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Abstract

Civil society is essential to governance, especially where laws and authority are weak. We study how a core strategy of international civil society groups—informing and publicizing human rights abuses—impacts those tied to abuse. Our study focuses on a major trend at the center of on-going international media campaigns: the assassination of civil society activists involved in mining activity. Collecting and coding 20 years of data on assassination events, we use Event Study Methodology to study how publicity of these events impact the asset prices of firms associated with abuse. We show that publicizing abuses has a significant impact on multinationals. Firms associated with an assassination have large, negative abnormal returns following the event. We calculate a median loss in market capitalisation of over 100 million USD, ten days following violence. We highlight the role of media publicity in our results. We show negative returns from assassinations are stronger during periods of low media pressure, versus when they coincide with competing newsworthy events. As well, we argue our results are driven by events where companies are explicitly named in media publicity, using a set of placebo events where no firms were identified by news coverage. Furthermore, we reject that our results are driven by other forms of unrest and conflict. Last, we show activist assassinations are positively related to the royalties paid by firms to domestic governments.

1 Introduction

Multinational corporations are political institutions. By revenue alone, the largest global corporations rival the size of nations.[1] As political actors, multinationals operate in a world without a single authority. Often where laws are murky and states are weak. The political power of multinationals poses a key question of governance for social scientists (Fukuyama,

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[1]Scholars have long juxtaposed multinational and states (Greene, 1983). Common means of doing so include revenue (Zingales, 2017) or market capitalisation. According to Global Justice Now (2018), in terms of revenue, 69 of the 100 largest economic entities are global corporations.

2016; Ruggie, 2018). For human rights scholars, civil society is seen as indispensable in holding powerful, international actors accountable (Acemoglu and Robinson, 2020). Yet without formal power, how does civil society govern? An influential scholarship argues that information and publicity are key weapons used by international activists to confront powerful agents (Brysk, 1993; Keck and Sikkink, 1998, 1999; Khagram et al., 2002).

The natural resource industry is a stark example of the tension between society and multinational power. Home to some of the largest global firms, the sector is a flashpoint across the developing world. Since 2002, more global environmental activists have been killed than Australian or U.K. soldiers in war zones (Butt et al., 2019). Figure 2 shows the rise in assassination of activists surrounding mining activity from 1998 to 2019. These abuses—-and those involved—-are the focus of global publicity campaigns by human rights groups and media (e.g. The Guardian and Global Witness). While a vibrant empirical scholarship has begun to explore how these strategies impact nations (Meernik et al., 2012; Hill Jr et al., 2013; Murdie and Peksen, 2014), what about private business? In the face of growing, targeted violence, how does the human rights spotlight impact sprawling multinationals?

Our paper explores the power of publicity in governing private actors. More precisely, we study how the informational strategies deployed by civil society (e.g. "naming-and-shaming" through the media) impacts multinational corporations. To answer this question we consider salient, well-publicized events at the heart of current international advocacy: the assassinations of environmental activists. We focus on one of the most eventful sectors for this abuse. Namely, the mining and mineral sector.

Our study estimates how publicity surrounding activist assassinations impacts the stock prices of multinationals. Specifically, the mining companies—and their operations—named in international media coverage of these events. To do so we collect and code 20 years of data on activist assassination across the developing world. Many of these events involved conflict surrounding mining concessions and activity. Thus, we draw from hundreds of these high-profile incidents to identify the mining projects associated with these events and hand-match them to publicly-listed corporations. Doing so allows us to explore how markets respond to news of violent events surrounding their operations.

We estimate the impact of human rights scrutiny with an event study analysis. We use two strategies to identify how publicity around these incidents is incorporated into the price of firms associated. First, we implement a canonical, large-sample event study to estimate the abnormal returns of companies connected to assassinations on the days before and after violence. On days leading up to assassinations, we find no evidence of abnormal returns for these companies. Importantly, after the event, we see significant, negative abnormal returns for "named" firms. Significant negative effects appear the day after a killing, and grow steadily for up to ten days after. Using new methods from financial econometrics, we show our findings are robust across a number of parametric and non-parametric test statistics.

Second, we build on classic event study methodology, and use the rich variation of our setting to estimate the impact of human rights publicity. We estimate these effects by comparing the abnormal returns for "associated" companies relative to controls. In particular, control firms operating in the same sector, event-country, and event period. Our regression estimates show a strong, robust pattern. Relative to control firms, companies named in news of assassinations have significant, negative abnormal returns directly following the assassination date–effects which accumulate through time. Thus, across both traditional and regression-based event studies, we estimate similar negative impacts of being connected to human rights abuses through publicity.

Together, we interpret our results as showing that information disclosed surrounding high-profile human rights news matters for investors. Importantly, the negative impact on publicly-traded firms is economically significant. For companies connected to events through publicity, we estimate that the median 10-day cumulative loss in market capitalisation is over 100 million USD. In other words, the informational strategies of international civil society impacts the bottom line of multinationals connected to the killing of activists.

Furthermore, our study explores the underlying mechanisms in three ways. First, we highlight the importance of media attention. To do so, we consider the likelihood that a media campaign reaches financial decision makers and examine how the impact of media attention varies over the news cycle. Using daily "news pressure" data (via Eisensee and Strömberg, 2007), we compare market reactions to assassinations during periods with more newsworthy events versus those with fewer newsworthy events. We show that the negative impact of assassinations disappears when they coincide with more active news cycles. However, the penalty survives when events occur during less eventual news periods. Second, we build on this media channel and show informationally-sensitive investor react significantly to assassination events, highlighting the potential role played by institutional investors. We find that institutional investors that follow event-based trading strategies, such as hedge-funds, divest more in mining companies following assassinations. Third, we highlight civil society's informational strategy of "naming-and-shaming" in the media. We do so by comparing companies named in connection to assassinations versus those merely operating close (geographically proximate) to events. We find that firms and operations in the vicinity event—though *not* named in media—are not penalised, relative to those whose operations are specifically named in publicity.

Last, we discuss the limits to bad publicity as a form of governance in this setting. Though incomplete, we do not find evidence that insiders trade off of prior knowledge of assassination events (see: Dube et al., 2011). We show a tentative explanation why mining companies get involved in assassinations in the first place, given that they can expect such large negative reactions from the market–especially in the wake of media coverage of these events. Collecting data on taxes and royalties paid by mining companies to the domestic governments, we find that the occurrence of assassinations is positively related to the royalties paid to the domestic governments. While preliminary, downstream multinationals may not have full control over the predatory behavior of local affiliates. Especially where local operations collude with governments and paramilitary forces.

We contribute to a rich literature in empirical political economy and forensic economics. In particular, recent work using asset prices to understand how markets and international political events. In method and spirit, our work relate to large(r) sample event studies of global political shocks (Dube et al., 2011; Girardi, 2020).[2] Girardi (2020) uses a large sample of international election events to study how internal asset prices respond to (formal) political shocks across democracies. Using the sample of CIA-authorized coupes, Dube et al. (2011) explore how the market responded favorably to US multinationals who stood to benefit for US coup authorisations. Our results stand on these papers, and suggest the use of asset prices to study firms as global political institutions unto themselves.

Our results suggest that market participants may expect (reputation or legal) costs to outweigh potential gains from conflict and malfeasance. In doing so, we build on seminal forensic analysis on how firm assets respond to conflict in the developing world. Focusing on Egypt's

[2]More broadly, a number careful event studies explore how firm and sectors respond to formal political transitions. Including important examples in the UK (Herron, 2000) and US (Knight, 2006; Wagner et al., 2018).

Arab Spring, Acemoglu et al. (2017) finds returns fall significantly on days of street-protests for companies with ties to the incumbent government, as investors adjust their expectations about their future rent-seeking ability.[3] For example, Guidolin and La Ferrara (2007) show international diamond firms reacted negatively to deescalation of conflict in Angola. More broadly, 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 arms trades in countries under arms embargo, and find that companies headquartered in corrupt countries may profit from increased conflict intensity. Our work suggests that the tools of civil society may help diminish returns to corporate misbehavior in the developing world.

Our results highlight the intimate link between civil society and media—two actors we see as intertwined. In doing so, we contribute to a growing literature on the political economy of the media. In particular, research emphasizing the role of media in intermediation. Work by Miller (2006) and Dyck et al. (2010), stress importance of media in uncovering corporate fraud and promoting corporate accountability. Specifically, the impact of headline-grabbing cases that appeal to a broad audience and provide career incentives. The media's role as a monitor is furthermore illustrated by the effect of public scrutiny and negative sentiment on managerial compensation (Kuhnen and Niessen, 2012) as well as capital decision in the form of acquisition abandonments (Liu and McConnell, 2013). The importance of news dissemination on the efficiency of financial markets is further illustrated by Peress (2014), who uses newspaper strikes to show that the absence of the information channel on strike days is not only accompanied by significant drops in trading volumes but also leads to lower dispersion and intraday volatility of stock returns. Dougal et al. (2012) show that not only the sentiment in financial news matters, as shown by Tetlock (2007), but journalists themselves possesses a causal influence on asset price returns.[4] We extend this literature, highlighting the complementary role of media and civil society.

Our results show how civil society and media work together to promote accountability. In doing so, we contribute to rich empirical literature on the role of media and information in promoting political accountability. Ferraz and Finan (2008) and Larreguy et al. (2020) highlight how information on malfeasances through local media and audits, respectively, disciplines local politicians. Seminal work (Snyder and Strömberg, 2010) shows how media coverage interacts with electoral institutions to discipline congressional behavior in the US.[5] In China, Qin et al. (2017) discuss the tacit approval of central autorities of controversial media as means of overseeing local-level corruption. Our study builds on this work and emphasising the potential use of media not only in constraining the behavior of conventional political actors, but also those of large multinational actors.

Our study highlights the role of international media and human rights advocacy in governance in the developing world. In particularly, in weakly-institutionalised democracies. These findings relate to a literature on market reactions to firm misconduct, though mostly in developed countries. Empirical studies in this area found evidence that investors negatively react to convictions as a result of illegal activities, such as corporate fraud (e.g. Miller, 2006; Dyck et al., 2010) or the introduction of laws that constrain unethical behaviour by companies (Cousins et al., 2020). Dai et al. (2015) show that media reports on past insider trading curb the

[3]This follows a large literature on political connections, including Fisman (2001)'s work on the political connections to General Soeharto in Indonesia.

[4]See for instance also Fang and Peress (2009), Bushee et al. (2010), Griffin et al. (2011), and Birz and Lott Jr (2011) on the media influence on financial markets as an information intermediary.

[5]In seminal work on the impact of television and radio exposure in Indonesia, Olken (2009) shows the importance of media in the governance of local road projects. While Olken (2009) reports negative effects on the attendance of village meetings responsible for planning and monitoring road construction, he finds no impact of television exposure on corruption associated with these projects.

profits of future insider transactions. Recent papers by Krüger (2015), Capelle-Blancard and Petit (2019) and Cui and Docherty (2020) also show that stock markets react negatively to negative news about environmental, social and governance (ESG) events. Though much of this literature is focused on corporate self-governance, and market response to malfeasance that occur in advanced countries, we find our results share much in common.[6] While focused on the discrimination, recent historic work Do et al. (2020) also highlights the importance of the media. The authors investigate market reactions to the "Dreyfus Affair." They find the infamous 19th-century episode of French antisemitism led to short-term devaluation of companies with Jewish connections, but show that (later) media revelations surrounding the scandal led to the rehabilitation of the French officer–and excess returns for these companies.[7]

To the best of our knowledge, this is the first empirical study that shows that international stock markets react—dare we say penalise—companies operating in association with high-profile human rights abuses. In our case, mining companies operating proximate to assassinations of civil society activists. Existing legal frameworks are often inadequate. Formal laws are unlikely to hold multinationals, or their subsidiaries, accountable for misconduct abroad (Ruggie, 2018). Though preliminary, our findings hint that the publicity strategies of human rights groups, which organise around and place a spotlight on such high-profile episodes, may have some bite. Specifically, by revealing information to international markets. Even where formal justice is rare, these strategies may nevertheless have impact.

The remainder of the paper is organised as follows: Section 2 introduces our data, section 3 describes our empirical methodology and presents the results; section 4 presents the analysis of the media channel, section 5 provides a tentative explanation for the occurrence of the assassinations; and section 6 concludes.

2 Data

We use event study methodology to study the impact of publicity surrounding 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 this data collection effort. We first describe the definition and scope of the assassination events used in our analysis. Second, we describe the collection and basic contours of our dataset. Third, we describe the coding of companies. We conclude by describing additional financial and GIS data.

2.1 Assassination Events and Descriptive Statistics

Assassination Definitions - Our focus on assassinations is intentional. Since the early 2000s, the international 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 natural resource activity. Figure 2 shows this trend using our newly collected dataset on global activist assassinations–events at the center of our study and a wave of current global human rights advocacy. Such events—including the victims and associated actors—embody the protagonists in international human rights campaigns. Where the "informational politics" groups focused on the names of victims and the naming of

[6] E.g. through fines, lengthy court cases, or the withdrawal of a business license

[7] In a related work on discrimination, Huber et al. (2019) examine how the dismissal of Jewish managers in Nazi Germany led to a decline in firm stock market value.

