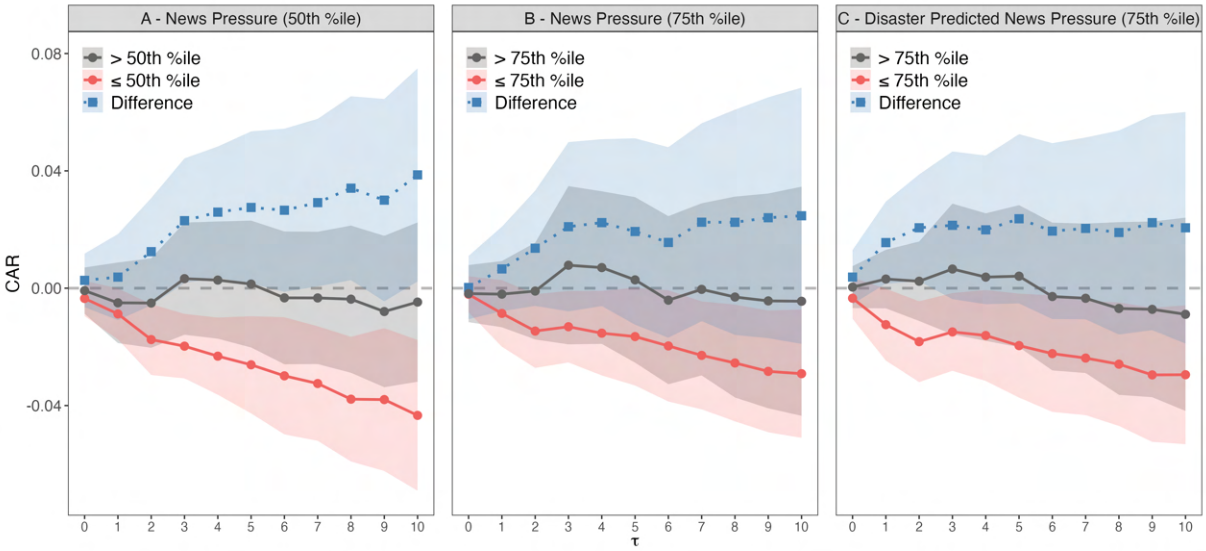
Notes: The thick black line in Panels A and B depicts the coefficient estimates for our baseline specification with event-fixed effects (c. Figure 5). Panel A presents CAR estimates when we consecutively drop one country at a time from the sample. The red lines in Panel A depict the estimates for dropping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

Figure 8: Long-Run Effect



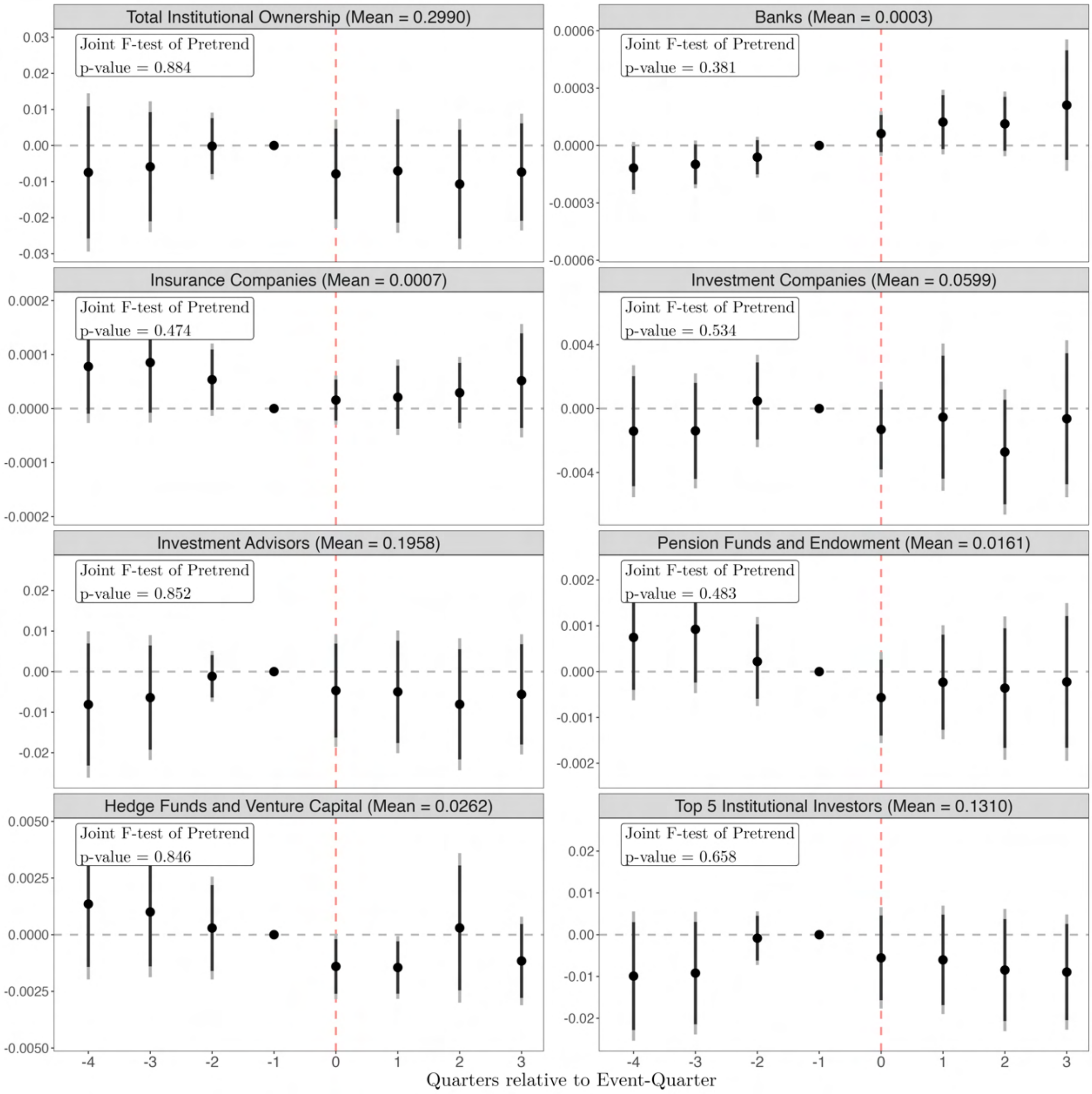
Notes: Bsed onthe market model, the average cumulative abnormal return (CAR) of mining companies associated with assassination events are displayed after the event day on $\tau = 0$ in Panel A and the buy-and-hold abnormal return (BHAR) in Panel B. Companies have to have been traded on 70 out of the 91 days following the event to be included in the sample.

Figure 9: The Influence of News Pressure on the Event Day



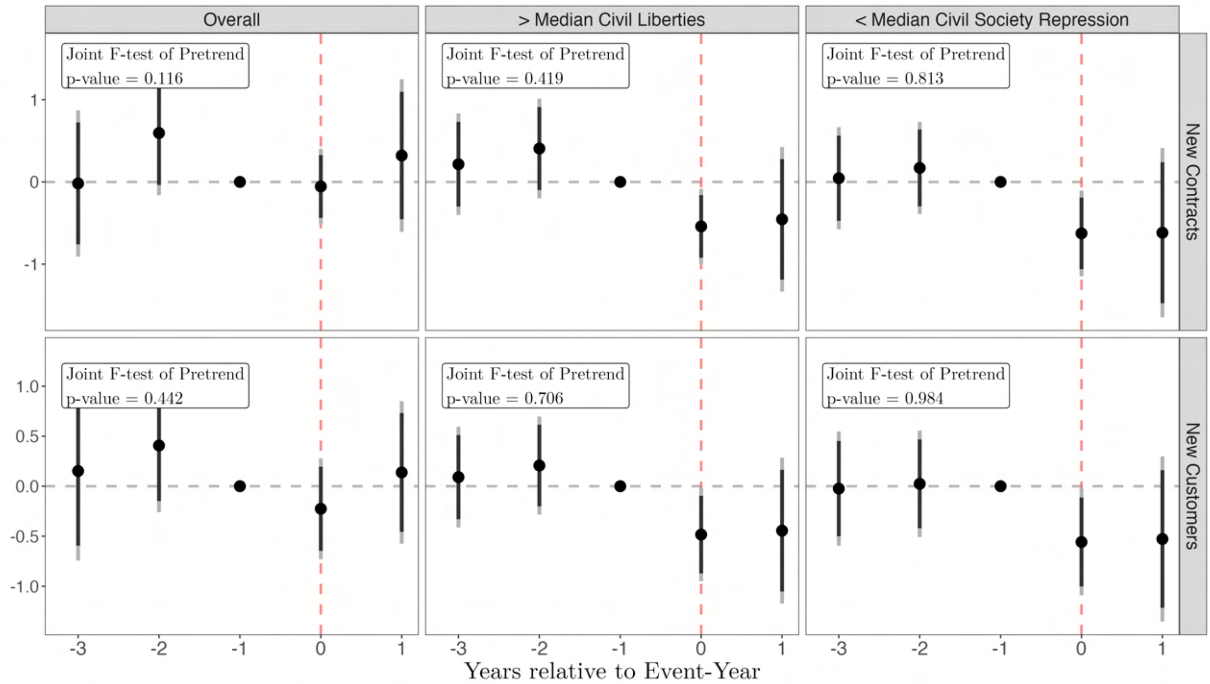
Notes: Red and black solid lines as well as dots denote the heterogeneous marginal treatment effect of assassination events on cumulative abnormal returns (CARs). The estimated (absolute) difference in treatment effects is represented by the dotted line and squares. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. In each panel, the treatment indicator D in our baseline specification in Equation 5 is interacted with a different binary indicator for high news pressure on the event day: *(i)* above themedian Eisensee and Strömberg (2007) news pressure day (Panel A); *(i)* above the 75th percentile Eisensee and Strömberg (2007) news pressure day (Panel C); *(iii)* above the 75th percentile disaster-predicted news pressure days (Panel C); 95% confidence intervals using robust standard errors clustered at the event level are displayed.

Figure 10: The Effect of Assassination Events on Institutional Investor Holdings



Notes: The effect of assassination events on institutional investors' holding position is presented. The control group comprises corporations active in the mining sector. The dependent-variable mean is presented in parentheses in the column 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



Notes: The effect of assassination events on supply chain contracting is presented. Column headers note dependent variables. Row headers refer to the "type" of contracts/customers considered in the respective specification. The horizontal axis represents the years before and after the event year $\tau = 0$. The control group comprises corporations active in the mining sector; 90% and 95% confidence intervals using robust standard errors clustered at the company level are displayed in black and gray, respectively.

Supplementary Online Appendix

A Technical Appendix

A.1 Test Statistics

For ease of notation (and without loss of generality), in this section we present the test statistics for one particular aggregation period from τ_1 to τ_2, where $T_1 < \tau_1 \leq \tau_2 \leq T_2$.[1]

t-Test

Following MacKinlay (1997), the null hypothesis H_0 of no event effect under the assumption of normally distributed security returns and in the absence of clustering can be tested using

$$\theta_1 = \frac{\overline{CAR}}{\sigma(\overline{CAR})} \sim N(0,1), \tag{A.1}$$

with $\overline{CAR}$ defined in Equation (4) and $\sigma(\overline{CAR})$ equaling

$$\sigma^2\left(\overline{CAR}\right) = \frac{1}{N^2}\sum_{j=1}^{N}\sigma^2\left(\widehat{CAR}_j\right). \tag{A.2}$$

For ease of exposition, the subscript $j = 1, ..., N$ denotes company–event pairs (ie). We refer to this test simply as the t-test.

BMP

Given the estimated abnormal and cumulative abnormal returns and their sample variance in Equations (2) and (A.2), the scaled abnormal (SAR) and cumulative abnormal (SCAR) returns during the event window $\tau = T_1 + 1, ..., T_2$ are defined as follows:[2]

$$SAR_{ie\tau} = \frac{\widehat{AR}_{ie\tau}}{\sigma(\widehat{AR}_{ie\tau})} \tag{A.3}$$

$$SCAR_{ie} = \frac{\widehat{CAR}_{ie}}{\sigma(\widehat{CAR}_{ie})}. \tag{A.4}$$

[1]This allows us to drop the suffix (τ_1, τ_2).

[2]Note that the definition for SARs is equivalent during the estimation window $\tau = T_0 + 1, ..., T_1$.

Boehmer et al. (1991) define the following test-static:

$$t_{BMP} = \frac{\overline{SCAR}\sqrt{N}}{\sigma(SCAR_{ie})}, \tag{A.5}$$

where $\overline{SCAR}$ constitutes the average SCAR on event day τ and $\sigma(SCAR_{ie})$ the cross-sectional standard deviation of the SCAR:[3]

$$\overline{SCAR} = \frac{1}{N}\sum_{j=1}^{N} SCAR_{ie} \tag{A.6}$$

$$\sigma(SCAR_{ie}) = \sqrt{\frac{1}{N-1}\sum_{j=1}^{N}(SCAR_{ie} - \overline{SCAR})^2}. \tag{A.7}$$

The rescaling of the SCARs by the cross-sectional standard deviation makes the BMP t-statistic robust to event-induced volatility.

ADJ-BMP

Kolari and Pynnönen (2010) relax the assumption of no clustering by allowing for covariance between the SARs. Under the assumption of equal variance of SARs, the authors show that the "true" cross-sectional variance of the SARs in this setting can be expressed as follows:

$$s^2(SAR_{ie}) = \frac{\sigma^2(SAR_{ie})}{N}\left(1 + (N-1)\overline{r}\right), \tag{A.8}$$

where $\sigma^2(SAR_{ie})$ is given in Equation (A.7) and $\overline{r}$ is the average of the sample cross-correlations of the abnormal returns during the estimation window. Using the variance formula in Equation (A.8), the adjusted BMP (ADJ-BMP) t-statistic is calculated as follows:

$$t_{ADJ-BMP} = \frac{\overline{SAR}}{s(SAR_{ie})} = \frac{\overline{SAR}\sqrt{N}}{\sigma(SAR_{ie})\sqrt{1+(N-1)\overline{r}}} = t_{BMP}\sqrt{\frac{1-\overline{r}}{1+(N-1)\overline{r}}} \tag{A.9}$$

[3]Note that (A.5)-(A.7) are calculated in the same way for the SAR.

The test statistic is equivalent for SCARs under the assumption of the square-root rule of the standard deviation of returns over different return periods (s. Kolari and Pynnönen, 2010, p. 4003).

GRANK

Kolari and Pynnönen (2011) re-standardize the SCARs defined in Equation (A.4) using the cross-section standard deviation of the SCARs defined in Equation (A.7) to transform the SCAR to a random variable with zero mean and unit variance as is the case for the other SARs defined in Equation (A.3):[4]

$$SCAR^{*}_{ie} = \frac{SCAR_{ie}}{\sigma(SCAR_{ie})}. \tag{A.10}$$

This allows Kolari and Pynnönen (2011) to define the generalized standardized abnormal return ($GSAR_{ie\tau}$) as follows:

$$GSAR_{ie\tau} = \begin{cases} SCAR^{*}_{ie}, & \text{for } \tau = \tau_1, ..., \tau 2 \\ SAR_{ie\tau} & \text{for } \tau = T_0 + 1, ..., T_1. \end{cases} \tag{A.11}$$

Intuitively, the CAR period is treated as if there were only one day, the "cumulative return day" at $\tau = 0$ (Kolari and Pynnönen, 2011). The demeaned standardized abnormal ranks ($U_{ie\tau}$) of the GSARs are as follows:

$$U_{ie\tau} = \frac{\text{Rank}(GSAR_{ie\tau})}{T+1} - \frac{1}{2}, \tag{A.12}$$

where $\tau \in \mathcal{T} = \{T_0 + 1, ...T_1, 0\}$ and T is equal to the length of the estimation window plus the "cumulative return day", i.e. $T = L_1 + 1 = T_1 - T_0 + 1$.
Since $U_{ie\tau}$ constitutes the demeaned rank of the GSAR, the null hypothesis of having no mean event effect, i.e. $H_0 : E[\overline{CAR}] = 0$, is equal to the expected rank of the GSAR being equal to zero for all company-event pairs on the "cumulative return day" ($E[U_{ie0}] = 0$).

[4]In the case of event-day clustering, it may be preferable to use the cross-correlation robust standard deviation $s^2(SCAR_{ie})$. Kolari and Pynnönen (2010) note, however, that this substitution should not substantially alter the results of rank tests (see footnote 7 on p. 4008).

Kolari and Pynnönen (2011) show that the t-statistic for testing this null hypothesis is as follows:

$$t_{GRANK} = Z\left(\frac{T-2}{T-1-Z^2}\right)^{\frac{1}{2}}, \tag{A.13}$$

where

$$Z = \frac{\overline{U}_0}{\sigma(\overline{U})} \tag{A.14}$$

with

$$\sigma(\overline{U}) = \sqrt{\frac{1}{T}\sum_{t\in\mathcal{T}}\frac{N_\tau}{N}\overline{U}_\tau^2} \tag{A.15}$$

$$\overline{U}_\tau = \frac{1}{N_\tau}\sum_{j=1}^{N} U_{ie\tau}, \tag{A.16}$$

where N_τ is the number of non-missing (valid) GSARs available at $\tau \in \mathcal{T} = \{T_0{+}1, \ldots T_1, 0\}$ and N is the number of all company-event pairs.

A.2 Modified Synthetic Matching Method of Acemoglu et al. (2016)

Following Acemoglu et al. (2016), we construct a synthetic match for each company i in the treatment group by solving the following optimization problem:

$$\underset{\{w_j^i\}_{j\in\text{Control group}}}{\arg\min} \sum_{t\in\text{Estimation Window}} \left[R_{it} - \sum_{j\in\text{Control group}} w_j^i R_{jt} \right]^2 \tag{A.17}$$

$$\text{s.t.} \quad \sum_{j\in\text{Control group}} w_j^i = 1 \tag{A.18}$$

$$w_j^i \geq 0, \tag{A.19}$$

where R_{it} and R_{jt} are the daily returns on day t for the treatment firm and the companies in the control group, respectively; $\{w_j^{i*}\}$ is the weight for control firm j in the optimal weighting for firm i. In line with our baseline analysis, the estimation window spans 250 trading days ending 30 days prior to the event day, and both treatment and control firms are required to be traded at least 200 out of the 250 trading days (and 8 out of the 11 days in the event window). Additionally, we require that control companies are traded on all non-missing trading days of the treated company to deal with missing values directly instead of relying on the assumption in Acemoglu et al. (2016) that missing values are equivalent to zero returns.

The aforementioned optimization problem amounts to a quadratic programming problem since the objective function is quadratic and the two constraints are linear. The problem can thus be rewritten as follows:

$$\underset{\mathbf{w}\in\mathbb{R}^J}{\arg\min} \quad f(\mathbf{w}) = \frac{1}{2}\mathbf{w}^\top \mathbf{D}\mathbf{w} - \mathbf{w}^\top \mathbf{b} \tag{A.20}$$

$$\text{s.t.} \quad \mathbf{A}_1 \mathbf{w} = 1 \tag{A.21}$$

$$\mathbf{A}_2 \mathbf{w} \leq \mathbf{0}, \tag{A.22}$$

where $\mathbf{w} \in \mathbb{R}^J$ is a vector containing the optimal weights for each of the $j = 1, ..., J$ companies; $\mathbf{D} =\in \mathbb{R}^{J\times J}$ is symmetric and equal to $\mathbf{R}^\intercal \times \mathbf{R}$ with matrix $\mathbf{R} \in \mathbb{R}^{T\times J}$ containing the returns in the estimation window of length T for all control companies J; $b \in \mathbb{R}^J$ and is equal to $\mathbf{R}^\intercal \times \mathbf{r}$ with $\mathbf{r} \in \mathbb{R}^T$ comprising the returns of the treated firm over the estimation window; and $A_1 \in \mathbb{R}^{T\times J}$ and $A_2 \in \mathbb{R}^{J\times J}$ are identity matrices and $\mathbf{0} \in \mathbb{R}^J$ a vector of zeros.

