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

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**Abstract**

We study how human rights publicity impacts multinationals. We collect 20 years of data on the assassination of environmental activists and use event study methodology to estimate the impact of the human rights spotlight on firm stock price. We find a median loss in market capitalization of 100 million USD, which is mediated by the strength of the news cycle. We highlight the role of economic mechanisms: events negatively impact supply chain contracts and responses by institutional investors, even where ESG scores are unchanged. With new public finance data, we also show the political economy mechanisms behind this equilibrium.

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# 1 Introduction

Multinational corporations are political institutions. By revenue alone, the largest multinationals rival the size of states (Zingales, 2017).[1] The activities of multinationals span the world without a single authority. The largest firms operate in weakly-governed territories and conflict-prone sectors, such as the natural resource industry. Mining, in particular, has become a flash point between civil society and multinational activity (Ruggie, 2013).[2] Since 2002, more global environmental activists have been killed than Australian and U.K. soldiers in war zones (Butt et al., 2019). Since 2020, attacks against business-focused human rights activists have occurred at a rate of one per day, with mining activity linked to a third of all attacks.[3] In a setting where civil society lacks formal power, what happens to the large firms caught in the human right's spotlight? Does the removal of opposition to mining activity benefit companies, or are firms caught in the spotlight penalized?

We study how human rights reporting impacts multinationals. We turn to well-publicized events at the heart of current advocacy: the assassination of environmental activists. These deaths in Figure 1 are the focus of global publicity campaigns by activist groups and journalists. We estimate how publicity surrounding activist assassinations impacts the stock price of multinationals "associated" with these events through human rights reporting. To do so, we collect and code 20 years of data on assassinations across the globe tied to the natural resource sector. We parse hundreds of assassination incidents and identify the mining projects *associated* with—that is, named in coverage of—violence, and match them to publicly-listed parent companies. We use the information surrounding these events to study how markets respond to the human rights spotlight. We then highlight the economic mechanisms behind our effects and the political economy of their persistence.

We deploy financial event study methodology to estimate the impact of human rights reporting, focusing on the assassinations of activists. These salient events are discrete, noteworthy, and, by definition, well-reported in the media.[4] Our focus is on high-profile events and their impact. As notable figures, their names and the circumstances around their death are often the focal point of human rights reporting, and thus appear in the international human rights spotlight (Ramos et al.,

[1] In 2018, 69 of the top 100 largest economic entities in the world were global corporations (Global Justice Now, 2018). Scholars have long juxtaposed the footprint of multinationals and states (Greene, 1983). These comparisons include market capitalization and revenue (Zingales, 2017).

[2] *Civil society* refers to a political space where voluntary—neither market nor state—associations shape the rules that govern social life. This definition follows Scholte (2002).

[3] See Hearon et al. (2020), and the Human Right's NGO Business and Human Rights Resource Centre report, Business and Human Rights Resource Centre (2021)

[4] These are notable individuals in the community, and we follow international press norms in defining assassination of these individuals. See Section 2.1.

2007; Hafner-Burton, 2008; Peksen et al., 2014).[5] The specificity of these events allows us to quantify their impact on stock prices using financial econometric methods.

Specifically, we use event study design to identify how publicity around human rights events is incorporated into the stock price of firms. We do so using two approaches and show a consistent causal story across two different counterfactuals. First *1)*, we consider the set of firms connected ("associated") to assassinations through media coverage. We then deploy a traditional event study and estimate abnormal returns, comparing actual returns to the expected firm returns over the event window. Second, *2)* we use a regression strategy to estimate the impact of assassinations, comparing the abnormal returns of "associated" firms against control firms—firms operating in the same event country, sector, and event period, but not otherwise identified in reporting. For classical financial event study estimates, our results are robust to alternative parametric and non-parametric test statistics, and OLS estimates are robust to a battery of fixed effects specifications and sensitivity checks.

Beyond OLS regression estimates, our regression results are also robust to using an implementation of a synthetic matching estimator. That is, to account for potentially unobserved differences between treated and control companies not fully captured by fixed effects and firm-level controls, we apply a modified version of the synthetic matching method introduced by Acemoglu et al. (2016).[6] Thus, our results are not only robust across types of event studies (traditional vs. regression-based estimates), but also different regression-based estimators (OLS vs. synthetic control-based studies).

Our results show that human rights reporting has substantial negative effects on multinationals. These effects are in contrast to firms benefiting from eliminated opponents, and we do not find that contrarian investors capitalize from these events. Instead, we estimate significant negative abnormal returns for firms associated with reported assassinations. These negative effects appear the (trading) day after an assassination occurs, and are amplified for the ten days after the event—and beyond. In other words, these effects do not mean revert, and investors do not trade against initial negative responses. Importantly, we show the negative impact of human rights events were likely unanticipated by the market. On days leading up to assassinations, abnormal returns are zero. Our estimates tell a consistent robust story using two counterfactual exercises.

[5]Since assassinations are implicitly high profile, this minimizes censoring. This study is concerned with reported assassinations in the international press.

[6]We provide an accompanying open source **R** package `synthReturn` that implements the method at https://github.com/davidkreitmeir/synthReturn.

We interpret our findings as evidence that disclosure of information on human rights violations meaningfully impacts markets, and does so where formal recourse is unlikely.[7] These impacts are also economically meaningful. For companies named in assassination news, the median 10-day cumulative loss in market capitalization is over 100 million USD. Our results suggest that the informational tools of civil society can impact the value of multinationals associated with human rights violations. Moreover, this information impacts firms where legal costs of these events are exceedingly rare (Christensen and Hausman, 2016). We do not find legal implications for any of the events in our dataset.

What drives these effects? Although the human rights spotlight impacts firms, we point to economic mechanisms over non-pecuniary mechanisms behind our main results. Whilst legal recourse is unlikely, we highlight the roles played by (i) media information dissemination, (ii) reactions from institutional investors, and (iii) the loss of potential purchasers in the global supply chain. Furthermore, we do not find that assassinations increased local protest and conflict, factors likely to impact short-run local production.

First, we establish the importance of the media channel and consider the likelihood that news of human rights events reaches financial decision-makers. We compare market reactions during periods with many newsworthy events to those with fewer newsworthy events, using daily "news pressure" data (Eisensee and Strömberg, 2007). We find that the penalty of human rights news disappears when they coincide with more active news cycles. However, the penalty survives when events occur during less eventful news periods—when news is less likely to be crowded out. We perform a placebo exercise to show that the visibility of the multinationals in press coverage—the firm names—is consequential. We find that firms operating in the vicinity of events, yet *not* named in media coverage, do not experience significant penalties, relative to companies explicitly named in the media.

Second, we find that informationally sensitive institutional investors respond significantly to assassination events. We show that institutional investors most likely to follow event-based trading strategies, such as hedge-funds, systematically divest from mining companies following assassination events. These results dovetail with work on the role institutional investors play in promoting social responsibility (Dyck et al., 2019), especially in emerging markets with weak institutions (Dyck et al., 2008).

[7] In our setting, firms may not be directly complicit; and if so, they are unlikely to face formal sanction. Human rights scholars legal scholars, like Ruggie (2013), document the complex reasons why punitive actions against multinationals are rare.

Third, we find that supply chain contracts may explain why these events negatively impact expected profitability. By collecting data at the corporate customer and supplier level, we analyze how assassination events impact new contracts and new corporate customers of the associated mining firms. Our results show that being associated with an assassination leads to a 32% reduction in new contracts and 39% decrease in new corporate customers from countries with a strong emphasis on human rights protection (i.e. North America and Europe). This is notable, as it appears that there may be demand-side repercussions for non-consumer facing firms and upstream commodities.

If violence against activists is costly to publicly-traded owners, why do these events occur? We explore the political economy behind this equilibrium. We collect international data on mining royalties, and show that assassinations correspond significantly with the importance of mining royalties paid to domestic governments. Multinationals may not have full control over costly actions of local affiliates, especially where local operations collude with governments and paramilitary forces. Thus, although we find the association with egregious human rights violations may be costly, there may be a multitude of reasons why these events continue. For example, formal liability and reputational costs may be insufficient to constrain local agents from engaging in socially deleterious behavior, or "rational wrongdoing" (Shapira and Zingales, 2017). In the case of human rights abuse, parent-company liability is likely also incredibly limited (Ruggie, 2013).

Our contributions are fourfold. First, we show the negative impact of the human rights spotlight on firm value. The violent removal of opposition does not benefit shareholders, even in a setting where firms are unlikely to face formal sanction. Our findings focus on weakly institutionalized settings, and contribute to work on the impact of publicity from *legally-binding* violations of human rights norms in developed economies (*e.g.* discrimination) (Au et al., 2019; Huber et al., 2021; Borelli-Kjaer et al., 2021) or U.S. firms being caught under the U.S. Foreign Corrupt Practices Act (FCPA) for paying bribes to officials abroad (Karpoff et al., 2017). By focusing on discrete, salient human rights violations, our findings build on earlier, incipient work of Kappel et al. (2009), which explored the impact of a multitude of human rights violations (*e.g.* labor misconduct, discrimination, etc.). Kappel et al. (2009) find UK and US investors "punish" firms accused of human rights violations: the median US-listed firm loses 47.31 million USD in value within 11 days following an event. Nevertheless, others have found that UK firms do not react to binding anti-slavery supply chain legislation. Beyond formal political mechanisms, we show firms based in developed countries may indeed be impacted by malfeasance in their global supply chain.

Thus, in a world reliant on global supply chains, our findings suggest that publicity can impact more obscure, upstream firms. Our results dovetail with earlier work on consumer-facing activism and downstream brands (Harrison and Scorse, 2010; Klymak, 2020; Borelli-Kjaer et al., 2021).[8] Likewise, the corporate responsibility literature has emphasized the effects of ESG events for high-profile brands Aouadi and Marsat (2018). The controversies in our setting are far from front page scandals involving well-known, consumer brands. The fallout, if any, may be lower for obscure, upstream commodity producers. Instead, investors may reward violent conflict (Guidolin and La Ferrara, 2010) and contrarian investors may seek to profit over controversies (Cui and Docherty, 2020; Schanzenbach and Sitkoff, 2020).

Second, our results suggests that informational tactics used by civil society, especially human rights groups, interact with global financial markets and impact firm value. An influential political science scholarship argues that human rights publicity is an important tool used by international activists to confront state entities (Brysk, 1993; Keck and Sikkink, 1998, 1999; Khagram et al., 2002); the impact on private actors is less established. We fill this area and quantify the impact on firm value, and build on important earlier work by Couttenier and Hatte (2016) who find that NGOs are able to target high-profile sponsors (*e.g.* Adidas and Coca Cola) during notable global sporting events (*e.g.* FIFA World Cup). We show human rights reporting impacts firm value in less high-profile and consumer product focused contexts. Reports by international human rights organizations highlight the challenges of local human rights groups in holding multinationals accountable and suggest that human rights advocacy is often a futile endeavor.[9] We show that global markets can react strongly to human rights reporting in less visible contexts.

Third, our results contribute to empirical studies of market reactions to controversial ESG events, and their impact on shareholder value.[10] Our findings support recent work finding markets react strongly to ESG news (Dyck et al., 2010; Krüger, 2015; Capelle-Blancard and Petit, 2019; Cui and Docherty, 2020). In particular, we contribute to the literature on which *types* of ESG events move markets, such as those which convey information about the quality of upstream operations or signal potential supply chain disruptions. Our mechanisms align with work by Serafeim and Yoon (2022), who emphasize that markets react to ESG news, not for non-pecuniary reasons, but that such events often convey new, meaningful economic information. We credibly address issues of measurement error found in this literature, by focusing on discrete, time-stamped events. In addition, we show that events in our study may not be reflected in common ESG measures used by investors and analysts.

[8] Klymak (2020) shows that "naming and shaming" campaigns from the US government have material consequences on the trade performance for more consumer-facing products versus upstream intermediate goods.

[9] For example, Amnesty International. (2016). This is what we die for: Human rights abuses in the Democratic Republic of the Congo power the global trade in cobalt. See: https://www.amnesty.org/en/documents/afr62/3183/2016/en/

[10] While our subject matter touches on ESG and corporate governance, throughout this study, our use of *governance* is conceptually different from that of the corporate finance literature and follows the broader concept used in public economics.

Last, we contribute to empirical work using asset price movements to understand political phenomena (Chaney, 2008; DellaVigna and La Ferrara, 2010; Guidolin and La Ferrara, 2010; Dube et al., 2011; Girardi, 2020; Baker et al., 2023). We build on forensic analyses of how firm assets respond to conflict, using asset prices where outcomes may be sparse (*e.g.* Chaney, 2008; Guidolin and La Ferrara, 2007). For Egypt's Arab Spring protests, Acemoglu et al. (2017) find returns fall for companies tied to the incumbent government, as investors adjust their expectations about the potential of future rents.[11] For example, Guidolin and La Ferrara (2010) document positive stock market reactions to the onset of conflict. DellaVigna and La Ferrara (2010) use similar methods to detect illegal weapons trade for countries under arms embargo.

The paper is organized as follows. Section 2 details our data and our coding process. Section 3 describes our empirical methodology and presents the results. Section 4 examines the mechanisms behind our baseline findings. Section 5 provides an explanations for the continued prevalence of the assassinations. We conclude with a brief discussion of our results in Section 6.

## 2 Data, Definitions and Context

We use event study methodology to study the impact of salient, well-reported human rights violations: assassinations. To do so, we collect 20 years of data on the assassination of activists connected to mining activity. The following sections describe the context of these events and how we code them. We do so in three steps. First, we describe the definition and assassination events used in our analysis, as well as the rationale for the mining sector case selection. Second, we describe the coding of human rights reporting and matching publicly listed firms to these events. Last, we describe the financial, mining, and geographic data used in our analysis.

### 2.1 Assassination Events

**Defining Assassinations and Sectoral Scope.** Figure 1 shows the global trend in activist killings since 2008. Data come from our data on global activist assassinations. These events—including the victims and associated actors—embody the protagonists in international human rights campaigns.

Our focus on assassinations is purposeful. Since the early 2000s, the global human rights community has drawn attention to a rising trend in violence toward environmental activists (Butt et al., 2019; Hale, 2020). Specifically, the killing of activists connected to campaigns around extractive activity. This strategy of "informational politics" of international human rights tends to coalesce around the

[11] This follows a large literature on political connections, including Fisman (2001).

names of victims and the naming of targeted (states and firms) associated with these events (Keck and Sikkink, 1998; McEntire et al., 2015).

**[Figure 1 about here]**

By *assassinations*, we mean the intentional killing of prominent members of society. Our definition is not idiosyncratic. Our usage largely tracks the journalistic standards (*e.g.* Associated Press and US National Public media standards) and those of the human rights scholarship (see: DeMeritt, 2012). By definition, assassinated persons are notable. In our data, many individuals are likewise notable in their communities—as advocates or key players—and we refer to their slayings as assassinations. These people include indigenous and tribal leaders; environmental and labor activists; members of the clergy; and more. Throughout the paper, we use the terms *assassination* and *extra-judicial killing* interchangeably.

By construction, the assassinations we study are relatively well-publicized. That is, these episodes are those that draw human rights and media attention. We follow scholarship on "naming-and-shaming" campaigns, and focus on the publicity of these events (Ramos et al., 2007; Hafner-Burton, 2008; Peksen et al., 2014).[12] Since we study the impact of news of these events, unpublicized killings are not the scope of our study.

The empirical advantage of focusing on assassinations, as opposed to other human rights violations, are numerous. In general, these events are less ambiguous than other forms of human rights abuse. As noted above, these events are salient, well-publicized events. Importantly, we focus on the most unambiguous human rights violations—and the most egregious. By far the most numerous human rights violations related to business activity encompass labor disputes and sexual harassment (Ruggie, 2013). For these events, there is great institutional capacity to arbitrate these disputes, thus their coverage in human rights databases is idiosyncratic.[13] Last, for the purpose of an event study, having a concrete timeline is important. Unlike other norm violations, we deal with events that have clear timelines. Thus, the gap between an assassination event and news in the media is minimal.

Our focus is on assassinations surrounding mining activity. We focus on mining activity for four reasons. First, for human rights scholars and legal practitioners, the sector has exemplified the weakness of current institutions in constraining human rights violations of multinationals (Ruggie, 2013). Second, as such, it is one of the deadliest sectors for activists (Business and Human Rights Resource Centre, 2021; Butt et al., 2019). Third, it is a capital-intensive sector and one where equity

[12]Scholarship in this area emphasized both individual dissemination of information from the NGOs themselves, the media reporting, and the UN.

[13]Noted by Ruggie (2013)'s UN fact finding assessments.

financing is common. As such, it is a sector where we can connect publicly traded firms to human rights events. Fourth, we limit our attention to relatively homogeneous, upstream product markets. By doing so, we attempt to limit the extent to which these products may face final consumer boycotts in response to human rights reporting.

Thus, a study of multinationals and human rights must focus on mining and extractive activity. Of the recent—and rare—human rights cases brought against multinationals, Hearon et al. (2020) show nearly half involve the extractive industry. For US based firms, this is the sector where the Alien Tort Statute has been notably deployed, unsuccessfully, against firms accused of human rights abuse (Christensen and Hausman, 2016).

**[Table 1 about here]**

**Event Collection.** Our data covers 354 assassinations (496 victims) over 20 years. Table 1 provides an overview of this data. Our first observation is recorded in 1998, and the sample expands to cover events across 31 countries. Peru and the Philippines are the most dangerous countries for mining activists. The geographic distribution of events is depicted in Figure 2.

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

Thus, activist assassinations are collected using both algorithmic and human searches of international full-text media archives. These include databases of the International Herald Tribune; the Associated Press wire archive; popular news APIs (*e.g.* the Guardian); and, importantly, global news databases (*e.g.* LexisNexis).[14] Coding is done by research assistants and cross-validated by principal investigators.

Of our event data, assassinations are mapped 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. In other words, over half of these firms have at least one assassination associated with them. But what exactly do we mean by "associated"? We turn to this now.

[14] We perform multilingual searches, for example, Spanish. However, core media databases provide translations of international news coverage, such as LexisNexis.

**[Figure 2 about here]**

## 2.2 Associated Multinationals

**Defining Company Association.** Table 1 shows publicly traded firms that have been matched to—or "associated" with—at least one assassination event in our sample. We use the following section to explain how we operationalize this definition and code these relationships.

For this study, *association* means that a company or their project is named in reporting surrounding an assassination event. A publicly traded company may be an indirect owner of a project where violence occurs. The association between a firm's operations and violence is coded directly from source material, all of which is sourced from publicly available human rights journalism or human rights reports.

A firm is connected to an event insofar as it—or its operations—are mentioned in the reporting of an event. It is worth stating that we do not take a stance on the relationship between a firm and an event, beyond their operations being named in human rights reporting. Thus, an "associated" company may not play any role in organizing or participating in violence. As we show later, it may be unlikely that multinationals themselves play active roles in these events, in the aggregate.

Figure 3 details the global distribution of assassinations and the headquarters of companies associated with assassinations. Colored panels (left) correspond to an event country; their height represents the total number of assassination events in that country. Gray panels (right) correspond to the headquarter country for publicly traded firms; their height represents the total number of events connected to firms headquartered in a given country. Figure 3 shows that most assassinations in our data are matched to firms headquartered in Canada, the United Kingdom, and the United States. Though these are advanced liberal, democracies, suits against multinationals for human rights abuses are exceedingly rare (Schrempf-Stirling and Wettstein, 2017).

**[Figure 3 about here]**

**Coding Associated Companies.** We hand-match the "nearest" publicly traded firm associated with each assassination. Matching public firms is done in three steps.

First, we determine whether a company named in human rights reporting is publicly traded.[15] For consistency, we consider the most direct, publicly traded companies—except when the international, corporate owner is specifically tied to the event in reporting.

Second, when a company named in human rights reporting is not publicly listed, we manually search for its parent company. We examine whether a named company is a subsidiary or joint venture of publicly traded companies (when the event occurred). We then verify this information using sources such as, but not limited to: official firm websites, corporate reports, SEC filings, public business registers. In many cases this process is non-trivial.

Third, reporting may refer to a mining project instead of the company responsible for the project. Rather than using firm names *per se,* it is common practice for geographic or administrative unit names to demarcate mining operations. For example, a report may refer to Rapu-Rapu Polymetallic Project (Philippines), as opposed to the ultimate firm, Lafayette Mining. In these cases, we attribute ownership of the project to the publicly traded company using step two.

Figure 4 illustrates our coding process. The figure shows an excerpt from the *Guardian* newspaper for an assassination event in our sample; color highlights indicate key information which we code for our data set.[16]. The piece identifies a victim, Ecuadorian indigenous leader José Isidro Tendetza Antún—shown in green—and establishes they were an activist working in opposition to mining activity. The latter is highlighted in purple. The piece in Figure 4 describes the victim's death, highlighted in yellow.

**[Figure 4 about here]**

The example in Figure 4 describes a specific mining project, rather than the firms. In this example, the death involved "Mirador copper and gold mine" (blue), owned by Corriente Resources Inc. through a subsidiary EcuaCorriente S.A. Our process codes the ultimate owner using a public records search. In this case, public data shows Corriente Resources Inc. was acquired in 2010; at the time of the event, it was owned by two publicly traded companies: China Railway Construction Corporation and TongLing Nonferrous Metals Group Holdings.[17] Thus, both listed companies (China Railway and TongLing) are coded as being "associated" with the event.

[15]A special case arises if another public mining company holds shares of the company at the time of the event: i.e. the named company is not the ultimate owner.

[16]The source article is available at https://www.theguardian.com/world/2014/dec/06/ecuador-indigenous-leader-found-dead-lima-climate-talks.

[17]See http://www.corriente.com/news/news.php and https://www.banktrack.org/project/el_mirador_copper_mine for more information.

Sometimes, specific projects or companies may not be named in reporting. In these cases, an assassination is then *not* associated with a company. Consider the example in Figure 4. In this example, if no identifying data (blue) is available, then the assassination is not matched to a specific firm. For a detailed example, refer to Appendix Figure D.1.

Figure 4 also provides an important example of how we consider ambiguity around event dates. For the example in Fig. 4, although the date of the crime is known to be November 28, 2014 (highlighted in pink), the event may have only made the news after the discovery of the body. In this case, financial markets are likely to react only days after the *de facto* event date. We turn to these issues in our estimation process.

## 2.3 Financial Outcomes and Geo-Location

We collect daily stock return data for mining companies associated with each event, as well as the returns for other companies operating within the same country, during the year the event took place. Daily return data for 1998–2019, and additional firm-level data, come from the Datastream database.[18]

For mining projects in our data, location and company ownership data comes from the SNL Minings & Metals database, which we matched to our assassination data.[19] The SNL database also allows us to identify a robust set of control companies for each event-year because we can identify other mining companies with operations in the geographic vicinity of the event mine.

Project ownership information is available annually and allows us to track treated and control companies over time. Consider an assassination in Colombia for 2013. In this case a control company set encompasses all publicly traded companies that own mining projects in Colombia that year, but are not named in association with the assassination. See Appendix Figure D.2 Panel A for an example.

**[Table 2 about here]**

Table 2 gives summary statistics for our financial data. We construct market returns using the Morgan Stanley Capital International (MSCI) stock indices, and match each mining company security to the MSCI country index where they are listed.

[18] Market holidays are removed from the closing price time series. We use financial variables in a common USD denomination to account for currency fluctuations.

[19] See: https://www.spglobal.com/marketintelligence/en/campaigns/metals-mining. Other recent studies using the SNL Minings & Metals database comprise Berman et al. (2017) and Knutsen et al. (2017), who explore the impact of local mining operations on conflict and corruption.

We require that companies are traded at least 200 days out of the 250 trading days in the *estimation window*, which we turn to in the next section. We deal with ambiguities in our data in standard ways. For example, if an assassination event falls on a non-trading day, the event date is assigned to the first trading day after the actual event date. Finally, we exclude thinly traded mining company securities from our sample. The financial data in Table 2 forms the basis of our event study analysis, which we turn to now.

# 3 Estimating the Impact of Assassinations and Stock Returns

This section estimates the impact of the human rights spotlight on firms with two different classes of financial event studies. First, section 3.1, uses classic event study methodology to estimate the abnormal returns (CARs) only for firms associated with assassinations. We also motivate our use of new non-parametric tests. Second, section 3.3, estimates the impact using our full set of data. We first use OLS to estimate the differences in CARs for associated (*i.e.* treated) firms versus control firms. We then show that these regression results are robust to using a application of synthetic control-type estimator—specifically, a modified application of Acemoglu et al. (2016) synthetic matching procedure.

## 3.1 Traditional Event Study and Inference

We first turn to the traditional event study setup for assassination events and discuss our preferred approach to statistical inference. Our setup follows the classical event study literature (MacKinlay, 1997). Consider the security of a company associated with an assassination event. Figure 5 presents the timeline around such an event. An assassination $e$ occurs at date $\tau = 0$, where $\tau$ denotes the time relative to the date of the assassination. Time is broken into two windows around this event, an *estimation* window and an *event* window.

**[Figure 5 about here]**

**Traditional Estimation.** The Abnormal Return (AR) is the difference between a firm's observed return and their expected return, absent an assassination. Around an assassination event $e$, we calculate the AR for company $i$ at time $\tau$ as

$$AR_{ie\tau} = R_{ie\tau} - E\left(R_{ie\tau} | X_\tau\right), \tag{1}$$

where expectations about "normal" returns are conditional on a set of information, $X_\tau$.