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

By *assassinations*, we mean the intentional killing of prominent members of civil society.[8] Since these individuals are locally prominent–as advocates or key players in their communities–we refer to their slayings as *assassinations.* These people include indigenous and tribal leaders; environmental and labour activists; members of the clergy; and more. Throughout the paper we use the terms *assassination* and *extra-judicial killing* interchangeably.

We study assassinations that are well-publicized. In other words, those that draw human rights media attention. We follow the human rights scholarship on "naming-and-shaming" and spotlight campaigns, and focus on international media publicity of these events (Ramos et al., 2007; Hafner-Burton, 2008; Peksen et al., 2014).[9] It bears repeating, while we study publicity surrounding violence, we are not studying the impact of violence *per se*, nor does this study focus on assassinations that go unpublicised (we discuss this issue in relation to our research design in the following Section). As such, we are interested in publicity that is likely to reach international markets.

We focus on assassinations surrounding mining and mineral extraction activity. We do so for three reasons. First and foremost, while mining and minerals are at the heart of conflict and violence, in our case, this sector is one of the most deadly sectors for activists.[10] Second, it is a highly capital-intensive sector. For the purpose of this study, one where equity financing is common. Given the attention of foreign capital, it is a sector where we are able to connect publicly traded firms to events. Third, we focus on more upstream, raw-materials products. In particular, commodities and non-differentiated materials that are not typically consumer-facing.

Event Data and Descriptive Statistics - Our assassination dataset covers 354 killings over 20 years. Our data collection process can, broadly, be summarised in the following way. First, we consider killings that are *publicly* reported in media 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 where the death occurred. Fourth, we code the mining companies or projects named (if any) in relation to the event. Below we describe our data collection and coding, and provide details in Appendix B.1.

Table 1 provides descriptive statistics of our assassination data. These events are selected using *both* algorithmic and human searches of international full-text media archives. These include full-texts collections of International Herald Tribune; the Associated Press wire archive; popular news APIs (Guardian); and global news databases (LexisNexis).[11] Coding is done by RA and cross-validated by principal investigators.

Table 1 shows, since our first observation in 1998, we record 496 victims of violence across 31 countries. Peru and Philippines are the most dangerous countries for mining activists. Figure 4 maps the geographic distribution of assassinations.

[8]This definition is not idiosyncratic. The operating definition in this paper and dataset tracks The Associated Press' style guide, as well as that of the US National Public Radio standard. Academically, our definition aligns with the definition used in human rights scholarship (for example, DeMeritt, 2012).

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

[10]See: https://www.globalwitness.org/en/press-releases/spotlight-criminalisation-land-and-environmental-defenders/

[11]These include multilingual searches. Some media databases provide translations of international news coverage as well, such as LexisNexis.

The alluvial plot in Figure 3 shows the global distribution of events in our dataset that are connected to publicly traded corporations. The coloured panels each correspond to a distinct country. Their height represents the number of events in a given country. Figure 3 shows the global breakdown of assassination events in two ways. The left column displays the distribution of events by the country where the assassination takes place. The right presents breaks events by the headquarter country of the company associated with each killing.

2.2 "Associated" Companies

In addition to assassinations, Table 1 shows the publicly traded firms "associated," or matched, to at least one event. Throughout this study, *association* means that a company, or their mining project, is named in media reports surrounding an assassination event. We take no stand on the nature of relationship between a firm and the violent event, and default entirely to the source articles for the match. Thus an "associated" company does not mean a company plays a role in organising or participating in violence. Rather, it is merely tied to the event insofar as it, or its operations, are mentioned in publicity surrounding the violence. This point is particularly important, as a publicly traded company may be an indirect owner of a project where the violence occurs.

Figure 3 shows a key feature of our data. A majority of assassinations in our data are matched to firms headquartered in Canada, the United Kingdom, and the United States. It is worth pointing out that, though these are advanced democracies, suits against multinationals for human rights abuses are exceedingly rare. Only recently have companies faced consequences in home country courts, highlighting the need for civil society to fill the legal vacuum left behind by international and national legislations.[12]

We hand-match information on the "nearest" publicly traded owner of the company. We do so in the following way: First, we determine whether the reported company named is publicly traded.[13] Second, if the company is not publicly listed, we then examine if the named company is a subsidiary or joint venture of publicly traded companies (when the event occurred). We verify this information using official firm websites; final year reports; SEC filings; and public business registers. Third, when reporting refers to a mining project–not the company overseeing the project–we attribute ownership of the project to the publicly traded company using the sources from step two.

The following example provides a concrete illustration of our coding process. Figure 5 presents an excerpt from a *Guardian* newspaper report for an assassination event in our dataset.[14] This article identifies the victim of the event (green), the Ecuadorian indigenous leader José Isidro

[12]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, 2019) after the Canadian Supreme Court had declined to hear similar cases in the past (The Guardian, 2020). In 2019, the UK Supreme Court ruled against *Vedanta Resources* for human rights abuses abroad can be heard. However, the ruling also clarified that the claimant needs to conclusively show that the violence is attributable to the company; a high hurdle given the often opaque web of subsidiaries, affiliates, and contractual parties, as demonstrated by the dismissal of a similar case against *African Minerals* (Morrison Foerster, 2020). For the US, Ruggie (2018) reports that of the 150 cases brought forward only one was heard by a jury and only two were settled outside court for modest sums.

[13]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. For consistency, we consider the most direct, publicly traded companies, except when the global corporate owner is specifically tied to the event in one of the articles.

[14]The source article can be found here: https://www.theguardian.com/world/2014/dec/06/ecuador-indigenous-leader-found-dead-lima-climate-talks .

Tendetza Antún, and establishes they were in opposition to the mining activity (purple). Furthermore, it describes the victim's violent death (yellow).

Importantly, this source assassination event in Figure 5 relates to conflict around a specific mining project: the "Mirador copper and gold mine (blue)", owned by *Corriente Resources Inc.* through its subsidiary *EcuaCorriente S.A.*. We code the ultimate owner using this information; searching public corporate data corresponding to this project reveals that *Corriente Resources Inc.* was acquired in 2010 and is ultimately owned by two publicly traded companies at the time of the event: *China Railway Construction Corporation* and *TongLing Nonferrous Metals Group Holdings*.[15] *Both* publicly traded companies are then coded as being "associated" with the event in our dataset. If a particular project (company) is not named in an article—that is, if only the purple, not the blue highlighted information is available—we record the mining-related assassination as not having a company tie.[16]

The previous example also shows an important point about event dates: while the date of the crime can be pinpointed as 28 November 2014 (pink), an event may make the news only after discovery of the body, etc. In this case, financial markets are likely to react only days after the *de facto* event date.

2.3 Further Data: Financial Outcomes, Geo-Location, and Control Companies

We collect daily stock returns data for publicly traded mining companies associated with event (see: Section 2.1 above), as well as returns for companies operating within same country, during the year the event took place. We refer to the former companies as "treated" (associated with an event), and the latter companies form "control" companies. Daily return data for 1998-2019, and additional firm-level data, come from the *Datastream* database.[17]

For mining projects in our dataset, geolocation and company ownership data comes from the *SNL Minings & Metals* database, which are then manually matched to our assassination data.[18] This 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 at the annual level allowing us to track (treated and control mining) companies operating in an event-country over time. For example, if we observe an assassination event in Colombia in 2013, the control company set for this event comprises all publicly traded companies that own mining projects in Colombia in 2013 but are not directly associated with the assassination in media or NGO reports. Figure C.2 Panel A in the Appendix provides a graphical illustration of this example case.

Our set of control companies has a number of advantages. First, we account for exposure to political risk events common 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, we

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

[16] Figure C.1 in the Appendix presents an example case in our dataset related to mining opposition but without a connection to a specific mining project or company.

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

[18] For more information 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, respectively corruption.

follow the rationale established by Guidolin and La Ferrara (2007); we wish to compare treated companies to those with a similar “comparative advantage” for operating in political risk environments (p.1987). Last, by limiting the control group geographically, we help control for commodity price fluctuations similarly impacting mining companies operating in similar commodity markets and the same domestic market.[19]

Summary statistics of the main variables are presented in Table 2. For the construction of market returns, we rely on the MSCI (Morgan Stanley Capital International) stock indices. We match mining company securities to their associated MSCI country index based on where they are listed. 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.[20]

3 Event Studies: Activist Assassinations and Stock Returns

We use event study methodology to study whether the market responds to companies associated with assassinations of environmental activists. Our goal is to determine how–and if–the average stock price of these companies change following publicity surrounding these killings. To do so, we examining the cumulative abnormal returns (CARs) of impacted companies. We estimate and track the returns of mining firms, starting on the date of violence and estimating 1-day to 10-day returns starting from the event date.

We use two related strategies to study global market response to publicity around assassinations. First, we employ a traditional (“classic”) event study methodology. We use a traditional market market model, and show that our results are robust to several parametric and non-parametric tests. Second, we consider the cumulative abnormal returns of affected (“treated”) versus control companies using cross-sectional OLS regressions.

In addition to examining the CARs of mining companies *after* publicity of assassination, we also consider returns *prior* to the date of violent events. We estimate the returns from the 10 to 1-days before the occurrence of an assassination, and test whether there are significant changes in the stock price prior to events occurring. These changes could indicate insider trading on prior knowledge of pre-meditated assassinations. More broadly, we are interested in whether firms associated with violence experience systematic changes in stock prices preceding violence.

3.1 The Classic Event Study: Estimation Strategy

Estimation and Event Timing - We first describe the classic event study methodology in the context of an assassination event timeline, shown in Figure 1.

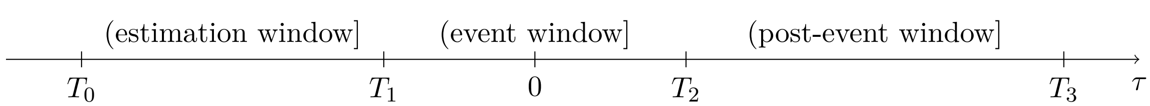


Figure 1: Event Study Time Line

[19] 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.

[20] For completeness, 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.

Abnormal returns are the difference between the observed return of company i and their expected return, absent the assassination event e. Abnormal returns AR around an assassination event can be written as

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

where X_τ is the information set that expectations about the "normal" returns are conditioned on, and τ denotes time relative to an event date.

Consider our timeline in Figure 1. The time around the assassination event at $\tau = 0$ can be split into three chunks, or windows. An *estimation*, *event*, and *post-event* window. Following the event study literature (MacKinlay, 1997,), normal returns for company i are determined by estimating a market model over the *estimation window* ($\tau = T_0 + 1, ..., T_1$). The market model's linear specification for firm i affected by assassination e is

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

where $R_{i,e,\tau}$ is the observed daily stock return for firm i, $R^M_{i,e,\tau}$ is the return of the market index where company i is listed, and ϵ is the residual.

For estimating the model parameters, we choose an *estimation window* of 250 trading days, ending 30 days *before* the event (day 0).[21] For this calculation, we require securities to be traded at least 200 out of the 250 trading days, and 8 out of the 11 trading days during the *event window* from $T_1 + 1$ to T_2 under our baseline specification.

Estimated abnormal returns, $\widehat{AR}_{i,e,\tau}$, and cumulative abnormal returns from τ_1 to τ_2, $\widehat{CAR}_{i,e}\left(\tau_1, \tau_2\right)$, during the *event window* are given by

$$\widehat{AR}_{i,e,\tau} = R_{i,e,\tau} - \hat{\alpha}_{i,e} - \hat{\beta}_{i,e}R^M_{i,e,\tau} \tag{3}$$

$$\widehat{CAR}_{i,e}\left(\tau_1, \tau_2\right) = \sum_{\tau=\tau_1}^{\tau_2} AR_{i,e,\tau}, \tag{4}$$

where $T_1 < \tau_1 \leq \tau_2 \leq T_2$. The conditional variance of the respective cumulative abnormal return is

$$\sigma^2\left(\widehat{CAR}_{i,e}\left(\tau_1, \tau_2\right)\right) = L_2\sigma^2_{\epsilon_{i,e}}\left[1 + \frac{L_2}{L_1} + \frac{L_2\left(\frac{\sum_{\tau=T_1+1}^{T_2} R^M_{i,e,\tau}}{L_2} - \mu^M_{i,e}\right)^2}{L_1\widehat{\sigma}^M_{i,e}}\right], \tag{5}$$

where $\sigma^2_{\epsilon_{i,e}}$ is the variance of the regression residual $\epsilon_{i,e,\tau}$, and $L_2 = \tau_2 - \tau_1 + 1$ is the length of the "aggregation" period. The terms $\mu^M_{i,e}$ and $\widehat{\sigma}^M_{i,e}$ are the mean and variance returns over the the estimation period, $L_1 = T_1 - T_0$. The second component of $\sigma_2\left(\widehat{CAR}_{i,e}\left(\tau_1, \tau_2\right)\right)$ accounts

[21] This follows Li and Lie (2006) and Acemoglu et al. (2016).

for the additional variance introduced by the sampling error in $\hat{\alpha}_{i,e}$ and $\hat{\beta}_{i,e}$ (Salinger, 1992; MacKinlay, 1997).[22]

Inference in "Classic" Event Studies - We now describe issues with inference in event study methodology, and motivate our use of multiple test statistics for inference. We show that Kolari and Pynnönen (2011)'s GRANK test specifically speaks to our setting where the day of pricing is likely to deviate from the actual assassination date.