Reformulating the optimization problem allows us to use the dual method of Goldfarb and Idnani (1982, 1983) for solving quadratic programming problems implemented in the **R** function `solve.QP` of the `quadprog` package.[5]

After finding the optimal weights, we obtain the abnormal return of the treated firm i, given by the difference between its return R_{it} and the return for the synthetic firm:

$$\widehat{AR}_{it} = R_{it} - \sum_{j\in\text{Control group}} w_j^{i*} R_{jt} \quad . \tag{A.23}$$

Following Acemoglu et al. (2016), we account for the goodness of the synthetic match when calculating the treatment effect across all companies in the treatment group:

$$\widehat{\phi}\,(0,k) = \frac{\sum\limits_{i\in\text{Treatment group}} \frac{\sum_{t=0}^{\tau_2} \widehat{AR}_{it}}{\widehat{\sigma}_i}}{\sum\limits_{i\in\text{Treatment group}} \frac{1}{\widehat{\sigma}_i}} \tag{A.24}$$

$$\text{where} \quad \widehat{\sigma}_i = \sqrt{\frac{\sum\limits_{t\in\text{Estimation Window}} \left(\widehat{AR}_{it}\right)^2}{T}}, \tag{A.25}$$

where $\widehat{\phi}\,(0,k)$ is the cumulative effect over the period τ_1=0 to τ_2 in the event window. The overall treatment effect is thus a weighted average of each assassination effect on a treated company, with greater weight given to the estimated effects for which the synthetic firm tracks the returns of the treated company more closely during the estimation window.

[5]Note that **D** has to be (semi-)positive definite. In the rare case that this condition is violated, we apply the algorithm of Higham (1988) to compute the nearest symmetric positive (semi-)definite matrix using the **R** function `make.positive.definite` of the `corpcor` package.

We construct confidence intervals by randomly drawing $K \times E$ *placebo* treatment groups of size N corresponding to the actual number of firms in the treatment group; K is the number of random draws at each event date e, with the number of event dates equaling E. In this study, we draw $25 \times 137 = 3{,}425$ placebo treatment groups, each comprising 167 firms. We compute the average treatment effect for each placebo treatment group and day τ_2 during the event window. The effect is significant at the 10%, 5%, or 1% level if the actual estimated treatment effect lies outside of the interval that contains the $[5, 95]$, $[2.5, 97.5]$, and $[0.5, 99.5]$ percentiles, respectively, of the placebo treatment effects.

We provide an accompanying open source **R** package `synthReturn` that implements the modified synthetic matching method at github.com/davidkreitmeir/synthReturn.

B Data Appendix

B.1 Assassination Dataset

In this appendix, we detail the compilation and coding of assassination events. The list of 354 extra-judicial killings of mining activists was retrieved from a range of sources that can broadly be categorized into the following categories: Human rights reports, international full-text archives and APIs, local newspaper archives, and published supporting material.

Below, we first describe our sources and the search process, our coding procedure (Section B.1.1), next our process for identifying "associated" firms implicated in events (Section B.1.2), and then our coding of additional geographic information (Section B.1.3). Finally, we show the relationship between our data and the Armed Conflict Location and Event Data Project (ACLED) project (Section B.1.4).

B.1.1 Search And Sources

We implement systematic searches of four types of sources:

1. **Reports from NGOs and human rights associations**. Major sources include leading organizations such as "Amnesty International", "Front Line Defenders", "Global Witness", "Bulatlat.". Many human rights projects of this study form the basis for larger monitoring efforts, such as ACLED.

2. **International news databases, archives, and APIs.** Major databases include Dow Jones' Factiva and Lexis-Nexis. Full-text databases draw from Gale's archives of the International Herald Tribune and Associated Press wire. We also use the Guardian international news API.

3. **Domestic news archives**. We search local newspapers. For example, for Spanish-language sources were used for Latin-American markers, such as "La Republica" in Peru, "El Universo" in Ecuador, "El Pais" in Mexico or "El Espectador" in

Colombia. These were queried from RAs in Latin America, and also by PIs in Australia.

4. **Supplemental material from published reports and books.** We rely on published reports (e.g. Holden and Jacobson, 2012; Doyle and Whitmore, 2014) and studies (e.g. Imai et al., 2017; Spohr, 2016; Hamm et al., 2013) for supplementary material and validation. These sources often provide supplementary information on cases such as event classifications: mining, deforestation, and mining project/company associations. For instance, Holden and Jacobson (2012) provided a list of mining projects and their owners at the time in Chapter 2 that can be matched with the mining projects mentioned in association with killings of anti-mining activists in Chapter 5.

We search archives and databases above for articles that contain a combination of "mining" keywords, "activist" keywords (e.g., activist, campaigner, indigenous), and "assassination" keywords (e.g., kill, assassin, abduct). Keyword lists were chosen semi-automatically. We use measures of cosine similarity between seed terms and those identified from the Web2Vec word vectors pre-trained on the Google News data set (c. for instance Keith et al., 2017).[6]

A deduplicated list of returned articles is then manually inspected for relevant events. Given the specificity of the type of events, PIs and RAs then evaluate individual articles by hand to sift out false positives. We note that we also experimented with training text classification models to automatically detect relevant articles. The specificity of our events, however, does not allow for the construction of a sufficient training corpus. Moreover, the data collection process revealed that many assassination events are covered by local newspapers or NGO reports, usually not available in news archives and APIs. Each event is substantiated by at least two sources and often prompts multiple pieces.

[6]The Google News data set comprises about 100 billion words. The pre-trained Web2Vec word vectors can be found here: code.google.com/archive/p/word2vec/.

Having identified assassination events against members of civil society—opposition leaders, Indigenous and tribal leaders, and local environmentalists, we assess whether the event is linked to the victim's opposition to a mining project. We require at least one source to indicate whether opposition to mining is the (suspected) reason for the attack. We are not able to establish a link to mining opposition for 211 of the 565 killings of activists identified. These 211 cases are either assassinations related to other sectors such as logging, pipelines, and hydro dams, or instances where the source articles failed to disclose definitive evidence of opposition.

B.1.2 Coding "Associated" Firms

Next, we establish company "ties" for the 354 mining-related events identified. We implement the following matching procedure:

1. **Identify publicly traded firm.** If a mining company is named in at least one event-related article, we check whether the reported company is publicly traded. As a convention, in cases where the named mining company is not the global ultimate owner, we consider only the "downstream" publicly traded companies, except where the global corporate owner is specifically named in reporting. For example, if the article states that the assassination is linked to a mining project owned by AngloGold Ashanti, a publicly traded mining company ultimately owned by Anglo American, we do not classify Anglo American as being "associated" with the event unless a source article also specifically mentions Anglo American.

2. **Validate firm operation.** For each firm, we cross-validate whether the company was active in the country at the time of the event. Given the scope of the dataset, we do this manually. We search for and validate this information using annual reports and the SNL Metals & Minings database. When the named company is privately owned, we record the company name and do not further discern the structure of ownership by private individuals.

3. **Identify joint ventures and subsidiaries.** If a named mining company is not publicly traded, we examine if the company constitutes a subsidiary or joint venture

of a publicly traded company at the time of the event by manually consulting corporate finance resources. Our primary sources for historical ownership links are Bureau Van Dijk's Orbis database and the SNL Metals & Mining database. We cross-validate and, where necessary, supplement this information with data from company websites, annual reports, SEC filings, and business registers. In rare cases, the articles only name the specific project are named in articles (e.g., The Acme Mining Project) without reference to the project's owner. For these cases, we also rely on the aforementioned sources to establish the ownership structure of the mining project at the time of the event. In both cases, all owners are matched to the respective event. If a private company is the (partial) owner of a subsidiary and/or joint venture, the name of the company is recorded, not the name of the private owners of the company.

B.1.3 Coding Additional Event Information

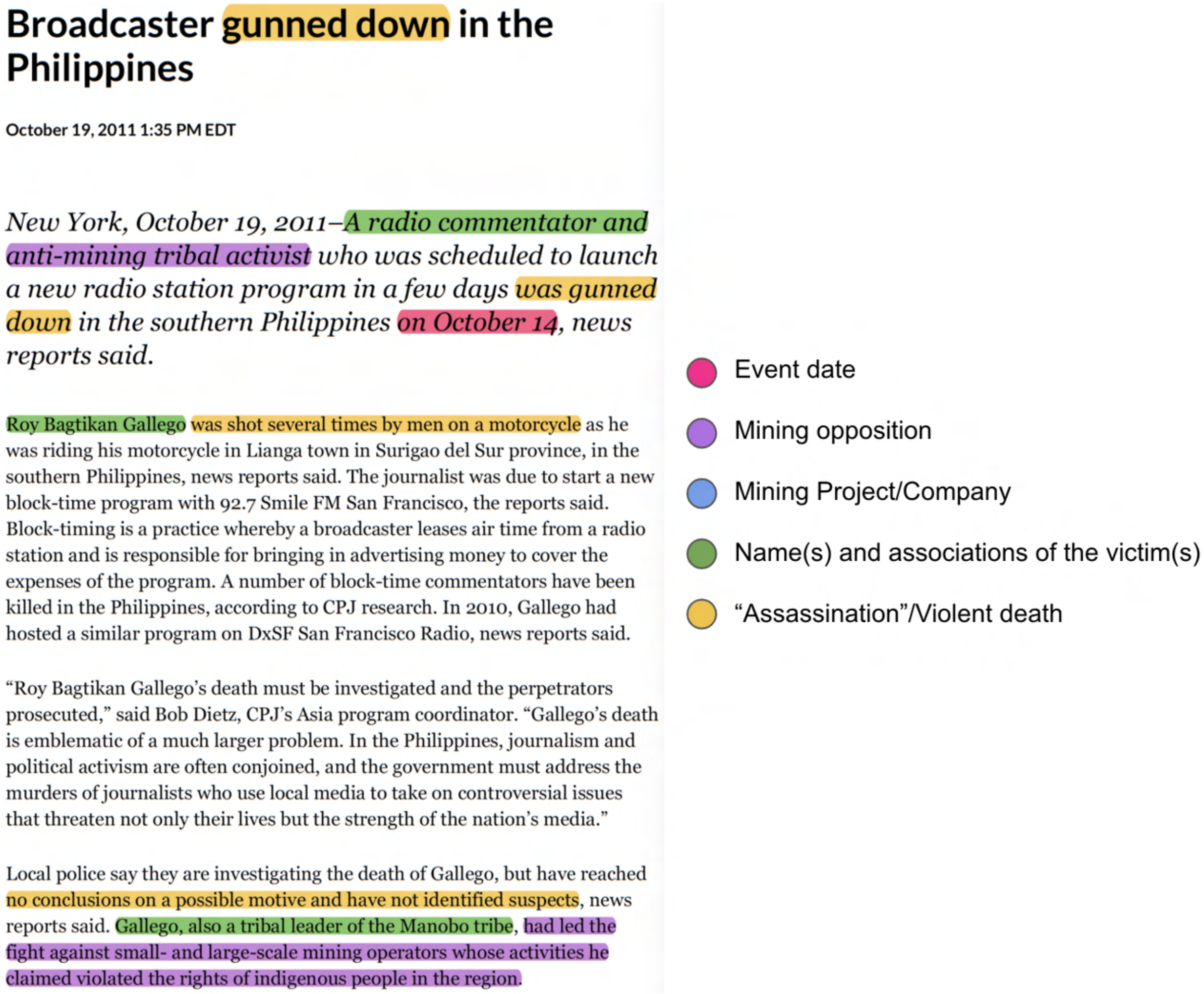
Apart from the company information, we manually code the following information: *(i)* the precise event date, *(ii)* the name and number of the victims, *(iii)* the geolocation of the assassination event, *(iv)* the event "circumstances" (e.g., whether an assassination attempt was successful, or if it happened during a protest), and *(v)* if known, the perpetrator (e.g., police, paramilitary forces, private security guards, hitmen). For most assassination events, we are able to establish the exact assassination location. If the location is not known precisely, but only at the municipality level, we use the (approximate) centroid of the municipality where the event occurred.

B.1.4 Coverage Of Assassination Dataset

The unique scope and coverage of our data set are highlighted by Figure B.2, which juxtaposes the coverage period of event countries in our study (1998-2019) to a high-quality benchmark dataset, the ACLED project. The ACLED data is "the highest-quality and most widely used near real-time source on political violence and protest data worldwide."[7] The dark cells in Figure B.2 show that ACLED covers only a fraction of

[7]Source: www.acleddata.com/data/, 27 May 2024.

Figure B.1: Example Case: Mining Opposition without Company Associations



Broadcaster gunned down in the Philippines

October 19, 2011 1:35 PM EDT

New York, October 19, 2011–A radio commentator and anti-mining tribal activist who was scheduled to launch a new radio station program in a few days was gunned down in the southern Philippines on October 14, news reports said.

Roy Bagtikan Gallego was shot several times by men on a motorcycle as he was riding his motorcycle in Lianga town in Surigao del Sur province, in the southern Philippines, news reports said. The journalist was due to start a new block-time program with 92.7 Smile FM San Francisco, the reports said. Block-timing is a practice whereby a broadcaster leases air time from a radio station and is responsible for bringing in advertising money to cover the expenses of the program. A number of block-time commentators have been killed in the Philippines, according to CPJ research. In 2010, Gallego had hosted a similar program on DxSF San Francisco Radio, news reports said.

"Roy Bagtikan Gallego's death must be investigated and the perpetrators prosecuted," said Bob Dietz, CPJ's Asia program coordinator. "Gallego's death is emblematic of a much larger problem. In the Philippines, journalism and political activism are often conjoined, and the government must address the murders of journalists who use local media to take on controversial issues that threaten not only their lives but the strength of the nation's media."

Local police say they are investigating the death of Gallego, but have reached no conclusions on a possible motive and have not identified suspects, news reports said. Gallego, also a tribal leader of the Manobo tribe, had led the fight against small- and large-scale mining operators whose activities he claimed violated the rights of indigenous people in the region.

Notes: The source article can be found here: https://cpj.org/2011/10/broadcaster-gunned-down-in-philippines/

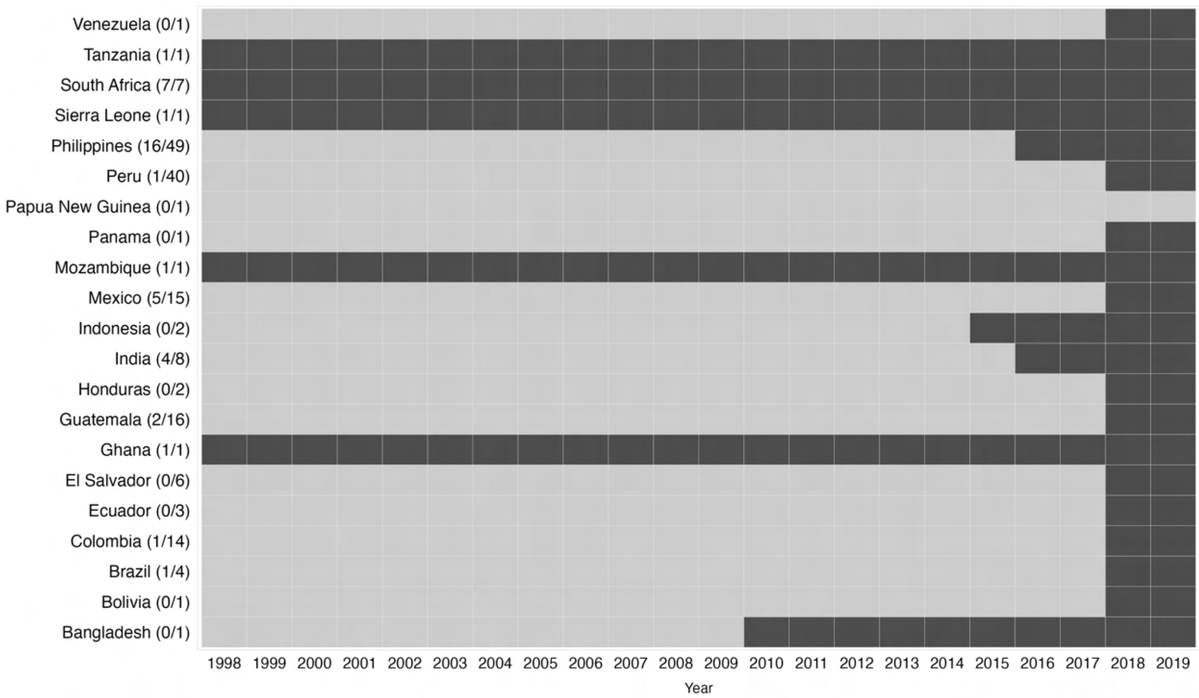
the study period for our event countries: only 41 of the 175 assassinations associated with publicly traded companies in our dataset fall into the period covered by ACLED. Moreover, we find that, at most, 29 of the 41 events are actually featured in ACLED.[8]

Although the scope of ACLED's conflict coverage includes assassination events, the temporal coverage is limited for some key regions in our data, such as South America and Southeast Asia: coverage starts in 2018 for Peru and 2016 for the Philippines. Importantly, for countries where ACLED has the most extensive coverage, such as Africa, our assassination data overlap nicely with ACLED's coverage (Ghana, Mozambique, Sierra Leone, South Africa, and Tanzania). The overlap may make sense, as many

[8] In detail, we are able to identify 23 events with certainty in ACLED, with 6 events as strong potential matches.

partner organizations working with ACLED, such as Frontline Defenders, are employed as sources of human rights reporting in our data collection effort.