We estimate the normal returns for firm $i$ over an *estimation window* ($\tau = T_0 + 1, ..., T_1$), using the following linear market model:

$$R_{ie\tau} = \alpha_{ie} + \beta_{ie} R^M_{ie\tau} + \epsilon_{ie\tau} \tag{2}$$

where $R_{ie\tau}$ is the daily return observed for firm $i$, and $R^M_{ie\tau}$ is the return for overall market index where they are listed. We estimate (2) using an estimation window of 250 trading days, ending 30 days before the event (Li and Lie, 2006; Acemoglu et al., 2016). We require securities to be traded at least 200 out of the 250 trading days, and 8 out of the 11 trading days for the period past and including the event day.

Using the estimates from (2), we then compute the daily abnormal returns for the event window:

$$\widehat{AR}_{ie\tau} = R_{ie\tau} - \hat{\alpha}_{ie} - \hat{\beta}_{ie} R^M_{ie\tau}. \tag{3}$$

We then calculate the cumulative abnormal returns (CARs) by aggregating the daily abnormal returns (3) over different windows from $\tau_1$ to $\tau_2$ within the event window for each firm and each event:

$$\widehat{CAR}_{ie}(\tau_1, \tau_2) = \sum_{\tau=\tau_1}^{\tau_2} AR_{ie\tau}, \tag{4}$$

with $T_2 + 1 \leq \tau_1 \leq \tau_1 \leq T_3$. This study considers the average impact of assassinations, thus we aggregate cumulative abnormal returns across $N$ company-event pairs in our data. The *average* CAR and its variance are equal to the following,

$$\overline{CAR}(\tau_1, \tau_2) = \frac{1}{N} \sum_{j=1}^{N} \widehat{CAR}_{ie}(\tau_1, \tau_2) \tag{5}$$

$$\sigma^2\left(\overline{CAR}(\tau_1, \tau_2)\right) = \frac{1}{N^2} \sum_{j=1}^{N} \sigma^2\left(\widehat{CAR}_{ie}(\tau_1, \tau_2)\right). \tag{6}$$

**Inference and Preferred Tests.** We present multiple test statistics for inference. Assuming normally distributed security returns, absent clustering (overlapping event-windows), $\overline{CAR}(\tau_1, \tau_2)$ follows a normal distribution with mean zero and variance $\sigma^2\left(\overline{CAR}(\tau_1, \tau_2)\right)$.[20] We refer to this test as the "Normality" test, which is simply a baseline.

We go beyond this standard test in three ways, following the literature in event study methodology and inference (Boehmer et al., 1991; MacKinlay, 1997). Our preferred method is Kolari and Pynnönen

[20] Following MacKinlay (1997), the normality assumption requires the absence of clustering in order to set the covariance terms in (6) to zero.

(2011)'s non-parametric generalized rank *t*-statistic, or *GRANK*, which is particularly fitting for our setting. We provide technical details for the following discussion in Appendix A.

The three tests are the following. First, we use a parametric test suggested by Boehmer et al. (1991) (BMP), which scales abnormal returns and adjusts for differences in the variance of pre-event residuals.[21] Intuitively, more volatile securities are down-weighted to prevent them from biasing estimates toward detecting average effects. Second, we use a refinement of BMP, adjusted BMP (ADJ-BMP), a parametric test which further accounts for event clustering (Kolari and Pynnönen, 2010). Though we suspect clustering is not an issue in our setting, we account for its potential role in bias.[22]

Third, our preferred approach relaxes parametric assumptions and introduces refinements that are suited to our setting (see recent applications by Luechinger and Moser, 2014; Barrot and Sauvagnat, 2016). We implement the non-parametric generalized rank *t*-statistic, or *GRANK*, proposed by Kolari and Pynnönen (2011). Kolari and Pynnönen (2011) show their generalized rank statistic outperforms both parametric and other non-parametric tests in studies where a) the exact event day may be unknown and b) long event windows are used. Both circumstances are useful for our environment, where sensitive information may percolate slowly into the market, and the precise day of the pricing—in contrast to the assassination date—is uncertain. Additionally, the GRANK statistic is robust to event-induced volatility, serial correlation and event-day clustering.

For completeness, we show results for the three parametric test-statistics in addition to our preferred non-parametric GRANK statistic. We turn to these estimates now.

**[Table 3 about here]**

## 3.2 Traditional Event Study Results

Table 3 shows our main results for the traditional event study. Overall, we find assassination events lead to negative abnormal returns for firms associated with violence. Table 3 shows that negative effects start soon after the date of the assassination, and these effects grow through time. Both in a) magnitude and b) significance. These effects are statistically significant across tests.

The steady pattern in Table 3 is consistent with sensitive information gradually diffusing through the market. On the day of an assassination, we see little market reaction, followed the next day by a (borderline) significant effect of around –0.7 percentage points. This initial reaction is followed by

[21] The BMP test builds on and goes beyond Patell (1976), by accounting for changes in event-induced volatility.

[22] In our study, *at most* four public firms are associated with any given event. The correction might, however, be warranted as our market model extracts *less* correlation from regression residuals than the alternative Fama-French model.

a stable decline over the next four days, and a steep—and robustly significant—decline from day 5 through to day 10 after the event. This cascade suggests that market participants may first gather additional information surrounding an event, before pricing the expected costs for an "associated" mining company.

On average, the cumulative abnormal return is –2.0 percentage points 10 days following the event. These estimates are significant at the 1 percent level using our preferred GRANK statistic (Column 6), and at the 5 percent level using the BMP (4) and adjusted BMP statistics (5). Findings in Table 3 suggest differences in volatility across securities could bias inference, comparing our adjusted and non-parametric statistics to the standard test in Column (3). The clustering issues in our setting seem negligible; the differences between the BMP and adjusted BMP tests are small (Columns 4 and 5, respectively).

The results above are robust to more conservative trading day criteria. Although our tests account for non-trading days of securities, adjusting for the length of the *estimation* and *event window*, Appendix Table C.1 shows our results are unchanged when we require companies to be traded each day within an event window, and 225 out of 250 days during the estimation window. This criteria drops seven company-event pairs and leads to a slight decline in the magnitude of our CARs to –1.5 percentage points ten days after an event. Nevertheless, these results are still highly significant and robust across tests. The difference in point estimates may be driven by a trading halt for highly affected securities. On the other hand, the securities of small mining companies are also less frequently traded; we may expect these firms are more vulnerable to disruption following an event. Thus, stricter requirements on trading frequency might disguise the true effect of assassination events, and we default to our original cut-off criteria.

Next, we turn to the days leading to the event. Doing so has two advantages. First, we formally consider pre-event movements in abnormal returns. Second, by considering the days before an assassination, we test whether market participants may have traded on foreknowledge of events. Furthermore, where events are planned, the "authorization" date of assassinations is unknown to us. 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. We test for this "prior knowledge"—and for pre-treatment movements more broadly—by aggregating abnormal returns *backwards* starting on the day before the event (see Dube et al., 2011).

Appendix Table C.2 reports the pre-assassinations results. The average abnormal return on the day before the event is positive, while the cumulative abnormal return over the ten days before an event is close to 0—slightly negative—and insignificant across test statistics. Thus, the results in Appendix Table C.2 indicate two findings. First, the market did not price prior knowledge of assassinations. 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. We test for pre-trends more thorough using our second estimator, which we turn to next.

### 3.3 Regression-Based Event Study With Treatment and Control Firms

Building on the traditional event study above, we now examine the relationship between the publicity around assassinations and stock returns of companies differentially exposed to violence. We do so by comparing the cumulative abnormal returns for companies whose projects are named in human rights reporting versus control companies: those operating in the same sector, same country, and same period of the assassination. For simplicity, we focus on our main estimates using OLS, and then show their robustness to using a synthetic matching procedure.

We consider the cumulative abnormal returns over a pre-specified period from $\tau_1$ to $\tau_2$, per firm for each assassination event. Abnormal returns for control companies are calculated similarly (equation 3). The regression model we consider for each period of the *event window* can be written as

$$CAR_{ie}\left(\tau_1, \tau_2\right) = \alpha + \delta D_{ie} + \boldsymbol{X}'_{ie}\phi + \gamma_e + \epsilon_{ie}, \tag{7}$$

where $CAR_{ie}\left(\tau_1, \tau_2\right)$ is the cumulative abnormal returns for company $i$ and event $e$ over the period from $\tau_1$ to $\tau_2$. The indicator $D_{ie}$ denotes treatment, and is equal to one if a company is associated with an event and zero otherwise.

The coefficient of interest $\delta$ captures the average difference in CARs between "treated" firms versus control firms. Our empirical strategy is valid if, absent association with violence over the event period, we would not observe systematic differences in the returns of treated versus the control firms.

Our set of control companies has a number of advantages, and our choice of control companies is guided by the political economy and finance literature. First, choosing firms with mining operations in the same country accounts for common exposure to political risk events to mining companies operating in a given location at the time of the event. This allows us to account, among other things, for incidents where violence against activists changes the national sentiment against the mining industry. Second, following Guidolin and La Ferrara (2007), we wish to compare treated companies to those with a similar "comparative advantage" for operating in high political risk environments (p.1987). Lastly, we control for commodity price fluctuations similarly impacting mining companies operating in similar commodity markets and the same domestic market.[23]

In other words, specification (7) uses event $\times$ firm-specific variation. It is important to note that the computation of cumulative abnormal returns for each event and firm, controls for the company-specific effect of market movements on the firm's stock price over the *event window*. Moreover, by re-estimating the market model for each event, we account for changes in the relationship between market and firm returns over time.

Our preferred specification (7) includes event-fixed effects, $\gamma_e$.[24] Including event-specific effects controls for common market reactions around dramatic events, such as shifts in market sentiment toward the event country, or increased excess volatility.

In addition, we present a specification which includes company fixed effects ($\gamma_i$) instead of event-specific effects, $\gamma_e$. In this case, we compare the effect of an assassination event on a company when the company was *merely* active in the country during an event-period compared to when the company was directly tied to an assassination.[25]

The baseline specifications include a set of time-variant, firm-level controls, $\boldsymbol{X}_{ie}$. These are firm size, 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 events in our study. Disruptions of projects may be more punitive for smaller (more leveraged) firms. To address issues of "bad controls," we use lagged values for controls. Standard errors are robust to heteroscedasticity and clustered at the event level.

**[Figure 6 about here]**

### 3.3.1 OLS Regression Event Study Results

Consider our OLS estimates first. Figure 6 reports our main results using (7). The top panel presents regression coefficients ($\hat{\delta}$) for our baseline specification with event fixed effects, the bottom panel presents estimates using company fixed effects. In total, 42 *individual* regression estimates are displayed, with the vertical axis showing the ($\tau$) days after (before) the event. We present 95 percent confidence error bands.

[23]For example, commodities mined in Columbia from 2013–2017 comprise coal, nickel, gold, emerald and iron according to EITI records, with about 70% of the companies active during those years mining coal.

[24]It is important to highlight that equation (7) does not specify a two-way fixed effects (TWFE) model because we use event $\times$ firm specific *cross-sectional* variation, a setting where the issue of negative weights (Goodman-Bacon, 2021, among others) does not apply.

[25]Note, that we have no convolution of the control and treatment groups, as a company cannot be part of the control and treatment group during the same event.

Figure 6 shows a clear pattern, similar to those from the traditional event study above (Section 3.1). In the days before the event, CARs are not significantly different between exposed and non-exposed firms. Returns for associated firms are, if anything, slightly positive. Following an assassination, we find a consistent decline in abnormal returns for treated versus control companies. We see no market reaction on the event day. Soon after, markets start responding to the assassination reporting. Two days following an assassination, CARs amount to –1,0, respectively –1.4 percentage points and are statistically significant. Thereafter, CARs remain negative, gradually dropping five days after the event. By day ten, the abnormal returns for exposed mining companies are between –2.2 to –2.4 percentage points.

These findings reinforce our interpretation of our classic event study results. Financial markets take time to absorb the publicity and assess risk for mining companies tied to the assassination event. In fact, the pattern in Figure 6 and the estimated magnitude of effects closely resembles that found in our traditional event study results, providing further support for our interpretation that the estimated effect is capturing the associated corporation being put under a magnifying glass, rather than gains by unnamed competitors in the aftermath of the event. The results are particularly strong for our specification using firm fixed effects (Figure 6, Row 2). Using within-firm variation, we see significant differences in CARs.

Furthermore, Appendix Figure D.3 shows that our baseline results are robust to accounting for potential non-linear effects of covariates when including cubics in size, leverage and additionally profitability (return on equity) (Column 2).[26] Moreover, the core pattern remains qualitatively and quantitatively unchanged for specifications with different fixed effects. These include less conservative sets of fixed-effects, like year ($\gamma_y$) and headquarter country ($\gamma_h$) effects (Rows 2 and 3).[27]

Figure 7 shows our results are economically meaningful. Figure 7 presents the estimated loss in market capitalization for the median treated company in our data. Results are shown for each day. Dots correspond to baseline estimates. Error bars correspond to the minimum and maximum loss across specifications (c. Figure D.3 in the Appendix). Figure 7 shows the median treated company is estimated to lose between 100 and 150 million USD in market capitalization over the ten days following the event.[28]

[26] C. for instance Acemoglu et al. (2016).

[27] In total, Figure D.3 presents 210 *individual* regression estimates. Columns correspond to two types firm-level controls used across different specifications. Each row shows five types of fixed effects. Each panel (cell), thus, shows a set of estimates for one of ten specifications. Corresponding regression tables are reported in Appendix Table C.3.

[28] Figure D.4 in the Appendix presents the distribution of losses across companies for the *baseline* specification. Demonstrating large, and economically meaningful losses for companies associated—even loosely—with violence. For exposition, we do not display losses above the 90th percentile due to long-tails.

**[Figure 7 about here]**

### 3.3.2 Robustness of OLS Regression Results

We show the OLS event study results above are not driven by an individual nation or company, nor are they sensitive to specific events or window size. The following section presents robustness results by considering robustness of our OLS results across these dimensions.

**[Figure 8 about here]**

Figure 8 presents a "leave-one-out" analysis of our OLS results, and shows that estimates are not driven by single countries or single firms. Our baseline regression results are shown in bold, and permutations are shown in light gray. In Figure 8 Panel A, we re-estimate our baseline specification, but sequentially drop individual event-countries from our sample. Visually, the negative effects are similar across sample permutations. Panel A shows a clear and gradual decrease in abnormal returns over the 10 days following the event. As well, abnormal returns in the days leading up to the event remain slightly positive, though close to zero. Importantly, the core pattern of our baseline results is robust to excluding the two deadliest countries for mining activists: Peru and the Philippines (shown in dashed red).

Figure 8 Panel B repeats the same exercise, now sequentially dropping firms. Here too our baseline results are qualitatively similar after sequentially dropping individual firms. The homogeneity across these light gray bands indicate that our findings are not driven by particular "bad actors". This suggests our results are applicable to publicly traded mining multinationals, broadly.

Our OLS results are also robust across different types of assassinations and similar events. Table C.4 in the Appendix shows that post-event assassination estimates are qualitatively unchanged when we exclude unsuccessful assassinations (Columns 1 to 5)—or, assassination attempts. Similarly, when we exclude activist killings during protests (Columns 6 to 10). Although the point estimates here follow a similar pattern to our baseline results, we exclude a substantial number of events. Excluding 42 observations, as in the previous case, reduces the power and significance of the results.

Our baseline results are not driven by outliers. Table C.4 in the Appendix shows dropping CARs larger than the 99th percentile or smaller than the 1st percentile (Columns 11 to 15) does not alter the substance of our results. The assassination coefficients are similar in magnitude. In fact, they are more precisely estimated across specifications, with the exception of company fixed effects. This latter case may indicate that the association with an assassination event constitutes an extreme observation for several companies that have been tied to an event at least once. Broadly, this supports the idea that assassinations can have substantial effects on single firms.

Next, we expand the event window to twenty days prior and post the event day to investigate the possibility of pre-trends more closely and to test for a potential reversal in the estimated effects after day 10. Appendix Figure D.5 in the Appendix plots the assassination coefficient estimates and the 95 percent confidence intervals for our baseline specification from over 40 specifications.[29] After the event date, we observe a monotonic decrease in returns until Day 13 that seems to be permanent. Before the event date, estimates are positive and insignificant. We conclude that the effect of publicized assassination events is persistent and cannot be explained by pre-existing trends in the stock price.[30]

Finally, we ask whether our results are driven by disruptions surrounding the violence *per se*. Rather than the market pricing the impact of being named in connection with the killing of an activist, our results may reflect other, physical disruptions surrounding the event or unobserved regional violence correlated with the assassination event. We address this concern by comparing the asset price responses of firms publicly exposed to the event to untreated mining companies operating within the same administrative unit in the event country. To deal with this, we construct a new control company set by matching the geolocation of our assassination events to properties in the *SNL Minings & Metals* database. Specifically, we connect assassination events to mining projects in the same *Admin1* region (see Figure D.2 Panel B in the Appendix for a graphical illustration).[31] For 92 events in our sample, we were able to match at least one publicly traded control company operating in the same region as a company "exposed" to a violent event.

Our results are robust to using these finer control firms who would be most exposed to common, localized political fallout from assassinations. Results (using 7) for this narrow control group definition are presented in Panel B of Appendix Figure D.8.[32] Although this geo-matching shrinks out sample size, we still observe a gradual relative decrease in the CARs for treated firms. CARs are –1.4 percentage points lower, ten days after an event for our baseline specification. Appendix Table C.5 shows that our estimates are broadly negative across specifications and of similar magnitude. For

[29]Note that we adjust the minimum trading days required for this analysis: for companies to be considered, they have to be traded on at least 15 of the 21 days post the event, respectively 15 of the 20 days prior to the event.

[30]Figure D.6 in the Appendix provides further support for the persistence of the publicity effect. Graphing the average CARs for *associated* companies in the 90 days following the event reveals that there is no reversal in the estimated effect even when considering very long-time horizons. Note that D.6 simply displays average CARs when underlying the market model (see Section 3.1), not OLS estimates.

[31]Given the geographic resolution of assassination events and projects in the database, there will be uncertainty in the precision of our "matches" within an Admin1 region. However, even with this uncertainty, we suspect these matches are nevertheless informative. Opposition to mining usually arises due to a local land conflict—such as conflict over indigenous property rights—and these conflicts are geo-spatially correlated.

[32]Note that Panel A displays the estimates for our baseline sample and specification.

the unrestricted specification in column 1, coefficient estimates are precisely estimated and significant at the 5 percent level.

Overall, our results indicate that the negative impact we observe is likely not driven by the proximity to violence and spatial disruptions from these events. Rather, our estimates likely reflect the association of firms with violence in media and NGO reports. Interestingly, Panel C of Figure D.8, moreover, suggests that traders may be well informed about circumstances surrounding events, even where information is more obscure. Using publicized assassination events of activists where no company was linked to the violence in reporting, we rerun our regression analysis for a slightly different set of treatment and control groups.[33] We consider *all* companies within the same *Admin1* region of the event as treated ($D = 1$). All other companies in the event-country are in the control group ($D = 0$). Our results reveal a delayed and subdued reaction of market participants.[34] This behavior is consistent with a lengthier information gathering process and lower expected costs for events that, *ex post*, have no direct company tie.

#### 3.3.3 Synthetic Matching: Robustness of OLS Results Using Alternative Estimator

To account for potentially unobserved differences between treated and control companies not fully captured by fixed effects and firm covariates, we alternatively apply a modified version of the synthetic matching method introduced by Acemoglu et al. (2016). This method allows us to compare the returns for each treated company to the returns of a synthetic match, independent of observed firm characteristics. A synthetic firm's returns are a weighted, convex combination of returns of control companies where the weights are chosen to optimally match pre-event returns for a given company in the treatment group.

For this purpose, we extend the synthetic matching procedure of Acemoglu et al. (2016) to accommodate multiple event dates.[35] We do so by determining a set of control firms for each event date and treatment company combination.[36] The estimated average treatment effect, $\hat{\phi}\,(\tau_1, \tau_2)$, for this method is computed as the average abnormal return for each company in the treatment group weighted by the "goodness" of its synthetic match during the *estimation window*; the abnormal return is the difference between a treated firm's actual return and the return of its synthetic match. To construct

[33]Note that we cannot rule out the possibility that reports establishing a link between an assassination and a mining company or project exist. However, we are confident that the information is—at least—not easily accessible given the extensive time that has been attributed to researching the events.

[34]The estimated coefficients across specifications are presented in Table C.6 in the Appendix.

[35]Note that our modified method also directly deals missing return values and does not rely on the assumption that returns are zero on those missing trading days.

[36]Note that control companies have to fulfill specific trading requirements. Specifically, we require securities to be traded at least 200 out of the 250 trading days during the *estimation window*, and 8 out of the 11 trading days during the *estimation window*. Additionally, control companies must be traded on all days the treated mining company is traded.

confidence intervals, we compute the average treatment effect for 3,425 placebo treatment groups.[37] For details on the modified synthetic matching method, see Section A.2 in the Appendix.[38]

**[Table 4 about here]**

Table 4 reports the results for the event day and the following ten trading days. In line with our baseline findings, the estimated effect of being associated with an assassination events is negative and increasing over the *event window*–with the exception of day 10. While the magnitude of the treatment effect is attenuated in comparison to our main OLS results, all abnormal return estimates lie outside of the interval $[0.005, 0.995]$ for the placebo treatment group and are significant at the 1% level.[39]

# 4 Mechanisms

This section points three economic mechanisms behind our main results above and rejects competing explanations. We highlight the roles played by (i) media information dissemination, (ii) reactions by institutional investors, and (iii) the loss of potential purchasers in the global supply chain. Summarizing our results: First, we show the importance of media and how it effects the likelihood that events reach investors. Second, we show investors whose strategy is more sensitive to negative news quickly decrease their holdings in stocks to avoid short-term losses. We then consider economic rationales behind these these negative reactions, and show the loss of (mostly Western) customers in the supply chain after assassination events. Thus, we do not find support for non-pecuniary mechanisms. In addition, this section shows that assassinations do not lead to disruptions of domestic operations (Section 3.3.3), and instead emphasize the material impact through the supply chain. We also rule out the impact of changing ESG scores and the potential of future legal costs.

## 4.1 Mechanisms: The Role of the Media

The role of the media in this context is twofold. First, journalists are often among the first to shed light on activist assassinations and media outlets communicate these stories to a broader audience. In most cases, media outlets are also the main source of information for investors about these types of market-relevant events. Second, increased media attention for an assassination event, leads to more negative publicity and a higher chance of public backlash against the resource company tied to the

[37] 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.

[38] We provide an implementation of the modified synthetic matching method in the `synthReturn` **R** package: https://github.com/davidkreitmeir/synthReturn.

[39] Figure D.7 in the Appendix graphically illustrates the size of actual treatment effect relative to the distribution of placebo treatment effects.

assassination. As such, media attention for an event can be one of the main mechanisms driving our baseline results.

A major empirical challenge in analyzing the media channel is that proxies for media attention are likely to be endogenous. For example, simply counting the number of news reports about a particular assassination may reflect media attention, but may also be driven by negative stock market reactions to the assassination event. Instead of relying on endogenous measures of media attention, we consider an exogenous variable which influences the likelihood that an assassination receives media attention. Following the literature on media economics, we use the daily news pressure index of Eisensee and Strömberg (2007).[40] We expect the likelihood of an event getting reported on, or gaining attention, to be lower if it coincides with a "high news pressure day".[41]

To study the role of media, we expand our baseline regression model and consider the following specification:

$$CAR_{ie}\left(\tau_1, \tau_2\right) = \alpha + \alpha^N N_e + \delta D_{ie} + \delta^N D_{ie} N_e + \boldsymbol{X}'_{ie}\phi + \gamma_e + \epsilon_{ie}. \tag{8}$$

where we add an interaction term between the treatment indicator $D_{ie}$ and a dummy variable $N_e$, equal to one if the event falls on a high news pressure day, and zero otherwise. We allow for different intercepts of high and low news-pressure days to account for generic differences in trading behavior on high news pressure days.

**[Figure 9 about here]**

Figure 9 plots the influence of news pressure on market reactions across three different measures of "high news pressure" days. In Columns 1 and 2, a high news pressure day is is defined as an above median, and above 75th percentile news pressure day for the Eisensee and Strömberg (2007) news pressure index for the period from 1998 to 2018, respectively.[42] The red line in the top panel corresponds to the cumulative abnormal return if the event day coincides with a high news pressure day ($\delta + \delta^N$), whereas the black line captures the effect of being tied to an event if the assassination

[40] Eisensee and Strömberg (2007) define *daily news pressure* as the median number of minutes a news broadcast devotes to the top three news segments in a day. For more details on the construction of the daily news pressure see Eisensee and Strömberg (2007) Section II.C and Appendix V.B.

[41] Even if investors obtain information from private sources, news pressure should serve as an indicator for the information demand of financial markets on the event day. For instance, news about natural disasters or recession forecasts are likely to both dominate trading behavior and feature in the top news segments. In contrast, assassinations of mining activists are highly unlikely to be broadcast in the top three news segments, making daily news pressure an arguably exogenous measure in our setting.

[42] Recall, 1998 constitutes the first year in our assassination dataset and 2018 is the last year for which the news pressure is available.

date falls on a low news pressure day ($\delta$). The bottom panel graphs the difference in estimated effects ($\delta^N$). Each panel reports 95% confidence intervals.