First and foremost, our study is about the *average* effect of publicity surrounding assassinations. To consider overall inference of events, we must aggregate cumulative abnormal returns across all company-event pairs N. Thus, the average CAR, and its variance are,

$$\overline{CAR}(\tau_1, \tau_2) = \frac{1}{N} \sum_{j=1}^{N} \widehat{CAR}_{i,e}(\tau_1, \tau_2) \tag{6}$$

$$\sigma^2\left(\overline{CAR}(\tau_1, \tau_2)\right) = \frac{1}{N^2} \sum_{j=1}^{N} \sigma^2\left(\widehat{CAR}_{i,e}(\tau_1, \tau_2)\right) \tag{7}$$

Under the assumption of normally distributed security returns, and in the absence of clustering (overlapping event-windows), $\overline{CAR}(\tau_1, \tau_2)$ follows a normal distribution with mean zero and variance $\sigma^2\left(\overline{CAR}(\tau_1, \tau_2)\right)$ (MacKinlay, 1997).[23] We refer to this test as the "Normality" test.

There are many reasons researchers wish to relax "normality" assumptions.[24] We go beyond the standard ("Normality") tests in three ways.

First, we utilise two additional parametric tests suggested by Boehmer et al. (1991) ("BMP") and Kolari and Pynnönen (2010) ("Adjusted-BMP"). The first parametric test ("BMP") scales abnormal returns, adjusting for differences in the variance of pre-event residuals (building on Patell (1976)). Intuitively, more volatile securities are down-weighted to prevent them from biasing the detected average event effect; and the "BMP" test goes beyond Patell (1976)'s test by accounting for changes in event-induced volatility. Second, we use a refinement of "BMP" and account for event clustering. Though clustering issues should be minor in our setting, we nevertheless account for the potential bias from cross-correlation of abnormal returns. We do so by using Kolari and Pynnönen (2010)'s adjustment ("ADJ-BMP").[25]

Our third—and preferred—approach, relaxes the assumption of normally distributed returns altogether, and specifically deals with informational environment around the assassinations in our sample. We implement the non-parametric generalized rank t-statistic ("GRANK") of Kolari and Pynnönen (2011). While the GRANK test relaxes the parametric assumptions that have plagued the event study literature, it is also particularly suited to the context of this study. As Kolari and Pynnönen (2011) show, GRANK outperforms other parametric tests (as well as non-parametric sign and rank tests) in event study settings where 1) the

[22] It follows from (5), that the variance of the abnormal returns during the *event window* is given by $\sigma^2(\widehat{AR}_{i,e,\tau}) = \sigma^2_{\epsilon_{i,e}}\left[1 + \frac{1}{L_1} + \frac{(R^M_{i,e,\tau} - \mu^M_{i,e})^2}{L_1 \sigma^M_{i,e}}\right]$.

[23] The normality assumption requires the absence of clustering in order to set the covariance terms in (7) to zero.

[24] Following a long literature in event study methodology (Boehmer et al., 1991; MacKinlay, 1997).

[25] 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.

exact event day may be unknown and 2) long event windows are used. In particular, pre-event information in our setting given its sensitivity may leak slowly into the market and the precise day of the pricing–in contrast to the assassination date–is uncertain. In addition, the GRANK t-statistic has been shown to be robust to event-induced volatility, serial correlation and event-day clustering (Kolari and Pynnönen, 2011). Details on the respective test-statistics can be found in A.1 in the Appendix.

Thus, we show that results from our core event study across tests. Which we now turn to.

3.2 Classic Event Study: Results

Table 3 shows our main results. Overall, we find assassinations 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. Importantly, the effects are significant across tests.

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

The average 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 with the "BMP" (4) and adjusted BMP statistics (5). Furthermore, by considering multiple test statistics, our findings in Table 3 suggest differences in volatility across securities could bias inference if not accounted for: comparing our adjusted test and non-parametric statistics to the "normal" test in column (3). Meanwhile, the clustering issues in our setting seem negligible—the differences between the BMP and adjusted BMP tests are small (columns 4 versus 5).

All our test-statistics account for non-trading days of securities, adjusting for the length of the *estimation* and *event window*. However, our results are robust to more conservative trading day criteria. Appendix Table D.1 shows our results are unchanged when we require companies i) be traded each day of the *event window* and ii) 225 out of 250 days during the *estimation window*. This adjustment drops seven company-event pairs, leading to a marginal decline in the magnitude of the CARs to -1.5 percentage points ten days after an event. These results are still highly significant and robust across tests. This difference 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 to be more vulnerable to disruption following an event. Thus, strict requirements on the frequency of trading 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. By considering the days prior to the assassination, we test whether market participants had foreknowledge of assassinations. Furthermore, where assassinations are planned and executed by insiders, the "authorization" data 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 this "prior knowledge" hypothesis, and also test for any pre-trends in stock returns, by aggregating abnormal returns *backwards* starting on the day before the event (c. for instance also Dube et al., 2011). Appendix Table D.2 reports these 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. These results indicate two key 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.

3.3 OLS Regression

We now expand on the "classic" event study design from the previous section by exploiting the relatively large number of events in our sample. More specifically, relative to many small-N event studies, our extensive event data (over 160 companies-event pairs) introduces rich panel and cross-sectional variation to further explore market reactions to assassinations. This section introduces an intuitive, yet powerful OLS estimation strategy, whose results allow us to develop an even stronger empirical case.

3.3.1 OLS Regression: Empirical Strategy

Our regression framework examines the relationship between the publicity around assassinations and stock returns of companies differentially exposed to violence. Intuitively, we compare the cumulative abnormal returns for companies whose projects are directly associated (named) with these events versus companies operating in the same sector, country, and period of the assassination. The regression model we consider can be written as

$$CAR_{i,e}\left(\tau_1, \tau_2\right) = \alpha + \delta D_{i,e} + \boldsymbol{X}'_{i,e}\phi + \gamma_e + \epsilon_{i,e}, \tag{8}$$

where $CAR_{i,e}\left(\tau_1, \tau_2\right)$ is the cumulative abnormal returns for company i. These CARs are estimated for periods τ_1 to τ_2 for each event e. The indicator $D_{i,e}$ denotes treatment, and is equal to one if a company is "associated" with an event, and zero otherwise. Our coefficient of interest is δ, which captures the average difference in cumulative abnormal returns between firms associated with an event versus control firms—companies with operations in the same country, sector, over the event period. Our empirical strategy is valid if, absent violence during the event period, we would not observe systematic differences in the returns of treated ("associated") versus control firms.

Our preferred specification, (8), includes a vector of event-specific effects, γ_e. 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. However, including this relatively conservative set of event-specific effects also absorbs a lot of potentially useful variation. Thus, alongside the preferred equation above, we present alternative specifications, which include combinations of year (γ_y), company (γ_i), and headquarter country (γ_h) effects.[26] Comparing results between our preferred specification (8) and specifications with less restrictive fixed effects allows us to potentially characterise aspects of bias. Specifications with firm-level fixed effects, fully leverage our data structure. Depending if a company was

[26] Recall that a calendar year and event-periods are distinct time periods.

merely active in the country during an event-preriod or directly tied to an assassination it can be part of both, the control and treatment group, for different assassination events.

In addition, we include a set of time-variant firm-level controls, $\boldsymbol{X}_{i,e}$ in our baseline specification: firm size, (log) total assets, and leverage (total debt to capital). For example, small or highly-leveraged firms may be more dependent on specific mining projects, and thus differentially impacted by events. Similarly, firms with these characteristics may be more prone to engage in violence. Disruptions of projects may be more punitive for smaller (more leveraged) firms. We also present a specification which includes cubics in size, leverage and additionally profitability (return on equity) to account for potential non-linear effects as well as previous firm performance (c. Acemoglu et al., 2016). To alleviate the issue of "bad controls", we use lagged values of the covariates. Standard errors are robust to heteroscedasticity and clustered at the event level.

3.3.2 OLS Regression: Results

We report our main results of equation (8) in Figure 6 which compactly displays the estimates from 210 individual regressions. [27] Columns correspond to two types firm-level controls used in our specifications. Rows show five types of fixed effects. Each panel (cell), thus, shows a set of estimates for one of our ten specifications. Points are regression coefficients (δ) for *individual* regressions, with the vertical axis showing the (τ) days after (before) the event. We present 95 percent confidence bands in grey.

Figure 6 conveys a clear pattern that is consistent across ten specifications: In the days *prior* to the event, cumulative abnormal returns are not significantly different between exposed and non-exposed firms. Returns for associated firms are, if anything, slightly positive.[28] Our results are particularly strong for specifications using company fixed effects (Figure 6, row 5). That is, when we use only within-firm variation, the significant cumulative abnormal return provides strong evidence for the presumption that the factual or expected disclosure of a company's role in the assassination event is at the root of the estimated effect, a mechanism we will investigate further in Section 4.

Furthermore, Figure 6 confirms the pattern found in our traditional event study results. We see no market reaction on the event day, but markets start responding to the assassination soon after. We see that two days following an assassination, estimated CARs are statistically significant, and lie between -1.1 and -1.3 percentage points. Thereafter, CARs remain negative before gradually declining around five days after the event. By day ten, the abnormal returns for exposed mining companies are between -2.2 to 3.3 percentage points. Qualitatively, 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.

What is the magnitude of these results? We quantify our findings in Figure 7. Panel A presents the estimated loss in market capitalization for the median "treated" company (those associated with an assassination). Dots correspond to our baseline regression estimates, and the minimum (and maximum) loss across specifications are captured by the error bar. Panel A shows that the median "treated" company is estimated to loose between 100 and 150 million USD in market capitalization over the ten days following the event. Panel B shows

[27] Full regression tables are reported in Appendix Table D.3. Throughout this section, tables corresponding to regression figures are presented in our Appendix.

[28] Note: Recall, for pre-event periods, we aggregate backwards. Here, CARs start with the trading day before the event.

the distribution of losses across companies for the baseline specification. Demonstrating large, and economically meaningful losses for companies associated–even loosely–with violence.[29]

3.3.3 Robustness for OLS Results

In this section, we conduct a number of robustness check. First, we investigate if our results are driven by a particular, individual nation or company. We answer this question through a "leave-one-out" analysis, presented in Figure 8. We plot our baseline regression results in bold. In Panel A, we re-estimate our baseline specification, but sequentially drop individual event-countries from our sample. We plot the estimates for these regressions in light grey. Panel A shows, visually, that our negative effects are similar. The core pattern of out baseline results are robust to excluding the two deadliest countries for mining activists: Peru and the Philippines (plotted dashed and in red). Across this analysis, A shows a clear and gradual decrease in abnormal returns over the 10 days following the event, while the abnormal returns in the days leading up to the event remain slightly positive, though close to zero.

Figure 8 Panel B repeats the same exercise, but for individual firms. Unsurprisingly, our baseline results are qualitatively similar. The light grey bands indicate that our findings are not driven by particular "bad actors," and show that our results are more broadly applicable to publicly traded mining multinationals.

One may worry that our results are driven by certain types of events. Table D.4 shows that the post-event assassination coefficients are qualitatively unchanged if we exclude unsuccessful assassination attempts (columns 1 to 5 in Table D.4). Similarly, when we exclude activist killings during protests (columns 6 to 10). While the point estimates here follow a similar pattern to our baseline results, we exclude a substantial number of events. Excluding 42 event observations in this case, reduces the power and significance of the results.

Next, we test for the effect of outliers on our results by dropping cumulative abnormal returns larger than the 99th percentile or smaller than the 1st percentile (columns 11 to 15 in Table D.4). The assassination coefficient is similar in magnitude and more precisely estimated across specifications with the exception of company fixed effects. The latter indicates that the association with an assassination event constitutes an extreme observation for several companies that have been tied to an event at least once, providing further support for the substantial impact of assassination events on asset prices.

Finally, we expand the event window to twenty days prior and post the event day to investigate the possibility of pre-trends more closely and test for a potential reversal in the estimated effect after day 10. Figure C.3 in the Appendix graphs the assassination coefficient estimates and the 95% confidence intervals for our baseline specification from now 41 separate regressions.[30] Considering CARs after the event date, we observe a monotonic decrease in returns until day 13 that seems to be permanent. In contrast, estimates are positive and never significant prior to the event date. We conclude that the effect of publicised assassination events is persistent and cannot be explained by pre-existing trends in the stock price.[31]

[29]For clarity, due to long-tails, we do not display losses above the 90th percentile in Panel B.

[30]Note that we adjust the minimum trading days requirement 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.

[31]Figure C.4 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 C.4 simply displays average CARs when underlying the market model (see Section 3.1), not OLS estimates.

4 Mechanisms

This section sheds light on the underlying mechanisms that explain how civil society campaigns around activist assassinations have such an immediate and relatively large impact on the stock price of associated companies. The overall process, can be summarised as follows: Civil society groups' main strategy is to name the mining companies linked with the event and use the global news media to make the general public and the financial markets aware of the human rights abuses. The likelihood that a story about an activist assassination gets wider attention and reaches financial decision makers, depends on the amount of other newsworthy events on the days around the assassination. Investors whose strategy is more sensitive to (negative) news will relatively quickly decrease their holdings in associated stocks to avoid short-term losses. This sudden reaction by institutional investors who follow an event based strategy, leads to the short-term drop in share prices depicted in our main results.