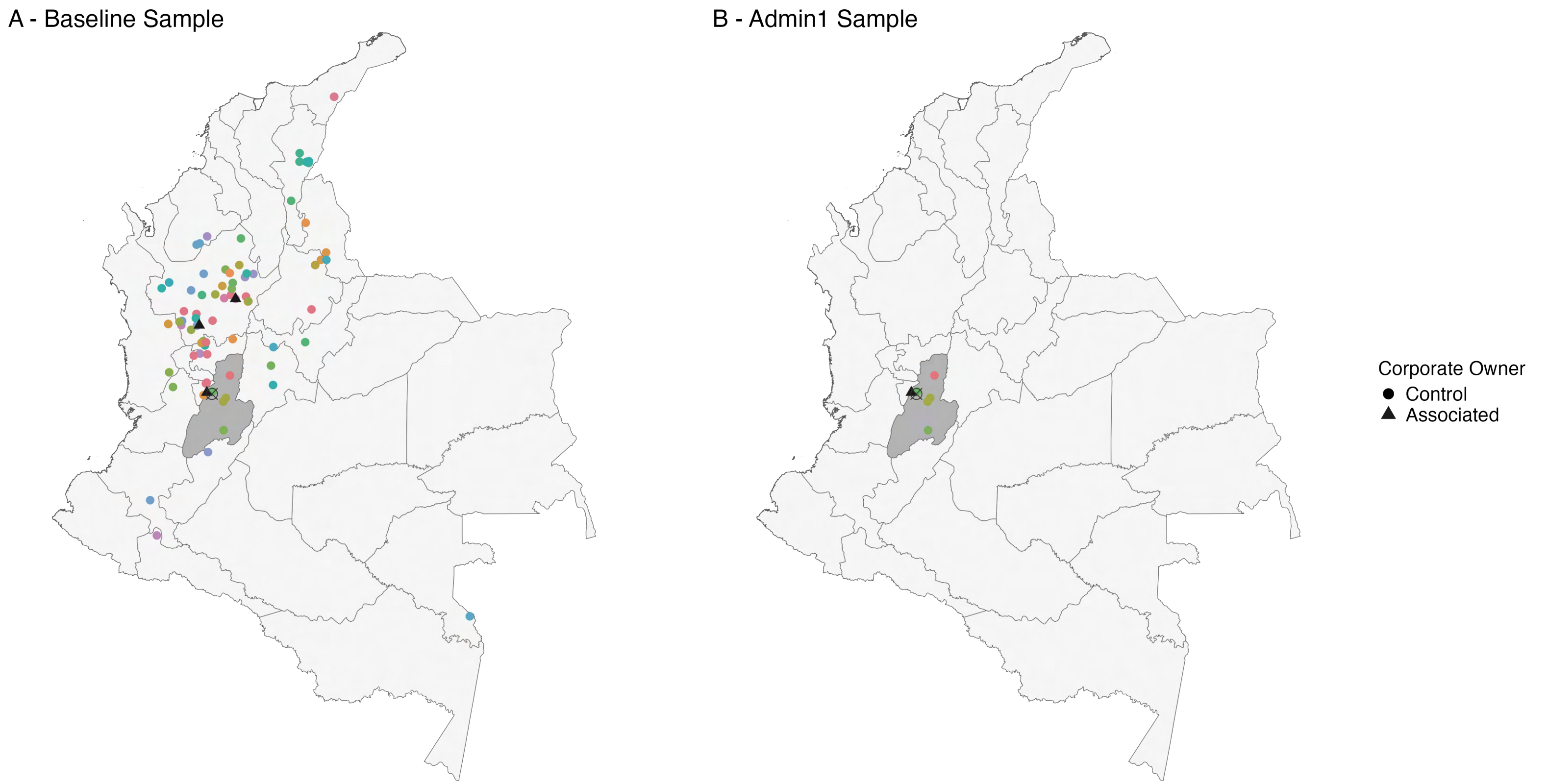
Figure B.2: ACLED Event-Country-Specific Data Coverage Periods



Notes: The number of assassinations associated with publicly traded companies in a country that falls into the ACLED coverage period relative to the total number of assassinations is presented in parentheses after each event-country name. Country-specific coverage-period information for ACLED was obtained from the official documentation at https://acleddata.com/knowledge-base/country-time-period-coverage/ (last accessed 26 May 2024).

B.2 Construction of OLS Control Group

Figure B.3: An Example Case from Colombia



Notes: The map displays the Admin1 regions of mainland Colombia. The dark gray area corresponds to the Admin1 region in which the assassination event took place, while the location itself is marked by the black circled cross. Triangles (dots) correspond to mining projects owned by companies linked to the assassination event (or not), with the corporate owner(s) differentiated by color. Panel A displays all mining projects in the SNL database with ownership information in the event year (here: 2013). Panel B restricts the mining projects to the ones present in the Admin1 region of the assassination location.

B.3 ESG Scores Dyck et al. (2019)

We follow Dyck et al. (2019) to create "equally weighted" indicator variables based on the ASSET4 ESG environmental and social indicator values. In particular, for questions with a positive direction (i.e., a "yes" answer or a higher number is associated with better social performance), we translate the answers to Y/N questions into 0 (N) and 1 (Y); the answers to double Y/N questions into 0 (NN), 0.5 (YN or NY), and 1 (YY); and the answers to numerical questions into 0 (value is less or equal than zero; or value is less than or equal to the median).[9] For questions with a negative direction (i.e., a "no" answer or a lower number is associated with better social performance), the opposite coding applies.[10]

Table B.1: Social Indicator Variables

		Description	Direction	Question Type	Translation Numeric Values
A.	Community Category				
1)	Bribery, Corruption, Fraud Controversies (so_so_co_o10_v)	Is the company in the media spotlight because of a controversy linked to bribery and corruption, political contributions, improper lobbying, money laundering, parallel imports, or any tax fraud?	Negative	Y/N	

[9]Column "Translation Numeric Values" in Table B.1 provides detailed information on the translation of each numerical question.

[10]Note that, in contrast to Dyck et al. (2019), we do not consider the indicator "Total Donations" (so_so_co_o01_v) due to almost exclusively missing values and rely on the "Effective Tax Rate" indicator instead of "Income Taxes", since the latter was not available in our version of the Asset4 database (data last retrieved on 26 September 2021).

Table B.1: Social Indicator Variables *(Continued)*

		Description	Direction	Question Type	Translation Numeric Values
2)	Business Ethics Compliance (so_so_co_o11_v)	All real or estimated penalties, fines from lost court cases, settlements or cases not yet settled regarding controversies linked to business ethics in general, political contributions or bribery and corruption, price-fixing or anti-competitive behavior, tax fraud, parallel imports or money laundering in U.S. dollars.	Negative	Number	Zero
3)	Corporate Responsibility Awards (so_so_co_dp074)	Has the company received an award for its social, ethical, community, or environmental activities or performance?	Positive	Y/N	
4)	Crisis Management (so_so_co_o08_v)	Does the company report on crisis management systems or reputation disaster recovery plans to reduce or minimize the effects of reputational disasters?	Positive	Y/N	

Table B.1: Social Indicator Variables *(Continued)*

		Description	Direction	Question Type	Translation Numeric Values
5)	Critical Countries, Indigenous People Controversies (so_so_co_-o06_v)	Is the company in the media spotlight because of a controversy linked to activities in critical, undemocratic countries that do not respect fundamental human rights or to disrespecting the rights of Indigenous people?	Negative	Y/N	
6)	Donations in General (so_-so_co_o02_v)	Does the company make cash donations? AND Does the company make in-kind donations, foster employee engagement in voluntary work, or fund community-related projects through a corporate foundation?	Positive	Double Y/N	
7)	Implementation (so_so_co_-d02_v)	Does the company describe the implementation of its community policy through a public commitment from a senior management or board member? AND Does the company describe the implementation of its community policy through the processes in place?	Positive	Double Y/N	
8)	Improvements (so_so_co_-d04_v)	Does the company set specific objectives to be achieved regarding its reputation or its relations with communities?	Positive	Double Y/N	

Table B.1: Social Indicator Variables *(Continued)*

		Description	Direction	Question Type	Translation Numeric Values
9)	Effective Tax Rate (so_so_co_o03_v)	The effective tax rate is defined as income taxes (credit) divided by income before taxes and expressed as a percentage. If there is an income tax credit, the result is a not meaningful (NM)	Positive	Number	Median
10)	Monitoring (so_so_co_d03_v)	Does the company monitor its reputation or its relations with communities?	Positive	Y/N	
11)	Patent Infringement (so_so_co_o07_v)	All real or estimated penalties, fines from lost court cases, settlements or cases not yet settled regarding controversies linked to patents and intellectual property infringement in U.S. dollars.	Negative	Number	Zero
12)	Policy (so_so_co_d01_v)	Does the company have a policy to strive to be a good corporate citizen or endorse the Global Sullivan Principles? AND Does the company have a policy to respect business ethics, or has it signed the UN Global Compact, or does it follow the OECD guidelines?	Positive	Double Y/N	

Table B.1: Social Indicator Variables *(Continued)*

		Description	Direction	Question Type	Translation Numeric Values
13)	Public Health Controversies (`so_so_co_o09_v`)	Is the company in the media spotlight because of a controversy linked to public health or industrial accidents harming the health & safety of third parties (non-employees and non-customers)?	Positive	Y/N	
B.	Human Rights				
1)	Child Labor Controversies (`so_so_hr_o03_v`)	Is the company in the direct or indirect (through suppliers) media spotlight because of a controversy linked to child labor?	Negative	Y/N	
2)	Freedom of Association Controversies (`so_so_hr_o02_v`)	Is the company in the direct or indirect (through suppliers) media spotlight because of a controversy linked to freedom of association?	Negative	Y/N	
3)	Human Rights Controversies (`so_so_hr_o04_v`)	Is the company in the direct or indirect (through suppliers) media spotlight because of a controversy linked to general human rights issues?	Negative	Y/N	
4)	Implementation (`so_so_hr_d02_v`)	Does the company describe the implementation of its human rights policy?	Positive	Y/N	
5)	Improvements (`so_so_hr_d04_v`)	Does the company set specific objectives to be achieved in its human rights policy?	Positive	Y/N	

Table B.1: Social Indicator Variables *(Continued)*

		Description	Direction	Question Type	Translation Numeric Values
6)	Monitoring (`so_so_hr_dp021`)	Does the company monitor human rights in its or its suppliers' facilities?	Positive	Y/N	
7)	Policy (`so_so_hr_d01_v`)	Does the company have a policy to guarantee the freedom of association universally and independent of local laws? AND Does the company have a policy for the exclusion of child, forced, or compulsory labor?	Positive	Double Y/N	
8)	Suppliers Social Impact (`so_so_hr_dp026` AND `so_so_hr_dp029`)	Does the company report or is it shown to use human rights criteria in the process of selecting or monitoring its suppliers or sourcing partners? AND Does the company report, or is it shown to be ready to end a partnership with a sourcing partner if human rights criteria are not met?	Positive	Double Y/N	

B.4 Protest Data

i. MM. The Mass Mobilization Data Project (MM) (Clark, David and Regan, Patrick, 2016) provides information on the start and end dates of protests and protesters' demands. We filter out, to the best of our abilities, protests that may be connected to the assassination event in our database by restricting the set of protests to those that are motivated by at least one of the following four issue categories:

- **Police brutality or arbitrary actions**: The beating or jailing of people for seemingly arbitrary reasons, the brutality by police or other authority figures against a group or individual
- **Political behavior/process**: Aspects of the political process that determines who rules and how, who can participate in elections or decisions, choices made by leaders that influence a range of political outcomes from domestic subsidies to foreign policy
- **Removal of corrupt or reviled political person**: Official corruption or the corruption of a particular individual
- **Land tenure or farm issues**: Access to or restrictions imposed on the use of land (e.g. expropriation of land for a dam project)

Since location information in MM is only available in an unstructured string format, with entries varying between city, district, and state, among others, we use the GeoNames API combined with manual matching to assign each protest to the ADMIN1 (ADM1) polygons used in our study. Protests which cannot be assigned to an ADMIN1 region are dropped from the analysis.

ii. GDELT. Using the CAMEO code classification of events in the Global Database of Events, Language, and Tone (GDELT) (Schrodt et al., 2009), we consider the following event codes matching the socio-environmental conflicts surrounding mining projects:

- **14**: Protests
- **1121**: Accuse of crime, corruption
- **1122**: Accuse of human rights abuses
- **113**: Rally opposition against
- **181**: Abduct, hijack, or take hostage
- **1822**: Torture
- **1823**: Kill by physical assault
- **185**: Attempt to assassinate
- **186**: Assassinate

We further filter the relevant protests by restricting the set of *primary* generic domestic role codes to

- **COP**: Police forces, officers, criminal investigative units, protective agencies
- **OPP**: Political opposition: opposition parties, individuals, anti-government activists
- **GOV**: Government: the executive, governing parties, coalitions partners, executive divisions
- **JUD**: Judiciary: judges, courts
- **MIL**: Military: troops, soldiers, all state-military personnel/equipment

and the *secondary* generic domestic role codes to

- **AGR**—Agriculture: individuals and groups involved in the practices of crop cultivation, including government agencies whose primary concern is agricultural issues
- **CVL**—Civilian individuals or groups sometimes used as catch-all for individuals or groups for whom no other role category is appropriate
- **ENV**—Environmental: entities for whom environmental and ecological issues are their primary focus, includes wildlife preservation, climate change, etc
- **HRI**—Human Rights: actors for whom their primary area of operation or expertise is with documenting and/or correcting human rights concerns
- **CRM**—Criminal: corresponding to individuals involved in or allegedly involved in the deliberate breaking of state or international laws primarily for profit
- **DEV**—Development: individuals or groups concerned primarily with development issues of varying types, including infrastructure creation and democratization.

Additionally, we require that the geographic location of an event is known at least at the ADMIN1 level.

C Additional Results

C.1 Descriptive Statistics

Figure C.1: Distribution of Assassination Events over Time.

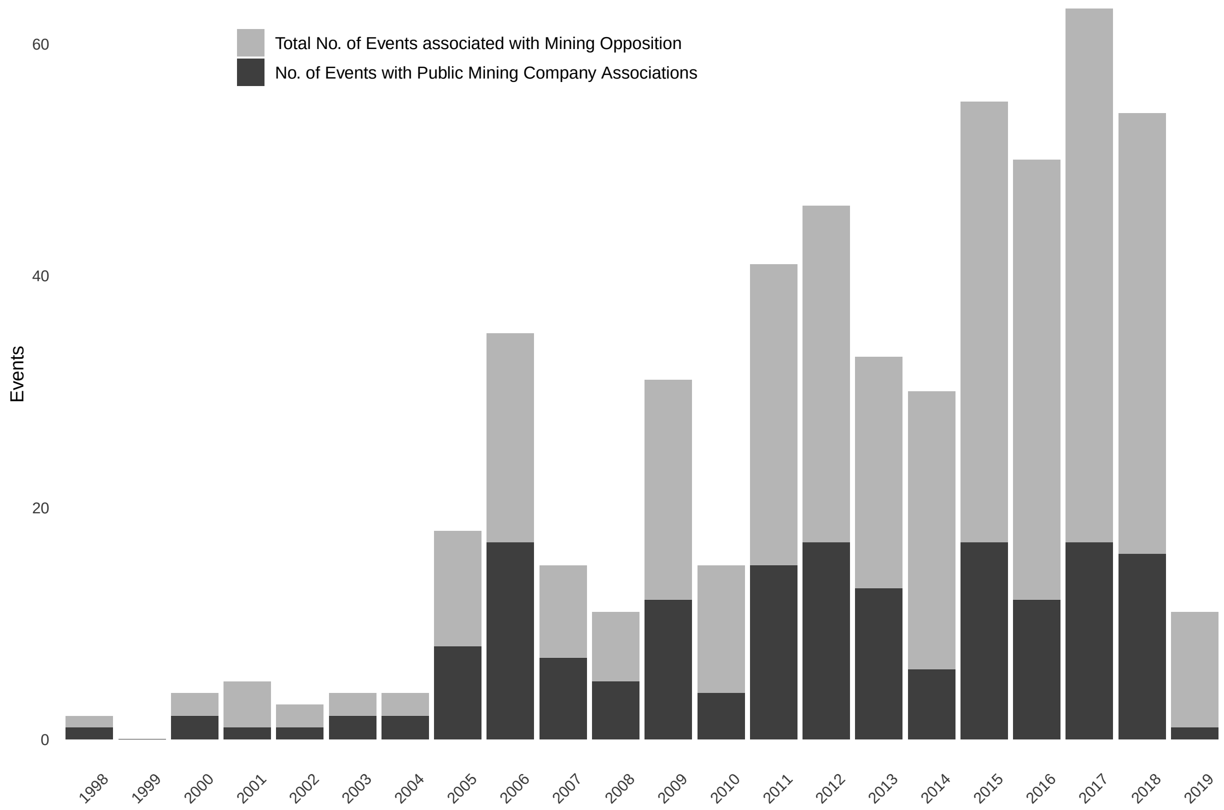
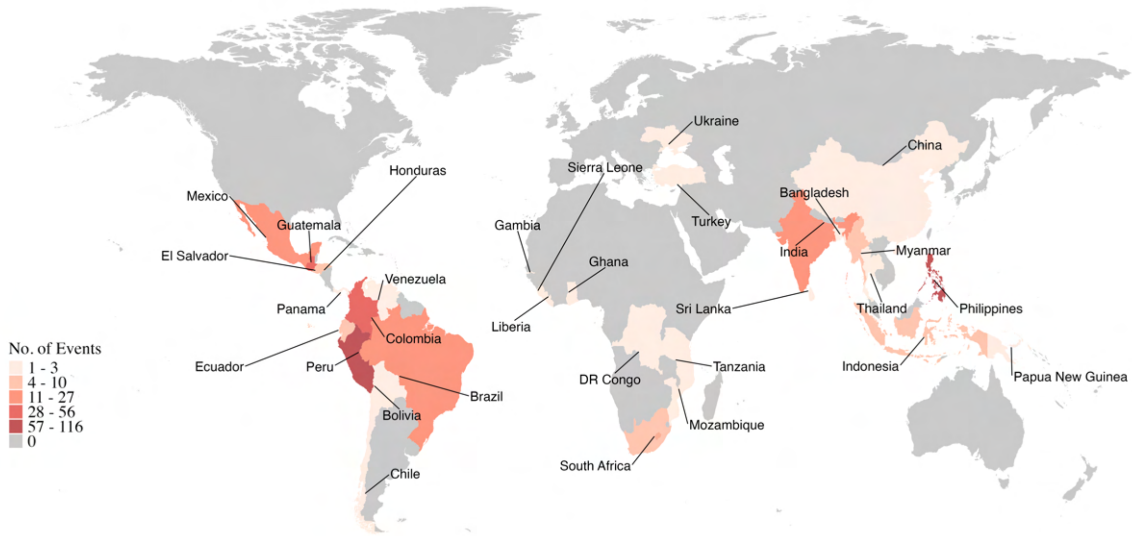


Figure C.2: The Spatial Distribution of Assassinations (1998-2019)