Results show a significant and continuous decline in abnormal returns for associated companies if the assassination event falls on a low news pressure day, whereas the coefficient is indistinguishable from zero if the event coincides with a high news pressure day. The bottom panel plots the gradual divergence in cumulative abnormal returns. By Day 10, the difference in cumulative abnormal returns between above and below median days is 3.9 percentage points and significant at the 5% level in Column 1. A qualitatively similar pattern is observed if the event falls on an above 75th percentile news pressure day in Column 2. Quantitatively, the divergence is less precisely estimated and slightly attenuated in this more demanding specification.

A potential concern is that perpetrators may strategically time their attacks to fall on high news pressure days to minimize public scrutiny. To address this concern, we base our indicator variable on a "disaster predicted news pressure" index. Based on the empirical strategy outlined in Jetter and Walker (2022), we regress the daily Eisensee and Strömberg (2007) 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 US 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.[43] The disaster coefficient estimate is 0.247 and significant at the 1% level using Newey–West standard errors (with a lag of one day). We use these parameter estimates to predict the news pressure on any day $t$ over the sample period from 1998-2018 and create an high news pressure indicator using only statistically unpredictable variation in news pressure. Estimates in Column 3 are quantitatively and qualitatively stable when the event coincides with an above 75th percentile *predicted* news pressure index day. These results reinforce our findings for the media mechanism above.

Our media results are robust to a number of checks. Figure D.9 in the Appendix presents further sensitivity checks for our baseline results. Column 1 and 2 show that our results 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.[44] Moreover, the estimates are qualitatively similar if we estimate equation 8 for the control group set

[43] For disasters in the EM-DAT database to be included in the analysis, they need to have information on the start and end date of the disaster and 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. For more details on conditions and host country sample definitions, see Jetter and Walker (2022).

[44] Eisensee and Strömberg (2007) note that media market integration increased the availability of breaking news stories, visible in the slight upward trend in the daily news pressure for the 1968–2003 period.

restricted to companies active in the Admin1 region of the assassination event (Column 3 and 4). However, the loss in sample power results in less precisely estimated coefficients.

Next, we turn to a related question. Can more transparency in the event country's mining industry support human rights organizations—in cooperation with media outlets—in their mission to hold corporations accountable for misconduct? We address this question by interacting our assassination indicator with a dummy variable for event country membership in the Extractive Industries Transparency Initiative (EITI) at the time of the event. The EITI initiative commits member countries to fully disclose taxes and payments made by mining companies to the government and serves as an indicator for quality of governance (Fukuyama, 2016).[45] For instance, Berman et al. (2017) find that EITI membership reduces the likelihood of mining-related, localized conflicts in Africa. The results in Column 1 of Appendix Figure D.10 provide further support for the notion that transparency can amplify the publicity effect on associated multinationals' stock value. Published assassination events that occurred in a country which was an EITI member at the time of the event have a relatively stronger, negative effect on the associated mining company's market value compared to events that happened in non-EITI member countries.

## 4.2 Mechanisms: Information-Driven Institutional Investors React Negatively

The previous sections showed that investors react negatively to assassination events. Next, we investigate whether institutional investors—sophisticated, informationally sensitive "big players" (e.g. Puckett and Yan, 2011; Hendershott et al., 2015)—respond to reports of human rights violations for companies in their portfolio. For instance, Mccahery et al. (2016) report that socially "irresponsible" corporate behavior is considered a very important trigger for shareholder activism as evidenced by 72% of the surveyed 143 institutional investors.[46] Large scale sovereign wealth and pension funds have been early adopters of ethical investment policies and are increasingly divesting from stocks that do not meet certain ESG criteria.[47] Therefore, certain types of institutional investors could be more responsive to activist assassinations and sell off their holdings in the companies tied to the event.

We examine the relation between news about company's reported "ties" to assassination events and consider the event study specification from Cengiz et al. (2019). This strategy accounts for multiple

[45]Data on "join" and "leave" dates of member countries is retrieved from the EITI API version v2. For more details on EITI, see Section 5 and https://eiti.org/ .

[46]Other recent studies (e.g. Dyck et al., 2019; Chen et al., 2020) have also found evidence for a positive relationship between institutional ownership and corporate social responsibility scores.

[47]See for example "Norway prepares to dump up to $3.7b in Aussie shares", Jun 13 2019, Australian Financial Review, or "Norwegian wealth fund blacklists G4S shares over human rights concerns", Nov 14 2019, The Guardian.

events per unit of observation in the panel and non-continuous treatment. In particular, we estimate,

$$IO_{it} = \alpha + \sum_{\tau=-4}^{4} \delta_\tau D_{it}^{\tau} + \boldsymbol{X}_{it}'\phi + \gamma_i + \lambda_t + \epsilon_{it}, \tag{9}$$

where $IO_{it}$ is the ratio (as percent of market capitalization) held by institutional owners for quarter $t$ of company $i$. The "treatment" dummy $D_{it}^{\tau}$ equals one if company $i$ was associated with (at least) one assassination event $\tau$ quarters from quarter $t$. This definition implies that $\tau = 0$ represents the quarter in which an event took place. Following Dyck et al. (2019), we control for a set of annual firm characteristics $\boldsymbol{X}_{it}$—i.e. size, asset tangibility, leverage, Tobin's q, and profitability lagged by one year. Our benchmark specification also accounts for company and quarter fixed effects, $\gamma_i$ and $\lambda_t$. We cluster our standard errors by company.

We use the estimated parameter $\delta_\tau$ from Equation (9) to calculate the change in the holding position of institutional investors in response to the assassination event. The difference-in-differences estimate between event date $-1$ and $\tau$ can be calculated as $\delta_\tau - \delta_{-1}$.

Data on institutional ownership comes from the Factset Ownership database. Factset has been widely used in empirical finance (Aggarwal et al., 2011; Dyck et al., 2019) and reports institutional investors' equity holdings collected globally from fund reports, regulatory authorities (e.g. 13F reports in the United States), fund associations, and fund management companies.[48]

Importantly, Factset data spans 2000 to 2017, at quarterly frequency, and covers 83 out of the 87 traded mining companies associated with assassination events (153 event-company pairs) in our sample. For those companies in this data, the average probability of an assassination event in a given quarter is 3.6 percent. For our baseline estimates, the set of control companies includes all corporations for which data is available throughout the entire sample period. We show in the Appendix that our results are robust to different control group specifications: *e.g.*, if we restrict the control group set to firms active in the "extractive" sector (Figure D.12), or draw a random sample of firms *not* active in the "extractive" sector (Figure D.13).[49]

**[Figure 10 about here]**

Figure 10 shows that the total institutional ownership share, on average, does not significantly change in response to assassination events in the quarter of the assassination event. However, the estimated

[48] In particular, 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 obtained from the *Factset Fundamentals database*.

[49] We define corporations as operating in the "extractive" sector if their TRBC Business Sector classification is "Energy - Fossil Fuels", "Uranium" or "Mineral Resources". These corporations are excluded *prior* to randomly drawing 1000 corporations for the second robustness control company set.

coefficient decreases over time and becomes statistically significant three quarters after the event. A possible explanation is that average institutional investors with holdings in mining companies follow a more long-term strategy and are less responsive to short-term events.

Institutional investors differ in their objectives and investment strategies (*e.g.* pensions versus hedge funds), which may lead to differences in the timing and magnitude of investor's reaction to assassination events. With this in mind, we disaggregate our data by type of institutional investor and separately re-estimate specification (9) for each. The results are presented in Figure 10. Overall, we find that hedge funds, investment companies and investment advisers, decrease their holdings in associated mining companies in the quarters following an assassination. The estimated coefficient is only statistically significant for hedge funds which decrease their average holding position over the first two quarters by about 12.1 percent compared to their holding position in the quarter before an event.[50] These results are consistent with the literature on hedge fund strategy which shows that hedge funds have a shorter investment horizon (Cella et al., 2013), are more inclined to monitor corporate behavior and respond rapidly to costly information disclosure (Gargano et al., 2017), in particular, following revelatory news events (Huang et al., 2020). Relative to other types of institutional investors, investment companies and advisors are also more likely to apply an event-based trading strategy, and thus are more likely to react to singular news events.

In contrast, institutional investor types with a long-term, strategic view of investment portfolios, such as pension funds or insurance companies, do not seem to systematically change their holdings for mining companies in the aftermath of an assassination event. Results are quantitatively unchanged if we account for overlapping event windows (Figure D.11).[51]

These results may appear surprising because among institutional investors, pension funds are often considered to engage companies on human rights issues and make public statements to divest from mining companies accused of environmental and human rights misconduct. There are a number of reasons, why our results differ from this public perception. First, although the sector has been acknowledging that it has responsibility to acknowledge human rights in their investment decisions, more credible public commitments have been a very recent development.[52]

[50]Note that $\frac{0.0025}{0.0206} \times 100 \approx 12.1\%$.

[51]To account for overlapping event windows we include an indicator variable taking the value 1 for the 4 quarters after an event if company $i$ expires another event within that 4 quarter period—a strategy also applied by Dube et al. (2011) to control for the fact that the current investor reaction might partially capture the response to a previous event when the events are sufficiently close together.

[52]In 2019, the National Council in Switzerland voted to support a bill introducing a broad mandatory human rights due diligence regime that received backing from 27 institutional investors representing over US$808 billion in assets under management. Investors representing over US$4 trillion assets under management filed multiple statements in support of the Australian Modern Slavery Act in 2018. In 2018, more than 70 large Dutch pension funds with combined assets of almost €1.2 trillion signed a covenant committing to worldwide cooperation aimed at promoting sustainable investment based on respect for human and labor rights. In late 2018, investors representing over US$5 trillion in assets under management

Second, despite these written commitments, even investors who actively engage companies on human rights issues appear to be reluctant to set clear timelines for divestment even in cases where investee companies contributed to severe human rights abuses over an extended period of time (UN Working Group on Business and Human Rights, 2021). It is possible that the few cases where institutional investors divested from companies accused of human rights violations are exceptions, and divestment is a tool of last resort among a variety of strategies to engage with investee companies on human rights issues.[53] Third, information costs and lack of internal capacity makes it often difficult for some institutional investors to obtain reliable information human rights violations and formulate a change in a funds long-term investment strategy based on individual incidents (Business and Human Rights Clinic, 2018). As a result, investment managers may also rely on external indicators about a company's corporate social responsibility.

### 4.3 Mechanisms: Events Lead to a Loss of Supply Chain Contracts

What may drive the negative responses from investors? We now investigate whether the loss of contracts and customers in the aftermath of assassinations is one of the fundamental factors guiding financial market reactions. Recall, our robustness section (3.3.3) showed transitory, weak effects of assassination events on conflict around mining operations. Beyond domestic operations, we now consider the effects along the supply chain.

We consider supply chain data at the corporate customer-supplier level from *FactSet Revere* with firm fundamentals from *Worldscope* and our event data set. With this data, we employ a difference-in-difference estimation strategy to estimate the impact of assassinations on supply chain contracting. This difference-in-difference specification is motivated by *(i)* the presumption that "human rights conscious" customers should not expand their business with mining corporations after they have been publicly tied to assassinations. Although those customers could have a "short memory", the majority of treated corporations in the sample are named in association with deadly violence against activists in multiple years, either consecutively or with only short spells between event-years. Thus, we make the conservative assumption that corporations are continuously treated after the first year of being tied to a killing and focus on an event window of four years prior and two years post the event. This allows us to have a sufficiently long pre- and post-period to both, *(i)* test for pre-trends

called on the US Securities and Exchange Commission to mandate corporate disclosure of ESG, including human rights information (see "The Investor Case for Mandatory Human Rights Due Diligence.", April 21 2020, Investor Alliance for Human Rights, https://investorsforhumanrights.org/sites/default/files/attachments/2020-04/The%20Investor%20Case%20for%20mHRDD%20-%20FINAL_3.pdf).

[53]For example, in December 2019, the largest Danish pension fund, ATP, decided to divest 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. "ATP resorts to DKK13m divestment after Mexican mining giant fails to engage.", Jan 31 2020.

and *(ii)* capture potential treatment dynamics, while safeguarding against the risk of importing confounding events over a longer period. This is in line with recent work by Darendeli et al. (2022) on the relationship between corporate social responsibility and supply chain contracting.

We consider a two-way fixed effects model for firm $i$ and calendar year $t$:

$$\begin{aligned} C_{it} =& \gamma_i + \lambda_t + \alpha \sum_{\tau<-4} D_{it}^{\tau} + \sum_{\tau=-4}^{-2} \delta_\tau D_{it}^{\tau} + \sum_{\tau=0}^{3} \delta_\tau D_{it}^{\tau} \\ &+ \beta \sum_{\tau>3} D_{it}^{\tau} + \boldsymbol{X}_{it-1}'\phi + \theta EC_{it} + \epsilon_{it}, \end{aligned} \tag{10}$$

where $C_{it}$ is either the number of new contracts or the number of new corporate customers of mining firm $i$ in year $t$. The term $D_{it}^{\tau}$ is an indicator of firm $i$ being $\tau$ periods away from initial treatment with $\tau = -1$ being the excluded relative time period. The set of firm-specific, time-varying controls is $\boldsymbol{X}_{it-1}$.[54] We also include the number of contracts to expire in year $t$, $EC_{it}$, to control for the independent effect of re-contracting on our outcome variables.

To avoid issues with two-way fixed effects estimators and staggered treatment adaption highlighted in the literature (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021), we employ the methodology suggested by Sun and Abraham (2021) to estimate the treatment effect for each cohort of corporations with the same initial treatment year and average the event-specific treatment effects across event-cohorts.

In this exercise, our control group comprises a set of never-treated companies in the mining sector for which information on supply chain contracting was available during the sample period from 2003 to 2019.[55] In total, our unbalanced panel comprises 36 treated and 110 control mining corporations.[56]

**[Figure 11 about here]**

Figure 11 presents are results for eq. (10). Column 1 gives the estimated impact on the total number of *(i)* new contracts (first row) and *(ii)* new customers (second row). The total average impact of assassinations is insignificant for the number of new contracts and new customers, though slightly negative for the latter.

[54]In specific, the firm-fundamentals identified in the literature (e.g. Darendeli et al., 2022) comprise a squared term of firm size (log of total assets), leverage (total debt to total assets), Tobin's Q, operating profitability (market capitalization of equity plus total debt divided by total assets), sales growth (change in revenues), investment intensity (roperty, plant, and equipment scaled by total assets), institutional ownership share, as well as cash (cash from operations scaled by total assets and constraints) and financing constraints (Whited and Wu (2006) financial constraints index).

[55]We require that control corporations have at least 7 years of non-missing information, i.e. the time of the event-window.

[56]For this analysis, we exclude treated corporations which are not primarily situated in the mining sector such as Barclays whose supply chain contracting as a bank is fundamentally different to the ones of mining corporations–the focus of this study. The results are qualitatively unaltered if we do not make this sample restriction (results not reported for brevity but available upon request).

The results are different, however, if we consider only new supply chain relations with customers from countries with high human rights protection and awareness. Column 2 of Figure 11 shows that the number of both, new contracts and customers, from countries with strong human rights protection—measured as a country being in the upper quartile of the *V-Dem Civil Liberties* index (Coppedge et al., 2022)—decreases significantly.[57] Relative to the mean, mining corporations caught in the human rights spotlight saw a reduction in new contracts of 32% and a loss of clients of 39% from these countries.[58] Since this specification chiefly considers the defection of customers from developed countries in North America and the European Union, our results suggest that the change in the customer base of treated firms significantly affects their future revenue prospects and economic health.[59]

We perform two main sensitivity checks: First, we use an alternative indicator for the level of human rights protection and awareness: *V-Dem Civil Society Organization (CSO) Repression* (Coppedge et al., 2022). Figure D.14 in the Appendix shows that customers from countries with the lowest repression of CSOs (bottom quartile) significantly defect after assassination events. Second, we conduct a placebo test where we replace our existing outcome variables with the annual number of expired contracts. Given an average contract period of on average 360 days in our sample, it is unlikely that customers would opt for the costly preliminary termination of contracts that would otherwise expire automatically (Darendeli et al., 2022). As such, we do not anticipate that assassination events have a systematic impact on the quantity of expired contracts in the event year. This is confirmed by the results in Figure D.15 in the Appendix, which shows that assassination events do not possess a significant effect on the number of expired contracts of the associated mining company in the event year.

## 4.4 Mechanisms: Assassinations Do Not Change Corporate Social Responsibility Scores

Do ESG scores react to the human rights events in our study? Investment managers and institutional funds may rely on external ESG indicators as a source of information about human rights violations of a company. We examine if the long-term reaction of institutional investors is itself a reaction to changes in environmental and social performance (E&S) indicators. To do so, we obtain data on firms' E&S performance from the Thomson Reuters ASSET4 ESG database. Information is acquired

[57]Results are robust to extending the sample to customers from countries with an above median civil liberties score (column 1 in Figure D.14 in the Appendix).

[58]Note that that the pre-trend F-test suggested by Sun and Abraham (2021), that all all of the coefficients on the pre-event relative time indicators are jointly zero, cannot be reject at the 10% level for any of our presented specifications.

[59]Note that the exact value of each contract is not available in the *FactSet Revere* database.

from stock exchange filings, CSRs, annual reports, non-government organization websites, and news sources for large, publicly traded companies at *annual* frequency for the period 2002–2019. The data covers 46 firms experiencing at least one event during this period, and in total 104 event-years.

Our baseline tests examine the relation between assassination events and E&S performance using the specification:

$$Log(Score_{it}) = \alpha + \delta D_i + \boldsymbol{X}'_{it-1}\phi + \gamma_i + \lambda_t + \epsilon_{it}, \tag{11}$$

where $Log(Score_{it})$ is the log (plus one) of the environmental or social scores of company $i$ in year $t$, $D^{\tau}_{it}$ 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 $\gamma_i$ and $\lambda_t$ are year and company fixed effects, respectively.[60]

**[Table 5 about here]**

We report the results in Table 5. 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 particularly discarded (ESGC score) as provided by Thomson Reuter. In Columns 3 and 4 we focus on the ESG categories which should be most impacted by the events in our data: human rights and community scores. For either category, we do not find a significant impact of our events on the score. While Thomson Reuter use rank-based scores relative to all other companies for categories, Dyck et al. (2019) rely on indicator based scores.[61] Columns 5 and 6 present the results when underlying their scoring method. No significant effect is detected but the negative sign of the estimated coefficients is in line with expectations. The results are qualitatively unchanged when we account for the potential impact of institutional investors on ESG scores (c. Dyck et al., 2019) by including the total institutional owner share at the end of year $t-1$ as an additional control variable (Table C.7).

Our results mirror survey responses Business and Human Rights Clinic (2018) by institutional investors about the human rights information in external ESG indicators. The responses reveal a concern in the industry that ESG indicators often lack coverage of large companies operating in emerging markets. In some instances, responsible investment managers often have to work directly with NGOs to receive information about human rights violations. One interviewed investment manager even stated that civil society accounts of companies' activities are a "fundamental component

[60]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.

[61]Details on the calculation of the category scores in the manner of Dyck et al. (2019) are presented in Section B.2 in the Appendix.

of his organization's tools for ensuring that they invest responsibly" (Business and Human Rights Clinic, 2018, p. 10).

## 4.5 Mechanisms: Direct Legal and Financial Costs Are Unlikely

Could legal costs also explain the potential effects? We now turn to this potential mechanism, and are skeptical of investors expecting costly legal fallout from human rights violations. In more developed economies, there are prominent examples where companies have faced large fines due to environmental or social misconduct. However, this has not been the case for violations in developing economies. Consider the events in this study. We were unable to find evidence that any of the events in our sample resulted in convictions or meaningful legal fines for the associated companies occurring during our sample period[62].

More formally, we conduct an empirical analysis to see if the effect of being associated with an activist's assassination varies by the likelihood of being indicted for the human rights violation and/or face legal consequences. We combine our event level data with data on the quality of the judicial system in a country ("Law and Order") from The International Country Risk Guide (ICRG) from *The PRS Group*. In particular, we access information about the quality of the judicial system both for the mining company's HQ country and the country in which the assassination took place. Intuitively, if investors expect that the association with an activist assassination will result in a legal indictment and subsequent financial losses due to fines, the effect of the event on the associated company's $CAR$ should increase in the quality of the (HQ and event) country's judicial system.

Our empirical analysis applies a similar logic as specification (8) in section 4.1. To analyze if the effect of assassination varies by the HQ (event) country's quality of the judicial system, we include an interaction term between the treatment indicator $D_{ie}$ and a dummy variable that switches to one if the HQ (event) country's quality of the judicial system is high, and zero otherwise.

The results using the quality of the judicial system in the event and HQ country are presented in columns 2 and 3 of Figure D.10 in the Appendix, respectively. In both cases, we do not find

[62] Court cases against multinational mining companies to hold them legally responsible for human rights violations are only a very recent phenomena. In a landmark case in 2019, Canadian mining company Tahoe Resources Inc. admitted that it "infringed the human rights" of protesters when security guards opened fire to break them up on April 27, 2013, (*The Conversation*,15 August 2019) after the Supreme Court had declined to hear similar cases in the past (*The Guardian*, 28 February 2020). During 2019 the UK Supreme Court ruled that Vedanta Resources was (potentially) liable for misconduct of its Zambian subsidiary KMC, in particular if "there are serious obstacles to claimants obtaining justice in their domestic jurisdictions" (*Morrison & Foerster*, 8 June 2020). The Thai Appeal Court decision in 2020 to allow against Asia's largest sugar producer, Mitr Phol., moreover, paved the way for Asia's first transboundary class action on human rights abuses (*Forum Asia*, 31 July 2020). To speak of an international trend, however, is premature, as the hurdles to hold multinationals accountable for human rights violations remain high, as exemplified by the rejection of the case against UK based African Mine Ltd. for excessive force by Sierra Leonean police in relation to its iron ore mine in Tonkolil (*Morrison & Foerster*, 8 June 2020).

any evidence that the magnitude of the assassination effect differs by the likelihood of facing legal challenges and fines (as proxied by the quality of the country's judicial system). Taking into account the lack of evidence for legal indictments resulting in any substantial penalties for the associated mining companies, we suspect that investors' reactions are not driven by expectations about future legal indictments for human rights violations.

Noteworthy, the human rights spotlight may, however, in isolated cases induce governments in the event country to apply other forms of punishment that can have severe financial implications for the associated mining companies. For example, in the case of Bear Creek Mining Corp.'s operations in southern Peru the public outcry over the killing of five demonstrators induced the Peruvian government to revoke the mining concession granted to Bear Creek.[63] This became public knowledge and is most likely the reason for the 43.6 percentage point drop in the Bear Creek's stock in the days following the event.

## 4.6 Mechanisms: Events Do Not Inspire Costly Local Opposition

Do assassinations, and the resulting media spotlight, provoke protest and, thus, costly disruptions to activity? The killing of mining activists may ignite even further backlash from the local community and increase opposition against a mining project. Protests, non-violent or violent, against a company's operations can lead to temporary disruptions in resource extraction, interruptions in logistics, physical damages to assets, or even a permanent shut-down of the operation. As such, the negative CAR in the subsequent days and weeks might not only be a result by civil society's informational campaign, but other, local anti-mining activities that have direct costs for the associated company.

To examine the role of this local opposition channel, we construct a new dataset at the ADM1 and day level for the period 1998 to 2019 for countries where we observe at least one assassination event in association with a publicly traded company over this period. The resulting data set is a balanced panel for each day $t$ and 486 ADM1 regions, $i$ from 21 countries, $c$. For this sample we have 161 event-ADM1-day pairs, which will be our treatments.

Data on local protests comes from the Mass Mobilization Data Project (Clark, David and Regan, Patrick, 2016). This dataset contains more reliable information about protests than other data sources and also has an almost global coverage. Importantly, the project's dataset codes the type and cause of the protest. Unfortunately, location information only contains string variables of the name of the location in an unstructured form with entries varying between city, district and state, among others.

[63] In 2011 Canadian based Bear Creek Mining Corp. faced local opposition by Aymara Indian activists against the company's planned Santa Ana silver mining project in the Puno region of Peru.

Thus, we used the GeoNames API[64] combined with manual matching to assign each protest to the ADM1 polygons used in our study.

To formally study the role of protests, we consider the following specification:

$$P_{it} = \alpha + \sum_{\tau=-10}^{10} \delta_\tau D_{it}^{\tau} + \phi_{cmy} + \gamma_i + \lambda_t + \epsilon_{it}, \tag{12}$$

where $P_{it}$ is one on day $t$ when we observe at least one protest in the Admin1 region $i$ of country $c$ and zero otherwise. We also consider the probability that a protest breaks out or ends. In this case, $P_{it}$ takes the value one if a protest starts (ends) on Day $t$ in Admin region $i$, episodes without a protest are coded zero from the beginning till the end, and ongoing protest days are coded as missing values (c. for instance Collier and Hoeffler, 2004). The "treatment" dummy $D_{it}^{\tau}$ equals 1 if the Admin1 region $i$ experiences (at least) one assassination event $\tau$ days from date $t$. This definition implies that $\tau = 0$ represents the day on which an event took place. Our specification accounts for region and day fixed effects, $\gamma_i$ and $\lambda_t$, as well as for country $\times$ month $\times$ year fixed effects ($\phi_{cmy}$). Specifically, we use within country-month-year variation when estimating the linear probability model.

The estimated coefficients of $\delta_\tau$ from equation are presented in Figure D.16 in the Appendix. Each cell corresponds to a different dependent variable with column labels depicting the type of protest and rows capturing if the dependent variable considers incidence, start, or the end of protests.