4.1 Vicinity vs. Associations to Assassinations

So far, our classic event study, and the regression results, present a consistent story: assassination events significantly—and negatively—impact the returns for companies associated with violence. In this section we further unpack what may be driving these results. Specifically, 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 be driven by other, physical disruptions surrounding the event or unobserved regional violence correlated with the assassination event.

In this section we address this concern by comparing the asset price responses of firms publicly exposed to the event to other companies operating within the same administrative unit in the event country. We use the regression framework from above to compare the CARs of treated firms to untreated firms operating in the same sector and same administrative unit as the treated firm.

For this robustness check, we construct a new set of control companies. We do so 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 C.2 Panel B in the Appendix for a graphical illustration).[32] 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 an violent event. Figure 9 shows the results of our analysis. The post-event assassination coefficient estimates for our Admin1 subsample are presented in Panel B, and Panel A displays the estimates for our baseline sample.

While we lose observations through our geo-matching process, we nonetheless 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 D.5 shows that our estimates are broadly negative across specifications and of similar magnitude. With the exception of column 9. Column 1 shows the specifications where we use maximal variation of our data, excluding fixed effects. Here, the average CAR is -2.8 percentage points, significant at the 5% level. Our results indicate that the negative impact we observe are likely not driven by the

[32] We do so through ArcPy/ArcGIS. 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.

proximity to violence and spatial disruptions from these events. This reifies our story that our results are likely driven by firms being associated to violence through the media. Furthermore, our results suggest that traders may be well informed about circumstances surrounding the events, which we investigate further in Panel C.

In Figure 9 Panel C, we use publicised assassinations events of activists where no company was linked in the media reports. In other words, we consider assassinations where no firm has been publicly associated with the violence.[33]

With this in mind, we rerun our regression analysis. However, we consider slightly different treatment and control groups for these "unattributed events." As we did before, we geolocate the "unattributed events" in our sample. Then, using this location information, we consider *all* companies within the same *Admin1* region of the event as "treated" ($D = 1$). All other companies in the event-country constitute our control group ($D = 0$).

The results reveal an interesting—though again, imprecisely estimated—pattern. Market participants appear to react with a time lag.[34] This behaviour is consistent with a lengthier information gathering process for events that *ex post* have no direct company tie. Further, the results suggest that financial markets might have access to information that is not publicly available.

4.2 The Role of the Media

We now turn our attention on the nexus between publicity of activist assassinations and the media. The role of the media in this context is twofold: First, journalists are often among the first ones to shed light on activists assassinations and media outlets help to communicate these stories to a broader audience. In most cases, media outlets are also the main source of information for investors about these type of market relevant events. Second, increased media attention for an assassination event, means more negative publicity and a higher chance of public backlash against the resource company tied to the assassination. As such, media attention for an event can be one of the main mechanism driving our baseline results.

A major empirical challenge analysing 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 might be a good measure for media attention, but at the same time could be driven by negative stock market reactions to the assassination event.

Instead of relying on a direct, but endogenous, measure for media attention for assassination events, we use an exogenous variable driving the likelihood that an assassination receives media attention. In particular, we use the *daily news pressure* index developed by Eisensee and Strömberg (2007).[35] In particular, we expect the likelihood of the event to get reported or gain attention to be smaller if the event coincides with a "high news pressure day"–defined as an above median news pressure day for the period from 1998 to 2018.[36] Even if investors obtain information from private sources and not the media, news pressure should serve as an

[33] Note that we cannot rule out that reports exist that establish a link between the assassination and a mining company or project. 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 D.6 in the Appendix.

[35] 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.

[36] Recall that 1998 constitutes the the first year in our assassination dataset and 2018 is the last year for which the *daily news pressure* is available.

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 broadcasted in the top three news segments, making *daily news pressure* an arguably exogenous measure in our setting.

To estimate the heterogeneity of the assassination effect by the likelihood of media attention, we expand our baseline regression model by an interaction term between the treatment indicator $D_{i,e}$ and and a dummy variable N_e equaling one if the event falls on a high news pressure day, and 0 otherwise:

$$CAR_{i,e}(\tau_1, \tau_2) = \alpha + \alpha^N N_e + \delta D_{i,e} + \delta^N D_{i,e} N_e + \boldsymbol{X}'_{i,e}\phi + \gamma_e + \epsilon_{i,e}. \tag{9}$$

Note that we allow for differential intercepts of high and low news-pressure days to account for generic differences in trading behavior on high news pressure days.

Figure 10 shows the influence of news pressure on market reactions graphically for all three samples. 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 date falls on a below median news pressure day (δ). The bottom panel graphs the difference in estimated effects (δ^N). Each panel also reports 95% confidence intervals.

For the baseline sample, the 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 ten, the difference in cumulative abnormal returns is 3.9 percentage points and significant at the 5% level. An attenuated but similar divergence in effects is visible for the Admin 1 control subsample in column 2, while a noisily estimated inverse pattern emerges for the sample without explicit company ties (column 3). The latter, however, should be interpreted with caution, as the absence of direct company associations suggests an inferior role of the media in the dissemination of information.

A potential concern with this identification strategy is that the integration of media markets over time increased the availability of breaking news stories. Figure C.5 in the Appendix shows that the results are virtually unchanged if we detrend the *daily news pressure* before applying the median split.[37] Moreover, the estimates are qualitatively similar if high news pressure days are defined as a daily news pressure above the 75th percentile or if we consider the day after the event (Figure C.6 in the Appendix). In the latter case, we still find a visible but attenuated difference in effects, which is consistent with the notion that breaking news stories dominate the news for more than a day.

Overall, we believe that the most plausible interpretation of the results is that financial markets price in the company's continuous risks and costs of the initial public exposure of the company's association with human rights abuses.

Next, we turn to a related question: can more transparency in the event country's mining industry support human rights organisations–in cooperation with media outlets–in their mission to hold corporations accountable for misconduct. We address this question empirically 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

[37]Eisensee and Strömberg (2007) note a slight upward trend in the daily news pressure for the 1968-2003 period.

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).[38] For instance, Berman et al. (2017) find that EITI membership reduces the likelihood of mining related, localised conflicts in Africa. The results in Figure C.7 in the Appendix provide further support 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.

4.3 Who reacts to publicity?

Our findings in the previous sections show that investors, in aggregate, react negatively to assassination events. In the next step, we are going to investigate if institutional investors–sophisticated and informationally-sensitive "big players" (e.g. Puckett and Yan, 2011; Hendershott et al., 2015)–respond to reports about severe human right violations in connection to 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 by 72% of the surveyed 143 institutional investors.[39] 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.[40] 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 linkages to assassination events and institutional ownership using the specification:

$$IO_{i,t} = \alpha + \delta D_{i,t} + \boldsymbol{X}'_{i,t}\phi + \gamma_i + \lambda_{1;i}t + \lambda_{2;i}t^2 + \epsilon_{i,t}, \tag{10}$$

where $IO_{i,t}$ is the ratio (in percent of market capitalization) that is held by institutional owners in quarter t of company i, $D_{i,t}$ a dummy variable equaling one if company i was associated with at least one assassination event e in quarter t, and a set of annual firm characteristics $\boldsymbol{X}_{i,t}$–i.e. size and leverage lagged by one year. Additionally, we control for company fixed effects (γ_i) as well as company-specific quadratic time-trends ($\lambda_{1;i}t + \lambda_{2;i}t^2$).
We obtained data on institutional ownership from the *Factset Ownership database*. Factset has been widely used (e.g. 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.[41] The data spans from 2000 to 2017 at the quarterly frequency and covers 83 out of the 87 publicly traded mining companies associated with assassination events in our database, corresponding to a coverage of 153 event-company pairs.

[38] 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/ .

[39] 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.

[40] 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.

[41] 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*.

Column 1 of Table 4 shows that the ratio of institutional ownership–on average–does not significantly change in response to assassination events. A possible explanation is that institutional investors with holdings in mining companies follow a more long-term strategy and are less responsive to short-term events. However, different types of institutions are likely to have different investment objectives and horizons. Re-estimating the regression model for each type of institutional investor (columns 2-8), reveals that hedge funds significantly decrease their average holding position by about 16.5%,[42] while we observe no significant reactions from other institutional investors.[43] The results are consistent with the notion that the hedge funds' relatively short investment horizon (c. for instance Cella et al., 2013) inclines them to monitor corporate behaviour closely and respond rapidly to costly information disclosure (Gargano et al., 2017), in particular, following revelatory news events (Huang et al., 2020). In contrast, institutional owners that invest heavily in mining companies appear to prioritze long-term financial goals and might first resort to shareholder activism rather than liquidation of their position.[44] Their inertia is highlighted by the positive–though imprecisely estimated–coefficient for the top 5 institutional owners, which make up on average about 42.6% of total institutional ownership.

This is not to say that moral is the guiding principle for hedge funds which have in the past not shied away from investing in companies associated with regimes responsible for severe human right violations.[45] Rather, reporting on the events is expected to prompt fast-moving, sophisticated investors in to action, anticipating reactions of other market participants. In an alternative setting without scrutiny from human right groups and the media, the "noiseless" elimination of opposition leaders may constitute a viable strategy, perceived as beneficial by informed investors.

Due to data limitations, it is difficult to further discern what type of investors react to news about assassination events. However, our event study results offer some indications. The negative reaction to the assassination event is not immediate but follows a more gradual, downward trend. This is consistent with the participation of retail investors who receive company news at a slower rate relative to their institutional counterparts. The attenuated market reaction on days dominated by other newsworthy events provides further support for the role of retail investors. Exclusive news coverage tailored to their needs by vendors such as *Bloomberg* make institutional investors less likely to miss news affecting their portfolios than non-professional investors, often relying on "classic" media channels such as newspapers to gather information. Combined with Table 4, our results suggest that skilled investors such as hedge funds are able to predict responses of retail investors.

[42] Note that $\frac{0.0034}{0.0206} \times 100 \approx 16.5\%$.

[43] The results are qualitatively and quantitatively unchanged when we restrict the analysis to the mining companies in the baseline event study sample (Table D.7).

[44] An illustrative example is the Church of England's sale of its Exxon Mobil Corp. shares in October 2020, after a failed attempt to push through a resolution to split the chief executive and chairman roles at the shareholders meeting in May earlier in the year, in response to the company's failure to provide an adequate response to the climate crisis. For details, see https://www.bloomberg.com/news/articles/2020-10-08/church-of-england-pensions-board-has-divested-from-exxonmobil .

[45] For instance, Och-Ziff Capital Management, one of largest publicly traded hedge funds, invested $150 million in Camec which used the newly raised money to purchase a joint venture with the state-owned Zimbabwe Mining Development Corp. (ZMDC). The deal funnelled about $100 million as a cash loan to Mugabe's government desperately in need of funds at the time. For more details, see: https://www.bloomberg.com/news/articles/2014-08-21/mugabes-bailout-och-ziff-investment-linked-to-zimbabwe-despot .

5 The Limits of Publicity - Tax Revenues and Assassinations

Given the substantial stock market losses that result from the negative publicity, one might wonder why publicly traded mining companies continue to be involved in these events. A potential explanation is that another, local, party that benefits from the mining project has an incentive to suppress or eliminate opposition but does not or only partially bear the costs. For example, local or federal governments might constitute such an agent, as an uninterrupted production will result in higher royalties and tax revenues. However, the expected gains should outweigh costs only for mining companies that make up a substantial share of the government's yearly revenue. If this premise is correct, the likelihood to observe an assassination event should be an increasing function of the share of the mining company's taxes in the host government's annual budget.

To shed light on this potential channel, we construct a novel dataset of mining companies' tax shares in the host country's annual government revenue using data published by EITI. Since members of the EITI commit to fully disclose all payments from local companies to the government, the reported revenue streams ordinarily cover payments from subsidiaries and joint venture. We consequently have to determine ownership structures to match EITI records with our assassination event dataset.

Specifically, 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, 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.[46] For instance, in 2014, *Anglo American* owned 81.90% of the Peruvian mining company *Anglo American Quellaveco S.A.* and *Mitsubishi* owned 18.10%. 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 5.

One caveat of EITI records is the limited coverage of event countries and years in our assassination dataset. We therefore opted to code ownership shares for all years available in the EITI database for countries which have experienced at least one assassination event in the past. Note that we do not exclude countries *a priori* if the assassination event falls outside of the EITI coverage period to retain potentially interesting cross-country variation. Noteworthy, we are not limited to public companies in this analysis and are able to additionally match private companies to assassinations in our dataset.

For our analysis, we rely on a linear probability model (LPM) to estimate the effect of a company's tax share on the likelihood to observe an assassination event. The functional specification of the LPM is:

$$\text{assassination}_{i,c,y} = \beta_1 \mathrm{T}_{i,c,y} + \gamma_{c,y} + \epsilon_{i,c,y}, \tag{11}$$

where $\text{assassinaton}_{i,c,y}$ is a dummy variable that takes the value one if an assassination event in country c in year y is associated with company i and $\mathrm{T}_{i,c,y}$ corresponds to the tax share

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

of company i in country c in year y. In our baseline specification we include country $\times$ year fixed effects ($\gamma_{c,y}$) to account for time-varying economic and political developments in countries. The focus on within country-year variation moreover alleviates concerns about sample selection.