(a) Number of Events by Country



(b) Number of Victims by Country

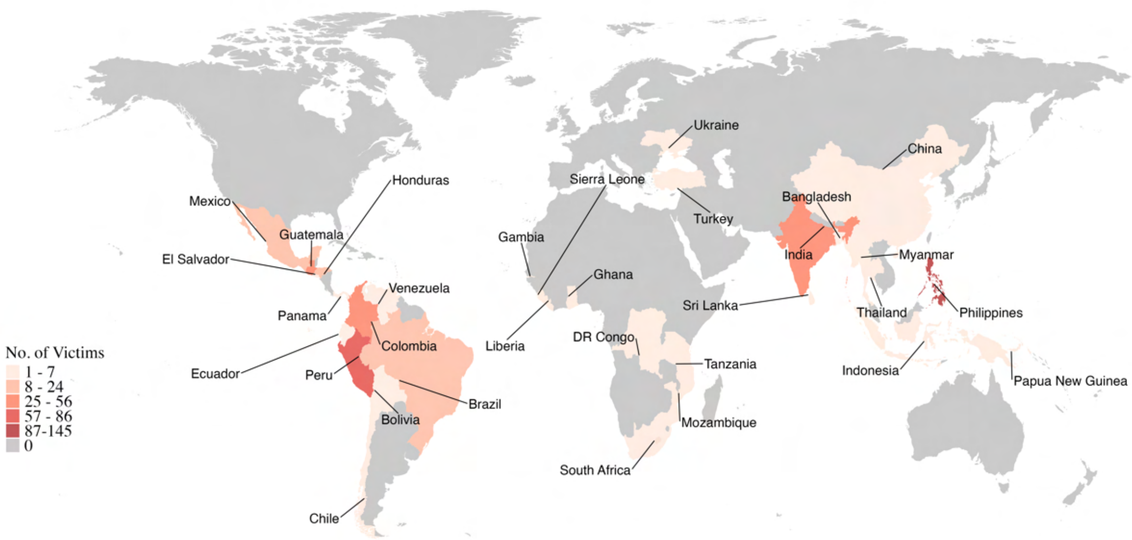
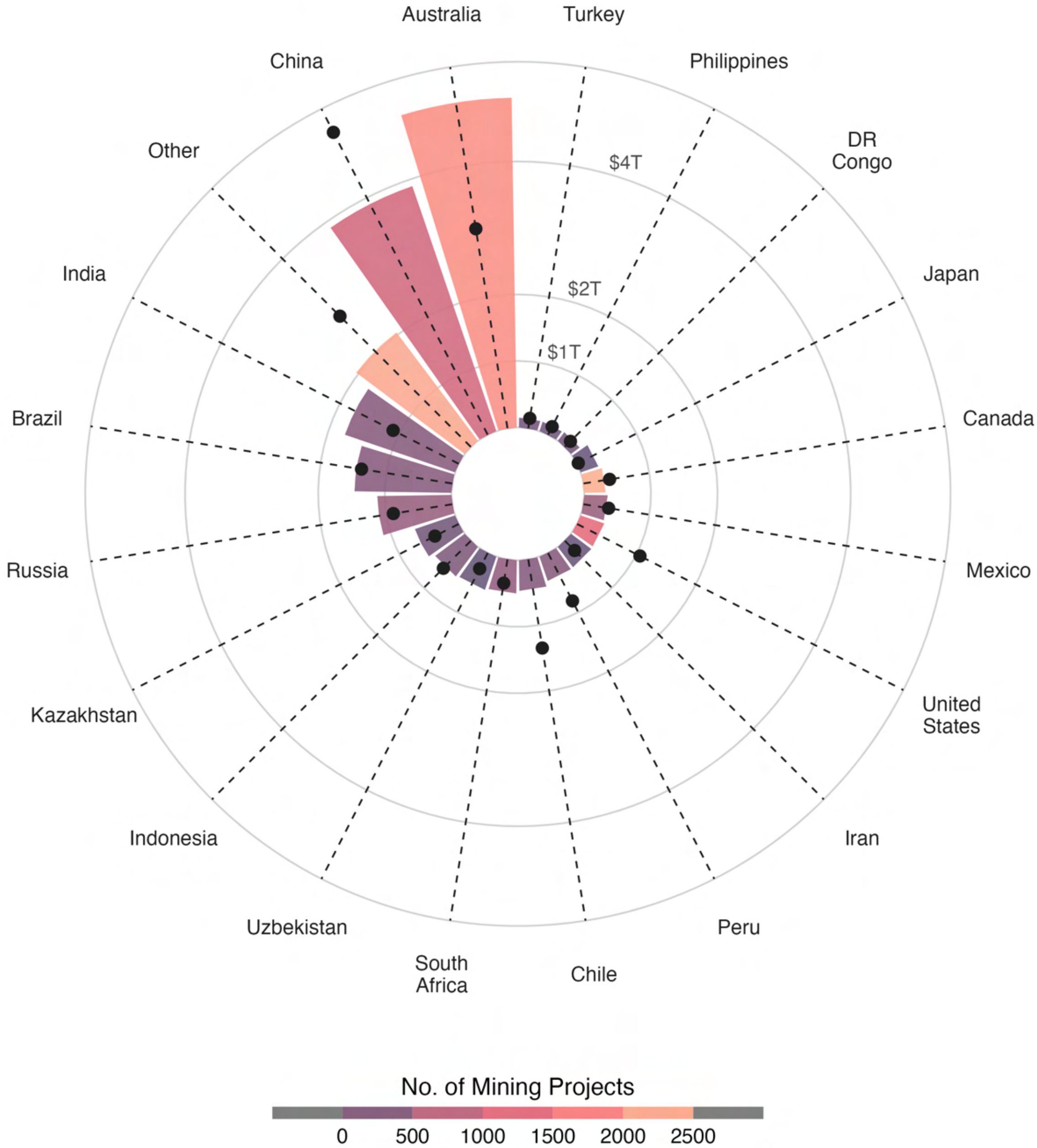


Figure C.3: Mineral Rents across Countries



Notes: Bars present mineral rents (in trillion USD) for particular countries in 2019 in which corporations are headquartered. Black dots depict the average mineral rent (in constant 2015 USD) for a country over the 1998-2019 period. The color scale indicates the number of active mining projects as of the end of 2018 in the SNL Metals and Mining database.

Figure C.4: Market Value of Mining Corporations Across Countries

Notes: Bars present the total market value in 2019 of mining corporations (in billion USD) headquartered in a particular country. Black dots depict the average market value of mining corporations over the 1998-2019 period. The color scale indicates the number of publicly traded mining countries headquartered in a country in 2019.

Table C.1: Summary of Financial Data – by Event Country

Event Country		R (%)			$\widehat{AR}$ (%)			Log(Size)			Leverage (%)		
		N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Bangladesh	Treatment	1	-0.09		1	0.01		1	11.3		1	0.00	
	Control	0			0			0			0		
Brazil	Treatment	4	0.08	0.27	4	0.00	0.02	4	17.6	0.98	4	0.17	0.08
	Control	198	0.08	0.33	198	0.00	0.02	193	13.9	3.4	175	19.2	18.3
Colombia	Treatment	14	0.01	0.28	14	-0.01	0.03	14	15.5	2.70	14	0.26	0.13
	Control	254	0.04	0.37	254	0.00	0.08	245	12.9	4.0	220	15.1	21.8
Ecuador	Treatment	5	0.06	0.13	5	-0.01	0.01	5	16.0	2.65	5	0.35	0.24
	Control	24	0.12	0.41	24	-0.01	0.04	22	12.1	3.4	22	11.3	18.4
El Salvador	Treatment	4	0.56	0.45	4	0.02	0.05	4	8.9	0.11	4	0.00	0.00
	Control	7	0.76	0.29	7	-0.01	0.02	3	9.2	0.0	3	0.0	0.0
Ghana	Treatment	1	0.05		1	0.01		1	16.4		1	0.14	
	Control	8	0.26	0.29	8	0.01	0.02	8	12.0	3.0	8	6.9	8.4
Guatemala	Treatment	17	0.01	0.21	17	0.00	0.01	15	14.7	2.13	15	0.06	0.05
	Control	43	0.08	0.25	43	0.00	0.01	43	14.8	3.8	40	13.0	12.0
Honduras	Treatment	1	0.09		1	0.03		1	16.9		1	0.13	
	Control	6	0.68	0.92	6	0.00	0.03	5	13.6	3.6	5	63.5	95.0
India	Treatment	8	0.02	0.12	8	0.00	0.01	8	16.7	1.36	8	0.29	0.23
	Control	105	0.08	0.30	105	0.00	0.02	105	14.7	2.7	97	27.4	25.5
Indonesia	Treatment	2	0.39	0.13	2	0.00	0.00	2	17.6	0.74	2	0.24	0.00
	Control	27	0.47	0.30	27	0.00	0.02	26	14.4	2.8	26	26.9	20.5
Mexico	Treatment	12	0.12	0.18	12	0.00	0.01	12	13.3	1.70	12	0.12	0.15
	Control	1168	0.12	0.32	1168	0.00	0.03	1097	11.7	3.0	937	14.0	37.6
Mozambique	Treatment	1	0.35		1	-0.01		1	18.4		1	0.24	
	Control	16	0.20	0.24	16	-0.01	0.04	16	13.4	4.1	12	21.6	18.9
Panama	Treatment	2	-0.17	0.04	2	0.00	0.00	2	16.9	1.76	2	0.39	0.24
	Control	1	-0.30		1	0.02		1	5.9		0		
Papua New Guinea	Treatment	1	-0.26		1	0.00		1	17.5		1	0.70	
	Control	14	0.32	1.59	14	0.01	0.02	13	14.1	3.5	13	20.0	20.3
Peru	Treatment	49	0.07	0.17	49	0.00	0.01	48	16.0	2.17	48	0.23	0.19
	Control	1460	0.12	0.32	1460	0.00	0.02	1403	13.9	3.3	1342	22.8	92.4
Philippines	Treatment	39	0.19	0.57	39	0.00	0.01	37	15.0	3.22	36	0.31	0.21
	Control	966	0.11	0.58	966	0.00	0.03	947	12.3	2.6	883	17.9	20.3
Sierra Leone	Treatment	1	0.15		1	0.00		1	15.0		1	0.18	
	Control	3	-0.34	0.77	3	0.00	0.01	3	12.2	4.2	3	18.2	27.2
South Africa	Treatment	6	-0.08	0.31	6	0.01	0.01	6	14.7	1.17	6	0.13	0.06
	Control	364	0.19	0.42	364	0.00	0.02	355	13.1	3.0	325	21.5	21.9
Tanzania	Treatment	2	-0.25	0.25	2	-0.01	0.02	2	15.6	1.93	2	0.16	0.16
	Control	26	-0.05	0.57	26	0.00	0.02	25	10.5	2.8	11	23.3	20.6
Venezuela	Treatment	1	-0.10		1	0.00		1	18.0		1	0.26	
	Control	2	-0.07	0.07	2	-0.01	0.01	2	12.2	1.5	2	19.3	17.9

Notes: Raw (R) and abnormal ($\widehat{AR}$) returns for each security are previously averaged over the *estimation* window from $\tau = -280$ to $\tau = -30$. Firm characterisitcs - i.e. size, leverage, and profitability - are based on the values in the event year.

Table C.2: Summary of Financial Data – by HQ Country

HQ Country		R (%)			$\widehat{AR}$ (%)			Log(Size)			Leverage (%)		
		N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Australia	Treatment	11	0.14	0.33	11	0.00	0.01	11	15	3.85	11	0.34	0.21
	Control	388	0.13	0.47	388	0.00	0.03	363	12	3.68	297	40.1	193.4
Belgium	Treatment	0			0			0			0		
	Control	24	-0.03	0.17	24	0.00	0.02	24	15	0.17	24	31.9	8.8
Brazil	Treatment	3	0.12	0.37	3	0.00	0.02	3	18	0.06	3	0.24	0.00
	Control	66	0.03	0.24	66	0.00	0.01	66	17	1.87	63	32.5	12.5
Canada	Treatment	53	0.18	0.51	53	0.00	0.02	49	13	2.82	48	0.10	0.14
	Control	2024	0.15	0.48	2024	0.00	0.04	1895	11	2.70	1653	10.4	26.0
China	Treatment	8	0.05	0.21	8	0.00	0.01	8	17	1.37	8	0.36	0.19
	Control	70	0.08	0.20	70	0.00	0.01	70	16	0.54	70	34.0	13.8
Colombia	Treatment	0			0			0			0		
	Control	3	-0.05	0.10	3	0.00	0.00	3	13	0.31	3	11.8	10.3
Finland	Treatment	0			0			0			0		
	Control	5	0.27	0.19	5	0.00	0.01	5	13	0.01	5	4.0	1.4
Hong Kong	Treatment	6	0.04	0.10	6	0.00	0.01	6	19	2.26	6	0.51	0.22
	Control	50	0.07	0.18	50	0.00	0.01	50	15	2.25	50	37.5	29.1
India	Treatment	4	0.04	0.06	4	0.00	0.01	4	16	1.46	4	0.18	0.20
	Control	111	0.07	0.20	111	0.00	0.02	111	15	2.20	107	32.2	25.5
Indonesia	Treatment	0			0			0			0		
	Control	6	0.47	0.10	6	-0.01	0.01	6	14	1.08	6	23.2	20.8
Japan	Treatment	2	0.15	0.19	2	0.00	0.00	2	15	0.15	2	0.15	0.18
	Control	230	0.06	0.10	230	0.00	0.01	230	17	1.17	230	40.9	10.1
Luxembourg	Treatment	2	0.15	0.07	2	0.00	0.01	2	16	0.18	2	0.30	0.01
	Control	10	0.01	0.19	10	0.01	0.01	10	16	0.14	10	19.9	3.6
Malaysia	Treatment	0			0			0			0		
	Control	27	0.02	0.14	27	0.00	0.01	27	12	0.25	27	47.3	5.8
Mexico	Treatment	6	0.03	0.16	6	0.00	0.01	5	17	0.19	5	0.31	0.06
	Control	54	0.05	0.22	54	0.00	0.01	54	15	1.35	54	25.8	12.6
Netherlands	Treatment	0			0			0			0		
	Control	33	0.08	0.21	33	0.00	0.01	33	18	1.52	33	20.5	4.6
Norway	Treatment	2	0.15	0.06	2	-0.01	0.01	2	17	0.04	2	0.10	0.01
	Control	37	0.04	0.36	37	0.00	0.02	37	11	2.59	37	3.2	9.4
Peru	Treatment	10	0.10	0.20	10	0.00	0.01	10	15	0.54	10	0.05	0.05
	Control	134	0.13	0.26	134	0.00	0.01	134	14	1.25	134	15.3	11.9
Philippines	Treatment	14	0.07	0.22	14	0.00	0.00	14	16	2.07	14	0.41	0.20
	Control	404	0.05	0.28	404	0.00	0.01	404	12	1.83	369	14.3	15.4
Russian Federation	Treatment	0			0			0			0		
	Control	8	0.07	0.22	8	0.00	0.00	6	17	0.31	6	48.2	12.8
Singapore	Treatment	0			0			0			0		
	Control	9	1.76	1.00	9	0.05	0.17	9	11	0.25	9	34.3	5.3
South Africa	Treatment	10	-0.04	0.26	10	0.00	0.01	10	16	0.18	10	0.26	0.15
	Control	213	0.08	0.33	213	0.00	0.02	209	15	1.46	208	19.5	12.8
South Korea	Treatment	0			0			0			0		
	Control	24	0.10	0.15	24	0.00	0.01	24	16	1.06	24	9.6	9.5
Thailand	Treatment	0			0			0			0		
	Control	2	0.39	0.03	2	-0.01	0.00	2	13	2.23	2	20.5	12.8
United Kingdom	Treatment	24	-0.02	0.27	24	0.00	0.01	24	16	2.95	24	0.21	0.16
	Control	455	0.11	0.42	455	0.00	0.02	448	14	3.97	421	16.8	14.5
United States	Treatment	16	0.07	0.16	16	0.00	0.01	16	16	0.70	16	0.26	0.14
	Control	305	0.11	0.35	305	0.00	0.02	292	14	3.11	282	23.1	45.4

Notes: Raw (R) and abnormal ($\widehat{AR}$) returns for each security are previously averaged over the *estimation* window from $\tau = -280$ to $\tau = -30$. Firm characterisitcs - i.e. size, leverage, and profitability - are based on the values in the event year.

Table C.3: Summary of Financial Data – by Listing Country

		R (%)			$\widehat{AR}$ (%)			Log(Size)			Leverage (%)		
Listing Country		N	Mean	SD	N	Mean	SD	N	Mean	SD	N	Mean	SD
Australia	Treatment	12	0.14	0.32	12	0.00	0.01	12	15	3.69	12	0.34	0.20
	Control	381	0.12	0.47	381	0.00	0.03	356	12	3.71	292	40.8	195.0
Belgium	Treatment	0			0			0			0		
	Control	24	-0.03	0.17	24	0.00	0.02	24	15	0.17	24	31.9	8.8
Brazil	Treatment	3	0.12	0.37	3	0.00	0.02	3	18	0.06	3	0.24	0.00
	Control	66	0.03	0.24	66	0.00	0.01	66	17	1.87	63	32.5	12.5
Canada	Treatment	38	0.27	0.57	38	0.00	0.02	36	12	2.22	35	0.07	0.10
	Control	1859	0.16	0.50	1859	0.00	0.04	1727	11	2.51	1485	10.0	27.0
China	Treatment	8	0.05	0.21	8	0.00	0.01	8	17	1.37	8	0.36	0.19
	Control	12	0.10	0.22	12	0.00	0.01	12	16	0.83	12	43.2	12.4
Colombia	Treatment	0			0			0			0		
	Control	3	-0.05	0.10	3	0.00	0.00	3	13	0.31	3	11.8	10.3
Finland	Treatment	0			0			0			0		
	Control	5	0.27	0.19	5	0.00	0.01	5	13	0.01	5	4.0	1.4
Hong Kong	Treatment	6	0.04	0.10	6	0.00	0.01	6	19	2.26	6	0.51	0.22
	Control	108	0.07	0.19	108	0.00	0.01	108	16	1.71	108	34.6	22.1
India	Treatment	4	0.04	0.06	4	0.00	0.01	4	16	1.46	4	0.18	0.20
	Control	111	0.07	0.20	111	0.00	0.02	111	15	2.20	107	32.2	25.5
Indonesia	Treatment	0			0			0			0		
	Control	6	0.47	0.10	6	-0.01	0.01	6	14	1.08	6	23.2	20.8
Japan	Treatment	2	0.15	0.19	2	0.00	0.00	2	15	0.15	2	0.15	0.18
	Control	230	0.06	0.10	230	0.00	0.01	230	17	1.17	230	40.9	10.1
Malaysia	Treatment	0			0			0			0		
	Control	27	0.02	0.14	27	0.00	0.01	27	12	0.25	27	47.3	5.8
Mexico	Treatment	6	0.03	0.16	6	0.00	0.01	5	17	0.19	5	0.31	0.06
	Control	54	0.05	0.22	54	0.00	0.01	54	15	1.35	54	25.8	12.6
Netherlands	Treatment	0			0			0			0		
	Control	33	0.08	0.21	33	0.00	0.01	33	18	1.52	33	20.5	4.6
Norway	Treatment	3	-0.09	0.32	3	-0.02	0.00	1	11		1	0.00	
	Control	37	0.04	0.36	37	0.00	0.02	37	11	2.59	37	3.2	9.4
Peru	Treatment	0			0			0			0		
	Control	115	0.14	0.27	115	0.00	0.01	115	14	1.22	115	16.6	12.2
Philippines	Treatment	14	0.07	0.22	14	0.00	0.00	14	16	2.07	14	0.41	0.20
	Control	404	0.05	0.28	404	0.00	0.01	404	12	1.83	369	14.3	15.4
Singapore	Treatment	0			0			0			0		
	Control	9	1.76	1.00	9	0.05	0.17	9	11	0.25	9	34.3	5.3
South Africa	Treatment	3	0.15	0.28	3	0.01	0.02	3	16	0.20	3	0.10	0.07
	Control	206	0.09	0.33	206	0.00	0.02	202	15	1.39	201	19.7	12.5
South Korea	Treatment	0			0			0			0		
	Control	24	0.10	0.15	24	0.00	0.01	24	16	1.06	24	9.6	9.5
Thailand	Treatment	0			0			0			0		
	Control	2	0.39	0.03	2	-0.01	0.00	2	13	2.23	2	20.5	12.8
United Kingdom	Treatment	24	-0.02	0.27	24	0.00	0.01	24	16	2.95	24	0.21	0.16
	Control	459	0.11	0.41	459	0.00	0.02	455	14	3.96	426	16.6	14.5
United States	Treatment	48	0.03	0.18	48	0.00	0.01	48	16	0.95	48	0.21	0.16
	Control	517	0.09	0.30	517	0.00	0.02	502	14	2.64	492	19.3	35.8

Notes: Raw (R) and abnormal ($\widehat{AR}$) returns for each security are previously averaged over the *estimation* window from $\tau = -280$ to $\tau = -30$. Firm characterisitcs - i.e. size, leverage, and profitability - are based on the values in the event year.