We find that protest activity precedes the assassination events; after the event there is uptick in protest activity, but this is transitory. The plots in the first row indicate that protest incidence is higher in the days prior and the day of the event, although the coefficients are not statistically significant. This is in line with anecdotal evidence that increased protest activity precedes the assassination event.[65] In the first day after the killing, the incidence of local protests significantly increases. Again, this is in line with anecdotal evidence that the local population mobilizes in the immediate aftermath of the killing of one of their community members. However, the incidence rate immediately drops after that and stays close to zero.

The second row of Figure D.16 shows that the likelihood of new protests is higher (although not statically significant) 5-6 days *prior* to the assassination event. The probability increases on the event day and the following day, but drops to zero on the day after that and remains at zero from the remaining time. This result is also reflected by the plots in the third row, which plot the likelihood

[64] http://www.geonames.org/source-code/javadoc/org/geonames/WebService.html

[65] Recall, as we do not find any pre-trends in our main results, the stock price does not seem to systematically reflect this increased protest activity.

that a protest episode ends. We find that protest episodes systematically end on the day after the event.

Taken together, these results reveals that the killing of an activist results in a very-short term, one day, protest by the local community but protest activity subsides quickly. The trend in protests is in contrast to the steady decline in the abnormal return for companies ten days following the assassination. We interpret this as an indication that the negative reactions of the stock market are not due to increased local protest mobilization that might interfere, directly, with the company's mining operation.[66]

# 5 Why Might Events Persist? The Political Economics of Local Rents and Assassinations

Given the economic losses associated with assassinations, why might this issue persist? Multinationals are sprawling and complex, and the boundaries of the multinational are blurry. Their upstream operations may be operated by actors whose incentives differ from their owners. Local parties benefiting from mining projects may have an incentive to suppress or eliminate opposition, and may not bear the loss that shareholders do. Domestic governments and state-aligned paramilitaries are examples of such agents, where uninterrupted production will result in higher royalties and tax revenues. The relative gains of engaging in malfeasance can be higher for mining operations that earn significant revenue from mining projects. If so, the likelihood of observing an assassination should be increasing in the share of taxes paid to a host-country.

We explore this equilibrium by constructing new data on the local public finance of mining companies, which allows us to explore the relationship between violence and local rents. We construct this data by hand from reports published by the Extractive Industries Transparency Initiative (EITI), an international civil society organization. We use these reports to construct annual measures of tax shares paid to host governments by mining firms. As members of the EITI, nations commit to disclosing payments from local companies to the government; these revenue streams ordinarily cover payments from subsidiaries and joint venture. We consequently must determine the ownership structures to match EITI records with our assassination event data set.

Thus, we hand-code ownership shares using information published in annual reports of publicly traded companies and—if not available—we rely on Bureau van Dijk's *Orbis* database. As a convention,

[66] Figure D.17 in the Appendix presents the estimated effect relative to the day before the assassination event.In particular, the difference-in-differences estimate between event date $-1$ and $\tau$ is calculated as $\delta_\tau - \delta_{-1}$.

private companies are coded as their own owner, i.e. we do not account for ownership shares of private individuals. Tax revenues of subsidiaries and joint ventures are distributed to owners in accordance with their shares.[67] For instance, in 2014, *Anglo American* owned 81.90 percent of the Peruvian mining company *Anglo American Quellaveco S.A.* and *Mitsubishi* owned 18.10 percent. Consequently, 202,232 USD of the 246,925 USD in taxes and royalties to the Peruvian government are attributed to *Anglo American* while the remainder is attributed to *Mitsubishi*. For each country-year pair (report), revenues are subsequently aggregated at the owner-level and divided by the total amount of tax revenues from the mining industry to obtain the *tax share*. Summary statistics on the tax shares, disaggregated by event-country are presented in Table C.10 in the Appendix.

We code ownership shares for all years available in the EITI database for countries which have experienced at least one assassination event in the past. One caveat with EITI records is that they cover a limited set of countries and years in our assassination data set. It is worth noting, that we are not limited to public companies in this analysis. Thus we are able to additionally match private companies to assassinations in our data. For completeness, we retain potentially interesting cross-country variation and do not exclude countries if an assassination event falls outside of EITI coverage period.

We then use a linear probability model to estimate the relationship between a company's tax share and the likelihood to observe an assassination event. Specifically, we estimate the following:

$$\mathrm{K}_{icy} = \beta_1 \mathrm{T}_{icy} + \gamma_{cy} + \epsilon_{icy}, \tag{13}$$

where $\mathrm{K}_{icy}$ 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 $\mathrm{T}_{icy}$ corresponds to the tax share of company $i$ in country $c$ in year $y$. In our baseline specification we include country $\times$ year effects ($\gamma_{cy}$), which account for time-varying economic and political developments in countries. Standard errors are clustered at the company $\times$ country level.

**[Table 6 about here]**

Table 6 shows a positive relationship between the tax share and the probability of observing an assassination event. Column 1 presents the significant and positive unconditional correlation coefficient of 13.8 percentage points. The magnitude of the coefficient increases to 17.4 percentage points after we account for differences in the mining industry and average prevalence of assassinations with country

[67] Note that we account for changes in ownership shares over time as well as for acquisitions.

fixed effects (Column 2). Adding year effects has little effect on our estimates. This seems reasonable, as the tax share is—by construction—expressed in relative terms.

According to our preferred specification (Table 6 Column 4), a hypothetical mining company (and sole tax payer) is associated with an 18 percentage points higher probability of an assassination. This translates to an average effect of about 1.1 percentage points, as the average tax share in the sample is 5.9 percent, which constitutes a 26 percent increase in the average probability of observing an event.[68]

Nevertheless, these estimates should be interpreted with care. This exercise presents correlations and we cannot entirely rule out confounders. For example, journalists may have an incentive to report assassinations related to notable companies, or those that are most compelling to readers. Since tax revenues are expected to be proportional to the value of projects owned by companies in the event country, reporting could simultaneously increase with the tax share. We attempt to deal with reverse causality in Appendix Table C.11. Across specifications, we show an insignificant, positive impact of assassination events on the change in tax shares (Δ Tax Share).

## 6 Conclusion

We study the power of human rights publicity and its impact on multinationals. To answer this question, we turn to prominent, well-publicized events at the heart of current advocacy: the assassination of environmental activists. We turn to the sector at the center of this conflict, the global mining sector. Our study evaluates the impact of publicizing human rights violations on value of the multinational companies connected with violations. To do so, 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 daily stock market returns of those companies and use event study methodology to estimate the effect of the killings on the abnormal daily returns of companies' stocks.

Our results show that publicity of human rights abuse has substantial negative effects on multinationals. Following assassinations, we estimate significant negative abnormal returns for firms associated with these events. Negative effects appear on the (trading) day after an assassination occurs. These negative effects are amplified for the ten days after the event, and continue. 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 statistically

[68] The average probability to observe an assassination event in the sample is 4.16 percent.

significant lower returns, with a cumulative median loss of over USD 100 million in the 10 days following the event.

We highlight the critical role of the media in this context, 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 human rights events varies over the news cycle. Using daily "news pressure" data (Eisensee and Strömberg, 2007), we find that the penalty of human rights news is mediated by the strength of the new cycle. Furthermore, we show the visibility of the multinationals in this coverage—that is, the firm names—also matter. 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—though *not* named in media coverage—do not experience significant penalties relative to companies explicitly named in the media. The human rights tactics of "naming-and-shaming" may thus carry currency.

Accordingly, informationally-sensitive institutional investors may play important roles in our effects. We find institutional investors that follow event-based trading strategies—such as hedge-funds—divest in mining companies after assassination events. These results dovetail with work on the role institutional investors play 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 human rights protection systematically reduce their contractual relationships with the mining companies named in 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 have in fact an impact on multinational corporations, by making ongoing societal conflict more salient. Being linked to human rights abuses can significantly influence a company's stock 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. In particular, as institutional investors incorporate ESG events into their portfolio. Some blame the lack of quantitative evidence of the financial implications of human rights violations as the main barrier to elevate 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 associated company's valuation. This can help investment managers to better incorporate human rights impacts in their long-term investment strategy and might ultimately help that stock prices better reflect human rights violations associated with the underlying 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: Summary Table - Financial Data

| Group | Variable | Mean | St.dev. | Min | Max | Observations |
|---|---|---|---|---|---|---|
| Treatment | Raw return | 0.0007 | 0.0029 | -0.0041 | 0.0233 | 171 |
| Control | Raw return | 0.0011 | 0.0038 | -0.0593 | 0.0476 | 4692 |
| Treatment | Abnormal return | -0.0002 | 0.0010 | -0.0044 | 0.0043 | 171 |
| Control | Abnormal return | 0.0000 | 0.0014 | -0.0317 | 0.0149 | 4692 |
| Treatment | Size | 15.2984 | 2.6828 | 7.7807 | 20.6501 | 166 |
| Control | Size | 12.9156 | 3.2477 | 4.3307 | 20.6965 | 4512 |
| Treatment | Leverage | 0.0022 | 0.0019 | 0.0000 | 0.0072 | 165 |
| Control | Leverage | 0.1907 | 0.5757 | 0.0000 | 16.8088 | 4124 |
| Treatment | Profitability | -0.0002 | 0.0046 | -0.0440 | 0.0063 | 162 |
| Control | Profitability | -0.2659 | 2.2810 | -51.3538 | 17.9823 | 4389 |

*Notes:* Raw and abnormal returns for each security are previously averaged over the period from $\tau = -280$ to $\tau = +20$. Firm characterisitcs - i.e. size, leverage, and profitability - are based on the values in the event year.

Table 3: The Effect of Assassinations on Stock Returns

| | Mean | SD | p-value | | | |
|---|---|---|---|---|---|---|
| | | | Normality | 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 (c. equations (5) and (6) in Section 3.1). 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.

Table 4: 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 5: 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 | X | X | X | X | X | X |
| Company FE | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| 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.2 in the Appendix. Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

Table 6: 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 | | X | X | |
| Year FE | | | X | |
| Country × Year FE | | | | X |
| R-squared | 0.006 | 0.051 | 0.080 | 0.143 |
| 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: Distribution of Assassination Events over Time.

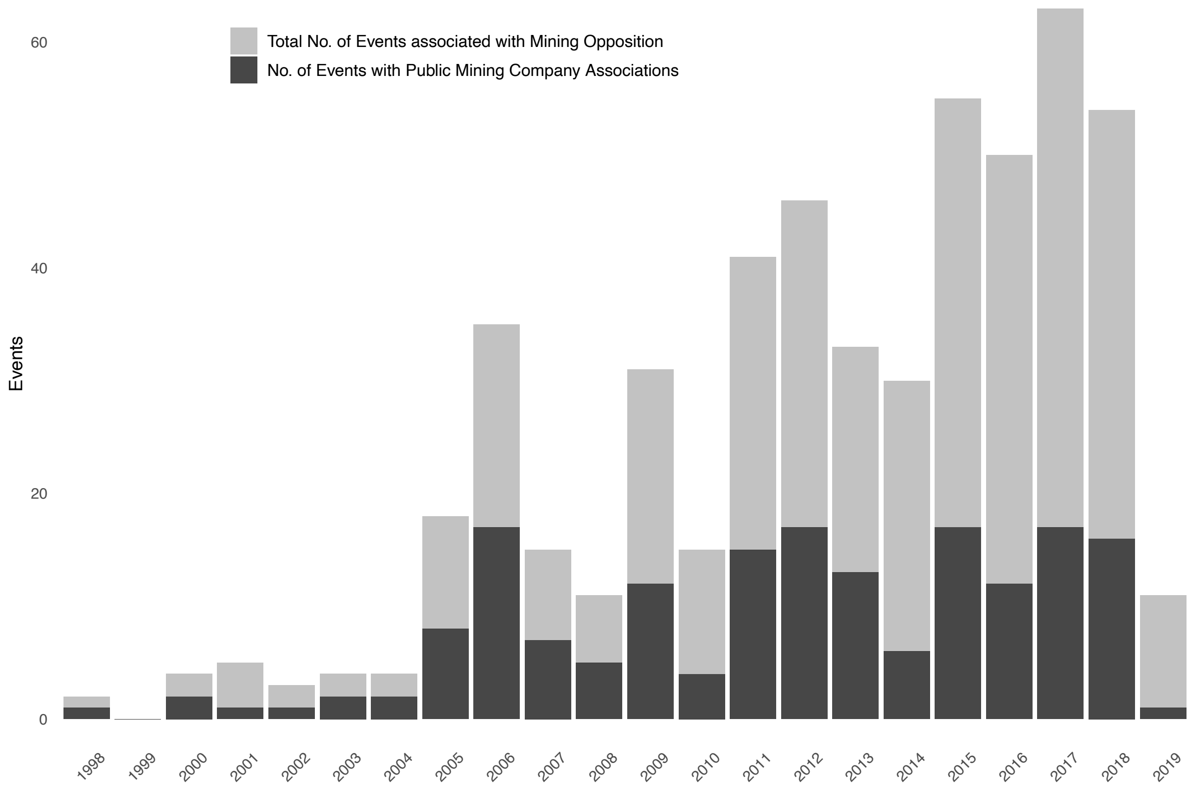
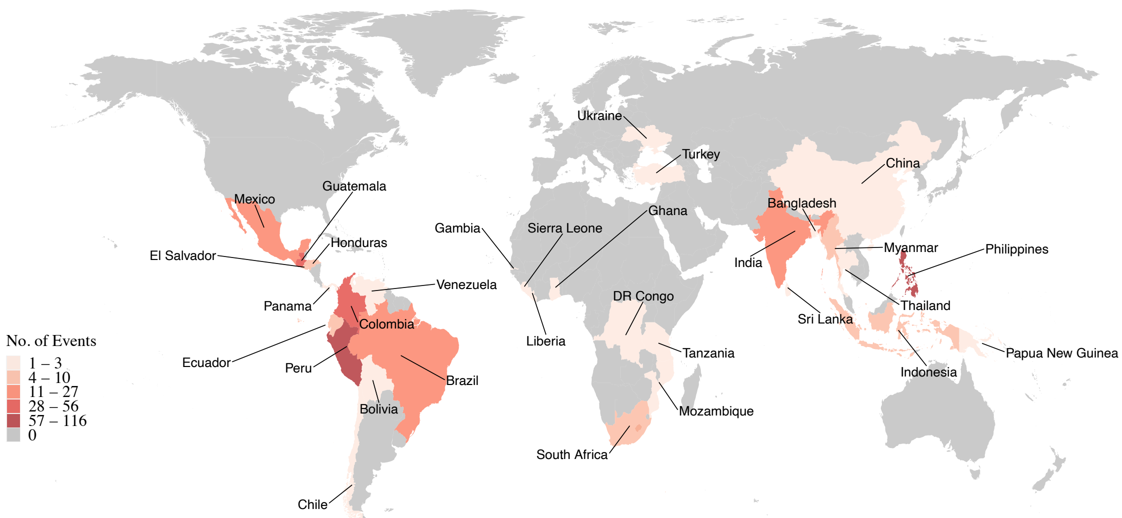
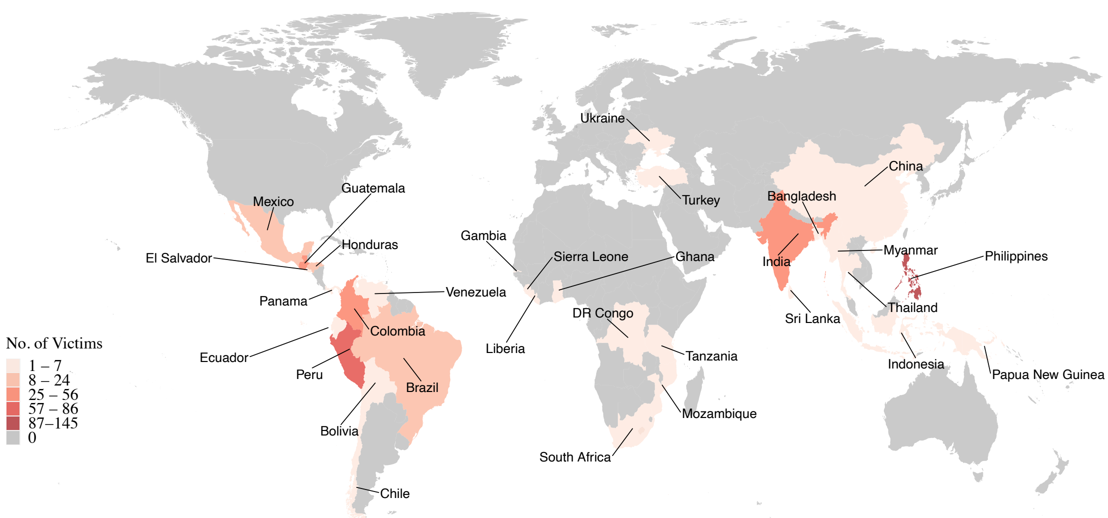


Figure 2: The Spatial Distribution of Assassinations (1998-2019)




(a) Number of Event Incidences by Country




(b) Number of Victims by Country

Figure 3: Event Country Activity and Company Headquarter Locations

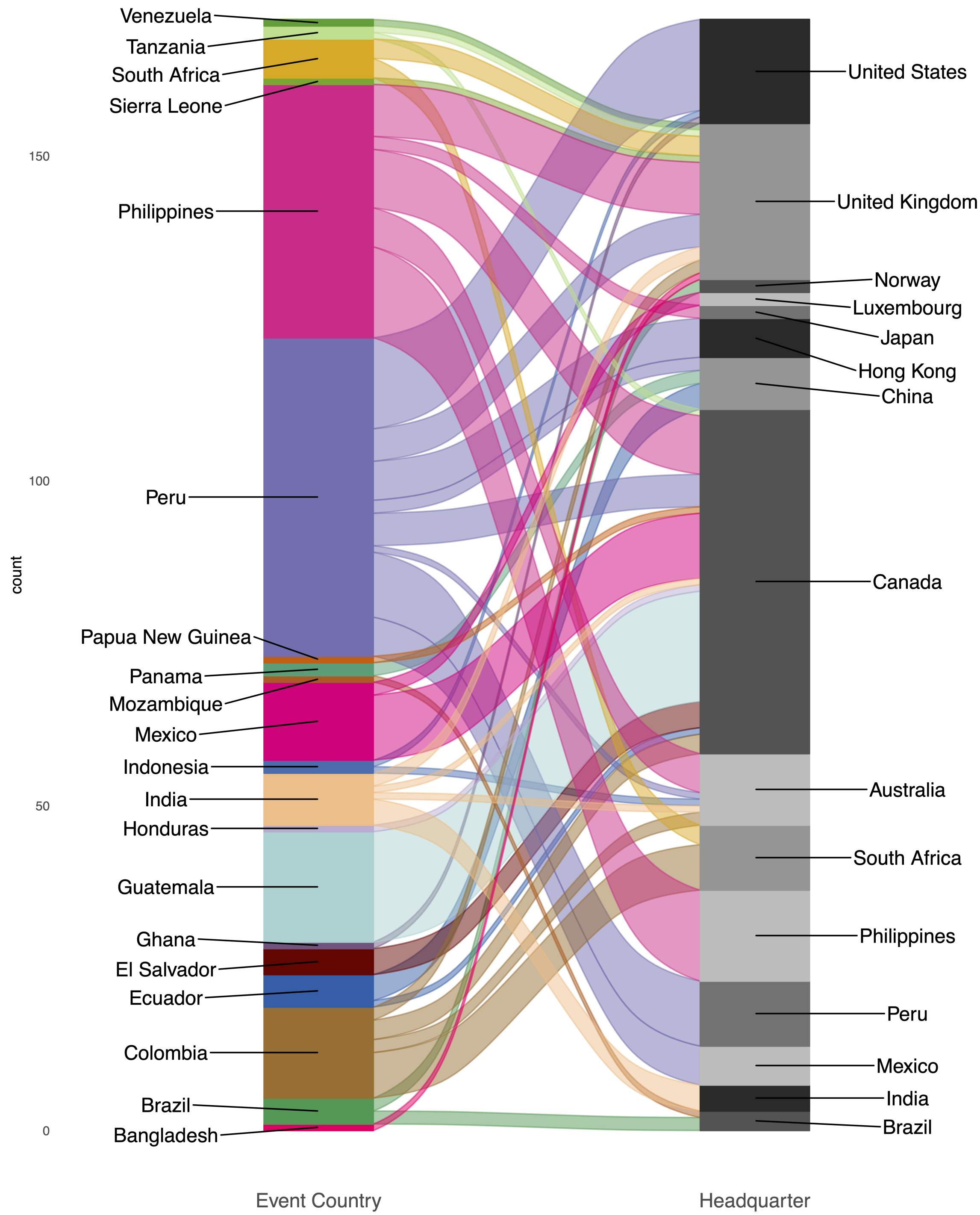


Figure 4: Extracting Events and Company Associations from NGO and Media Reports - Example Case

## 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*

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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.

Event date | Mining Project/Company | "Assassination"/Violent death

Mining opposition | Name(s) and associations of the victim(s)

Figure 5: Event Study Time Line

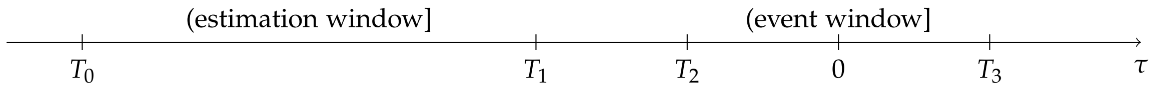
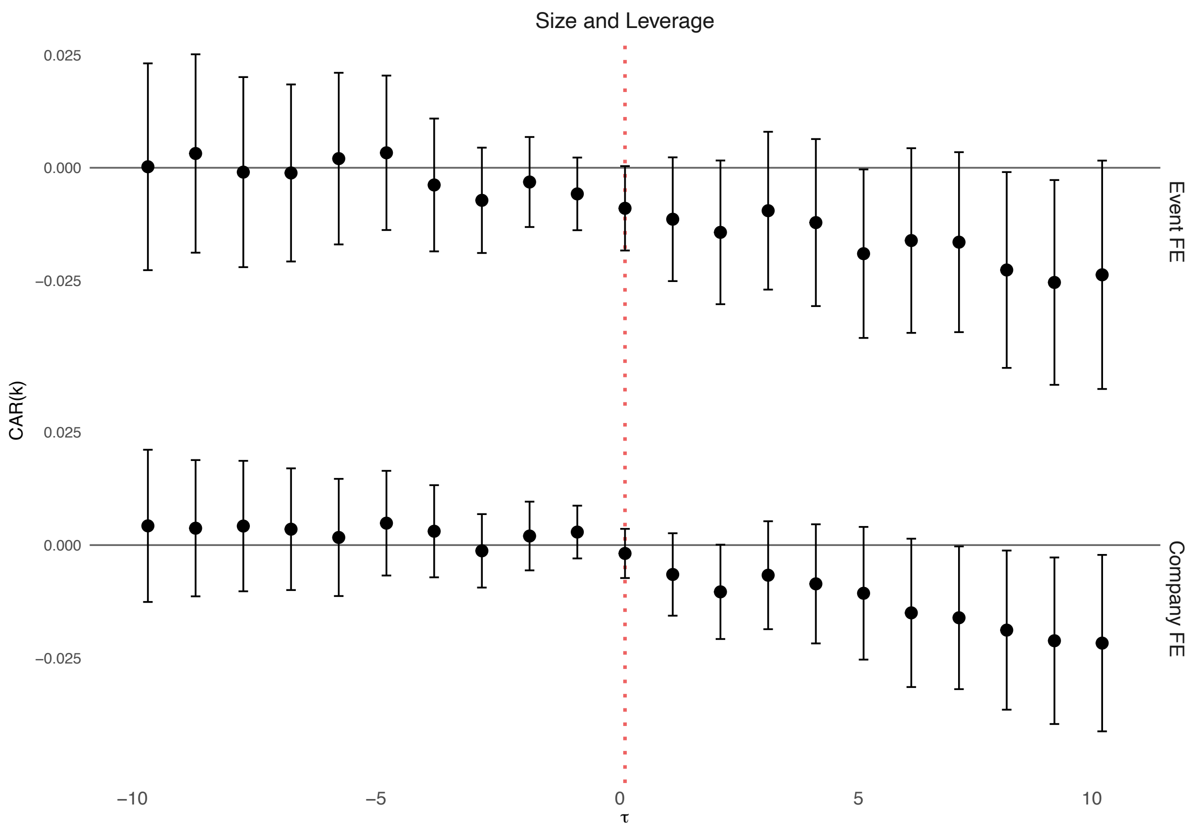
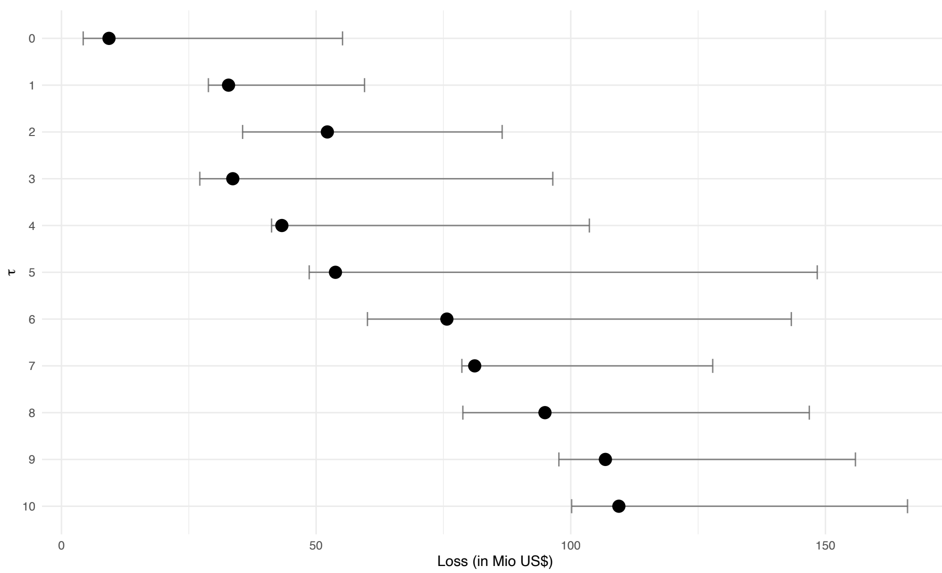



Figure 6: The Treatment Effect of Assassination Events on Mining Companies



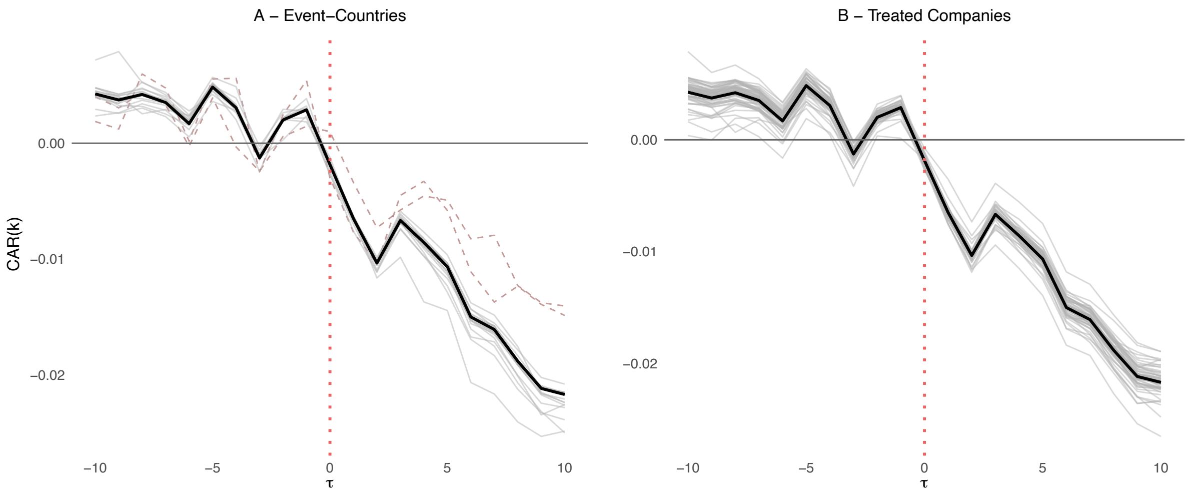

*Notes:* The coefficients when regressing the respective cumulative abnormal return (CAR) on an indicator for being tied to an assassination event is represented by the black dots. The horizontal axis label denotes the trading days before and after the event on $\tau = 0$. CARs are aggregated backwards before the event date and forwards 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. The top panel displays the point estimates for $\hat{\delta}$ when event fixed effects are included, the bottom panel estimates for the specification with company fixed effects. In total the coefficients of 42 regressions are displayed. 95% confidence intervals using robust standard errors clustered on the event-level are depicted.