The results presented in Table 6 reveal the hypothesized positive relationship between the tax share and the probability of observing a published assassination event. Column 1 presents the significant and positive unconditional correlation coefficient of 13.8 percentage points. The magnitude of the estimated coefficient increases to 17.4 percentage points when we account for structural differences in the mining industry and the average prevalence of assassinations in an event country via country fixed effects (column 2). The additional introduction of year fixed effects leaves the estimated coefficient virtually unchanged, which is reasonable, as the tax share is–by construction–expressed in relative terms. For our preferred specification in column 4, a hypothetical mining company that is the sole tax payer is estimated to have an about 18 percentage points higher probability to experience an assassination event. This translates to an average effect of about 1.1 percentage points, as the average tax share in the sample is 5.9%, which constitutes a 26% increase in the average probability to observe an assassination event.[47] In column 5 we introduce company $\times$ country fixed effects, which results in the loss of significance, while leaving the magnitude of the estimated effect unchanged. The former can be explained by the limited intra-company variation of tax shares in a country over time.[48]

The estimated effects, however, should be interpreted with care, as we cannot entirely rule out potential confounding factors such as reporting biases. For example, journalists have an incentive to report about assassinations in association with large and renown companies that are of most interest 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. Reassuringly, the insignificant and positive effect of assassination events on the change in the tax shares (Δ Tax Share) across specifications reported in Table D.8 in the Appendix alleviates concerns about reverse causality.

6 Conclusion

Multinational companies are sometimes connected with human rights violations at the global periphery. In the absence of legal frameworks that govern corporate misbehaviour abroad, global civil society (e.g. activists, human rights groups, the media) are often the only institution to hold large multinational corporations accountable for their misbehaviour. In this paper, we evaluate the effect of publicising human right violations on the stock market value of the multinational companies connected with the abuse. In particular, we compile a unique 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 the companies' stocks. Our results show that killings of activists lead to statistically 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 making information available to a broader public in this context and show that the stock market's negative reaction to

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

[48] The average change in the tax share (Δ Tax Share) is only -0.14 percentage points. The change in the tax share–Δ Tax Share–is defined as: $\Delta T_y = T_y - T_{y-1}$.

publicising the assassinations is more pronounced during days which are not dominated by other news-worthy events. We believe that our results can build the foundation for a novel and important research strand on the political economy of multinational operations and the role of civil society in governing transnational corporate activities at the global periphery. Our findings show that informational campaigns by civil society have in fact an impact on multinational corporations and being linked to human rights abuses can significantly influence an associated companies' stock market value.

7 Figures

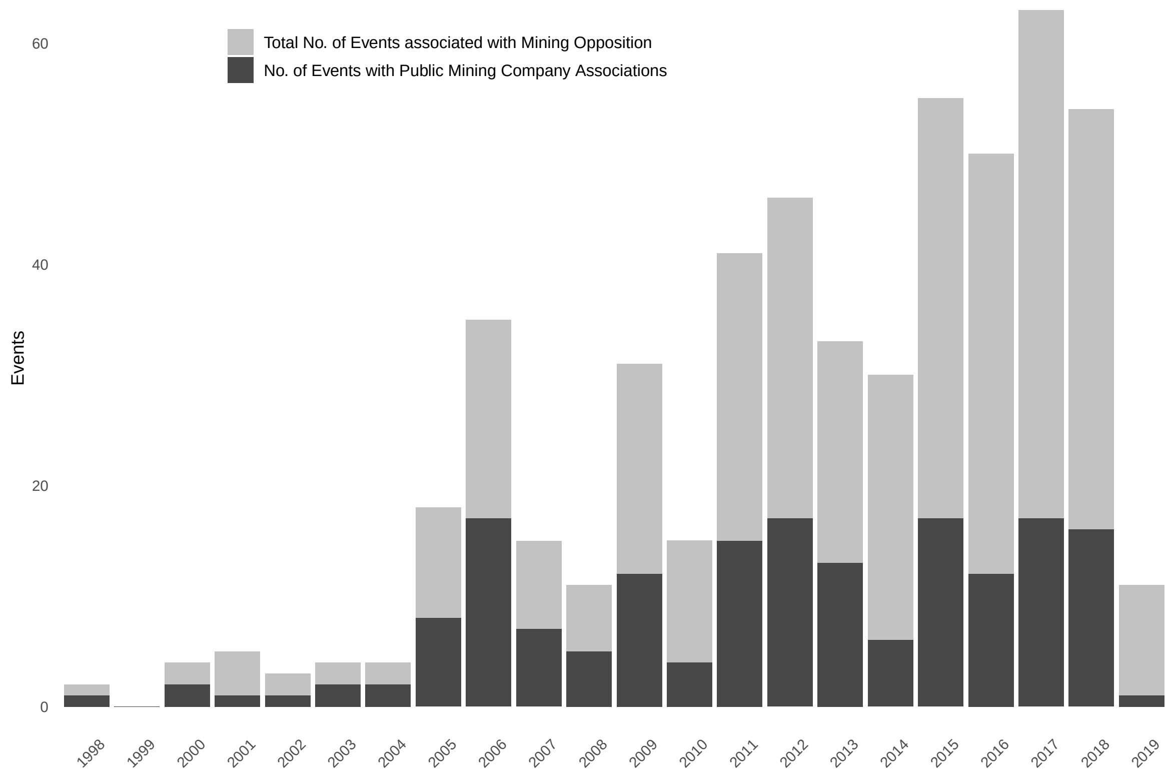


Figure 2: Distribution of Assassination Events over Time.

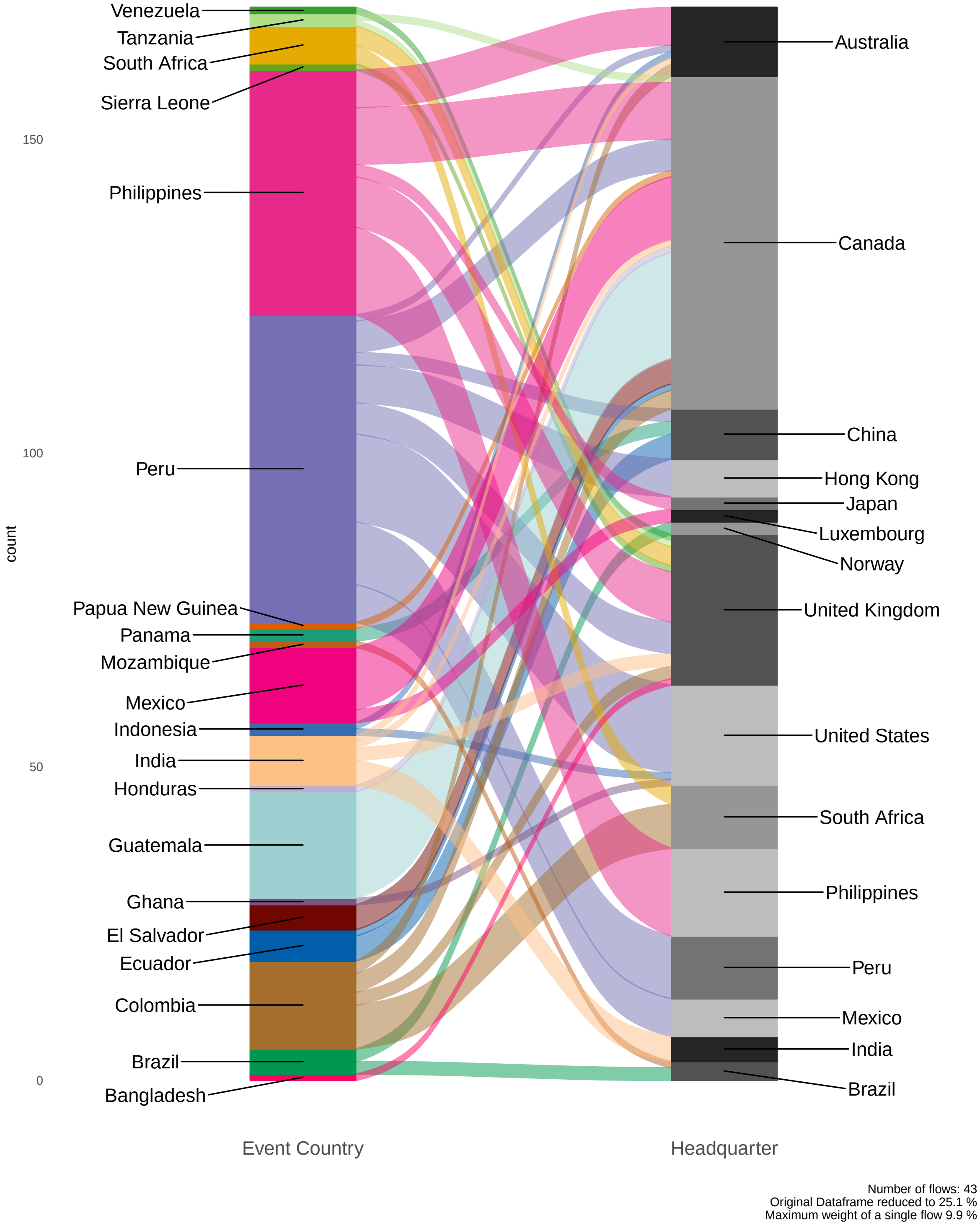


Figure 3: Event Country Activity and Company Headquarter Locations

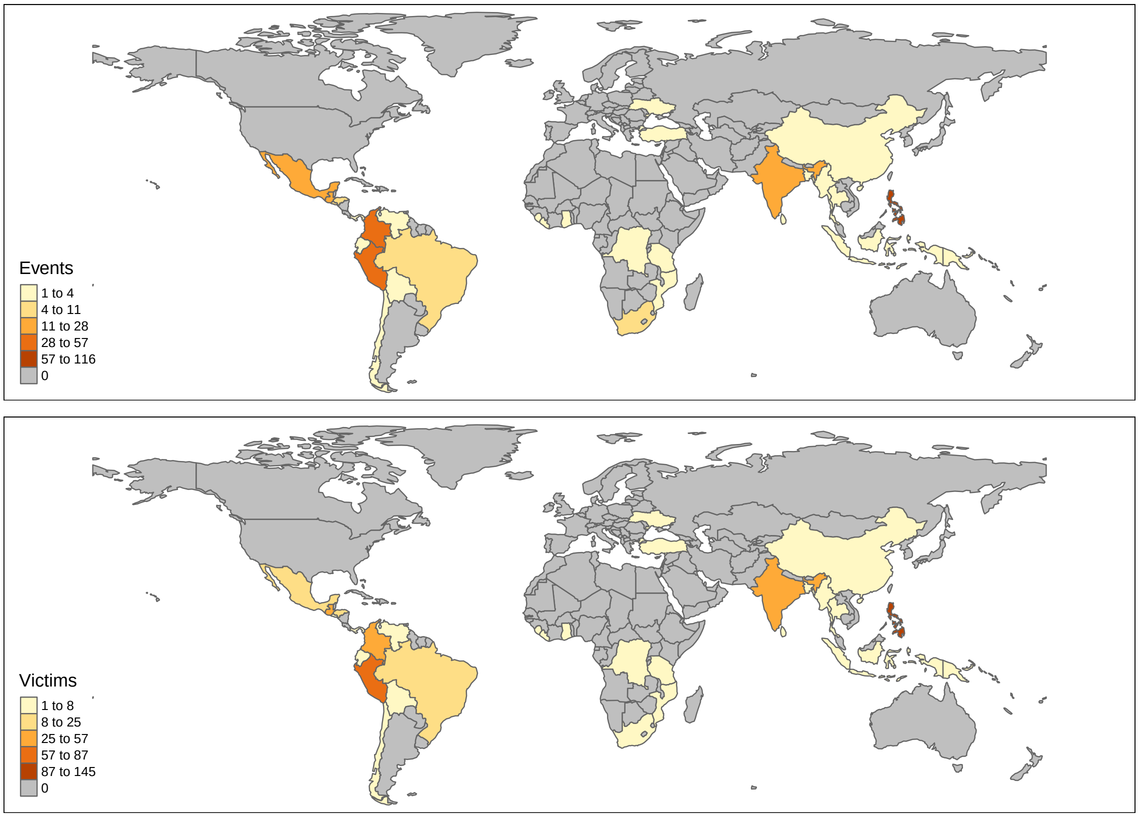
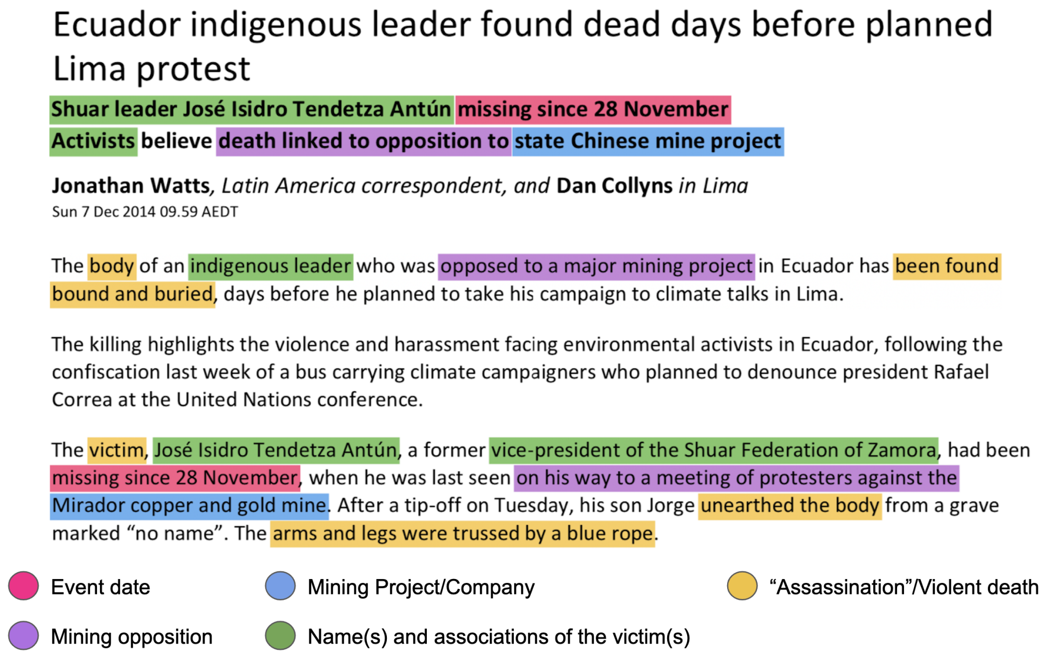


Figure 4: The Spatial Distribution of Assassination Events.