C.2 The Impact of Assassination on Firms

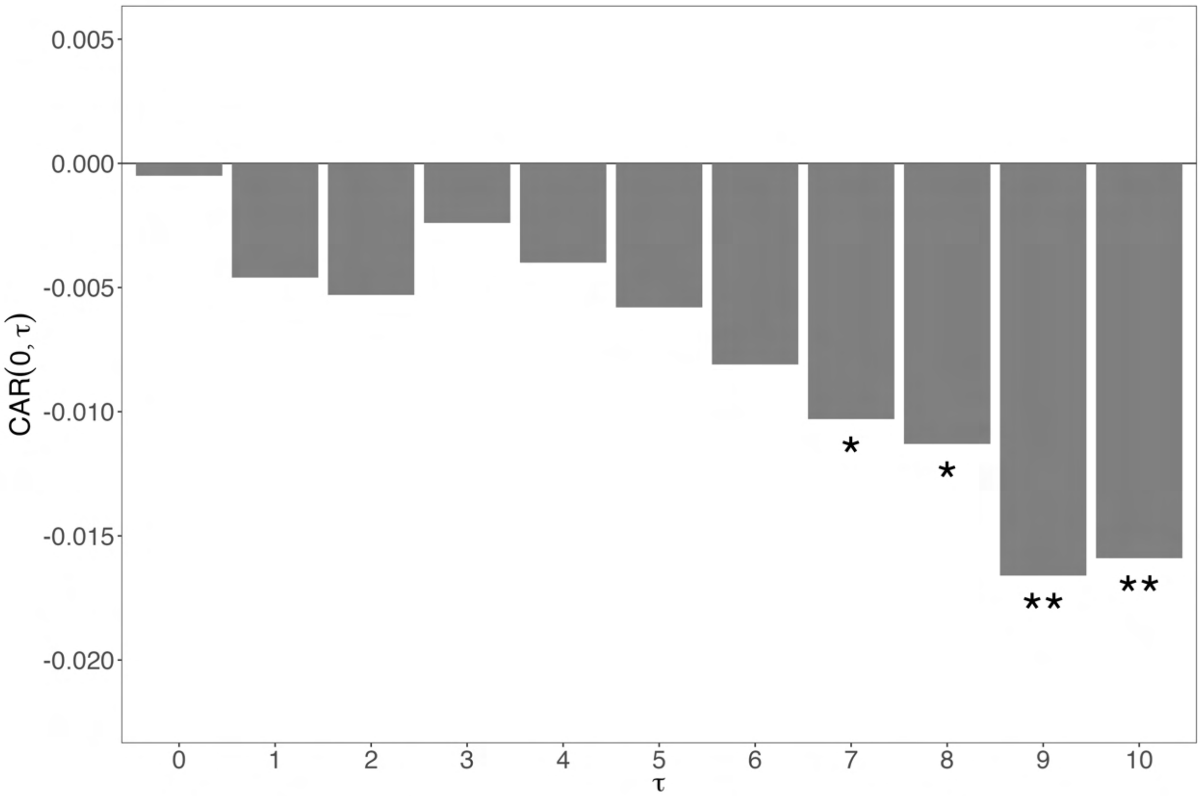
C.2.1 Traditional Event Study

Table C.4: The Effect of Assassinations on Stock Returns

			p-value			
	Mean	SD	t-test	BMP	ADJ-BMP	GRANK
$CAR\,(0,0)$	-0.0009	0.0034	0.789	0.406	0.432	0.700
$CAR\,(0,1)$	-0.0066	0.0047	0.161	0.067	0.083	0.217
$CAR\,(0,2)$	-0.0074	0.0058	0.202	0.102	0.122	0.140
$CAR\,(0,3)$	-0.0040	0.0067	0.547	0.186	0.211	0.048
$CAR\,(0,4)$	-0.0061	0.0075	0.415	0.119	0.141	0.037
$CAR\,(0,5)$	-0.0078	0.0082	0.344	0.137	0.160	0.061
$CAR\,(0,6)$	-0.0104	0.0087	0.233	0.064	0.080	0.033
$CAR\,(0,7)$	-0.0132	0.0094	0.160	0.032	0.043	0.016
$CAR\,(0,8)$	-0.0148	0.0099	0.135	0.027	0.037	0.011
$CAR\,(0,9)$	-0.0201	0.0105	0.055	0.013	0.019	0.001
$CAR\,(0,10)$	-0.0200	0.0110	0.070	0.023	0.031	0.004

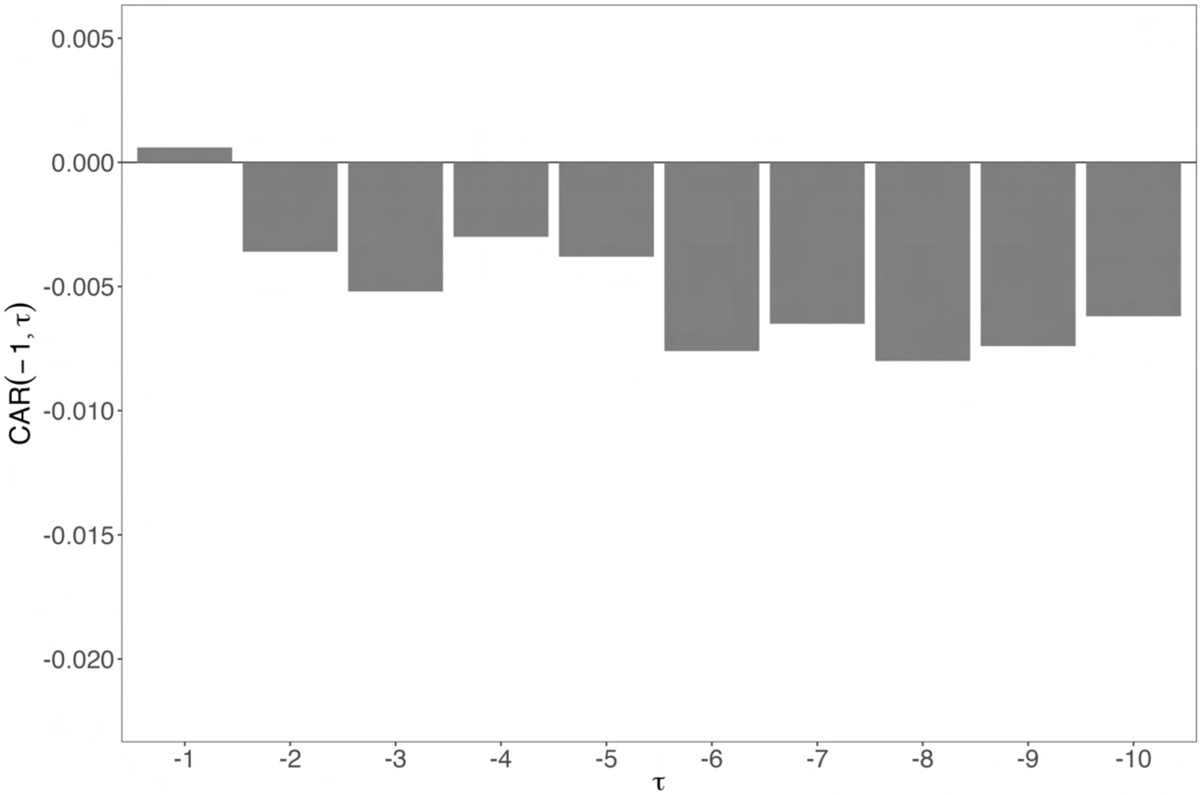
Notes: The number of company-event pairs N is 167. The respective average cumulative abnormal return (CAR) and its standard deviation (SD) is presented in columns 1 and 2. A minimum of 8 trading days during the *event window* from 0 to 10 is required. *The estimation window* spans from day -280 to -30 with a minimum of 200 trading days. Columns 3 - 6 show the p-value of the respective test-statistic. For details on the applied test-statistics see Appendix A.

Figure C.5: Fama-French 4-Factor Model



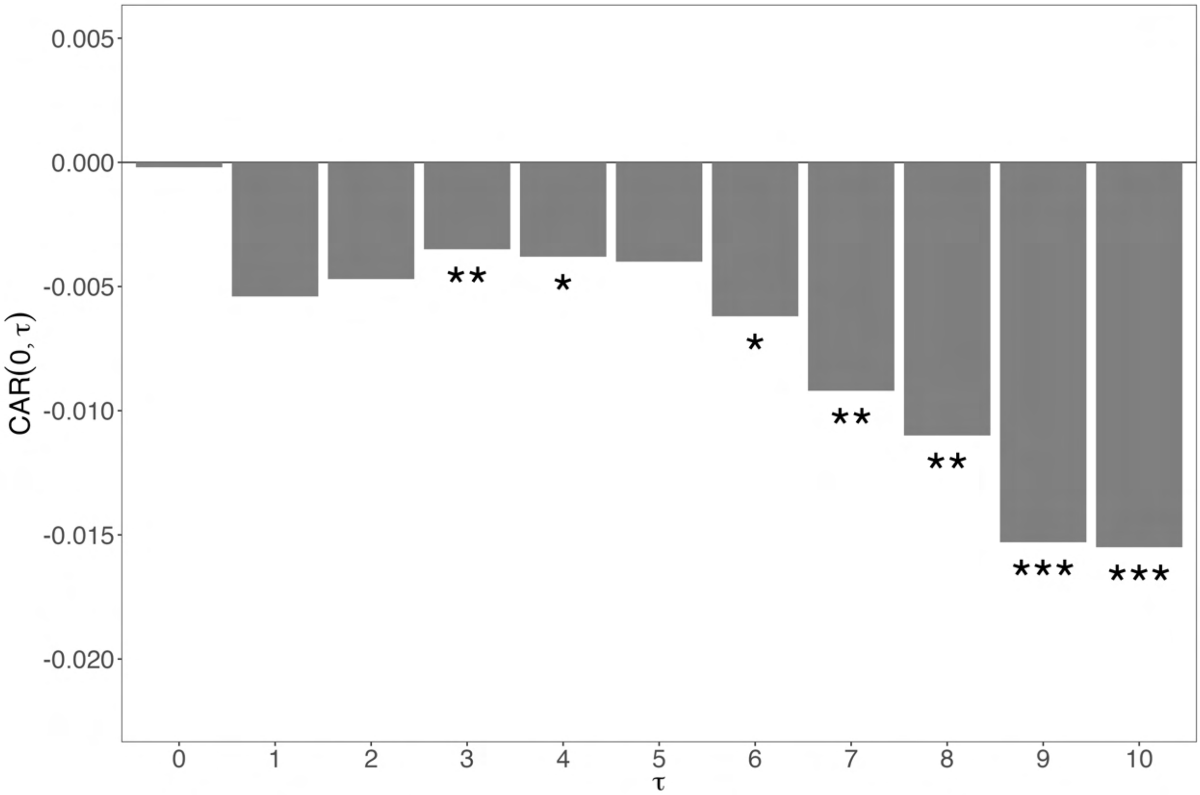
Notes: The average cumulative abnormal returns (CARs) for the Fama-French 4-Factor Model are presented. The number of company-event pairs, N, is 167. A minimum of 8 trading days is required during the event window from 0 to 10. The estimation window spans from Days -280 to Day -30 with a minimum of 200 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.

Figure C.6: Private Information and Pre-Trends



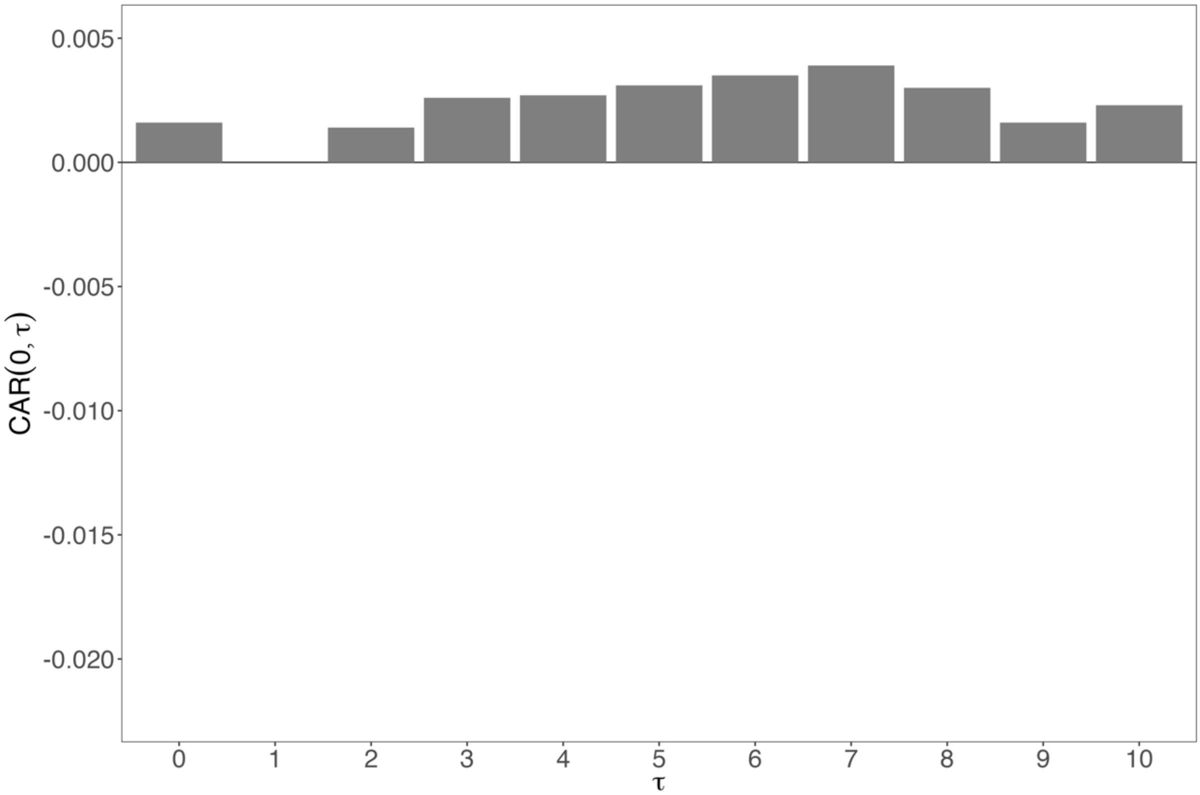
Notes: The average cumulative abnormal returns (CARs) for the baseline market model are presented. The number of company-event pairs, N, is 170. A minimum of 8 trading days during the event window from Days -1 to -10 is required. The estimation window spans from Day -280 to Day -30 with a minimum of 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.

Figure C.7: Alternative Threshold for Trading Frequency



Notes: The average cumulative abnormal return (CAR) for the baseline market model is presented. The number of company-event pairs, N, is 160. Stocks have to be traded on each trading day during the event window from 0 to 10 and on at least 225 days during the estimation window spanning from Day -280 to Day -30. Stars depict significance levels for the GRANK (Kolari and Pynnönen, 2011) test-statistic: *p<0.1, ** p<0.05, *** p<0.01

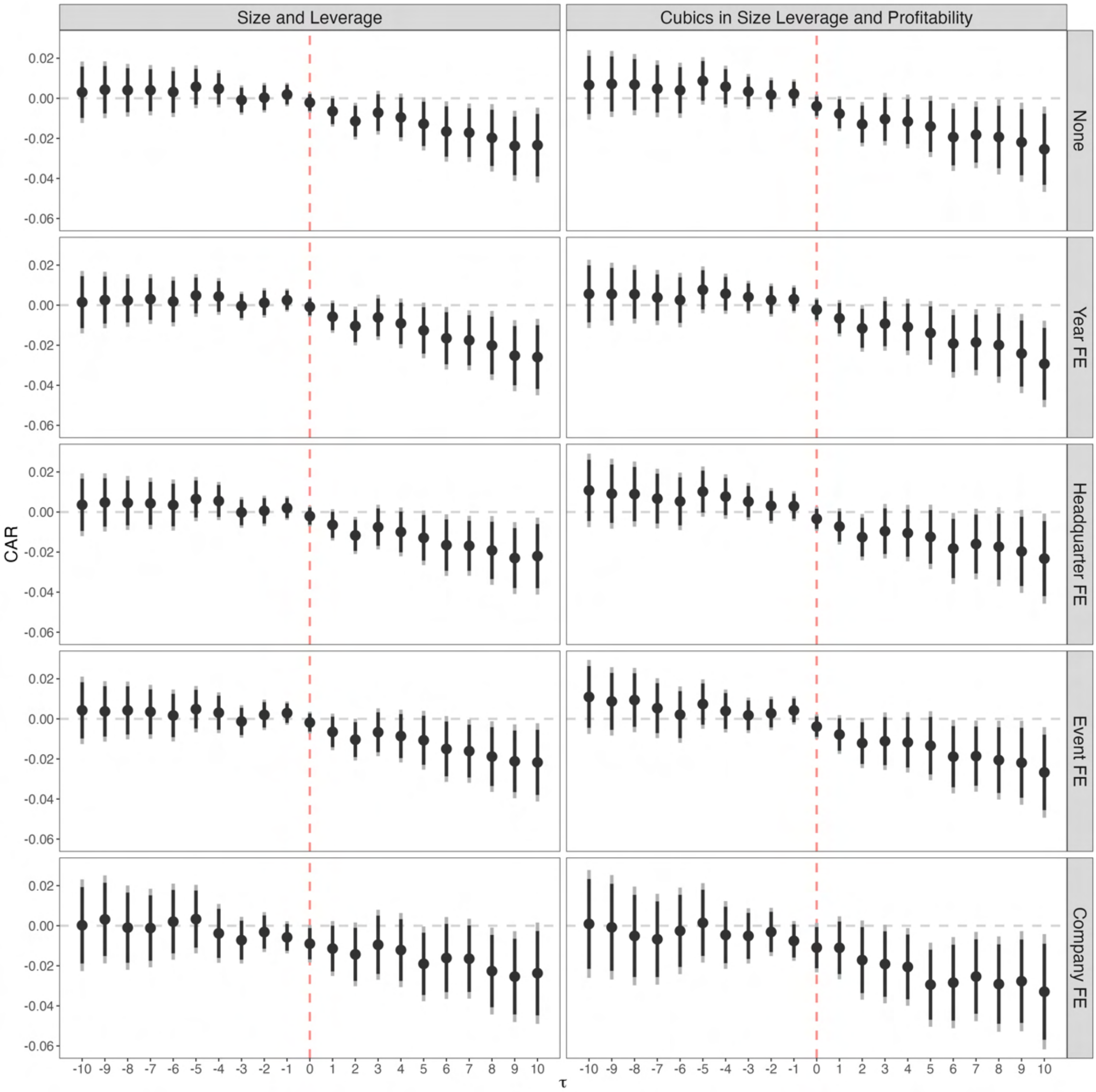
Figure C.8: Placebo Effect of Assassinations on Control Companies



Notes: The average cumulative abnormal return (CAR) for the baseline market model is presented. The number of company-event pairs, N, is 4527. A minimum of 8 trading days during the event window from Day -1 to Day -10 is required. 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