Figure 7: The estimated Economic Value of Assassination Events



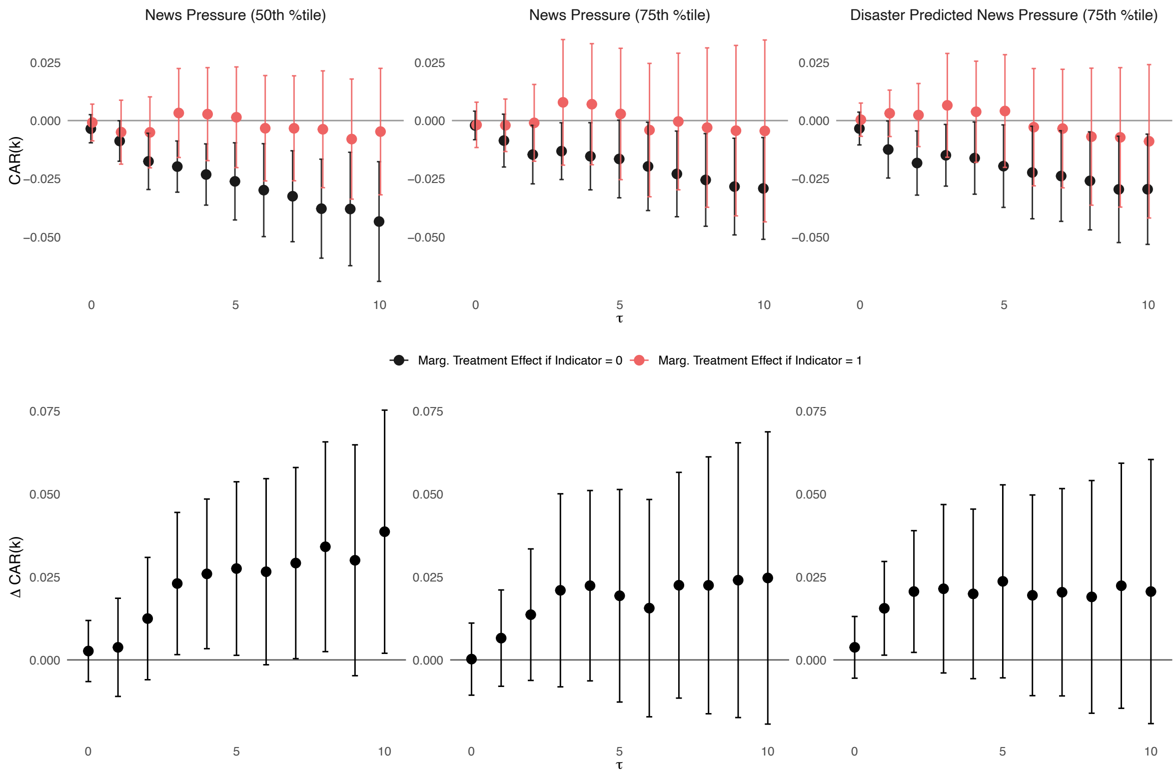

*Notes:* Dots correspond to the estimated loss in market capitalization of the median company for our event fixed effects specification. The grey bars display the estimated minimum and maximum loss in market capitalization for the median company across specifications presented in Figure D.3.

Figure 8: Robustness of Baseline Results - Leave-One-Out



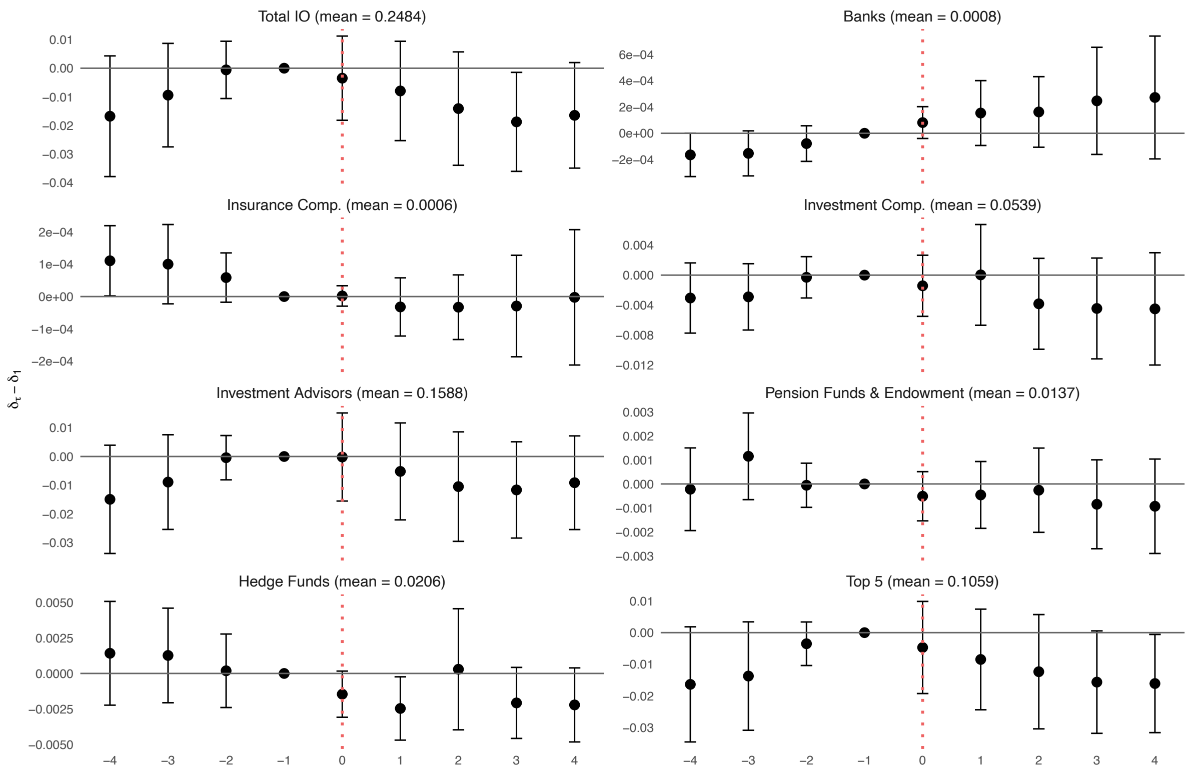

*Notes:* The thick black line in Panel-A and Panel-B corresponds to the event fixed effect specification's coefficient estimates for being tied to an assassination in the full sample. Panel-A presents the estimated coefficients when one country is consecutively dropped from the sample. The (red) dashed lines highlight the estimated coefficients when dropping events in the Philippines, respectively Peru from the sample. Panel-B displays the estimated coefficients when one treated company at a time is dropped from the sample.

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



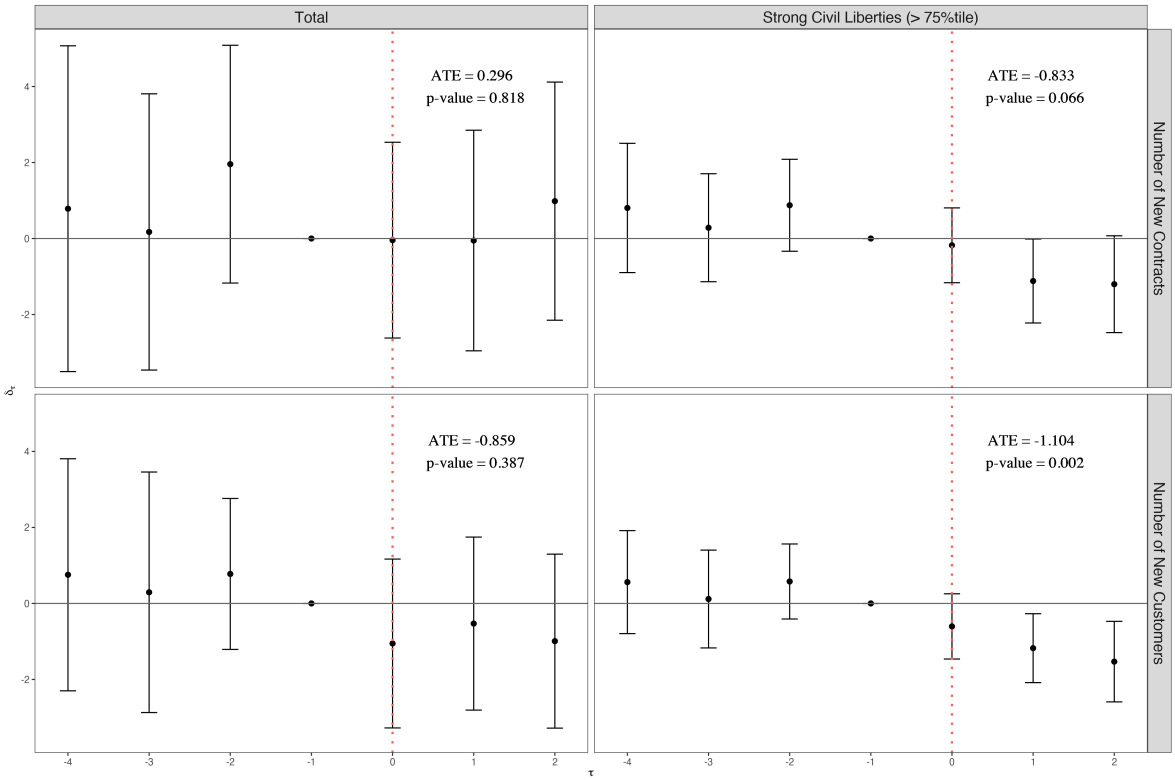

*Notes:* The top panel displays the heterogeneous marginal treatment effect of assassination events on the respective cumulative abnormal return (CAR). The difference in treatment effects is presented in the bottom panel. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. CARs are forwards starting with the event date. E.g. 5 refers to the CAR between days 0 and 5. Regression specifications include an interaction term of the assassination indicator and three different indicators for high news pressure. High news pressure days are defined as *(i)* above median Eisensee and Strömberg (2007) news pressure day in column 1, *(ii)* above 75th percentile Eisensee and Strömberg (2007) news pressure day in column 2, and *(iii)* and above 75th percentile disaster predicted news pressure day in column 3. 95% confidence intervals using robust standard errors clustered on the event-level are displayed in the top and bottom panel.

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




*Notes:* The figure shows the effect of an assassination event on institutional investor's holding position. Each cell displays the estimated effect relative to the quarter before the event ($\tau = -1$). The mean institutional investor holding position of companies that experienced at least one event during the sample period is presented in parentheses. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure 11: The Impact of Assassinations on Supply Chain Contracts




*Notes:* The figure shows the effect of an assassination event on supply chain contracting. Columns capture the supply chain contracts under investigations, while rows depict the corresponding dependent variable. Each panel displays the estimated effect relative to the year before the event ($\tau = -1$), with the corresponding average treatment effect (ATE) across all cohorts over the relative time period $[0;2]$ and its $p$-value are depicted in the upper right corner. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

# Internet Appendix

## A Technical Appendix

### A.1 Test Statistics

For ease of notation (and without loss of generality), we present test statistics for one particular aggregation period from $\tau_1$ to $\tau_2$ in this section, where $T_1 < \tau_1 \leq \tau_2 \leq T_2$.[69]

**Normality**

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

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

with $\overline{CAR}$ and $\sigma(\overline{CAR})$ defined in (5) and (6).

**BMP**

Given the estimated abnormal and cumulative abnormal returns and their sample variance in (3)-(6), the scaled abnormal (SAR) and cumulative abnormal (SCAR) returns during the *event window* $\tau = T_1 + 1, ..., T_2$ are defined as:[70]

$$SAR_{ie\tau} = \frac{\widehat{AR}_{ie\tau}}{\sigma(\widehat{AR}_{ie\tau})} \tag{A.2}$$

$$SCAR_{ie} = \frac{\widehat{CAR}_{ie}}{\sigma(\widehat{CAR}_{ie})}. \tag{A.3}$$

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

$$t_{BMP} = \frac{\overline{SCAR}\sqrt{N}}{\sigma(SCAR_{ie})}, \tag{A.4}$$

[69] This allows us to drop the suffix $(\tau_1, \tau_2)$.

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

where $\overline{SCAR}$ constitutes the average scaled abnormal return on event day $\tau$ and $\sigma(SCAR_{ie})$ the cross-sectional standard deviation of the SCAR:[71]

$$\overline{SCAR} = \frac{1}{N}\sum_{j=1}^{N} SCAR_{ie} \tag{A.5}$$

$$\sigma(SCAR_{ie}) = \sqrt{\frac{1}{N-1}\sum_{j=1}^{N}(SCAR_{ie} - \overline{SCAR})^2}. \tag{A.6}$$

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 boils down to:

$$s^2(SAR_{ie}) = \frac{\sigma^2(SAR_{ie})}{N}\left(1 + (N-1)\bar{r}\right), \tag{A.7}$$

where $\sigma^2(SAR_{ie})$ is given in (A.6) and $\bar{r}$ is the average of the sample cross-correlations of the ARs during the *estimation window*. Using the variance formula in (A.7) the adjusted BMP (ADJ-BMP) $t$-statistic is:

$$t_{ADJ-BMP} = \frac{\overline{SAR}}{s(SAR_{ie})} = \frac{\overline{SAR}\sqrt{N}}{\sigma(SAR_{ie})\sqrt{1+(N-1)\bar{r}}} = t_{BMP}\sqrt{\frac{1-\bar{r}}{1+(N-1)\bar{r}}} \tag{A.8}$$

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 (A.3) using the cross-section standard deviation of the SCARs defined in (A.6) to transform the SCAR to a random variable with zero mean and unit variance just as the other SARs defined in (A.2):[72]

$$SCAR^*_{ie} = \frac{SCAR_{ie}}{\sigma(SCAR_{ie})}. \tag{A.9}$$

[71] Note that (A.4)-(A.6) are equivalently calculated for the $SAR$.

[72] In 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 for rank tests (s. footnote 7 on p. 4008).

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

$$GSAR_{ie\tau} = \begin{cases} SCAR^*_{ie}, & \text{for } \tau = \tau_1, ..., \tau 2 \\ SAR_{ie\tau} & \text{for } \tau = T_0 + 1, ..., T_1. \end{cases} \tag{A.10}$$

Intuitively, the CAR period is treated as if there was 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:

$$U_{ie\tau} = \frac{\text{Rank}(GSAR_{ie\tau})}{T+1} - \frac{1}{2}, \tag{A.11}$$

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" ($E[U_{ie0}] = 0$). Kolari and Pynnönen (2011) show that the t-statistic for testing this null hypothesis is:

$$t_{GRANK} = Z \left( \frac{T-2}{T-1-Z^2} \right)^{\frac{1}{2}}, \tag{A.12}$$

where

$$Z = \frac{\overline{U}_0}{\sigma(\overline{U})} \tag{A.13}$$

with

$$\sigma(\overline{U}) = \sqrt{\frac{1}{T} \sum_{t \in \mathcal{T}} \frac{N_\tau}{N} \overline{U}^2_\tau} \tag{A.14}$$

$$\overline{U}_\tau = \frac{1}{N_\tau} \sum_{j=1}^{N} U_{ie\tau}, \tag{A.15}$$

where $N_\tau$ is the number of non-missing (valid) GSARs available at $\tau \in \mathcal{T} = \{T_0 + 1, ...T_1, 0\}$ 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:

$$\underset{\{w_j^i\}_{j \in \text{Control group}}}{\arg\min} \sum_{t \in \text{Estimation Window}} \left[ R_{it} - \sum_{j \in \text{Control group}} w_j^i R_{jt} \right]^2 \tag{A.16}$$

$$\text{s.t.} \quad \sum_{j \in \text{Control group}} w_j^i = 1 \tag{A.17}$$

$$w_j^i \geq 0, \tag{A.18}$$

where $R_{it}$ and $R_{jt}$ are the daily returns on date $t$ for the treatment firm, respectively for companies in the control group; $\{w_j^{i*}\}$ 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 during 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 boils down to a *quadratic programming problem*, since the objective function is quadratic and the two constraints are linear. I.e. the problem can be rewritten as:

$$\underset{\mathbf{w} \in \mathbb{R}^J}{\arg\min} \quad f(\mathbf{w}) = \frac{1}{2}\mathbf{w}^\intercal \mathbf{D} \mathbf{w} - \mathbf{w}^\intercal \mathbf{b} \tag{A.19}$$

$$\text{s.t.} \quad \mathbf{A}_1 \mathbf{w} = 1 \tag{A.20}$$

$$\mathbf{A}_2 \mathbf{w} \leq \mathbf{0} \tag{A.21}$$

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 during 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*; $A_1 \in \mathbb{R}^{T \times J}$ 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 problem* implemented in the **R** function `solve.QP` of the `quadprog` package.[73]

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

$$\widehat{AR}_{it} = R_{it} - \sum_{j \in \text{Control group}} w_j^{i*} R_{jt} \tag{A.22}$$

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:

$$\widehat{\phi}\,(0,k) = \frac{\sum\limits_{i \in \text{Treatment group}} \frac{\sum_{t=0}^{\tau_2} \widehat{AR}_{it}}{\widehat{\sigma}_i}}{\sum\limits_{i \in \text{Treatment group}} \frac{1}{\widehat{\sigma}_i}} \tag{A.23}$$

$$\text{where} \quad \widehat{\sigma}_i = \sqrt{\frac{\sum\limits_{t \in \text{Estimation Window}} \left(\widehat{AR}_{it}\right)^2}{T}}, \tag{A.24}$$

where $\widehat{\phi}\,(0,k)$ is the cumulative effect over the period $\tau_1$=0 to $\tau_2$ in the *event window*. The overall treatment effect is, hence, a weighted average of each assassination effect on a company in the treatment group, with greater weight given to the estimated effects for which the synthetic firm tracks the return of the treated company more closely during the *estimation window*.

To draw inference, we construct confidence intervals by randomly drawing $K \times E$ *placebo* treatment groups of size $N$ corresponding to the the actual number 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 = 3425 *placebo* treatment groups, each comprising 167 firms. We compute the average treatment effect for each placebo treatment group and day $\tau_2$ 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 $[5, 95]$, $[2.5, 97.5]$, respectively $[0.5, 99.5]$ percentiles of the *placebo* treatment effects.

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

[73]Note that **D** has to be (semi-)positive definite. For 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.

# B Data Appendix

## B.1 Assassination Dataset

In this appendix we describe in 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:

1. We obtain information from NGOs and human rights associations such as "Global Witness" and "Amnesty International", "Front Line Defenders" or "Bulatlat".

2. We use international full-text newspaper archives (e.g. Gale full-texts collections of the International Herald Tribune and Associated Press wire archives) and prominent APIs (e.g. Guardian) to locate events via algorithmic searches. Specifically, we query the APIs and news archives for articles that contain a combination of "activist" keywords (activist, campaigner, indigenous, etc. and additionally variations of mining) and "assassination" keywords (kill, assassin, abduct, etc.). Both keyword lists were chosen semi-automatically by looking up cosine similarities from the *web2vec* word vectors pre-trained on the Google News data set (c. for instance Keith et al., 2017).[74] The (deduplicated) list of returned articles is then manually inspected for relevant events. Note that we also experimented with training text classification models to automatically detect relevant articles. The specificity of our events in combination with the infrequent reporting, 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. These supplementary sources are described below.

3. We search local newspapers such as "La Republica" in Peru, "El Universo" in Ecuador, "El Pais" in Mexico or "El Espectador" in Colombia.

4. We rely on published books (e.g. Holden and Jacobson, 2012; Doyle and Whitmore, 2014) and studies (e.g. Imai et al., 2017; Spohr, 2016; Hamm et al., 2013). These sources often provide supplementary information on cases such as event classifications - i.e. mining, deforestation - and mining project/company associations. For instance, Holden and Jacobson (2012) provide 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.

[74]The Google News data set comprises about 100 billion words. The pre-trained *web2vec* word vectors can be found here: https://code.google.com/archive/p/word2vec/.

After locating assassination events of opposition leaders, indigenous and tribal leaders, and local environmentalists we ensure that the event is indeed linked to the victim's opposition to a mining project, i.e. we require at least one source to state that opposition to mining is the (suspected) reason for the attack. In particular, for 211 of the 565 killings of activists we collected we are not able to establish a link to mining opposition. These 211 cases either comprise assassinations in relation to other sectors such as logging, pipelines, and hydro dams or the source articles provides no conclusive information on the characteristics of the victim's activism. Next, we establish company "ties" for the 354 "mining related" events. We implement the following matching procedure:

1. If a mining company is named in at least one article we check if the reported company is publicly traded. As a convention, we consider only the "downstream" publicly treated companies for the case that the named mining company is not the global ultimate owner, except if the global corporate owner is specifically tied to the event in one of the articles. For instance, 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 specifically mentions *Anglo American* as well. Moreover, we cross-validate–to the best of our abilities–if the company was active in the country at the time of the event by inspecting–among others–annual reports. For the case that the named company is privately owned, we record the company name and do not further discern the ownership structure by private individuals.
2. If the stated mining company is not publicly traded, we examine if the company constitutes a subsidiary or joint venture of publicly traded companies at the time of the event by consulting–among others–company websites, annual reports, SEC documents and business registers. In case no company but a specific mining project could be identified, we 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/joint venture, the name of the company is recorded, not the name of the private owners of the company.

Apart from the company information, we hand-code (i) the precise event date, (ii) the name and number of the victims (iii) the geolocation of the assassination event[75] (iv) the event "circum-

[75]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 pick (approximately) the centroid of the municipality.

stances" (e.g. if the assassination attempt was successful or if it happened during a protest) (v) and–if known–the perpetrator (e.g. police, paramilitary forces, private security guards, hitmen).