Ecuador indigenous leader found dead days before planned Lima protest

Shuar leader José Isidro Tendetza Antún missing since 28 November

Activists believe death linked to opposition to state Chinese mine project

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

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

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

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

Figure 5: Extracting Events and Company Associations from NGO and Media Reports - Example Case

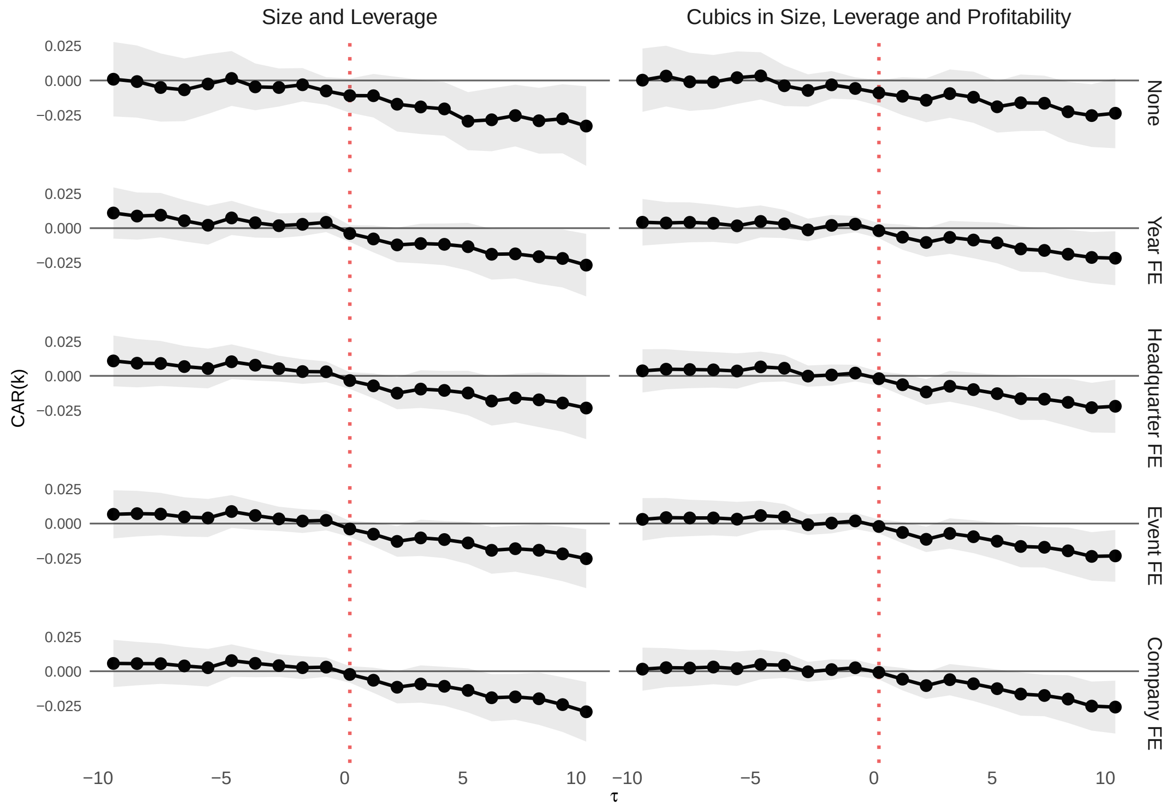


Figure 6: The Treatment Effect of Assassination Events on Mining Companies.

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 thick line (and 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.

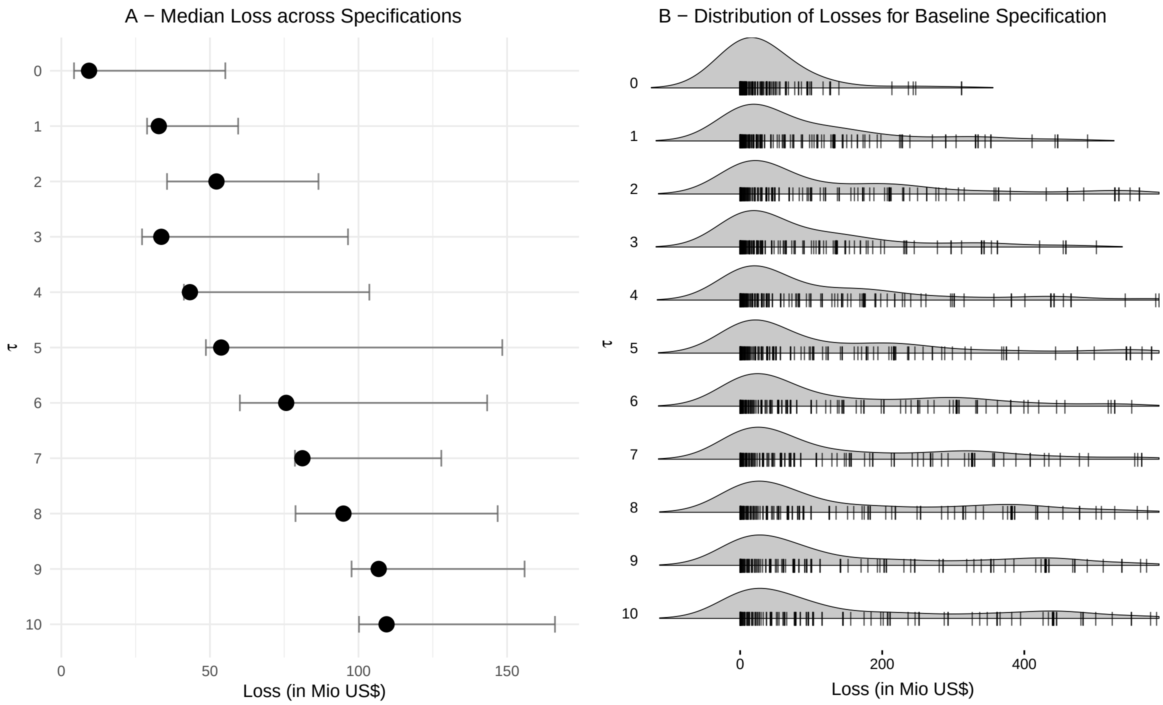


Figure 7: The estimated Economic Value of Assassination Events.

Notes: Dots in Panel-A correspond to the estimated loss in market capitalization of the median company for the baseline specification. The grey bars in Panel-A display the estimated minimum and maximum loss in market capitalization for the median company across specifications. Panel-B presents the distribution of market capitalization losses across companies for the baseline specification. For illustrative purposes losses above the 90th percentile are not displayed in Panel-B.

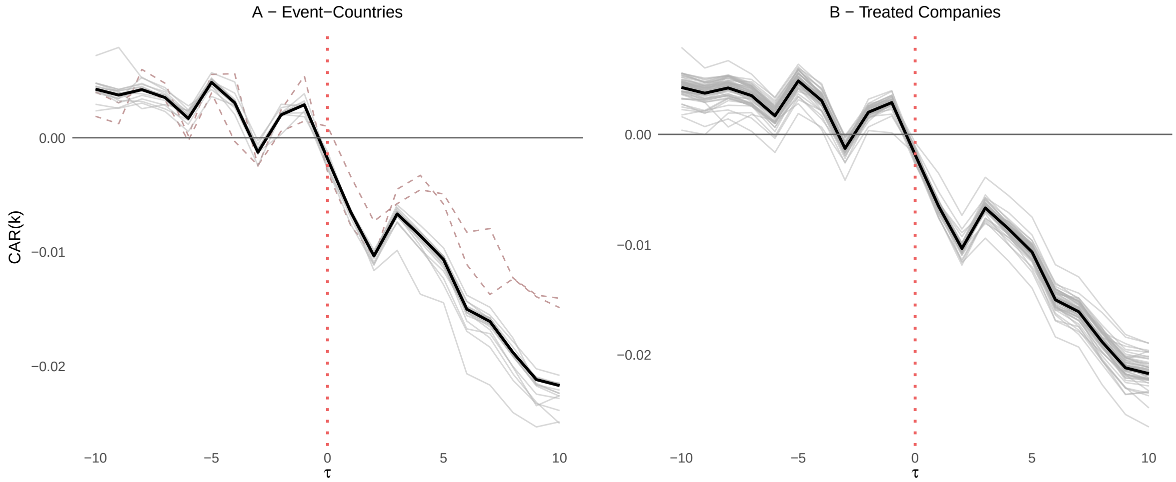


Figure 8: Robustness of Baseline Results - Leave-One-Out.

Notes: The thick black line in Panel-A and Panel-B corresponds to the baseline coefficient estimates for being tied to an assassination in the full sample. Panel-A presents the estimated coefficients when one country is consecutivley 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.

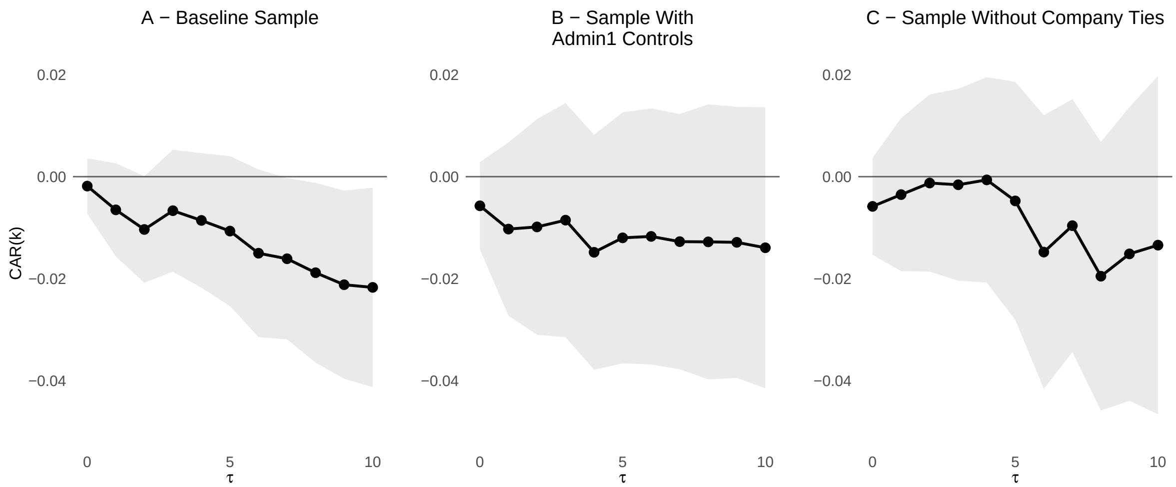


Figure 9: Vicinity vs. Media Ties.

Notes: The thick black lines correspond to the baseline coefficient estimates for being tied to an assassination. 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 assaciations; 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.