C.2.2 Regression-Based Event Study

Figure C.9: Robustness – Fixed Effects and Firm Characteristic



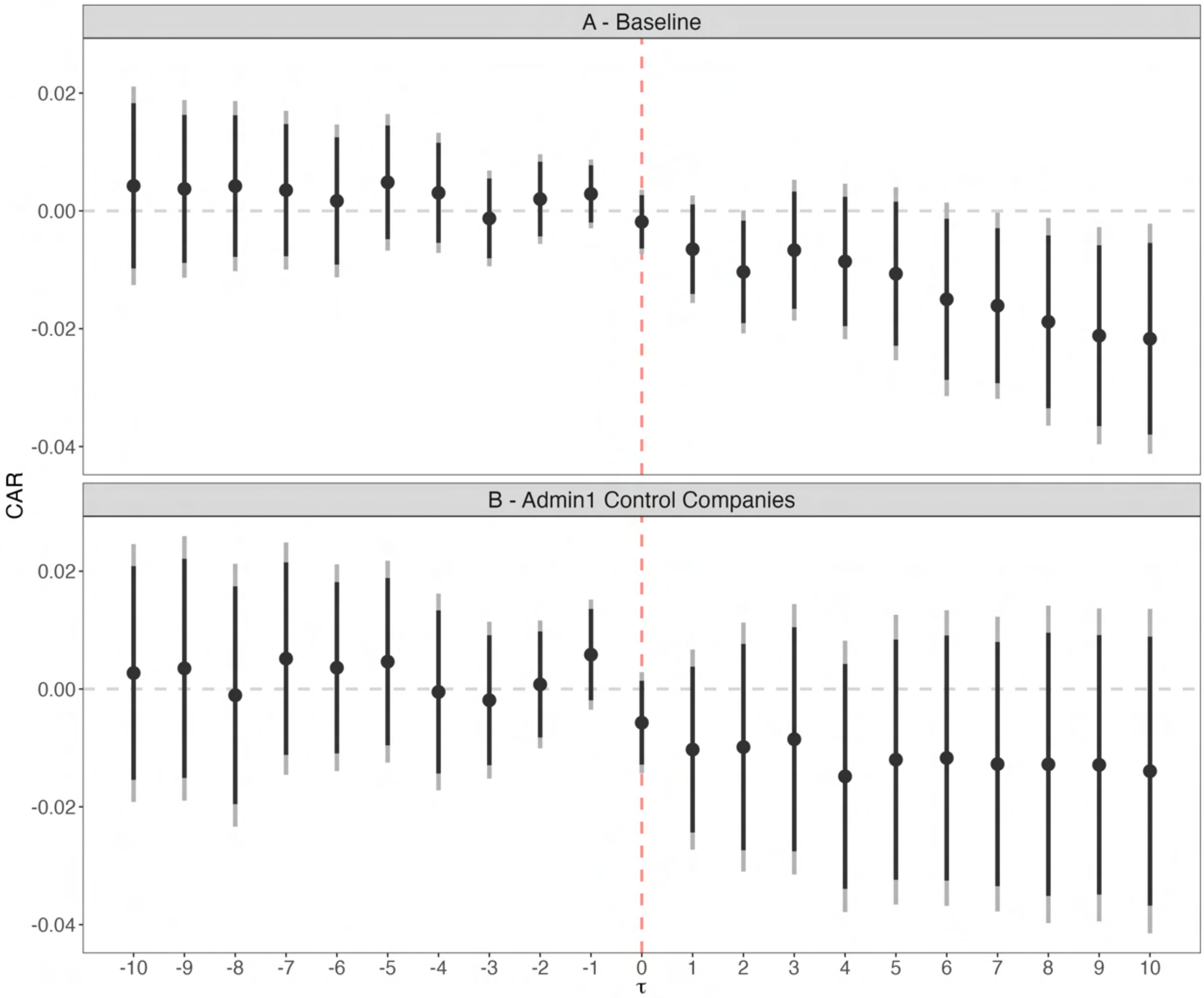
Notes: The coefficients when regressing the cumulative abnormal returns (CARs) on an indicator for being tied to an assassination event are represented by black dots. The horizontal axis represents the trading days before and after the event on $\tau = 0$. CARs are aggregated backward before the event date and forward starting with the event date (e.g., -5 refers to the CAR between -1 and -5 while 5 refers to the CAR between 0 and 5). Each cell corresponds to a different regression specification, with columns capturing control variable definitions and rows reflecting the inclusion of various fixed effects. In total, the coefficients of 210 regressions are displayed; 90% and 95% confidence intervals using robust standard errors clustered at the event level are depicted in black and gray, respectively.

Table C.5: Robustness – Sample Definitions

	Excl. Attempts					Excl. Protests					Winsorized				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
$CAR\,(0,0)$	-0.0024	-0.0024	-0.0012	-0.0018	-0.0093*	-0.0034	-0.0036	-0.0014	-0.0015	-0.0094	-0.0004	-0.0005	0.0007	0.0005	-0.0039
	(0.0027)	(0.0028)	(0.0028)	(0.0029)	(0.0053)	(0.0032)	(0.0032)	(0.0031)	(0.0031)	(0.0067)	(0.0023)	(0.0024)	(0.0023)	(0.0025)	(0.0032)
$CAR\,(0,1)$	-0.0068	-0.0068	-0.0062	-0.0064	-0.0123	-0.0091**	-0.0093**	-0.0074**	-0.0083**	-0.0096	-0.0046*	-0.0046*	-0.0037	-0.0045	-0.0048
	(0.0042)	(0.0043)	(0.0044)	(0.0050)	(0.0079)	(0.0036)	(0.0036)	(0.0036)	(0.0037)	(0.0073)	(0.0027)	(0.0027)	(0.0027)	(0.0029)	(0.0043)
$CAR\,(0,2)$	-0.0109**	-0.0111**	-0.0101*	-0.0095*	-0.0142	-0.0126**	-0.0129**	-0.0105*	-0.0097*	-0.0156*	-0.0082**	-0.0086**	-0.0072*	-0.0076*	-0.0054
	(0.0050)	(0.0051)	(0.0052)	(0.0057)	(0.0091)	(0.0053)	(0.0055)	(0.0053)	(0.0054)	(0.0093)	(0.0039)	(0.0039)	(0.0040)	(0.0041)	(0.0049)
$CAR\,(0,3)$	-0.0063	-0.0067	-0.0054	-0.0054	-0.0070	-0.0069	-0.0070	-0.0039	-0.0048	-0.0081	-0.0087**	-0.0092**	-0.0078*	-0.0083**	-0.0054
	(0.0060)	(0.0061)	(0.0062)	(0.0065)	(0.0098)	(0.0071)	(0.0073)	(0.0070)	(0.0070)	(0.0101)	(0.0040)	(0.0040)	(0.0041)	(0.0041)	(0.0057)
$CAR\,(0,4)$	-0.0096	-0.0100	-0.0088	-0.0076	-0.0106	-0.0062	-0.0061	-0.0030	-0.0028	-0.0061	-0.0102**	-0.0109**	-0.0097**	-0.0095*	-0.0102*
	(0.0065)	(0.0067)	(0.0067)	(0.0072)	(0.0103)	(0.0080)	(0.0082)	(0.0081)	(0.0082)	(0.0105)	(0.0046)	(0.0047)	(0.0047)	(0.0049)	(0.0058)
$CAR\,(0,5)$	-0.0131*	-0.0134*	-0.0125*	-0.0102	-0.0177*	-0.0125	-0.0123	-0.0092	-0.0076	-0.0173	-0.0141***	-0.0146***	-0.0140***	-0.0120**	-0.0132**
	(0.0072)	(0.0074)	(0.0075)	(0.0080)	(0.0104)	(0.0087)	(0.0090)	(0.0090)	(0.0091)	(0.0109)	(0.0052)	(0.0052)	(0.0053)	(0.0055)	(0.0062)
$CAR\,(0,6)$	-0.0184**	-0.0186**	-0.0177**	-0.0161*	-0.0162	-0.0160	-0.0160	-0.0124	-0.0122	-0.0132	-0.0172***	-0.0175***	-0.0175***	-0.0165**	-0.0118
	(0.0080)	(0.0082)	(0.0083)	(0.0088)	(0.0110)	(0.0098)	(0.0099)	(0.0099)	(0.0101)	(0.0116)	(0.0061)	(0.0062)	(0.0062)	(0.0064)	(0.0071)
$CAR\,(0,7)$	-0.0195**	-0.0196**	-0.0189**	-0.0172**	-0.0172	-0.0181*	-0.0179*	-0.0149	-0.0145	-0.0160	-0.0182***	-0.0183***	-0.0186***	-0.0176***	-0.0132*
	(0.0077)	(0.0079)	(0.0079)	(0.0085)	(0.0107)	(0.0096)	(0.0097)	(0.0095)	(0.0099)	(0.0117)	(0.0061)	(0.0062)	(0.0061)	(0.0064)	(0.0073)
$CAR\,(0,8)$	-0.0221**	-0.0220**	-0.0213**	-0.0203**	-0.0236**	-0.0161	-0.0164	-0.0122	-0.0116	-0.0213	-0.0202***	-0.0200***	-0.0206***	-0.0197***	-0.0172**
	(0.0090)	(0.0092)	(0.0092)	(0.0095)	(0.0117)	(0.0114)	(0.0115)	(0.0112)	(0.0114)	(0.0133)	(0.0069)	(0.0070)	(0.0070)	(0.0071)	(0.0083)
$CAR\,(0,9)$	-0.0261***	-0.0258***	-0.0263***	-0.0223**	-0.0276**	-0.0201	-0.0199	-0.0177	-0.0122	-0.0234	-0.0237***	-0.0235***	-0.0249***	-0.0232***	-0.0193**
	(0.0094)	(0.0097)	(0.0095)	(0.0100)	(0.0123)	(0.0122)	(0.0123)	(0.0120)	(0.0123)	(0.0143)	(0.0075)	(0.0077)	(0.0076)	(0.0079)	(0.0096)
$CAR\,(0,10)$	-0.0248**	-0.0239**	-0.0257**	-0.0216**	-0.0252*	-0.0202	-0.0191	-0.0192	-0.0136	-0.0216	-0.0201**	-0.0195**	-0.0222***	-0.0202**	-0.0089
	(0.0097)	(0.0100)	(0.0099)	(0.0104)	(0.0128)	(0.0125)	(0.0128)	(0.0125)	(0.0127)	(0.0152)	(0.0079)	(0.0080)	(0.0080)	(0.0084)	(0.0100)
Size and Leverage	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Headquarter FE		✓					✓					✓			
Year FE			✓					✓					✓		
Event FE				✓					✓					✓	
Company FE					✓					✓					✓
Observations	3877	3877	3875	3870	3799	2702	2702	2700	2697	2613	4107	4107	4105	4100	4020
Clusters	142	142	140	135	140	112	112	110	107	111	153	153	151	147	152

Notes: Robust standard errors are clustered on the event-level in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

Figure C.10: Robustness – Vicinity vs. Human Rights Spotlight



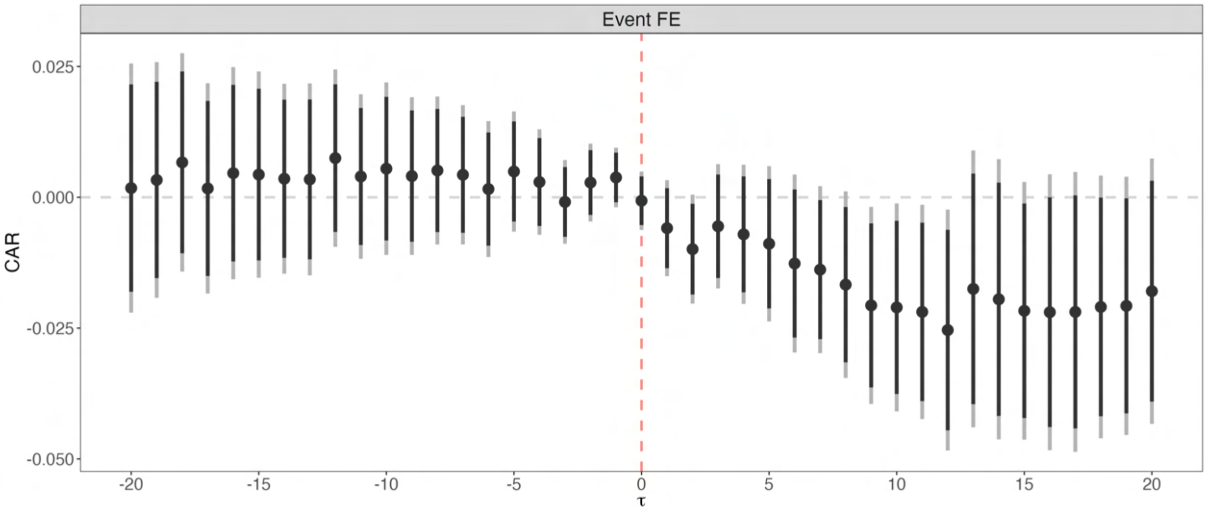
Notes: The black dots correspond to the coefficient estimates for being tied to an assassination in our event-fixed effect specification. Panel A presents the baseline sample estimates, while Panel B presents the results when restricting the control group to companies active in the Admin1 region of the assassination event; 90% and 95% confidence intervals using robust standard errors clustered at the event level are depicted in black and gray, respectively.

Table C.6: Robustness – Vicinity vs. Human Rights Spotlight across Different Specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$CAR\,(0,0)$	-0.0070	-0.0067	-0.0043	-0.0057	-0.0116	-0.0073	-0.0078	-0.0046	-0.0037	-0.0155
	(0.0046)	(0.0047)	(0.0039)	(0.0043)	(0.0076)	(0.0058)	(0.0067)	(0.0053)	(0.0055)	(0.0103)
$CAR\,(0,1)$	-0.0115	-0.0120	-0.0082	-0.0103	-0.0295*	-0.0071	-0.0092	-0.0029	-0.0031	-0.0275
	(0.0080)	(0.0086)	(0.0075)	(0.0085)	(0.0172)	(0.0101)	(0.0117)	(0.0103)	(0.0116)	(0.0214)
$CAR\,(0,2)$	-0.0132	-0.0134	-0.0090	-0.0099	-0.0192	-0.0054	-0.0078	0.0006	-0.0002	-0.0103
	(0.0098)	(0.0107)	(0.0096)	(0.0106)	(0.0179)	(0.0118)	(0.0138)	(0.0127)	(0.0132)	(0.0226)
$CAR\,(0,3)$	-0.0105	-0.0108	-0.0061	-0.0085	-0.0202	-0.0011	-0.0016	0.0059	0.0055	-0.0113
	(0.0108)	(0.0117)	(0.0100)	(0.0115)	(0.0185)	(0.0125)	(0.0144)	(0.0127)	(0.0143)	(0.0220)
$CAR\,(0,4)$	-0.0170	-0.0160	-0.0126	-0.0148	-0.0269	-0.0096	-0.0086	-0.0021	-0.0008	-0.0198
	(0.0108)	(0.0118)	(0.0103)	(0.0116)	(0.0189)	(0.0124)	(0.0143)	(0.0128)	(0.0144)	(0.0222)
$CAR\,(0,5)$	-0.0147	-0.0126	-0.0117	-0.0120	-0.0279	-0.0064	-0.0044	-0.0001	0.0065	-0.0241
	(0.0111)	(0.0122)	(0.0114)	(0.0124)	(0.0216)	(0.0120)	(0.0138)	(0.0128)	(0.0140)	(0.0251)
$CAR\,(0,6)$	-0.0163	-0.0151	-0.0130	-0.0117	-0.0180	-0.0092	-0.0080	-0.0037	0.0037	-0.0115
	(0.0117)	(0.0128)	(0.0121)	(0.0126)	(0.0221)	(0.0132)	(0.0148)	(0.0138)	(0.0146)	(0.0246)
$CAR\,(0,7)$	-0.0193*	-0.0162	-0.0164	-0.0127	-0.0233	-0.0105	-0.0059	-0.0044	0.0056	-0.0170
	(0.0112)	(0.0127)	(0.0119)	(0.0126)	(0.0233)	(0.0131)	(0.0147)	(0.0137)	(0.0139)	(0.0253)
$CAR\,(0,8)$	-0.0214*	-0.0187	-0.0177	-0.0128	-0.0302	-0.0140	-0.0107	-0.0078	0.0044	-0.0218
	(0.0125)	(0.0140)	(0.0131)	(0.0136)	(0.0254)	(0.0138)	(0.0157)	(0.0144)	(0.0145)	(0.0270)
$CAR\,(0,9)$	-0.0267**	-0.0207	-0.0235*	-0.0129	-0.0339	-0.0206	-0.0131	-0.0156	0.0010	-0.0263
	(0.0131)	(0.0148)	(0.0136)	(0.0134)	(0.0273)	(0.0154)	(0.0171)	(0.0161)	(0.0152)	(0.0290)
$CAR\,(0,10)$	-0.0281**	-0.0215	-0.0250*	-0.0139	-0.0308	-0.0195	-0.0103	-0.0145	0.0075	-0.0227
	(0.0136)	(0.0152)	(0.0143)	(0.0139)	(0.0275)	(0.0159)	(0.0174)	(0.0164)	(0.0152)	(0.0301)
Size and Leverage	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Profitability						✓	✓	✓	✓	✓
Cubic Terms						✓	✓	✓	✓	✓
Headquarter FE		✓					✓			
Year FE			✓					✓		
Event FE				✓					✓	
Company FE					✓					✓
Observations	676	675	676	673	605	658	657	658	653	586
Clusters	92	92	92	89	89	92	92	92	87	88

Notes: Robust standard errors are clustered on the event-level in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

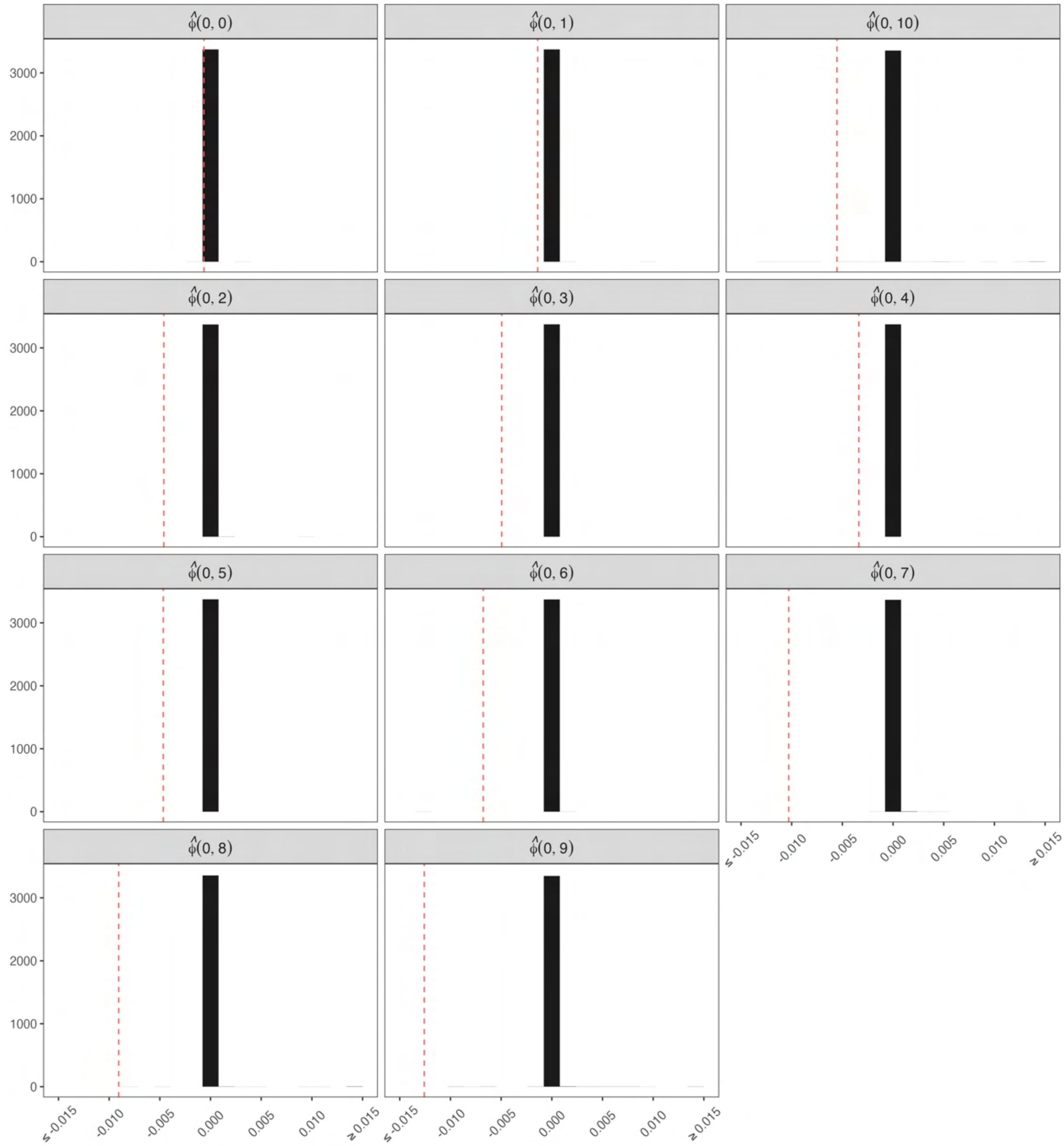
Figure C.11: Robustness – Wide Event Window +/- 20



Notes: The coefficients when regressing the cumulative abnormal returns (CARs) on an indicator for being tied to an assassination event is represented by black dots for our event-fixed effects specification. Each dot corresponds to a separate regression coefficient estimate. 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 (e.g., -5 refers to the CAR between -1 and -5 while 5 refers to the CAR between 0 and 5); 90% and 95% confidence intervals using robust standard errors clustered at the event level are depicted in black and gray respectively.