## B.2 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 greater 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 or equal than the median).[76] 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.[77]

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 under the spotlight of the media because of a controversy linked to bribery and corruption, political contributions, improper lobbying, money laundering, parallel imports or any tax fraud? | Negative | Y/N | |
| 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 behaviour, tax fraud, parallel imports or money laundering in U.S. dollars. | Negative | Number | Zero |

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

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

Table B.1: Social Indicator Variables (Continued)

| | | | | | |
|---|---|---|---|---|---|
| 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 reputation disasters? | Positive | Y/N | |
| 5) | Critical Countries, Indigenous People Controversies (so_so_co_o06_v) | Is the company under the spotlight of the media 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 provide funding of 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 on its reputation or its relations with communities? | Positive | Double Y/N | |
| 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 the Income Tax is a credit, the result is a Not Meaningful (NM) | Positive | Number | Median |

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Table B.1: Social Indicator Variables (Continued)

| | | | | | |
|---|---|---|---|---|---|
| 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 the company signed the UN Global Compact or follow the OECD guidelines? | Positive | Double Y/N | |
| 13) | Public Health Controversies (so_so_co_o09_v) | Is the company under the spotlight of the media 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 Labour Controversies (so_so_hr_o03_v) | Is the company under the direct or indirect (through suppliers) spotlight of the media because of a controversy linked to child labour? | Negative | Y/N | |
| 2) | Freedom of Association Controversies (so_so_hr_o02_v) | Is the company under the direct or indirect (through suppliers) spotlight of the media because of a controversy linked to freedom of association? | Negative | Y/N | |
| 3) | Human Rights Controversies (so_so_hr_o04_v) | Is the company under the direct or indirect (through suppliers) spotlight of the media 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 | |

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Table B.1: Social Indicator Variables (Continued)

| | | | | |
|---|---|---|---|---|
| 5) | Improvements (so_so_hr_d04_v) | Does the company set specific objectives to be achieved on its human rights policy? | Positive | Y/N |
| 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 applied independent of local laws? AND Does the company have a policy for the exclusion of child, forced or compulsory labour? | Positive | Double Y/N |
| 8) | Suppliers Social Impact (so_so_hr_dp026 AND so_so_hr_dp029) | Does the company report or show to use human rights criteria in the selection or monitoring process of its suppliers or sourcing partners? AND Does the company report or show to be ready to end a partnership with a sourcing partner if human rights criteria are not met? | Positive | Double Y/N |

## Appendix References


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# C Additional Tables

Table C.1: The Effect of Assassinations on Frequently Traded Companies

| | | | p-value | | | |
|---|---|---|---|---|---|---|
| | Mean | SD | Normality | BMP | ADJ-BMP | GRANK |
| $CAR\,(0,0)$ | -0.0002 | 0.0024 | 0.934 | 0.465 | 0.498 | 0.632 |
| $CAR\,(0,1)$ | -0.0054 | 0.0035 | 0.118 | 0.081 | 0.106 | 0.348 |
| $CAR\,(0,2)$ | -0.0047 | 0.0043 | 0.270 | 0.145 | 0.176 | 0.255 |
| $CAR\,(0,3)$ | -0.0035 | 0.0049 | 0.480 | 0.172 | 0.205 | 0.047 |
| $CAR\,(0,4)$ | -0.0038 | 0.0055 | 0.496 | 0.136 | 0.166 | 0.061 |
| $CAR\,(0,5)$ | -0.0040 | 0.0061 | 0.511 | 0.175 | 0.209 | 0.114 |
| $CAR\,(0,6)$ | -0.0062 | 0.0066 | 0.347 | 0.089 | 0.115 | 0.069 |
| $CAR\,(0,7)$ | -0.0092 | 0.0070 | 0.190 | 0.048 | 0.066 | 0.043 |
| $CAR\,(0,8)$ | -0.0110 | 0.0075 | 0.139 | 0.038 | 0.054 | 0.027 |
| $CAR\,(0,9)$ | -0.0153 | 0.0079 | 0.052 | 0.018 | 0.028 | 0.004 |
| $CAR\,(0,10)$ | -0.0155 | 0.0083 | 0.061 | 0.030 | 0.044 | 0.008 |

*Notes:* The number of company-event pairs $N$ is 160. The respective average cumulative abnormal return (CAR) and its standard deviation (SD) is presented in columns 1 and 2 (c. equations (5) and (6) in Section 3.1). A minimum of 11 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 225 trading days. Columns 3 - 6 show the $p$-value of the respective test-statistic. For details on the applied test-statistics see Appendix A.

Table C.2: Private Information and Pre-Trends

| | Mean | SD | p-value | | | |
|---|---|---|---|---|---|---|
| | | | Normality | BMP | ADJ-BMP | GRANK |
| $CAR\,(-1,-1)$ | 0.0006 | 0.0034 | 0.870 | 0.782 | 0.783 | 0.706 |
| $CAR\,(-1,-2)$ | -0.0036 | 0.0048 | 0.447 | 0.236 | 0.237 | 0.231 |
| $CAR\,(-1,-3)$ | -0.0052 | 0.0058 | 0.370 | 0.157 | 0.158 | 0.129 |
| $CAR\,(-1,-4)$ | -0.0030 | 0.0068 | 0.656 | 0.412 | 0.413 | 0.240 |
| $CAR\,(-1,-5)$ | -0.0038 | 0.0076 | 0.618 | 0.520 | 0.521 | 0.303 |
| $CAR\,(-1,-6)$ | -0.0076 | 0.0083 | 0.360 | 0.487 | 0.488 | 0.234 |
| $CAR\,(-1,-7)$ | -0.0065 | 0.0090 | 0.470 | 0.562 | 0.563 | 0.292 |
| $CAR\,(-1,-8)$ | -0.0080 | 0.0095 | 0.401 | 0.413 | 0.415 | 0.157 |
| $CAR\,(-1,-9)$ | -0.0074 | 0.0101 | 0.462 | 0.569 | 0.570 | 0.216 |
| $CAR\,(-1,-10)$ | -0.0062 | 0.0106 | 0.560 | 0.801 | 0.802 | 0.364 |

*Notes:* The number of company-event pairs $N$ is 170. The respective average cumulative abnormal return (CAR) and its standard deviation (SD) is presented in columns 1 and 2 (c. equations (5) and (6) in Section 3.1). A minimum of 8 trading days during the *event window* from -1 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.

Table C.3: The effect of assassination events on stock prices - OLS regression results.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| $CAR\,(0,0)$ | -0.0021 | -0.0020 | -0.0008 | -0.0019 | -0.0090* | -0.0039 | -0.0034 | -0.0023 | -0.0039 | -0.0109* |
| | (0.0026) | (0.0026) | (0.0026) | (0.0028) | (0.0047) | (0.0029) | (0.0031) | (0.0030) | (0.0031) | (0.0062) |
| $CAR\,(0,1)$ | -0.0065* | -0.0064 | -0.0057 | -0.0065 | -0.0114 | -0.0077* | -0.0072 | -0.0065 | -0.0078 | -0.0110 |
| | (0.0039) | (0.0040) | (0.0041) | (0.0046) | (0.0069) | (0.0044) | (0.0046) | (0.0046) | (0.0049) | (0.0080) |
| $CAR\,(0,2)$ | -0.0114** | -0.0117** | -0.0104** | -0.0104* | -0.0143* | -0.0129** | -0.0125** | -0.0116* | -0.0121* | -0.0172* |
| | (0.0047) | (0.0047) | (0.0049) | (0.0053) | (0.0081) | (0.0056) | (0.0059) | (0.0059) | (0.0063) | (0.0100) |
| $CAR\,(0,3)$ | -0.0072 | -0.0075 | -0.0061 | -0.0067 | -0.0095 | -0.0104 | -0.0096 | -0.0093 | -0.0111 | -0.0191* |
| | (0.0056) | (0.0057) | (0.0057) | (0.0060) | (0.0088) | (0.0066) | (0.0069) | (0.0068) | (0.0073) | (0.0099) |
| $CAR\,(0,4)$ | -0.0095 | -0.0099 | -0.0091 | -0.0086 | -0.0121 | -0.0116* | -0.0105 | -0.0109 | -0.0117 | -0.0206** |
| | (0.0060) | (0.0062) | (0.0063) | (0.0067) | (0.0094) | (0.0068) | (0.0072) | (0.0071) | (0.0076) | (0.0098) |
| $CAR\,(0,5)$ | -0.0128* | -0.0129* | -0.0126* | -0.0107 | -0.0190** | -0.0140* | -0.0124 | -0.0139* | -0.0134 | -0.0294*** |
| | (0.0067) | (0.0069) | (0.0070) | (0.0074) | (0.0094) | (0.0077) | (0.0081) | (0.0081) | (0.0087) | (0.0106) |
| $CAR\,(0,6)$ | -0.0166** | -0.0165** | -0.0165** | -0.0150* | -0.0161 | -0.0194** | -0.0182** | -0.0192** | -0.0189** | -0.0284** |
| | (0.0076) | (0.0078) | (0.0079) | (0.0083) | (0.0103) | (0.0086) | (0.0090) | (0.0086) | (0.0093) | (0.0115) |
| $CAR\,(0,7)$ | -0.0172** | -0.0168** | -0.0175** | -0.0161** | -0.0165 | -0.0182** | -0.0160* | -0.0185** | -0.0186** | -0.0254** |
| | (0.0074) | (0.0076) | (0.0076) | (0.0080) | (0.0101) | (0.0084) | (0.0089) | (0.0084) | (0.0090) | (0.0113) |
| $CAR\,(0,8)$ | -0.0197** | -0.0192** | -0.0201** | -0.0188** | -0.0226** | -0.0193** | -0.0173* | -0.0199** | -0.0206** | -0.0291** |
| | (0.0085) | (0.0087) | (0.0087) | (0.0089) | (0.0110) | (0.0095) | (0.0100) | (0.0096) | (0.0100) | (0.0120) |
| $CAR\,(0,9)$ | -0.0238*** | -0.0230** | -0.0252*** | -0.0212** | -0.0254** | -0.0219** | -0.0197* | -0.0241** | -0.0219** | -0.0277** |
| | (0.0089) | (0.0091) | (0.0090) | (0.0093) | (0.0115) | (0.0100) | (0.0105) | (0.0100) | (0.0106) | (0.0126) |
| $CAR\,(0,10)$ | -0.0234** | -0.0220** | -0.0259*** | -0.0217** | -0.0237* | -0.0254** | -0.0233** | -0.0293*** | -0.0267** | -0.0329** |
| | (0.0095) | (0.0097) | (0.0097) | (0.0099) | (0.0128) | (0.0108) | (0.0114) | (0.0109) | (0.0114) | (0.0146) |
| Size and Leverage | X | X | X | X | X | X | X | X | X | X |
| Profitability | | | | | | X | X | X | X | X |
| Cubic Terms | | | | | | X | X | X | X | X |
| Headquarter FE | | X | | | | | X | | | |
| Year FE | | | X | | | | | X | | |
| Event FE | | | | X | | | | | X | |
| Company FE | | | | | X | | | | | X |
| Observations | 4177 | 4177 | 4175 | 4170 | 4090 | 4029 | 4029 | 4027 | 4022 | 3944 |
| Clusters | 154 | 154 | 152 | 147 | 153 | 154 | 154 | 152 | 147 | 153 |

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

Table C.4: The effect of assassination events on stock prices - OLS robustness checks.

| | 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 | X | X | X | X | X | X | X | X | X | X | X | X | X | X | X |
| Headquarter FE | | X | | | | | X | | | | | X | | | |
| Year FE | | | X | | | | | X | | | | | X | | |
| Event FE | | | | X | | | | | X | | | | | X | |
| Company FE | | | | | X | | | | | X | | | | | X |
| 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.

Table C.5: The effect of assassination events on stock price - Admin1 control sample.

| | (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 | X | X | X | X | X | X | X | X | X | X |
| Profitability | | | | | | X | X | X | X | X |
| Cubic Terms | | | | | | X | X | X | X | X |
| Headquarter FE | | X | | | | | X | | | |
| Year FE | | | X | | | | | X | | |
| Event FE | | | | X | | | | | X | |
| Company FE | | | | | X | | | | | X |
| 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.

Table C.6: The effect of assassination events on stock prices without company ties.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| $CAR\,(0,0)$ | -0.0050 | -0.0049 | -0.0028 | -0.0058 | -0.0034 | -0.0041 | -0.0050 | -0.0023 | -0.0059 | -0.0029 |
| | (0.0061) | (0.0064) | (0.0046) | (0.0048) | (0.0072) | (0.0060) | (0.0063) | (0.0045) | (0.0045) | (0.0074) |
| $CAR\,(0,1)$ | -0.0019 | -0.0025 | -0.0022 | -0.0035 | -0.0047 | -0.0018 | -0.0022 | -0.0023 | -0.0046 | -0.0052 |
| | (0.0040) | (0.0042) | (0.0048) | (0.0075) | (0.0050) | (0.0040) | (0.0042) | (0.0045) | (0.0071) | (0.0051) |
| $CAR\,(0,2)$ | 0.0002 | -0.0004 | -0.0005 | -0.0012 | -0.0070 | -0.0009 | -0.0026 | -0.0012 | -0.0032 | -0.0086 |
| | (0.0052) | (0.0057) | (0.0062) | (0.0087) | (0.0080) | (0.0051) | (0.0058) | (0.0056) | (0.0079) | (0.0084) |
| $CAR\,(0,3)$ | -0.0014 | -0.0026 | -0.0009 | -0.0016 | -0.0102 | -0.0074 | -0.0109 | -0.0061 | -0.0077 | -0.0108 |
| | (0.0074) | (0.0078) | (0.0072) | (0.0094) | (0.0102) | (0.0072) | (0.0077) | (0.0066) | (0.0086) | (0.0107) |
| $CAR\,(0,4)$ | -0.0008 | -0.0016 | -0.0029 | -0.0006 | -0.0136 | -0.0066 | -0.0097 | -0.0082 | -0.0055 | -0.0135 |
| | (0.0067) | (0.0067) | (0.0075) | (0.0101) | (0.0096) | (0.0067) | (0.0069) | (0.0069) | (0.0095) | (0.0099) |
| $CAR\,(0,5)$ | 0.0007 | -0.0020 | -0.0031 | -0.0048 | -0.0132 | -0.0041 | -0.0094 | -0.0080 | -0.0102 | -0.0129 |
| | (0.0078) | (0.0081) | (0.0085) | (0.0117) | (0.0106) | (0.0082) | (0.0086) | (0.0082) | (0.0110) | (0.0110) |
| $CAR\,(0,6)$ | -0.0013 | -0.0052 | -0.0072 | -0.0148 | -0.0088 | -0.0069 | -0.0133 | -0.0127 | -0.0223* | -0.0093 |
| | (0.0094) | (0.0095) | (0.0102) | (0.0134) | (0.0119) | (0.0094) | (0.0095) | (0.0100) | (0.0128) | (0.0124) |
| $CAR\,(0,7)$ | 0.0031 | 0.0001 | -0.0009 | -0.0096 | -0.0007 | -0.0020 | -0.0068 | -0.0065 | -0.0169 | -0.0013 |
| | (0.0119) | (0.0118) | (0.0111) | (0.0124) | (0.0156) | (0.0123) | (0.0121) | (0.0114) | (0.0118) | (0.0164) |
| $CAR\,(0,8)$ | -0.0019 | -0.0039 | -0.0064 | -0.0195 | -0.0066 | -0.0058 | -0.0100 | -0.0111 | -0.0268** | -0.0066 |
| | (0.0120) | (0.0117) | (0.0111) | (0.0132) | (0.0166) | (0.0120) | (0.0116) | (0.0111) | (0.0122) | (0.0171) |
| $CAR\,(0,9)$ | -0.0003 | -0.0043 | -0.0047 | -0.0151 | -0.0100 | -0.0058 | -0.0121 | -0.0106 | -0.0233* | -0.0101 |
| | (0.0137) | (0.0136) | (0.0126) | (0.0144) | (0.0172) | (0.0138) | (0.0134) | (0.0122) | (0.0130) | (0.0177) |
| $CAR\,(0,10)$ | -0.0047 | -0.0059 | -0.0058 | -0.0134 | -0.0164 | -0.0093 | -0.0129 | -0.0110 | -0.0197 | -0.0168 |
| | (0.0174) | (0.0176) | (0.0151) | (0.0166) | (0.0193) | (0.0171) | (0.0170) | (0.0146) | (0.0153) | (0.0198) |
| Size and Leverage | X | X | X | X | X | X | X | X | X | X |
| Profitability | | | | | | X | X | X | X | X |
| Cubic Terms | | | | | | X | X | X | X | X |
| Headquarter FE | | X | | | | | X | | | |
| Year FE | | | X | | | | | X | | |
| Event FE | | | | X | | | | | X | |
| Company FE | | | | | X | | | | | X |
| Observations | 1472 | 1470 | 1472 | 1471 | 1403 | 1434 | 1432 | 1434 | 1433 | 1368 |
| Clusters | 62 | 62 | 62 | 61 | 62 | 62 | 62 | 62 | 61 | 62 |

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

Table C.7: The Effect of Assassination Events on ESG Scores - Controlling for Total IO

| | 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 | X | X | X | X | X | X |
| Total IO | X | X | X | X | X | X |
| Company FE | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| 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.2 in the Appendix. Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

Table C.8: The Effect of Assassination Events on ESG Scores - 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 | X | X | X | X | X | X |
| Lagged Dependent Variable | X | X | X | X | X | X |
| Company FE | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| 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.2 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: The Effect of Assassination Events on ESG Scores - First Differences

| | Asset4 z-Scores | | | | Dyck et al. (2019) | |
|---|---|---|---|---|---|---|
| Dep. Variable: | Δ Overall ESG | Δ Overall ESGC | Δ Human Rights | ΔCommunity | Δ Human Rights | Δ Community |
| Δ Assassination | 0.0066 | 0.0006 | -0.1287** | -0.0135 | -0.0264 | -0.0183 |
| | (0.0188) | (0.0342) | (0.0580) | (0.0247) | (0.0207) | (0.0162) |
| Company Controls | X | X | X | X | X | X |
| Year FE | X | X | X | X | X | X |
| R-squared | 0.014 | 0.012 | 0.004 | 0.007 | 0.013 | 0.087 |
| Observations | 46704 | 46704 | 19689 | 46329 | 46281 | 36918 |

*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.2 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: 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.

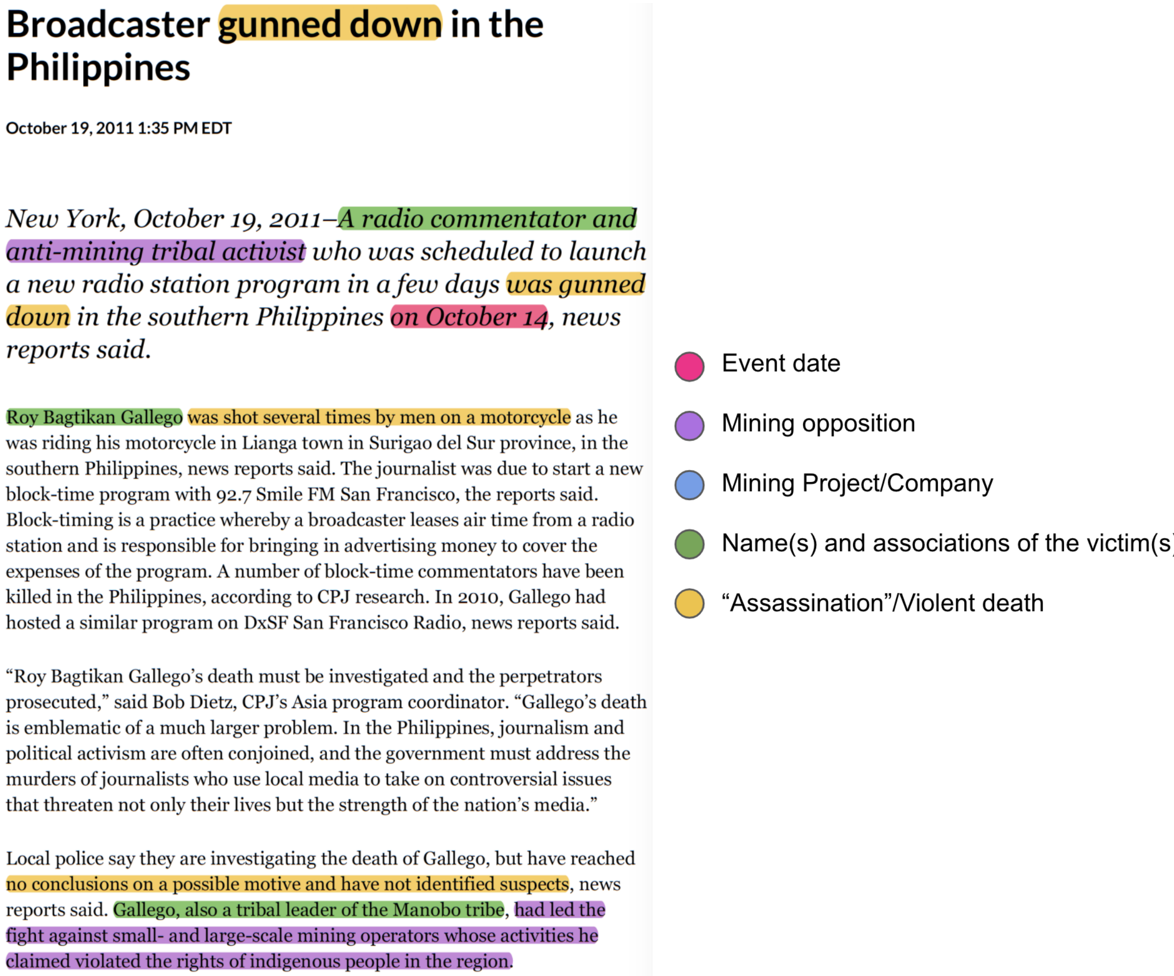
Table C.11: The Relationship between Assassinations and the Change in Tax Revenue Shares

| | 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 | | X | X | |
| Year FE | | | X | |
| Country × Year FE | | | | X |
| R-squared | 0.001 | 0.002 | 0.011 | 0.053 |
| 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.

# D Additional Figures

Figure D.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.

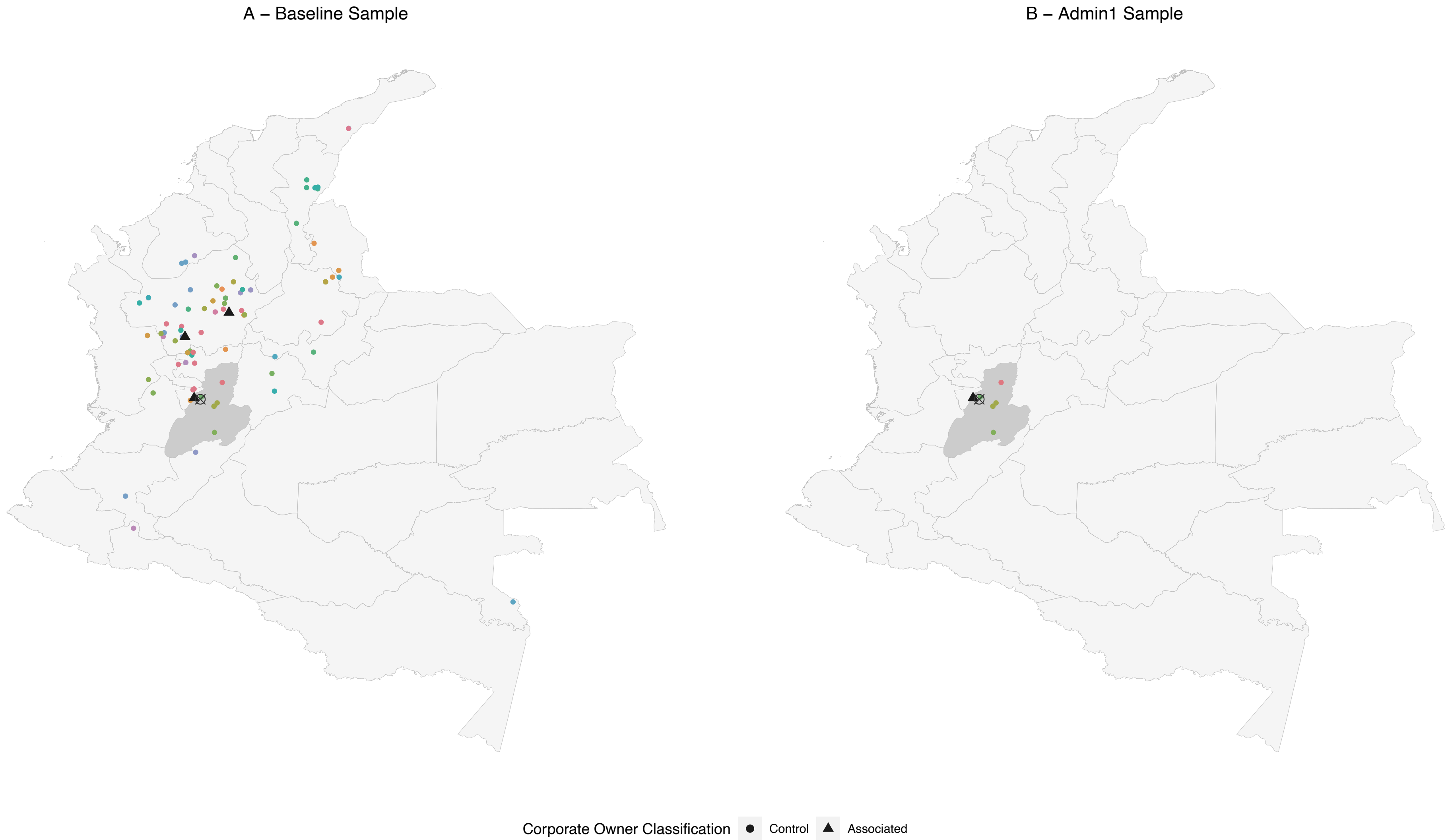
"Roy Bagtikan Gallego's death must be investigated and the perpetrators prosecuted," said Bob Dietz, CPJ's Asia program coordinator. "Gallego's death is emblematic of a much larger problem. In the Philippines, journalism and political activism are often conjoined, and the government must address the 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.