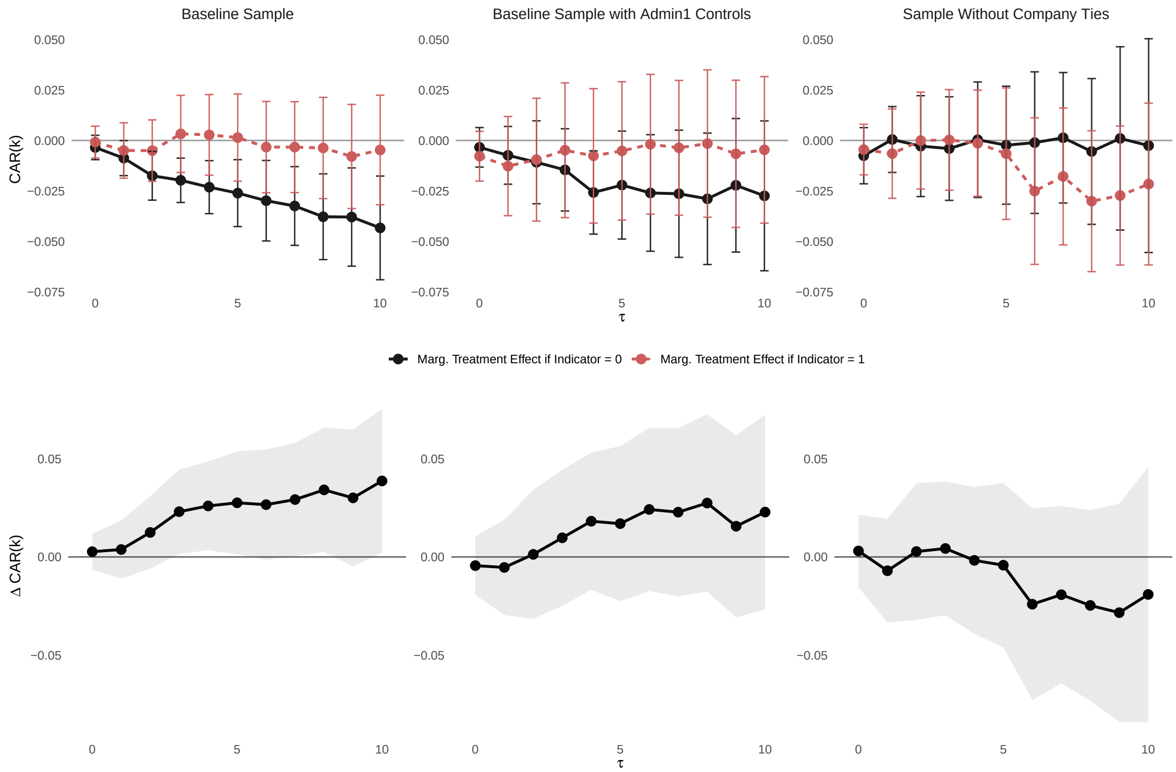


Figure 10: The Influence of News Pressure on the Event Day

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. Columns present regression specifications with the assassination indicator interacted the indicator variable displayed in the column header. 95% confidence intervals using robust standard errors clustered on the event-level correspond to the error bars in the top panel and the ribbon in the bottom panel

8 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	p-value BMP	p-value ADJ-BMP	p-value GRANK
CAR0to0	-0.0009	0.0034	0.789	0.406	0.432	0.700
CAR0to1	-0.0066	0.0047	0.161	0.067	0.083	0.217
CAR0to2	-0.0074	0.0058	0.202	0.102	0.122	0.140
CAR0to3	-0.0040	0.0067	0.547	0.186	0.211	0.048
CAR0to4	-0.0061	0.0075	0.415	0.119	0.141	0.037
CAR0to5	-0.0078	0.0082	0.344	0.137	0.160	0.061
CAR0to6	-0.0104	0.0087	0.233	0.064	0.080	0.033
CAR0to7	-0.0132	0.0094	0.160	0.032	0.043	0.016
CAR0to8	-0.0148	0.0099	0.135	0.027	0.037	0.011
CAR0to9	-0.0201	0.0105	0.055	0.013	0.019	0.001
CAR0to10	-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 (6) and (7) 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.1.

Table 4: The Effect of Assassination Events on Institutional Investor Holdings

		Institutional Investor Type						
Dep. Variable:	IO	Banks	Insurance Comp.	Investment Comp.	Investment Advisors	Pesion Funds & Endowment	Hedge Funds	Top 5
Assassination	0.0010	0.0002	-0.0001	-0.0005	0.0054	-0.0006	-0.0034***	0.0053
	(0.0047)	(0.0002)	(0.0000)	(0.0017)	(0.0041)	(0.0005)	(0.0010)	(0.0037)
Size and Leverage	X	X	X	X	X	X	X	X
Company FE	X	X	X	X	X	X	X	X
Company-specific quadratic time trend	X	X	X	X	X	X	X	X
R-squared	0.674	0.306	0.578	0.599	0.671	0.561	0.333	0.490
Observations	4136	4136	4136	4136	4136	4136	4136	4136
Mean	0.2484	0.0008	0.0006	0.0539	0.1588	0.0137	0.0206	0.1059

Notes: Robust standard errors clustered on the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

Table 5: 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 6: Tax Revenue Shares and the Likelihood to observe Assassinations

	Dependent Variable: Assassination				
	(1)	(2)	(3)	(4)	(5)
Tax share	0.138**	0.174***	0.174**	0.181**	0.191
	(0.060)	(0.067)	(0.069)	(0.071)	(0.181)
Country FE		X	X		
Year FE			X		
Country × Year FE				X	
Company × Country FE					X
R-squared	0.006	0.051	0.080	0.143	0.004
Observations	1081	1081	1081	1081	1081

Notes: Robust standard errors in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

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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 τ_1 to τ_2 in this section, where $T_1 < \tau_1 \leq \tau_2 \leq T_2$.[49]

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 (6) and (7).

BMP

Given the estimated abnormal and cumulative abnormal returns and their sample variance in (3)-(3), the scaled abnormal (SAR) and cumulative abnormal (SCAR) returns during the *event window* $\tau = T_1 + 1, ..., T_2$ are defined as:[50]

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

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

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

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

where $\overline{SCAR}$ constitutes the average scaled abnormal return on event day τ and $\sigma(SCAR_{i,e})$ the cross-sectional standard deviation of the SCAR:[51]

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

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

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

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

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

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

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:

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

where $\sigma^2(SAR_{i,e})$ is given in (A.6) and $\overline{r}$ 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:

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

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):[52]

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

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

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

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 ($U_{i,e,\tau}$) of the GSARs are:

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

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

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

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

where

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

with

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

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

where N_τ 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.

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:

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).[53] 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.

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:

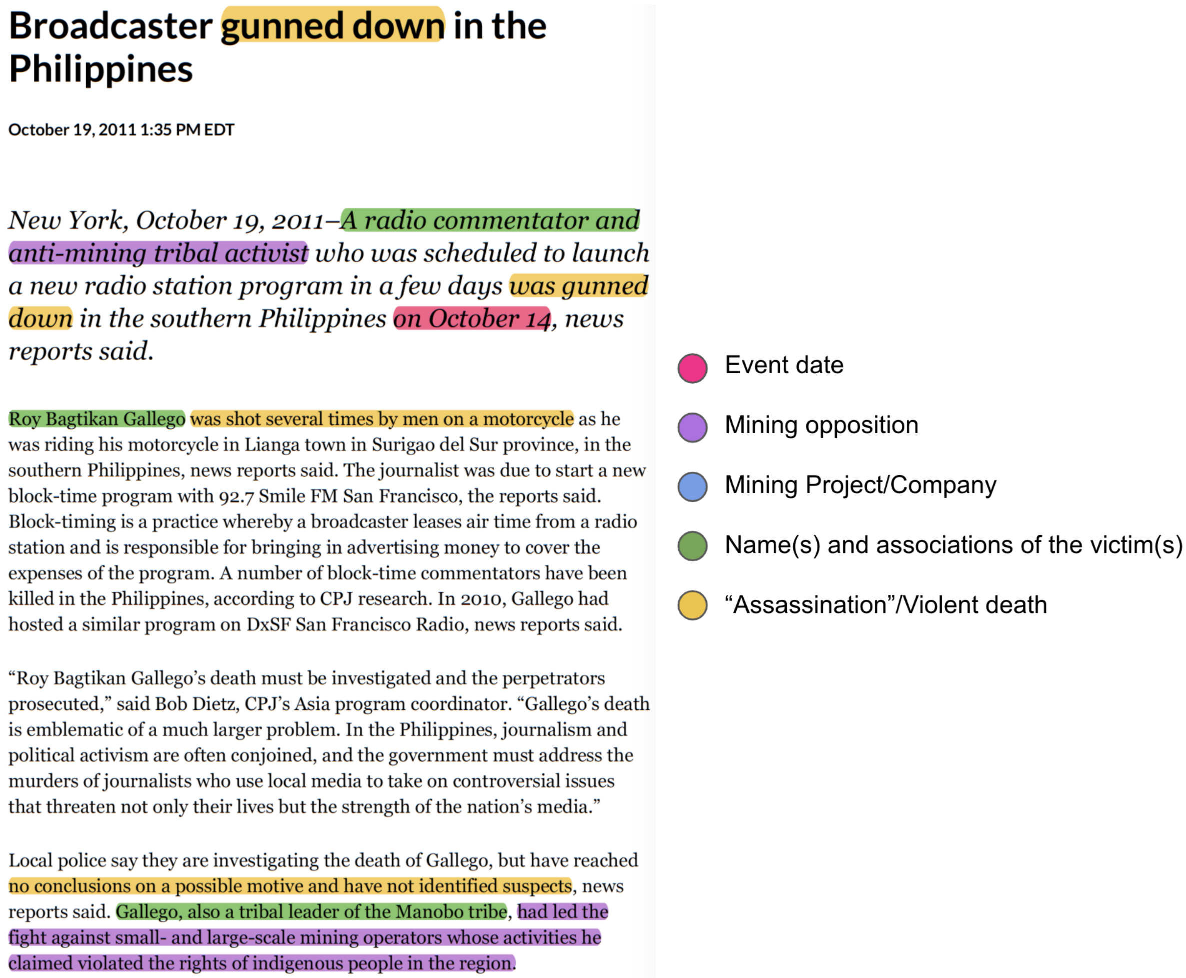
[53]The Google News data set comprises about 100 billion words. The pre-trained *web2vec* word vectors can be found here: https://code.google.com/archive/p/word2vec/.

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[54] (iv) the event "circumstances" (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).

[54]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.

C Additional Figures



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.

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

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

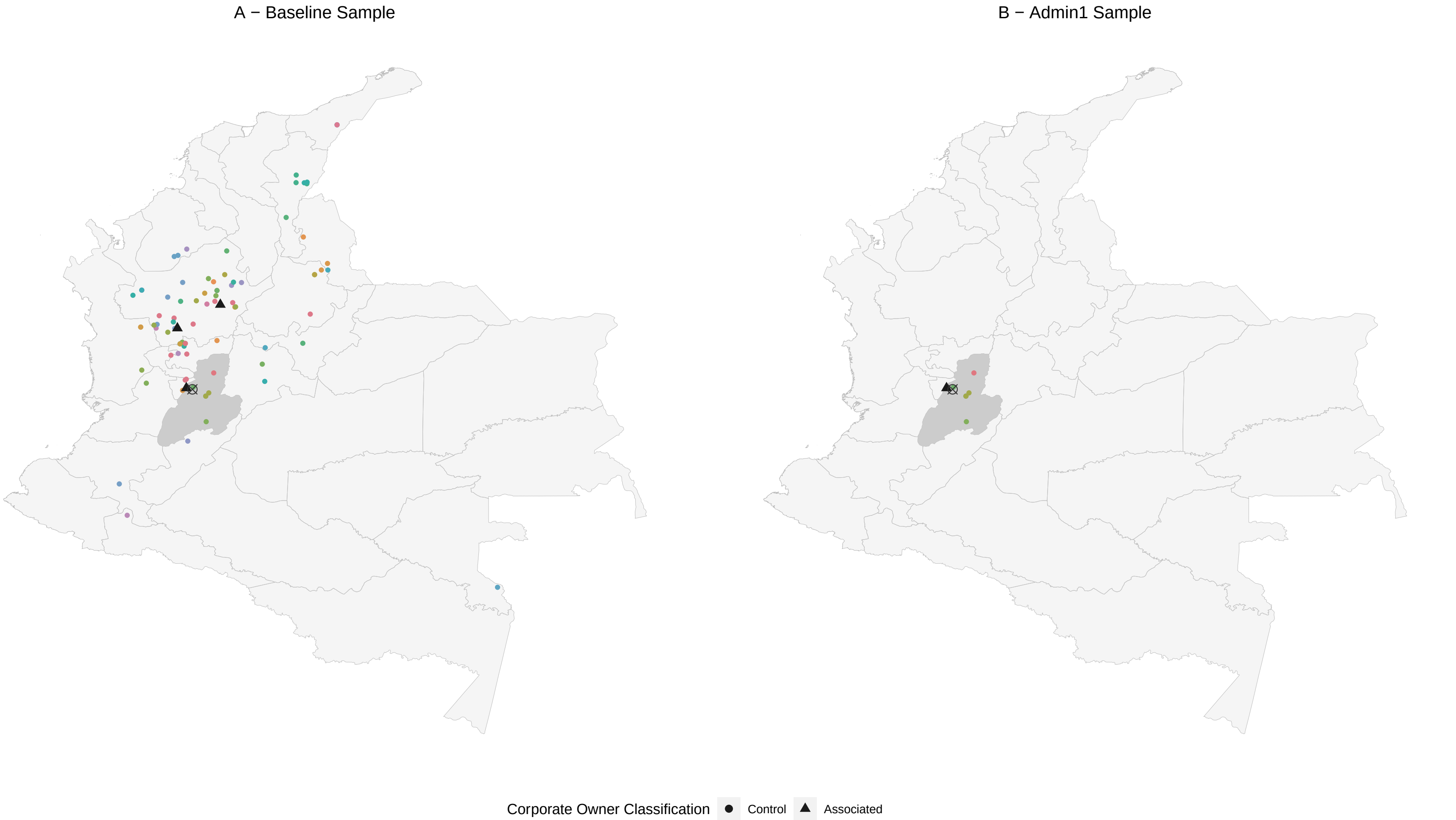


Figure C.2: The Construction of the Control Group - An Example Case from Colombia.