C.2.3 Synthetic Matching Estimation

Figure C.12: Distribution of Placebo Effects

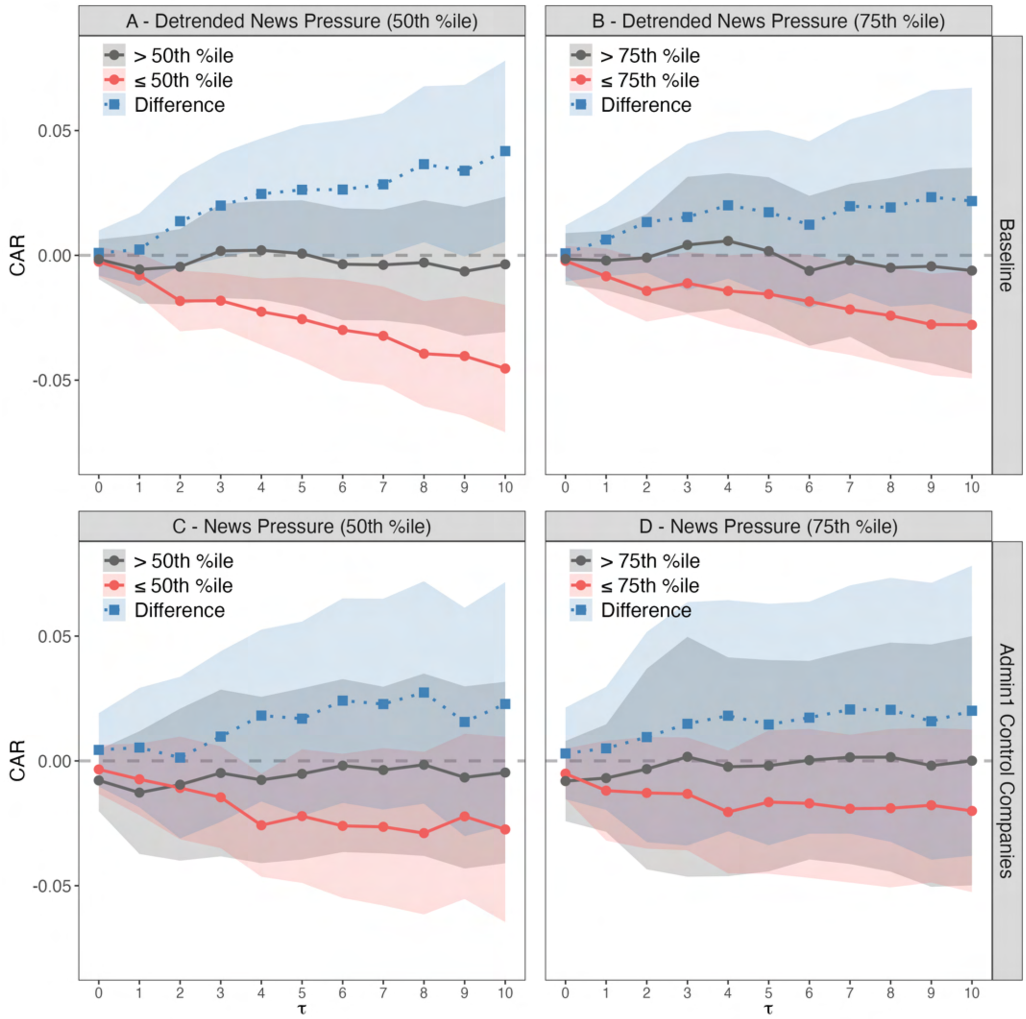


Notes: The red dashed line depicts the *actual* estimated effect of assassination events on returns using the modified synthetic matching method of Acemoglu et al. (2016) (for more details, please see Section A.2 in the Appendix). The distribution of the effects for the 3, 245 placebo treatment groups is presented in gray.

C.3 Mechanisms: The Media

C.3.1 Additional Robustness Checks

Figure C.13: Additional Robustness Checks



Notes: Red and black solid lines as well as dots denote the heterogeneous marginal treatment effects of assassination events on cumulative abnormal returns (CARs). The estimated (absolute) difference in treatment effects is represented by the dotted line with squares. The horizontal axis represents the trading days relative to the event day $\tau = 0$. In each panel, the treatment indicator D in our baseline specification in Equation 5 is interacted with a different binary indicator for a high level of news pressure on the event day: *(i)* above median Eisensee and Strömberg (2007) news pressure day (Panel A and C) and *(ii)* above the 75th percentile Eisensee and Strömberg (2007) news pressure day (Panel B and D). Panels A and B are estimated on the baseline sample using a "detrended" news pressure index; Panels C and D are estimated on the Admin1 control company sample using the standard news pressure index; 95% confidence intervals using robust standard errors clustered at the event level are displayed.

In this paragraph, we show that our benchmark results in Section V.1 are robust to a battery of additional sensitivity checks. Panels A and B (Appendix Figure C.13) show that the results above are virtually unchanged if we de-trend the daily news pressure index of Eisensee and Strömberg (2007) before applying the sample splits to account for the observed integration of media markets over time.[11] Furthermore, the estimates (Panels C and D) are qualitatively similar if the control group set is restricted to companies active in the Admin1 region of the assassination event; however, the smaller sample size reduces the power and precision of these estimates.

In Table C.7 we run a time-series model at the daily level for our sample period, where we regress assassination events on the news pressure of the day. The intuition behind this test is that the reporting of assassination is not affected by the level of high news pressure. This is confirmed by the estimation results using different sets of time-fixed effects.

Table C.7: Robustness – Daily News pressure and Assassination Events

	Dependent Variable: Assassination				
	(1)	(2)	(3)	(4)	(5)
	OLS		Probit		
News pressure	0.0012	0.0094	0.0029	0.0043	0.0043
	(0.0011)	(0.0085)	(0.0095)	(0.0097)	(0.0097)
Year FE	NO	NO	YES	YES	YES
Month	NO	NO	NO	YES	YES
DOW FE	NO	NO	NO	NO	YES
Observations	7520	7520	7162	7162	7162

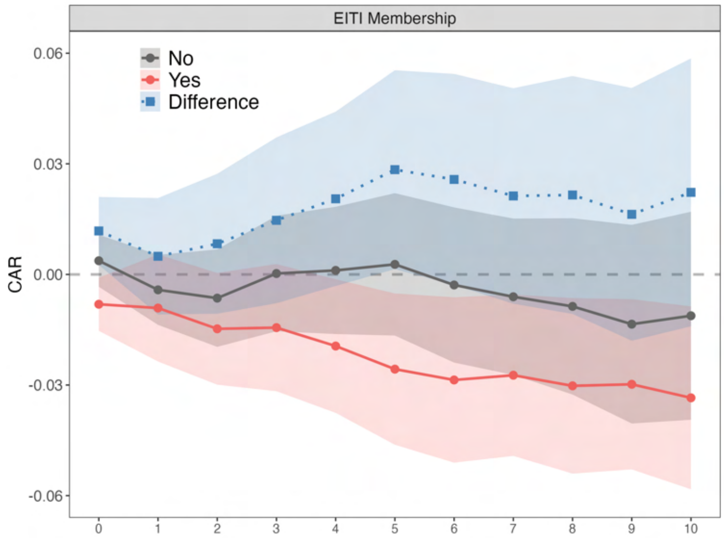
Notes: News pressure is the daily news pressure taken from Eisensee and Strömberg (2007). Robust standard errors are clustered on the event-level in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

C.3.2 Alternative Monitoring Institutions

Here, we turn to a related question: does more transparency surrounding the extractive industry impact asset prices? We show that membership in a major civil society program to increase monitoring of the mining industry—the global Extractive Industries Transparency Initiative (EITI)—also impacts market responses to assassination events. Among other things, EITI commits member countries to fully disclose taxes and payments made by mining companies to their governments; for further details, see Section VI. Data on "join" and leave" dates of member countries comes from the EITI API version v2. Appendix Figure C.14 Panel A shows that, like media, civil society transparency may amplify the publicity effect of assassination events. Assassination events that occurred in a country that was an EITI member at the time have a relatively

[11] Eisensee and Strömberg (2007) note that media market integration increases the availability of breaking news stories; this is seen in the slight upward trend in the daily news pressure for the 1968–2003 period.

Figure C.14: The Impact of Oversight

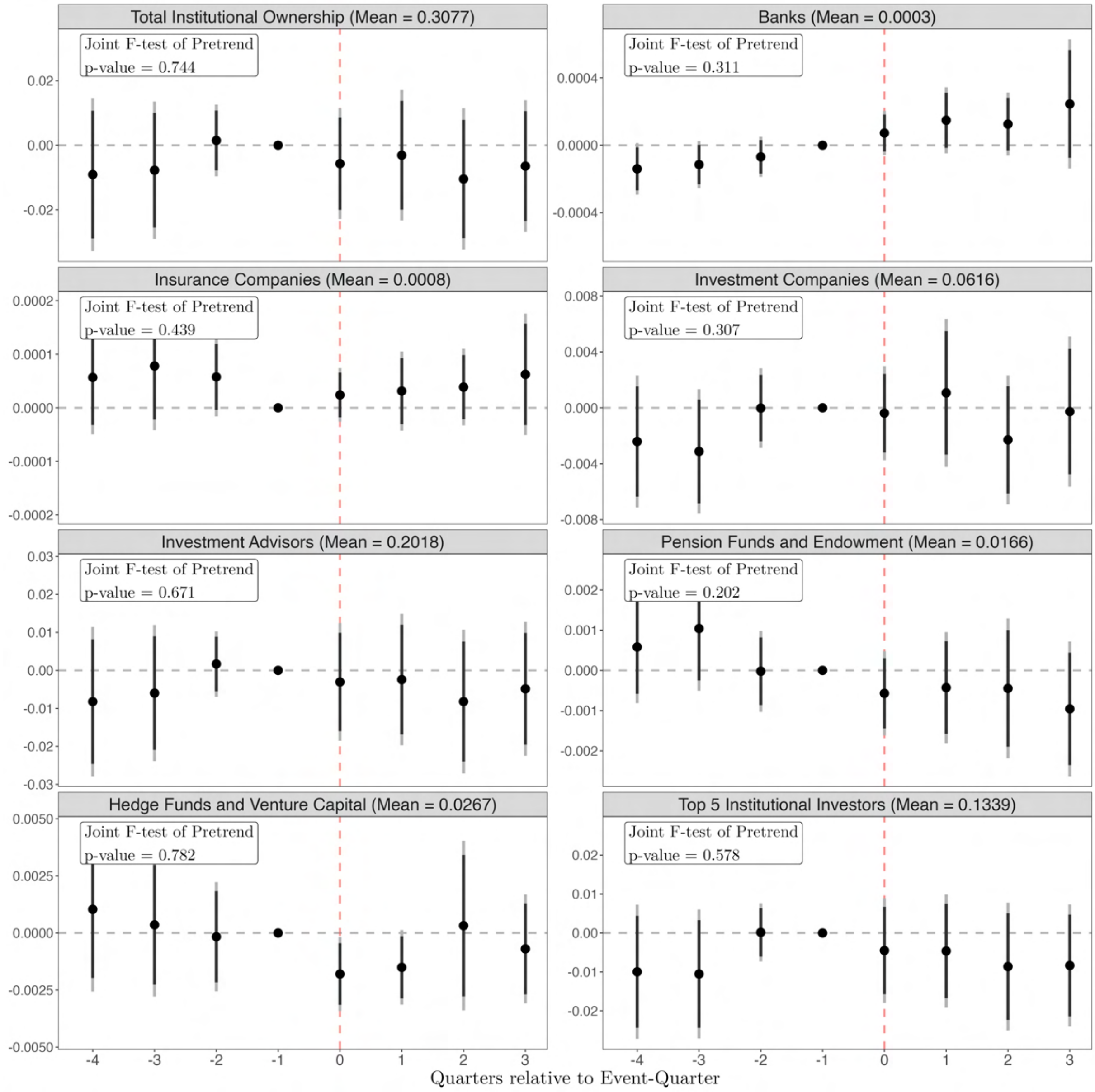


Notes: Red, respectively black solid lines and dots denote the heterogeneous marginal treatment effect of assassination events on cumulative abnormal returns (CAR). The estimated (absolute) difference in treatment effects is represented by the dotted line and squares. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. We interact the treatment indicator D in our baseline specification 5 with a binary indicator for EITI membership of the event country.

stronger negative effect on the associated mining company's market value than events that happened in non-EITI member countries.

C.4 Mechanisms: Institutional Investors

Figure C.15: Robustness – Time-varying Firm Characteristics



Notes: The effect of assassination events on institutional investors' holding positions is presented. The set of time-varying firm characteristics comprises size, leverage profitability, tangibility, and Tobin's Q, all lagged by one year. The control group comprises corporations active in the mining sector. The dependent variable mean is presented in parentheses in the column header; 90% and 95% confidence intervals using robust standard errors clustered at the company level are displayed in black and gray, respectively.

C.5 Mechanisms: Corporate Social Responsibility Scores

Investment managers and institutional funds may rely on external environmental, social, and governance (ESG) indicators as a source of information about human rights violations by a company. We consider if this key indicator for institutional investors itself reacts to the human rights violations in our study.

i. Data. We do so using data on firms' environmental and social (E&S) performance from the Thomson Reuters ASSET4 ESG database. Their information on relevant E&S actions of large, publicly traded companies is obtained from stock exchange filings, CSR and annual reports, and non-government organization websites. Annual ESG scores are available for the period 2002-2019 and cover a total of 104 event-years for 46 public mining firms.

ii. Empirical Framework. Since, by construction, human rights violations affect a company's ESG score only in the event year, our baseline tests examine the relation between assassination events and E&S performance using the following specification Dyck et al. (c., for instance, 2019):

$$\log\left(\text{Score}_{it}\right) = \alpha + \delta D_i + \boldsymbol{X}'_{it-1}\phi + \gamma_i + \lambda_t + \epsilon_{it}, \tag{C.1}$$

where $\log(\text{Score}_{it})$ is the log (plus one) of the E&S scores of company i in year t, D^{τ}_{it} is a dummy equaling 1 if company i was associated with (at least) one assassination event in year t, $\boldsymbol{X}_{it}$ is a set of firm-level controls in year $t-1$ (size, asset tangibility, leverage, Tobin's q, and profitability), and γ_i and λ_t are year- and company-fixed effects, respectively.[12]

iii. Results. We report the results in Table C.8. Columns 1 and 2 show that the assassination event has no impact on either the overall ESG performance score or the ESG score when controversies are specifically discarded (ESGC score), as provided by Thomson Reuters. In Columns 3 and 4, we focus on the ESG categories that should be most impacted by the events in our data: human rights and community scores. For each category, we find no significant impact of our events on the scores. While Thomson Reuters uses rank-based scores relative to all other companies for these categories, Dyck et al. (2019) rely on indicator-based scores.[13] Columns 5 and 6 present the results when applying their scoring method. The effects are still indistinguishable from zero,

[12] Following Dyck et al. (2019), we use logs of E&S scores to obtain better distributional properties and to reduce the impact of outliers. Our results are qualitatively unchanged when using raw scores instead.

[13] Details on the calculation of the category scores in the manner of Dyck et al. (2019) are presented in Appendix Section B.3.

although both point estimates now exhibit a negative sign. The results are qualitatively unchanged when we account for the potential impact of institutional investors on ESG scores (Dyck et al., 2019) by including the total institutional ownership share at the end of year t-1 as an additional control variable (Table C.9) or when we estimate a lagged dependent variable model (Table C.10).

Our results mirror the survey responses of institutional investors in Business and Human Rights Clinic (2018) regarding the human rights information provided by external ESG indicators. The responses reveal a concern in the industry that ESG indicators often fail to cover large companies operating in emerging markets. Responsible investment managers often have to directly liaise with NGOs to receive information about human rights violations. One investment manager interviewed even stated that civil society accounts of companies' activities are a "fundamental component of his organization's tools for ensuring that they invest responsibly" (Business and Human Rights Clinic, 2018, p. 10).