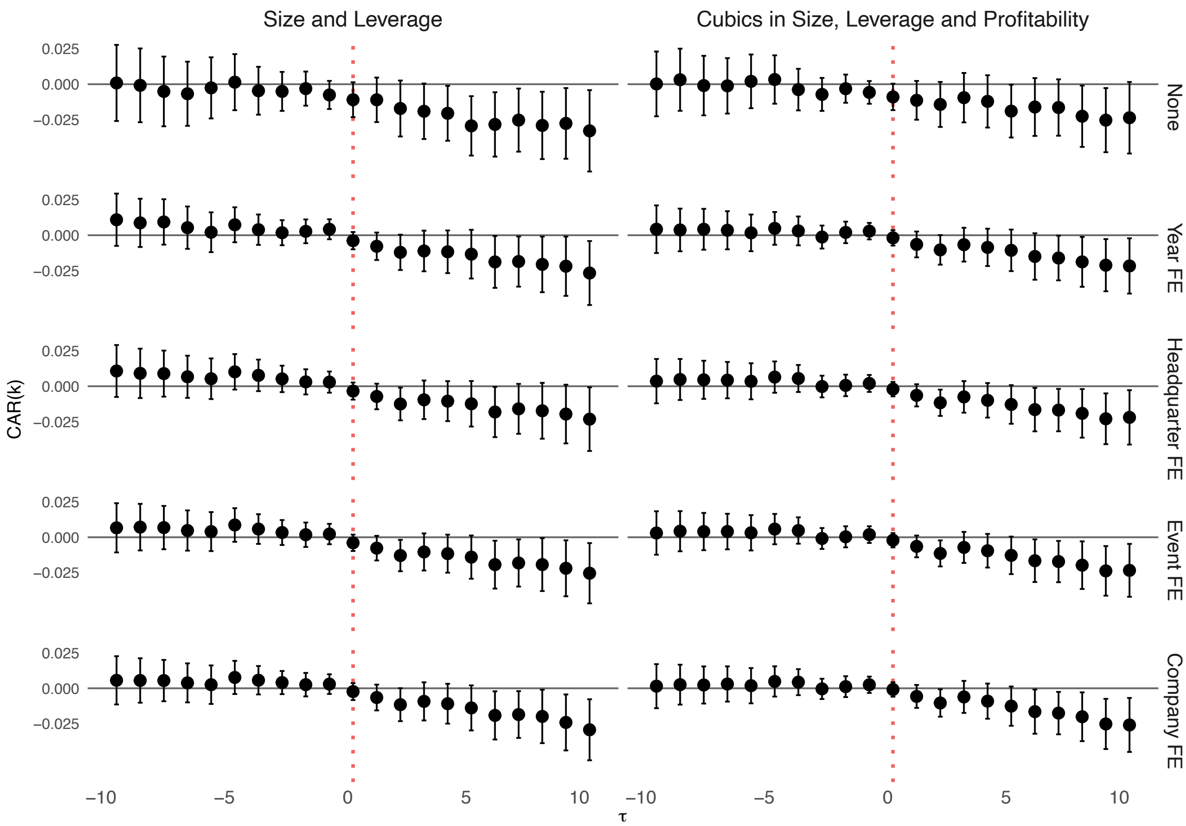
*Notes:* The source article can be found here: https://cpj.org/2011/10/broadcaster-gunned-down-in-philippines/

Figure D.2: The Construction of the Control Group - An Example Case from Colombia



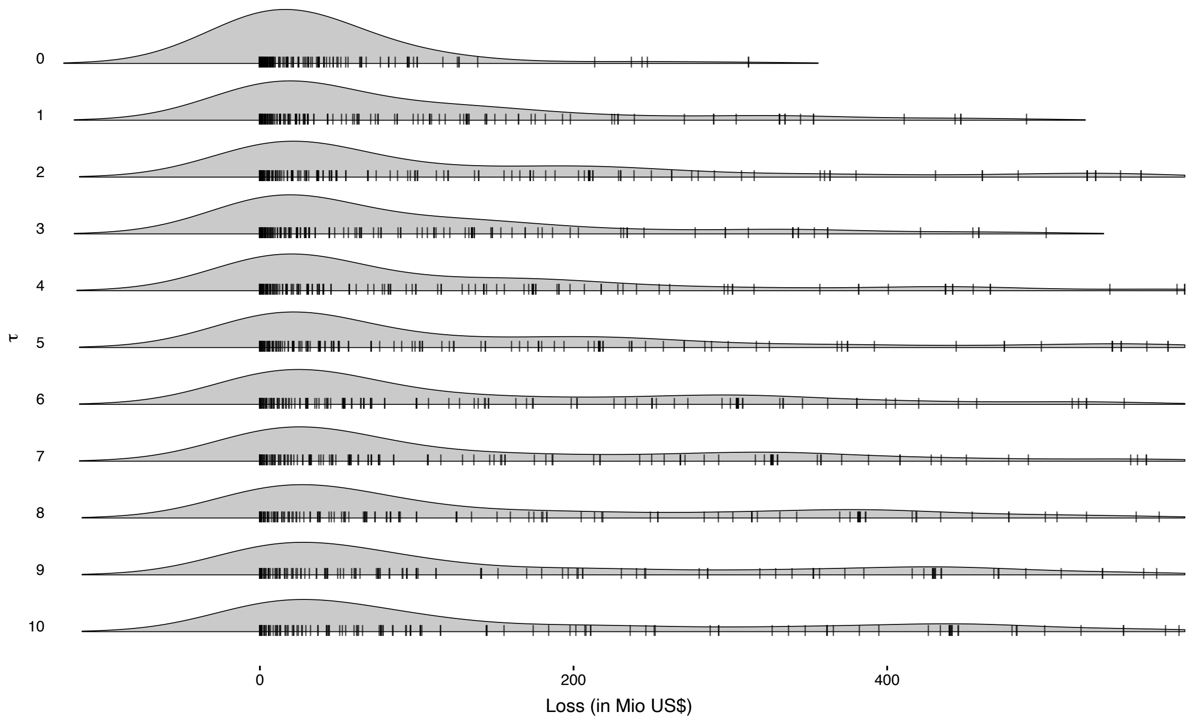

*Notes:* The map displays the Admin1 regions of mainland Colombia. The dark grey area corresponds to the Admin1 region, where the assassination event took place, while the location itself is marked by the black circle cross. Triangles (dots) correspond to mining projects owned by companies linked to the assassnation event (or not), with colours differentiating corporate owner(s). 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.

Figure D.3: 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 is represented by the black dots. The horizontal axis label denotes the trading days before and after the event on $\tau = 0$. CARs are aggregated backwards before the event date and forwards 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 the inclusion of various fixed effects. In total the coefficients of 210 regressions are displayed. 95% confidence intervals using robust standard errors clustered on the event-level are depicted.

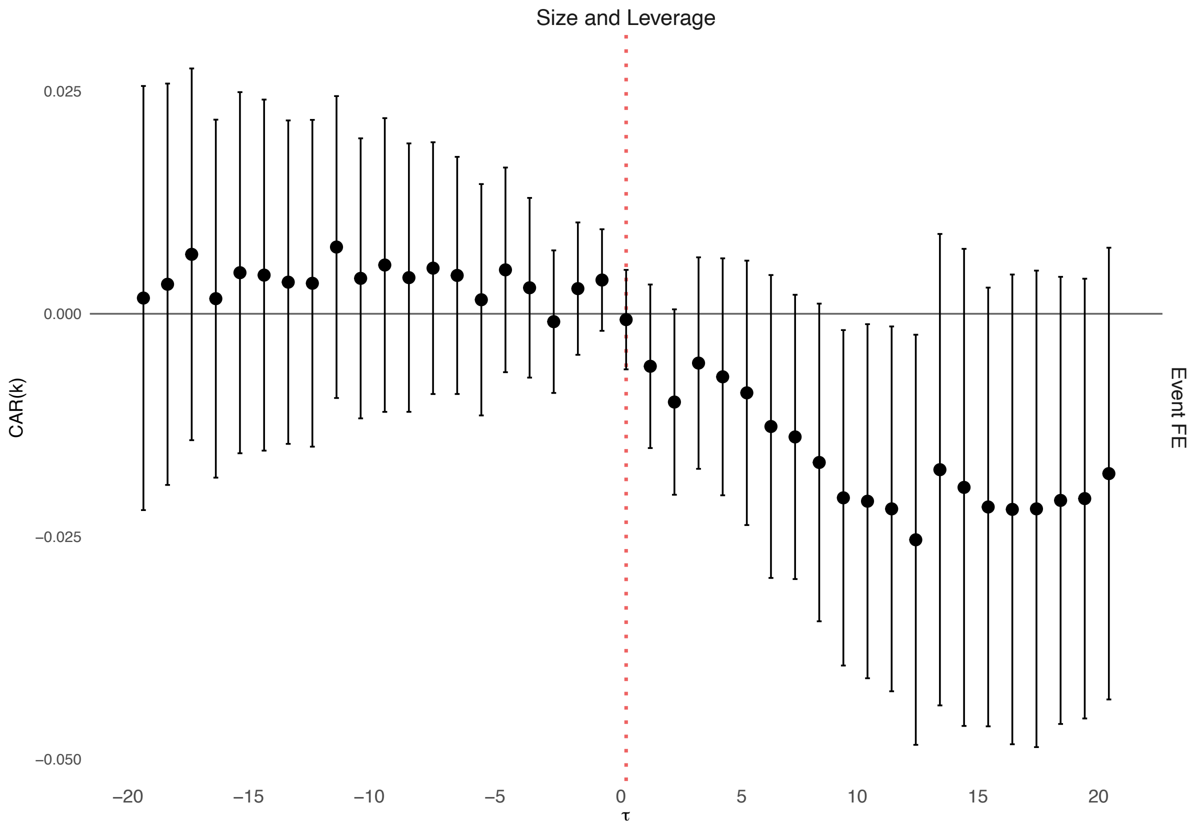
Figure D.4: The Distribution of the Economic Value of Assassination Events




*Notes:* The Distribution of the Economic Value of Assassination Events.

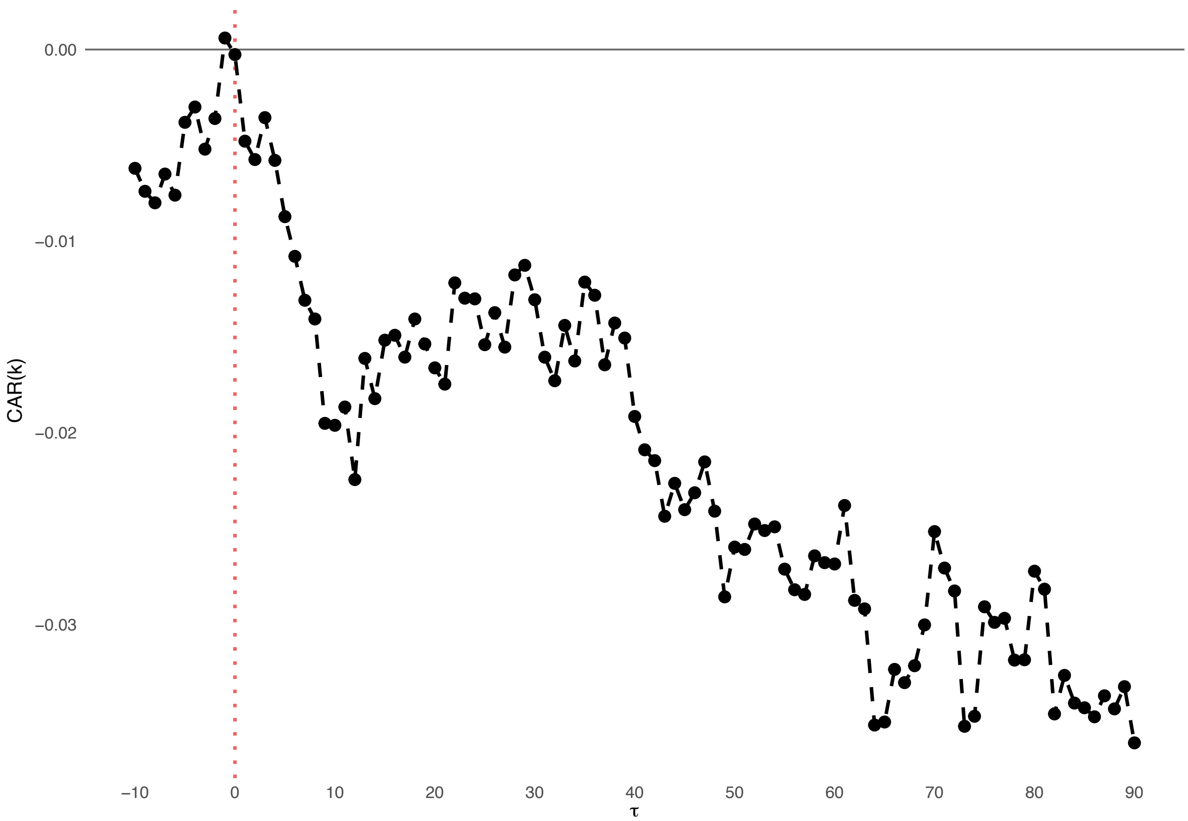
*Notes:* The distribution of market capitalization losses across companies for the baseline specification is displayed. For illustrative purposes losses above the 90th percentile are not displayed in Panel-B.

Figure D.5: The Treatment Effect of Assassination Events on Mining Companies - Wide Event Window



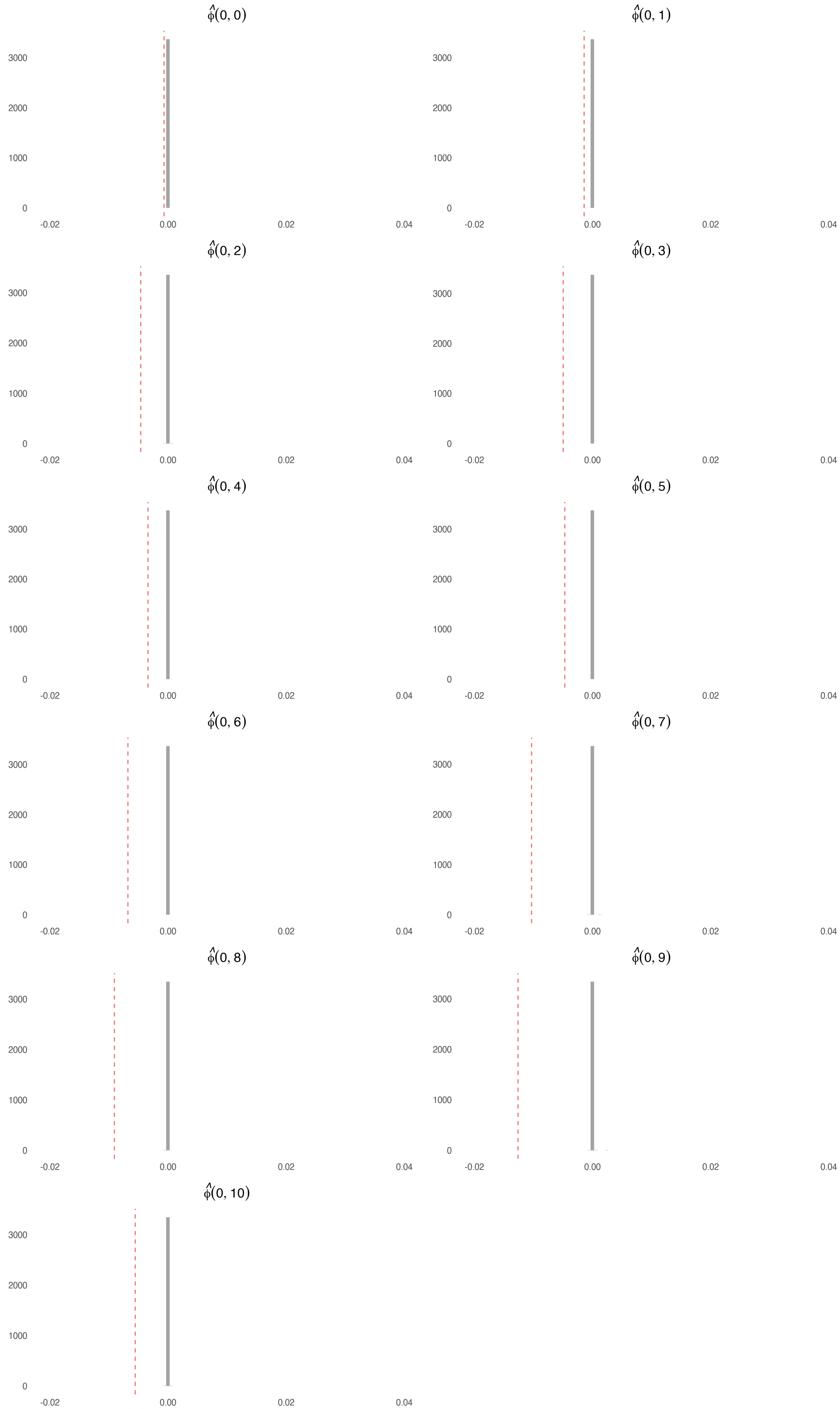

*Notes:* The coefficients when regressing the respective cumulative abnormal return (CAR) on an indicator for being tied to an assassination event is represented by the black dots for our event fixed effects specification. Each dot corresponds to a separate regression coefficient estimate. The horizontal axis label denotes the trading days before and after the event on $\tau = 0$. CARs are aggregated backwards before the event date and forwards 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. 95% confidence intervals using robust standard errors clustered on the event-level are depicted.

Figure D.6: The Long-Run Average Cumulative Abnormal Returns of Associated Companies



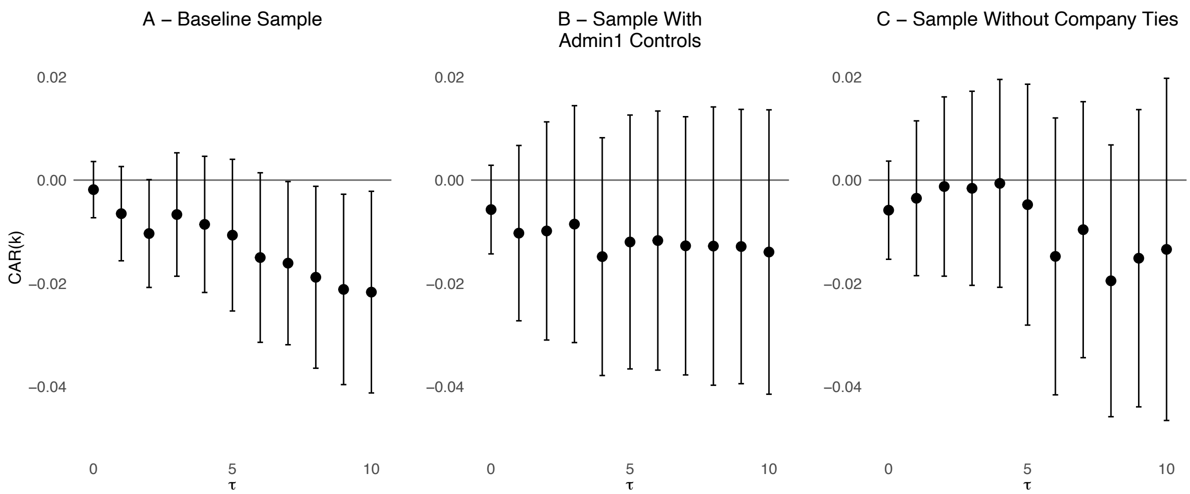

*Notes:* Underlying the Market Model, the *average* cumulative abnormal return (CAR) of mining companies associated with assassination events are displayed. CARs are aggregated backwards before the event date and forwards starting with the event date. E.g. −5 refers to the CAR between −1 and −5 while 5 refers to the CAR between 0 and 5. Companies have to be traded 70 out of the 91 days following the event and 8 out 10 days prior to the event.

Figure D.7: Distribution of Effects of Treated and Synthetic Matching Firms



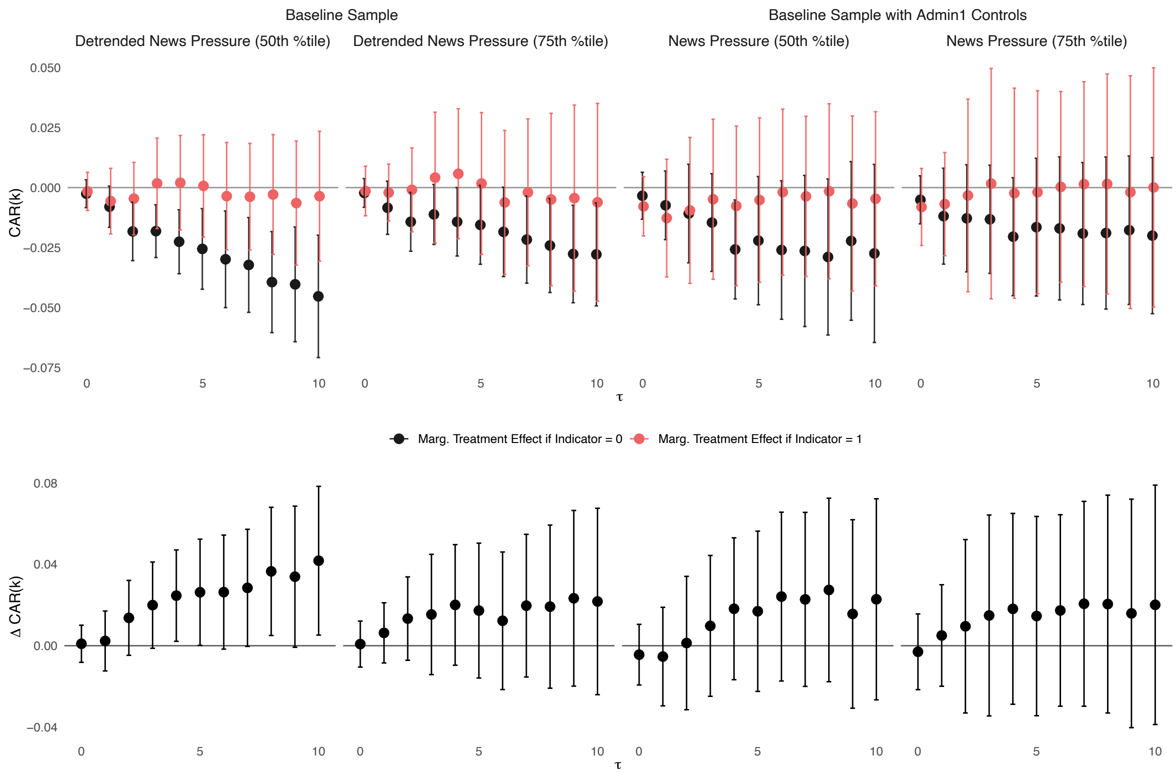

*Notes:* The red dashed line depicts the *actual* estimated effect of assassination events on returns using the modified synthetic matching method of Acemoglu et al. (2016) (For more details, please see Section A.2 in the Appendix). The distribution of the effects for the 3245 placebo treatment groups is presented in gray.

Figure D.8: Vicinity vs. Media Ties



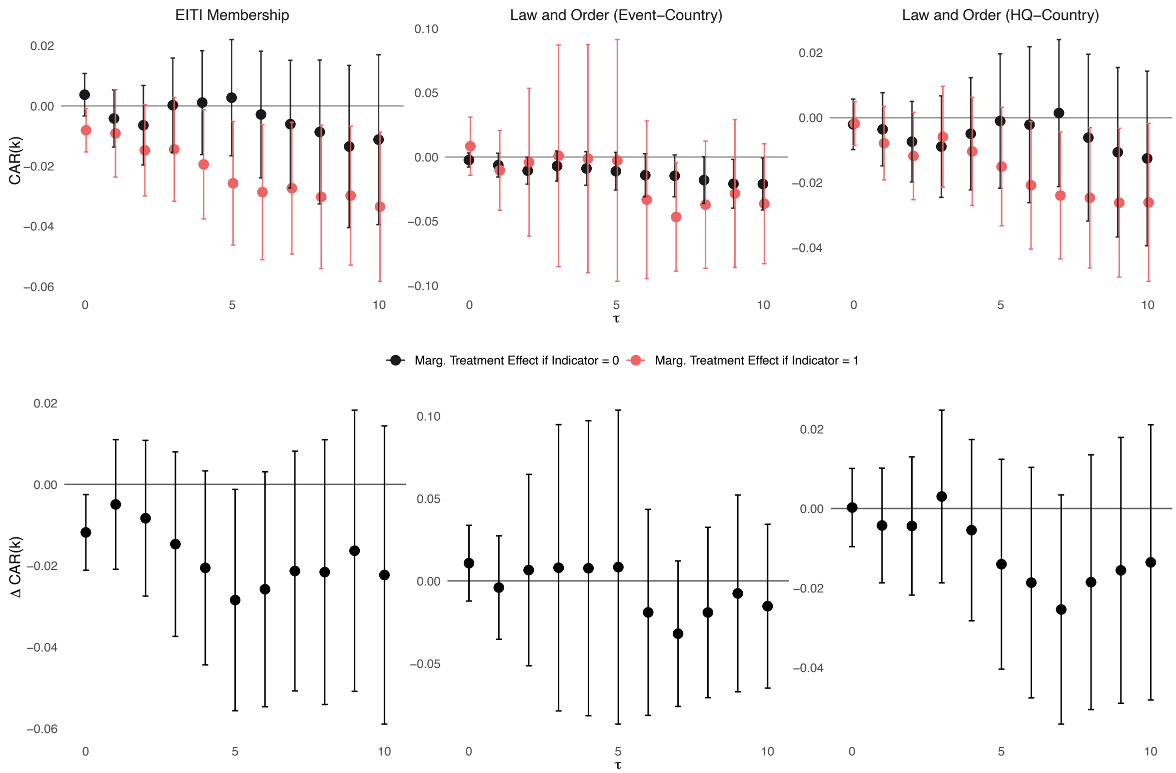

*Notes:* The black dots correspond to the coefficient estimates for being tied to an assassination in our event fixed effect specification. Panel-A presents the baseline sample estimates, while Panel-B presents the results when altering the control group to companies active in the Admin1 region of the assassination event. Panel-C shows the coefficient estimates for the sample with no public company associations; all companies within the same Admin1 region of the event are considered as treated, while all remaining companies in the event-country constitute the control group. 95% confidence intervals using robust standard errors clustered on the event-level are depicted.