Table C.8: The Effect of Assassination Events on ESG Scores

	Asset4 z-Scores				Dyck et al. (2019)	
Dep. Variable:	Overall ESG	Overall ESGC	Human Rights	Community	Human Rights	Community
Assassination	0.0061	-0.0143	-0.0496	0.1136	-0.0143	-0.0078
	(0.0300)	(0.0396)	(0.0813)	(0.0780)	(0.0278)	(0.0189)
Company Controls	✓	✓	✓	✓	✓	✓
Company FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
R-squared	0.815	0.793	0.676	0.740	0.754	0.751
Observations	53805	53805	23864	53541	53313	44895

Notes: Rank based Asset4 z-Scores provided by Thomson Reuter are presented in columns 1 to 4. Columns 5 and 6 present indicator based scores following the procedure outlined in Dyck et al. (2019) and detailed in Section B.3 in the Appendix. Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

Table C.9: Robustness – Controlling for Institutional Ownsership

	Asset4 z-Scores				Dyck et al. (2019)	
Dep. Variable:	Overall ESG	Overall ESGC	Human Rights	Community	Human Rights	Community
Assassination	-0.0081	-0.0314	-0.0328	0.0861	-0.0154	-0.0182
	(0.0320)	(0.0418)	(0.0907)	(0.0938)	(0.0292)	(0.0188)
Company Controls	✓	✓	✓	✓	✓	✓
Total IO	✓	✓	✓	✓	✓	✓
Company FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
R-squared	0.817	0.795	0.663	0.734	0.765	0.764
Observations	41912	41912	17953	41667	41665	35843

Notes: Rank based Asset4 z-Scores provided by Thomson Reuter are presented in columns 1 to 4. Columns 5 and 6 present indicator based scores following the procedure outlined in Dyck et al. (2019) and detailed in Section B.3 in the Appendix. Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

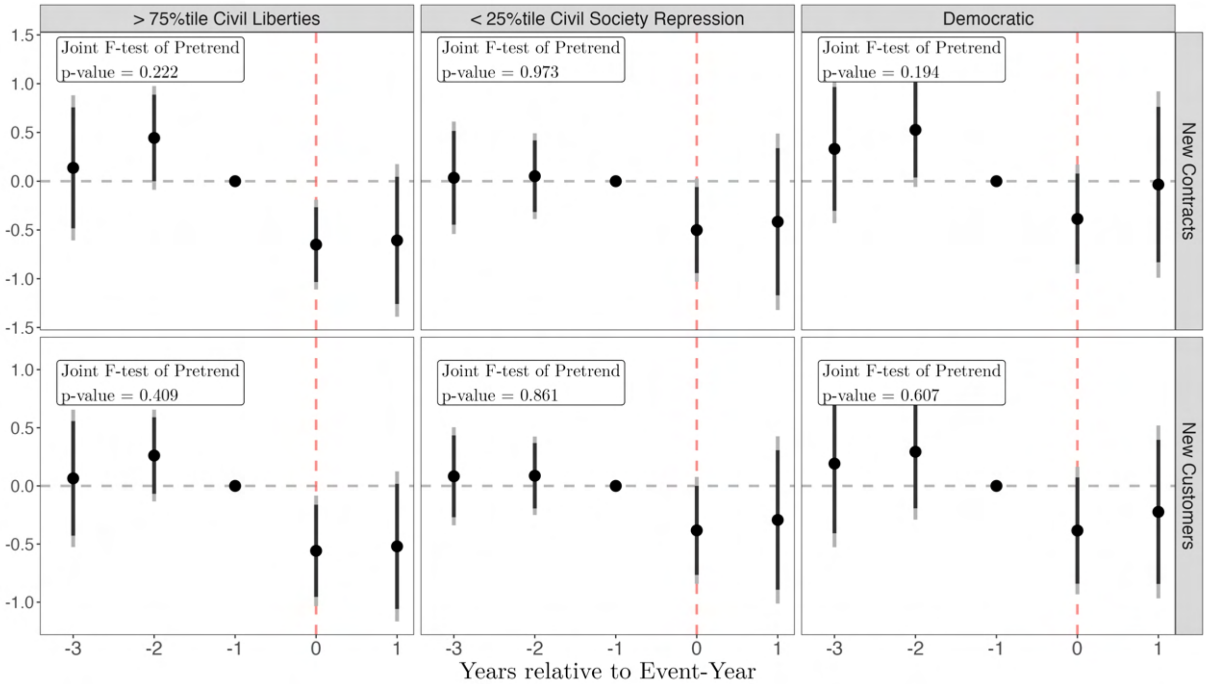
Table C.10: Robustness – Lagged Dependent Variable

	Asset4 z-Scores				Dyck et al. (2019)	
Dep. Variable:	Overall ESG	Overall ESGC	Human Rights	Community	Human Rights	Community
Assassination	-0.0155	-0.0288	-0.0650	0.0347	-0.0300	-0.0229
	(0.0235)	(0.0362)	(0.0611)	(0.0461)	(0.0223)	(0.0186)
Company Controls	✓	✓	✓	✓	✓	✓
Lagged Dependent Variable	✓	✓	✓	✓	✓	✓
Company FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
R-squared	0.877	0.852	0.750	0.805	0.851	0.797
Observations	47126	47126	19149	46744	46682	36883

Notes: Rank based Asset4 z-Scores provided by Thomson Reuter are presented in columns 1 to 4. Columns 5 and 6 present indicator based scores following the procedure outlined in Dyck et al. (2019) and detailed in Section B.3 in the Appendix. Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

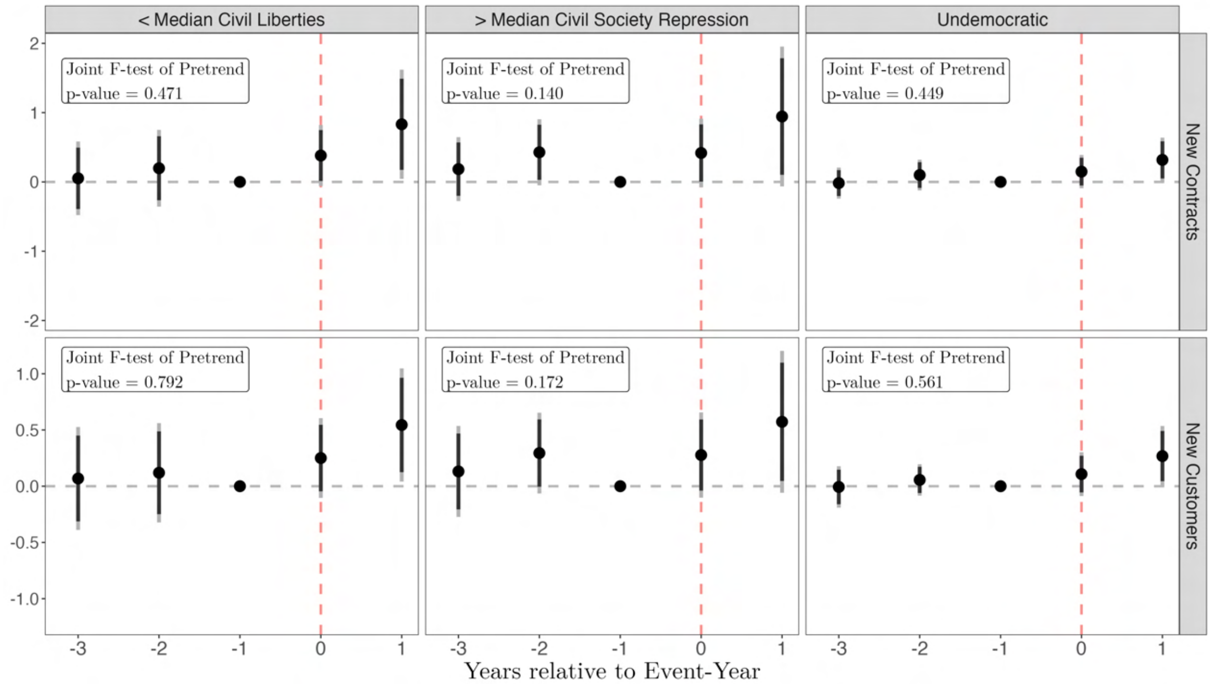
C.6 Mechanisms: Supply Chain

Figure C.16: Robustness – Alternative Indicators



Notes: The effect of assassination events on supply chain contracting is presented. Row headers specify dependent variables. Column headers refer to the "type" of contracts/customers considered in the respective specification. The horizontal axis represents the years before and after the event year, $\tau = 0$. The control group comprises corporations active in the mining sector; 90% and 95% confidence intervals using robust standard errors clustered at the company level are displayed in black and gray, respectively.

Figure C.17: Robustness – Authoritarian Regimes



Notes: The effect of assassination events on supply chain contracting is presented. Row headers specify dependent variables. Column headers refer to the "type" of contracts/customers considered in the respective specifications. The horizontal axis represents the years before and after the event year, $\tau = 0$. The control group comprises corporations active in the mining sector; 90% and 95% confidence intervals using robust standard errors clustered at the company level are displayed in black and gray, respectively.

C.7 Mechanisms: Direct Legal and Financial Costs

C.7.1 Anecdotal Evidence

Court cases intended to hold multinational mining companies legally accountable for human rights violations abroad are a relatively recent development. However, it is too early to identify this as an international trend, as there are significant obstacles remaining. Below, we discuss human rights legislation in key headquarters regions and countries for the mining corporations included in our sample.

- **Asia** The landmark 2020 decision by the Thai Appeal Court to allow a case against Mitr Phol, Asia's largest sugar producer paved the way for Asia's first transboundary class action on human rights abuses (*Forum Asia*, 31 July 2020).

- **Australia** Australia established the Australian National Contact Point (ANCP) in 2002 to promote the UN Human Rights Guidelines and handle specific complaints. However, an independent review commissioned by the Australian Treasury in 2017 found that the ANCP was underperforming and ranked among the weakest-performing NCPs globally. While reforms since the review have improved its efficiency, the ANCP only accepted a its first complaint in 2020/21 regarding potential human rights violations by Rio Tinto at its Panguna mine in Papua New Guinea (Booth and Wilde-Ramsing, 2021).

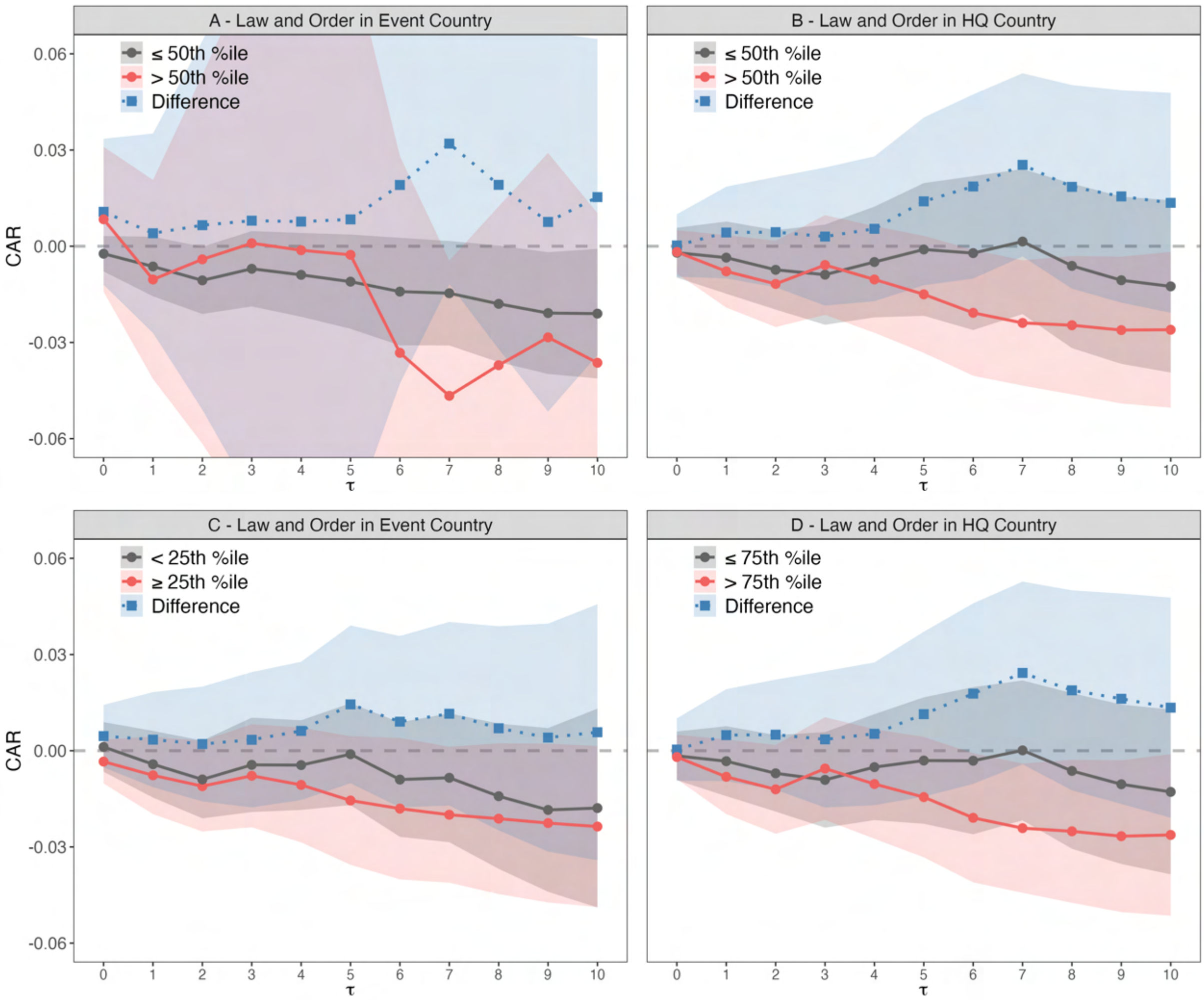
- **Canada** In 2019, Canadian mining company Tahoe Resources Inc. admitted to "infringing the human rights" of protesters after security guards opened fire on April 27, 2013, to disperse a protest (*The Conversation*, 15 August 2019). This was a landmark case; the Canadian Supreme Court had previously declined to hear similar cases (*The Guardian*, 28 February 2020).

- **United Kingdom** In 2019, the UK Supreme Court ruled that Vedanta Resources could potentially be held liable for the actions of its Zambian subsidiary, KCM, because claimants faced "serious obstacles" in seeking justice within their domestic jurisdictions (*Morrison & Foerster*, 8 June 2020). However, between 2012 and 2022, only 17 civil cases (and no criminal cases) were brought against UK companies for human rights violations abroad. Of these, only six were settled and eight remained ongoing (Percival et al., 2022). The remainder was dismissed such as the case against UK-based African Mine Ltd. for alleged excessive force by Sierra Leonean police at its Tonkolili iron ore mine (*Morrison & Foerster*, 8 June 2020).

- **United States** Ruggie (2018, p. 320) documented that "for nearly two decades, the US Alien Tort Statute(ATS) was also an exception, providing a means for foreign plaintiffs to bring suit in federal courts for egregious human rights abuses committed abroad. A California district court first agreed to extend it to corporations in 1997 (Joseph, 2004). More than 150 such cases were subsequently brought. The net result? The only case to go to a jury trial was won by the corporation. Two were settled for modest sums (the ATS is a civil statute, resulting in payment for damages if successful). The rest were dismissed on various procedural grounds."

C.7.2 Results

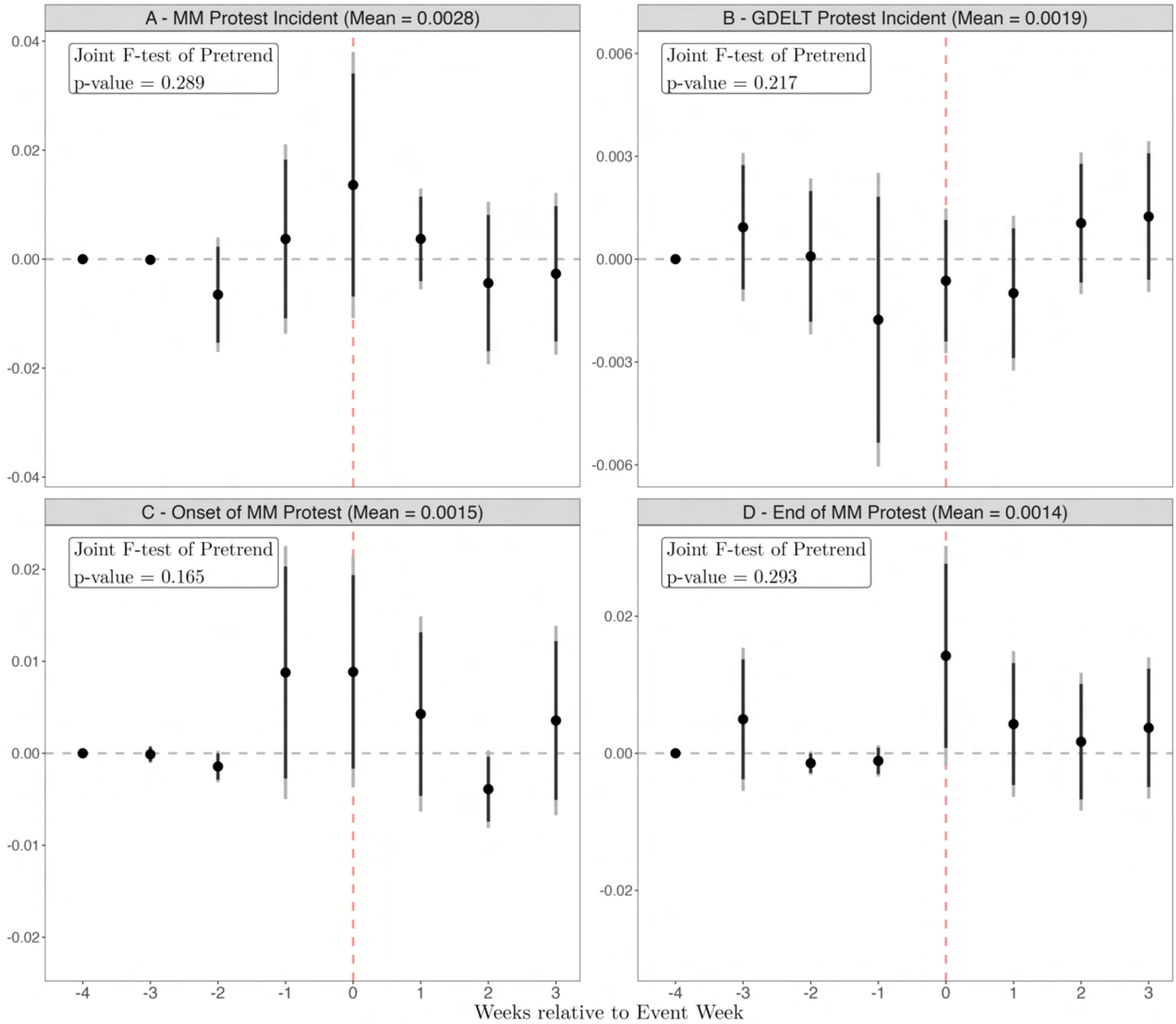
Figure C.18: Influence of Legal Institutions in Event and Headquarter Countries



Notes: Red and black solid lines as well as dots denote the heterogeneous marginal treatment effects of assassination events on cumulative abnormal returns (CAR). The estimated (absolute) difference in treatment effects is represented by the dotted line with squares. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. We interact the treatment indicator D in our baseline specification 5 with a binary indicator for the following: (a) above-median ICRG Law and Order score in the event country; (b) above-median ICRG Law and Order score in the corporation's headquarters (HQ) country; (c) below the 25th percentile ICRG Law and Order score in the event country; (d) above the 75th percentile ICRG Law and Order score in the HQ country; 95% confidence intervals using robust standard errors clustered at the event level are displayed.

C.8 Mechanisms: Local Opposition to Mining Projects

Figure C.19: The Effect of Assassination Events on Protests



Notes: Panel A presents the effect of assassinations on the incidence probability of protest in the Admin1 region of the event for two databases: *(i)* Mass Mobilization Data Project (MM) and *(ii)* GDELT. Panel B uses information from MM on protest start and end dates and depicts the effect of assassinations on the probability of conflict onset or ending. The horizontal axis label denotes the weeks before and after the event on $\tau = 0$; 90% and 95% confidence intervals using robust standard errors clustered at the Admin1 level are depicted in black and gray, respectively.

C.9 Persistence: Assassinations and the Political Economy of Local Rents

Table C.11: EITI Tax Revenue Share Data

Country	Years	Observations	Mean	St.dev.	Min	Max	Assassinations
Colombia	5	45	0.1111	0.1096	0.0001	0.3378	5
Ghana	13	138	0.0942	0.1170	0.0000	0.4927	0
Guatemala	2	23	0.0870	0.2781	0.0000	0.9901	3
Honduras	3	15	0.2000	0.2023	0.0062	0.5156	0
Mozambique	7	213	0.0329	0.1138	0.0000	0.9311	0
Papua New Guinea	5	40	0.1250	0.1822	0.0000	0.6291	1
Peru	13	331	0.0393	0.0796	0.0000	0.7864	28
Philippines	5	144	0.0347	0.0661	0.0000	0.4379	7
Sierra Leone	11	132	0.0833	0.1026	0.0006	0.4671	1

Notes: The number of events corresponds to the assassination events that can be matched to both, private and publicly traded mining companies with EITI tax records.

Table C.12: Robustness – Probit

	Dependent Variable: Assassination			
	(1)	(2)	(3)	(4)
Tax share	0.104**	0.206**	0.228**	0.411**
	(0.045)	(0.083)	(0.107)	(0.175)
Country FE		✓	✓	
Year FE			✓	
Country × Year FE				✓
Observations	1081	715	640	416

Notes: The *Tax Share* is defined as the taxes and royalties paid by a corporation to the host country government divided by the total tax and royalty revenues received from the mining industry. Marginal effects are reported. Robust standard errors clustered on the company-country level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table C.13: Robustness – Change in Tax Revenue Shares

	Dependent Variable: Δ Tax Share			
	(1)	(2)	(3)	(4)
Assassination	0.012	0.011	0.012	0.013
	(0.031)	(0.033)	(0.032)	(0.033)
Country FE		✓	✓	
Year FE			✓	
Country × Year FE				✓
Observations	784	784	784	784

Notes: The Δ *Tax Share* share is the first difference of the *Tax Share* defined as the taxes and royalties paid by a corporation to the host country government divided by the total tax revenue received from the mining industry.

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