Figure D.9: News Pressure - Additional Robustness Checks



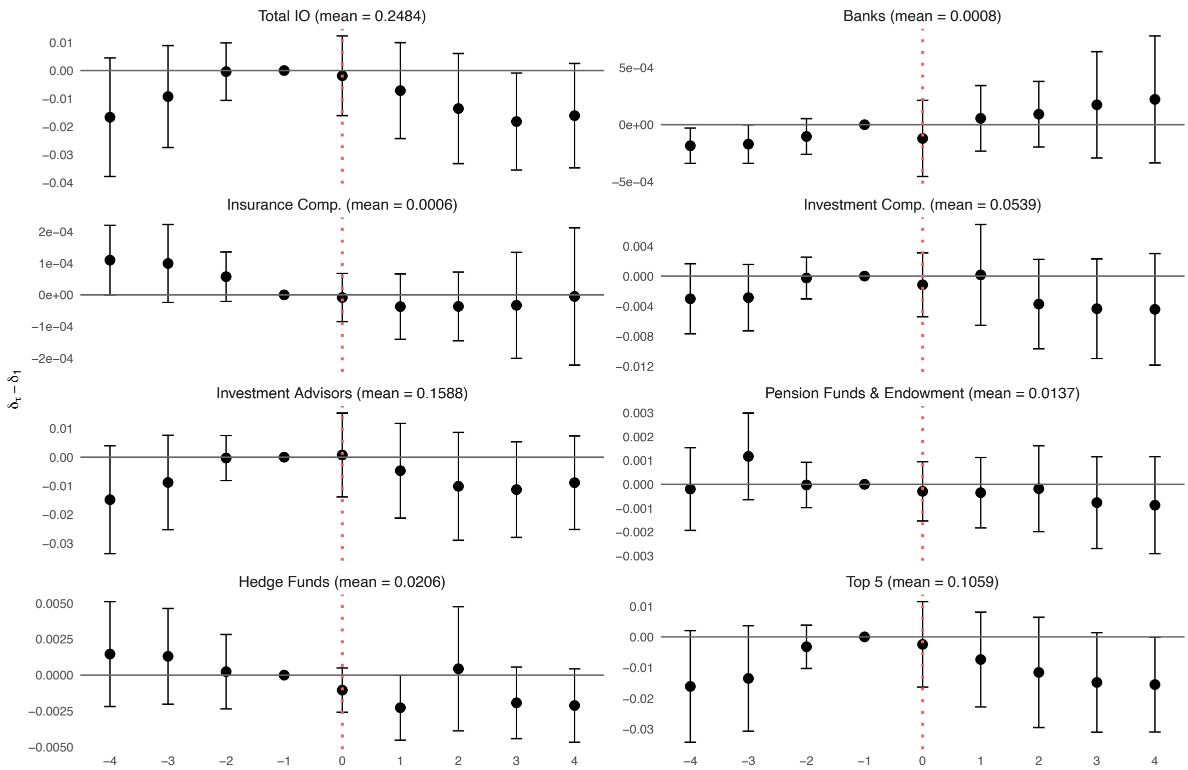

*Notes:* The top panel displays the heterogeneous marginal treatment effect of assassination events on the respective cumulative abnormal return (CAR). The difference in treatment effects is presented in the bottom panel. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. CARs are forwards starting with the event date. E.g. 5 refers to the CAR between days 0 and 5. Regression specifications include an interaction term of the assassination indicator and different indicators for the level of corporate oversight. Columns 1 and 2 present baseline sample estimates for an high news pressure days defined as an above median, respectively above 75th percentile *detrended* Eisensee and Strömberg (2007) news pressure day. Columns 3 and 4 present estimates for above median, respectively above 75th percentile Eisensee and Strömberg (2007) news pressure day with the control group set restricted to companies active in the Admin1 region of the assassination event. 95% confidence intervals using robust standard errors clustered on the event-level are displayed in the top and bottom panel.

Figure D.10: The Impact of Oversight



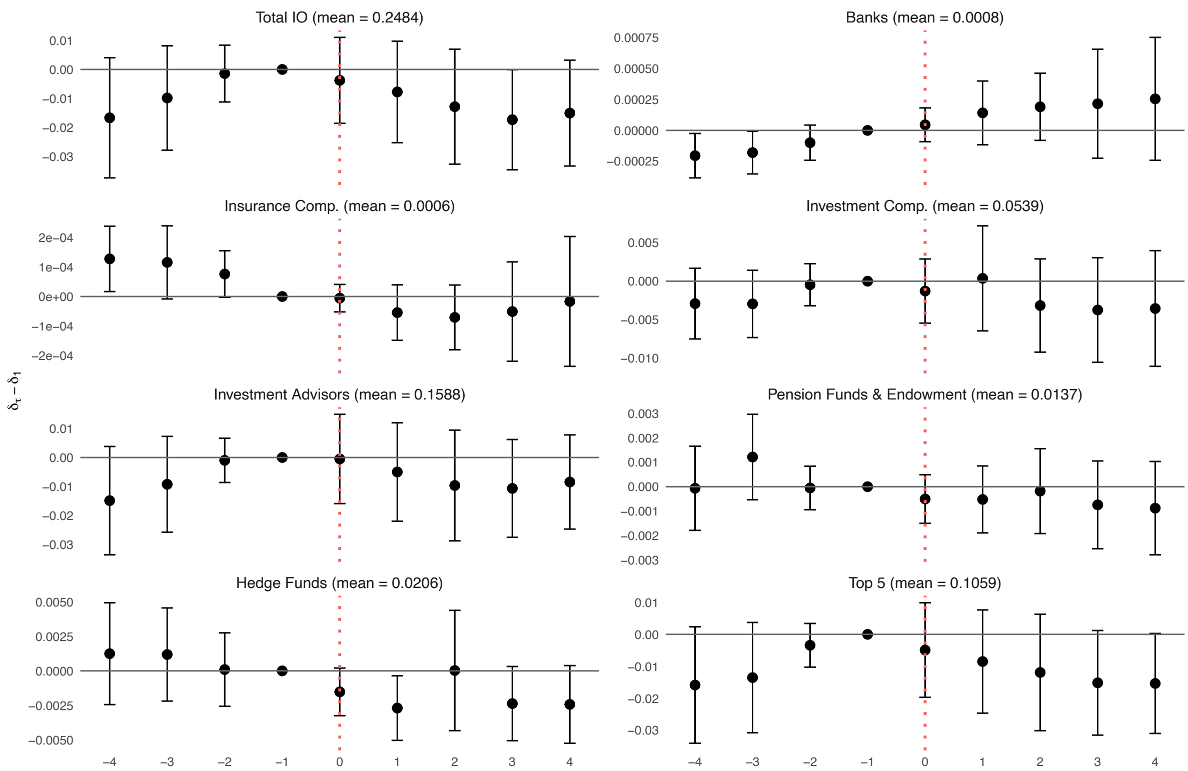

*Notes:* The top panel displays the heterogeneous marginal treatment effect of assassination events on the respective cumulative abnormal return (CAR). The difference in treatment effects is presented in the bottom panel. The horizontal axis label denotes the trading days relative to the event day $\tau = 0$. CARs are forwards starting with the event date. E.g. 5 refers to the CAR between days 0 and 5. Regression specifications include an interaction term of the assassination indicator and three different indicators for the level of corporate oversight. The oversight indicators are defined as *(i)* EITI membership of the event-country in column 1 *(ii)* above median ICRG *Law and Order* scores in the event-country in column 2, and *(iii)* above median ICRG *Law and Order* scores in the corporation's headquarter country in column 3. 95% confidence intervals using robust standard errors clustered on the event-level are displayed in the top and bottom panel.

Figure D.11: The Effect of Assassination Events on Institutional Investor Holdings - Accounting for Overlapping Event Windows



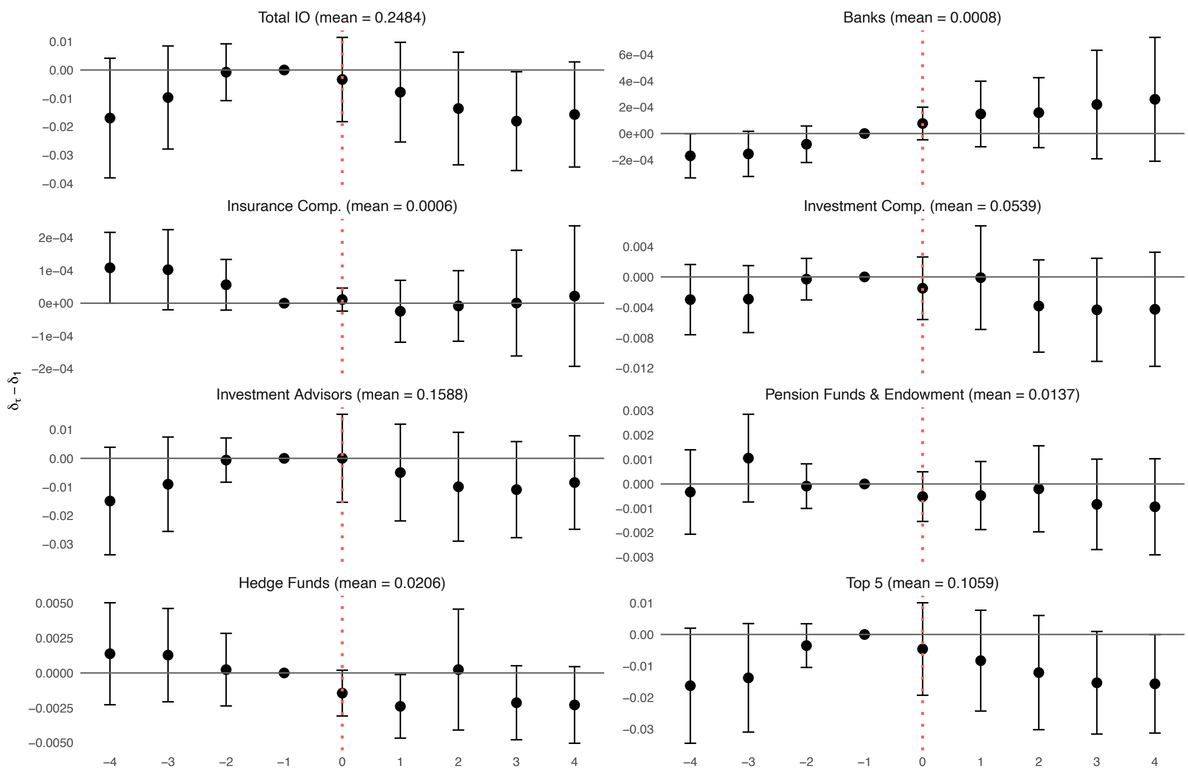

*Notes:* The figure shows the effect of an assassination event on institutional investor's holding position. The control group consists of corporations which are active in the extractive sector according to their TRBC code. Each cell displays the estimated effect relative to the quarter before the event ($\tau = -1$). The mean institutional investor holding position of companies that experienced at least one event during the sample period is presented in parentheses. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure D.12: The Effect of Assassination Events on Institutional Investor Holdings - Extractive Control Sample



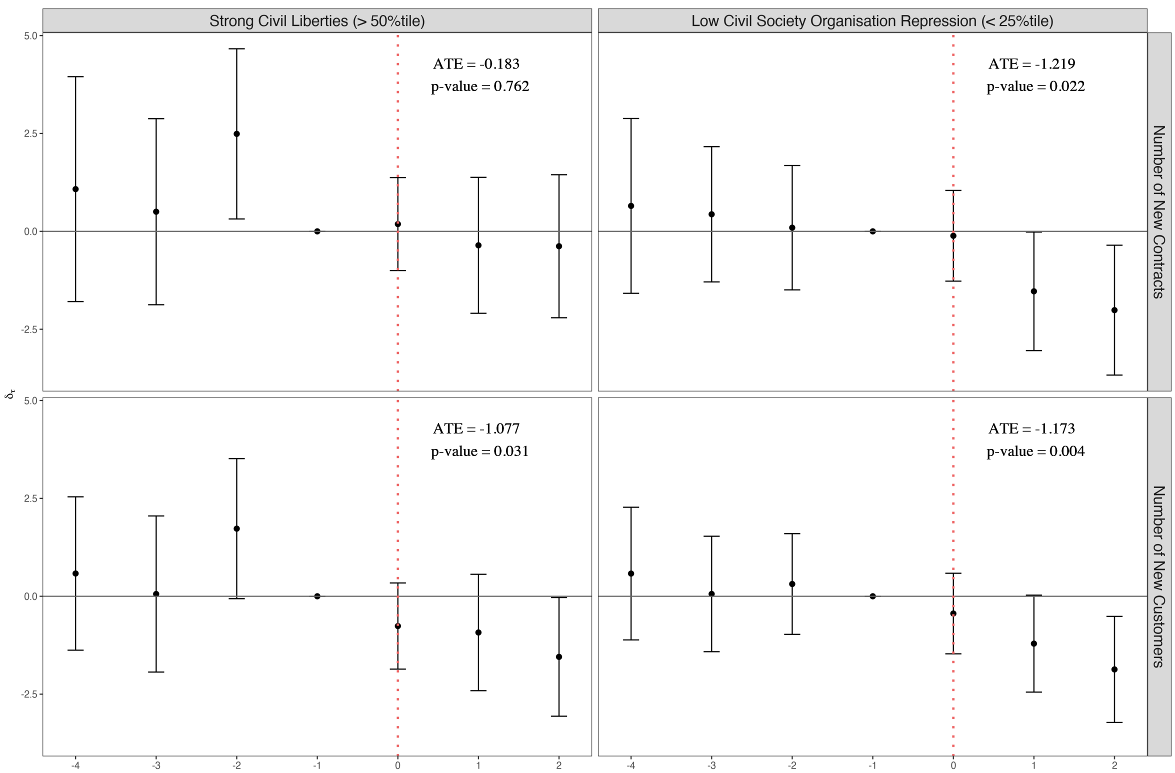

*Notes:* The figure shows the effect of an assassination event on institutional investor's holding position. The control group consists of corporations which are active in the extractive sector according to their TRBC code. Each cell displays the estimated effect relative to the quarter before the event ($\tau = -1$). The mean institutional investor holding position of companies that experienced at least one event during the sample period is presented in parentheses. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure D.13: The Effect of Assassination Events on Institutional Investor Holdings - Random Control Sample



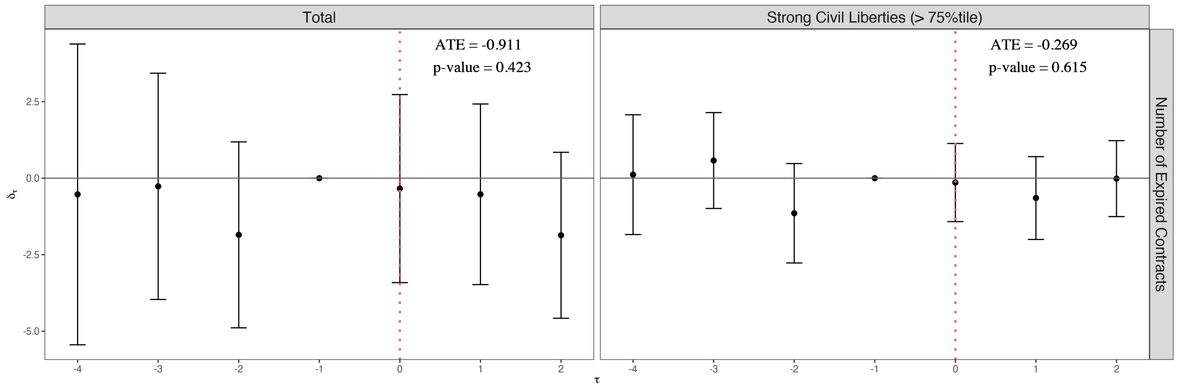

*Notes:* The figure shows the effect of an assassination event on institutional investor's holding position. The control group consists of 1000 randomly selected corporations which are not active in the extractive sector according to their TRBC code. Each cell displays the estimated effect relative to the quarter before the event ($\tau = -1$). The mean institutional investor holding position of companies that experienced at least one event during the sample period is presented in parentheses. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure D.14: Supply Chain Contracting - Robustness to Indicator Choice



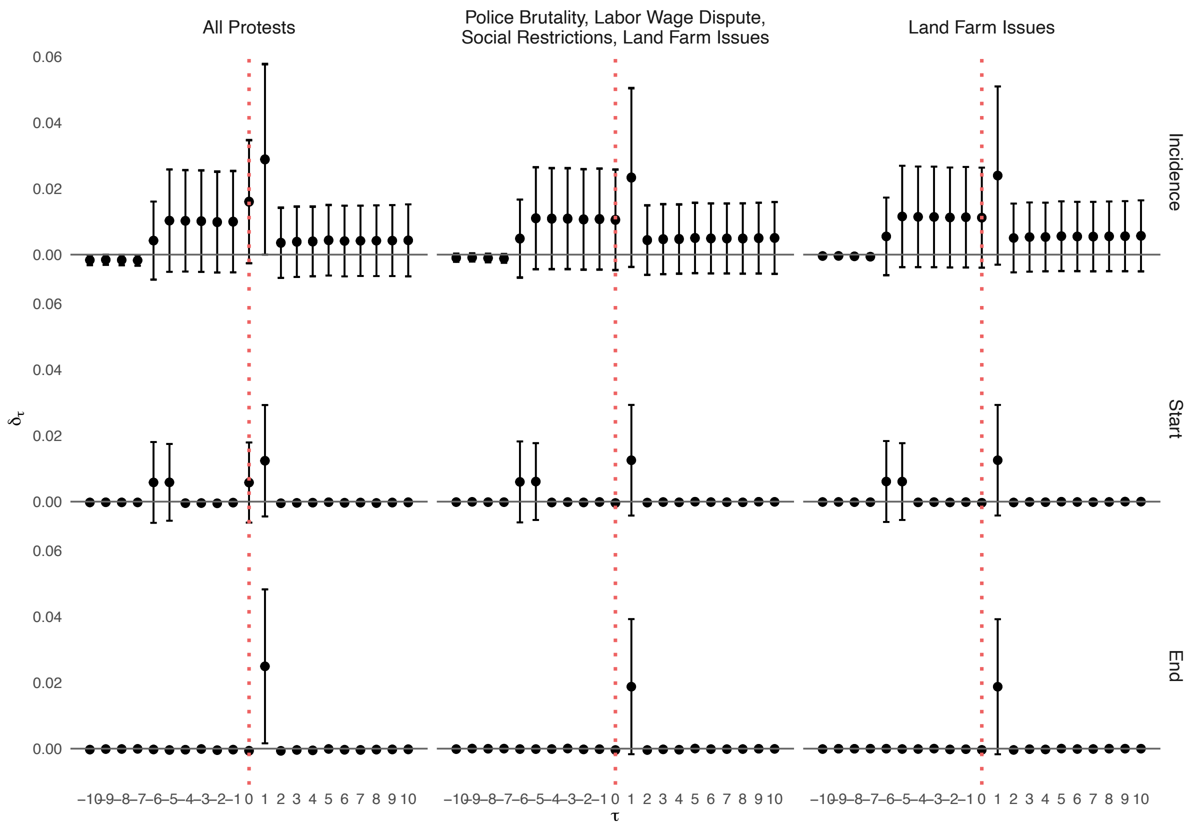

*Notes:* The figure shows the effect of an assassination event on supply chain contracting. Columns capture the supply chain contracts under investigations, while rows depict the corresponding dependent variable. Each panel displays the estimated effect relative to the year before the event ($\tau = -1$), with the corresponding average treatment effect (ATE) across all cohorts over the relative time period $[0; 2]$ and its $p$-value are depicted in the upper right corner. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure D.15: Supply Chain Contracting - Expired Contracts



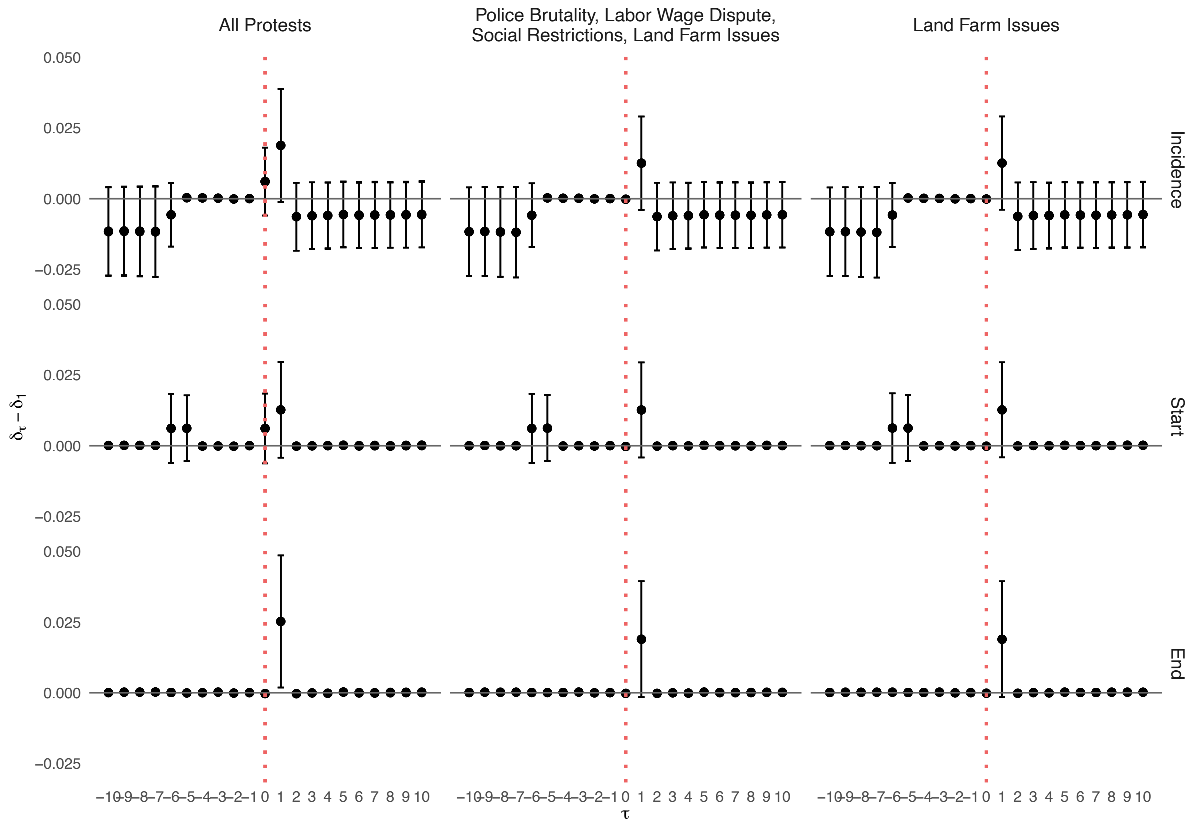

*Notes:* The figure shows the effect of an assassination event on supply chain contracting. Columns capture the supply chain contracts under investigations, while rows depict the corresponding dependent variable. Each panel displays the estimated effect relative to the year before the event ($\tau = -1$), with the corresponding average treatment effect (ATE) across all cohorts over the relative time period $[0;2]$ and its $p$-value are depicted in the upper right corner. 95% confidence intervals using robust standard errors clustered on the company-level are displayed.

Figure D.16: The Effect of Assassination Events on Protest




*Notes:* The figure shows the effect of assassination events on protest probability in an Admin1 region. The horizontal axis label denotes the days before and after the event on $\tau = 0$. Each cell corresponds to a different dependent variable with column labels depicting the type of protest and rows capturing if the dependent variable considers incidence, start, or the end of protests. 95% confidence intervals using robust standard errors clustered on the Admin1 level are depicted.

Figure D.17: The Effect of Assassination Events on Protest relative to the Day before the Event




*Notes:* The figure shows the effect of assassination events on protest probability in an Admin1 region. The horizontal axis label denotes the days before and after the event on $\tau = 0$. The estimated effect relative to the quarter before the event ($\tau = -1$) is presented. Each cell corresponds to a different dependent variable with column labels depicting the type of protest and rows capturing if the dependent variable considers incidence, start, or the end of protests. 95% confidence intervals using robust standard errors clustered on the Admin1 level are depicted.