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.

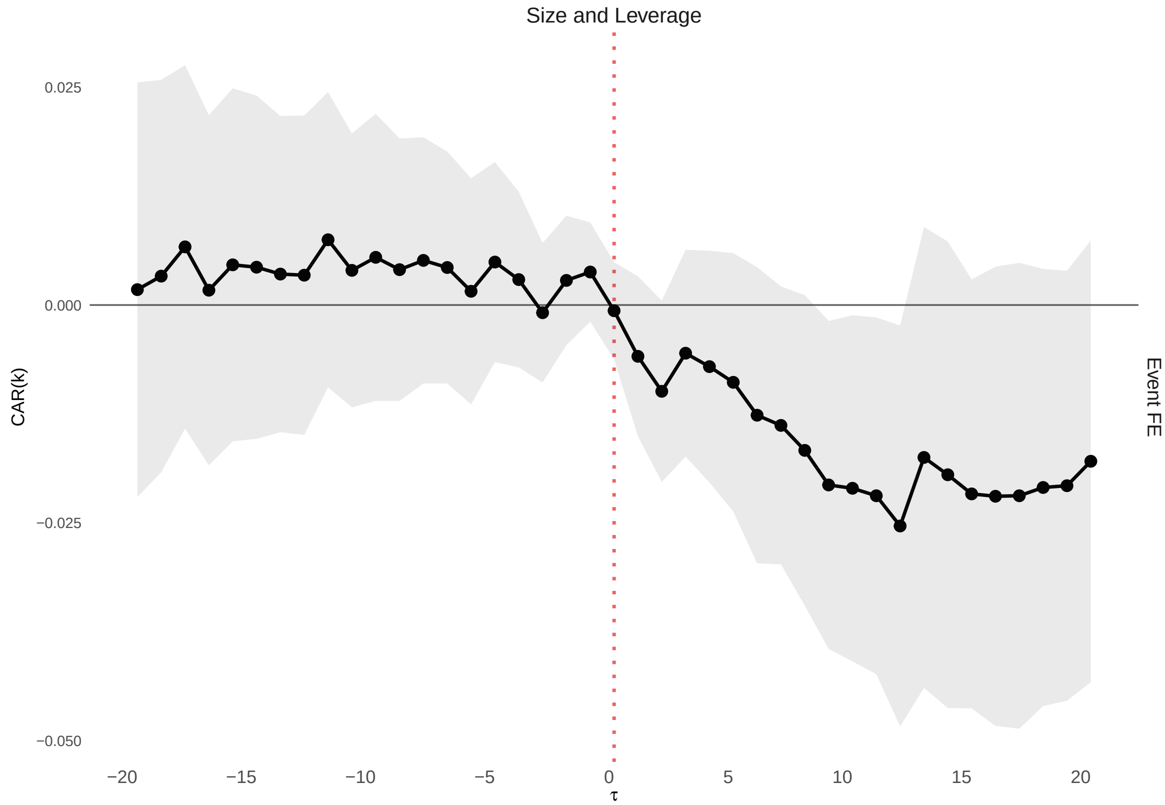


Figure C.3: The Treatment Effect of Assassination Events on Mining Companies - Wide Event Window.

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 thick line (and dots). Each dot corresponds to a seperate 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.

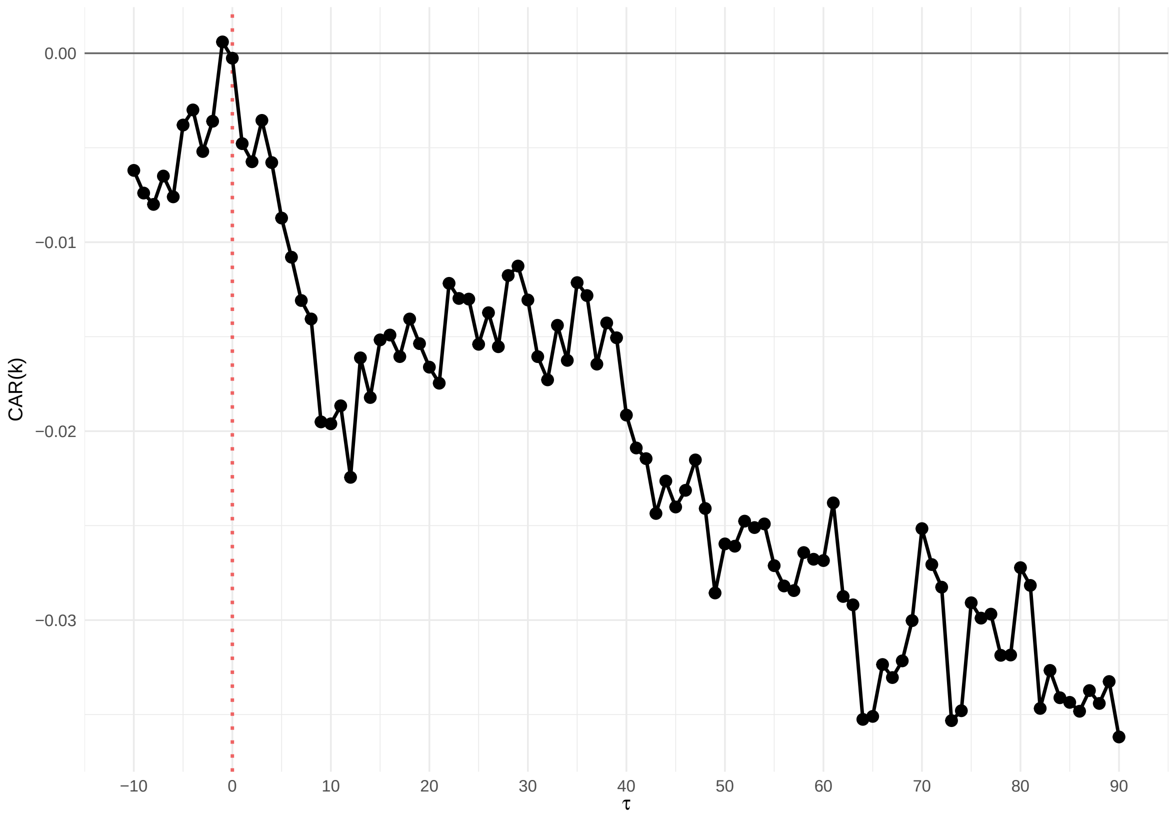


Figure C.4: The Long-Run Average Cumulative Abnormal Returns of Associated Companies.

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.

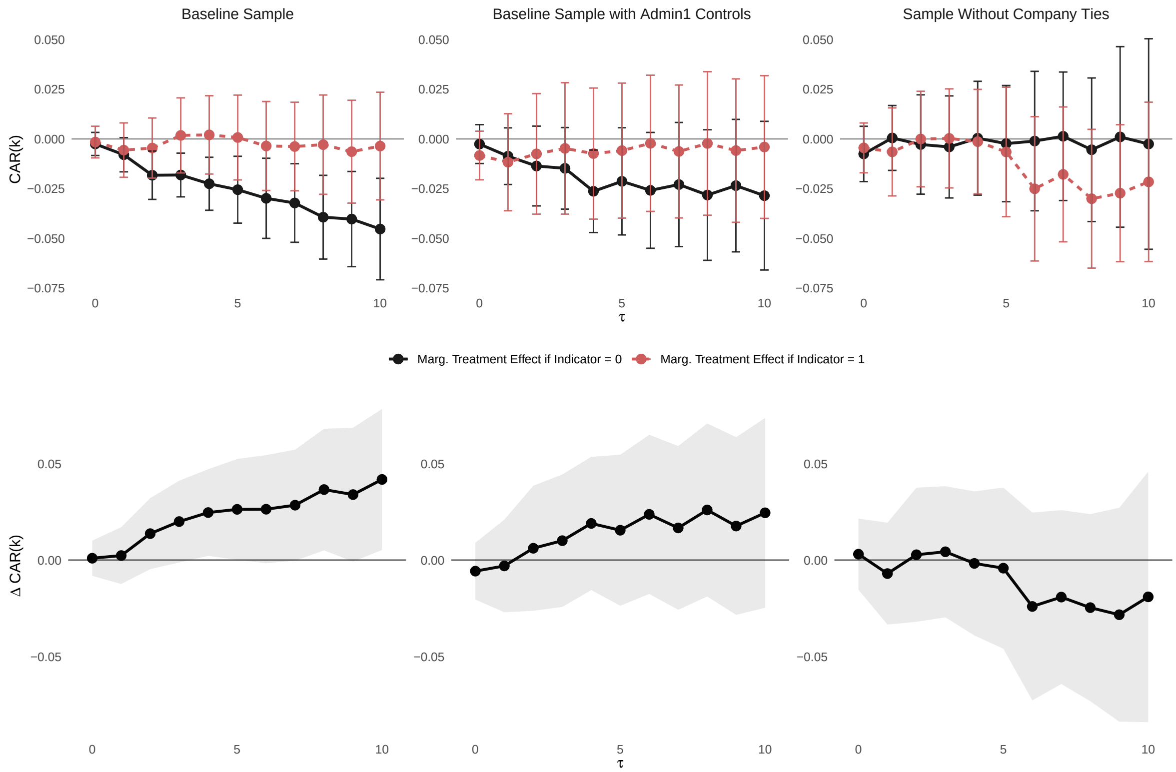


Figure C.5: The Influence of News Pressure on the Event Day - Robustness: Detrended

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. Columns present regression specifications with the assassination indicator interacted the indicator variable displayed in the column header. 95% confidence intervals using robust standard errors clustered on the event-level correspond to the error bars in the top panel and the ribbon in the bottom panel

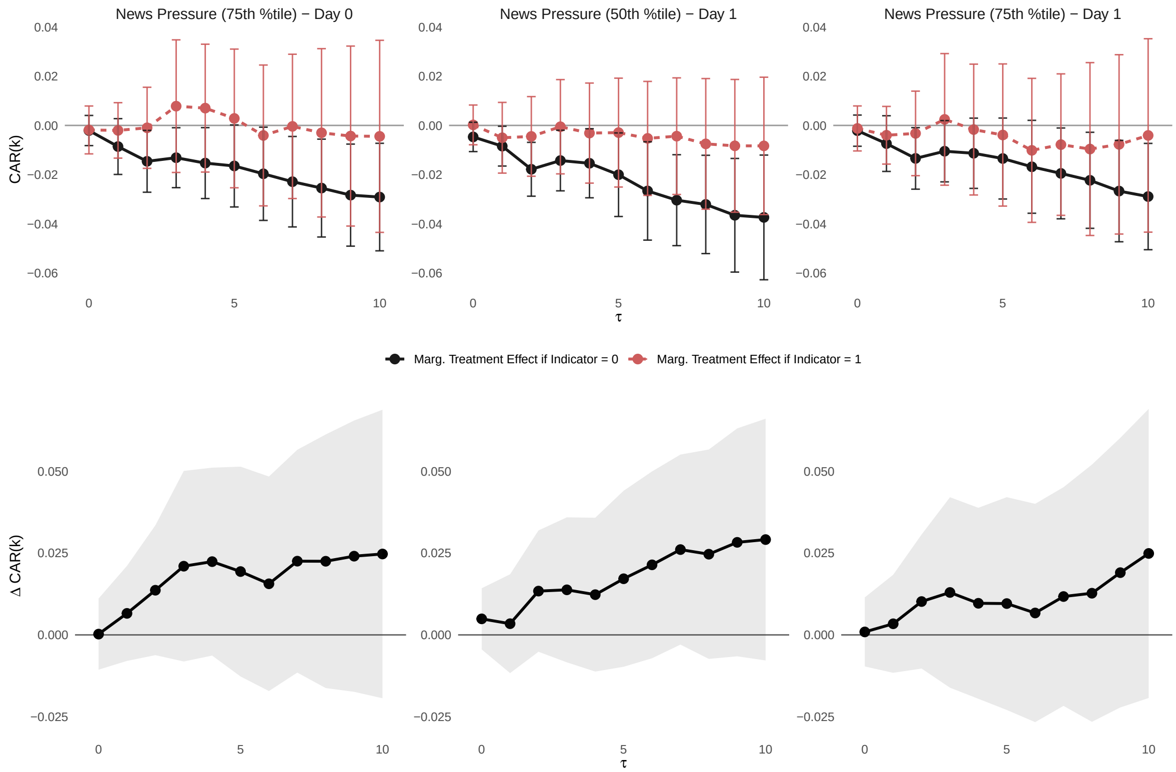


Figure C.6: The Influence of News Pressure - Robustness: 75th Percentile and Day 1

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. Columns present regression specifications with the assassination indicator interacted the indicator variable displayed in the column header. 95% confidence intervals using robust standard errors clustered on the event-level correspond to the error bars in the top panel and the ribbon in the bottom panel

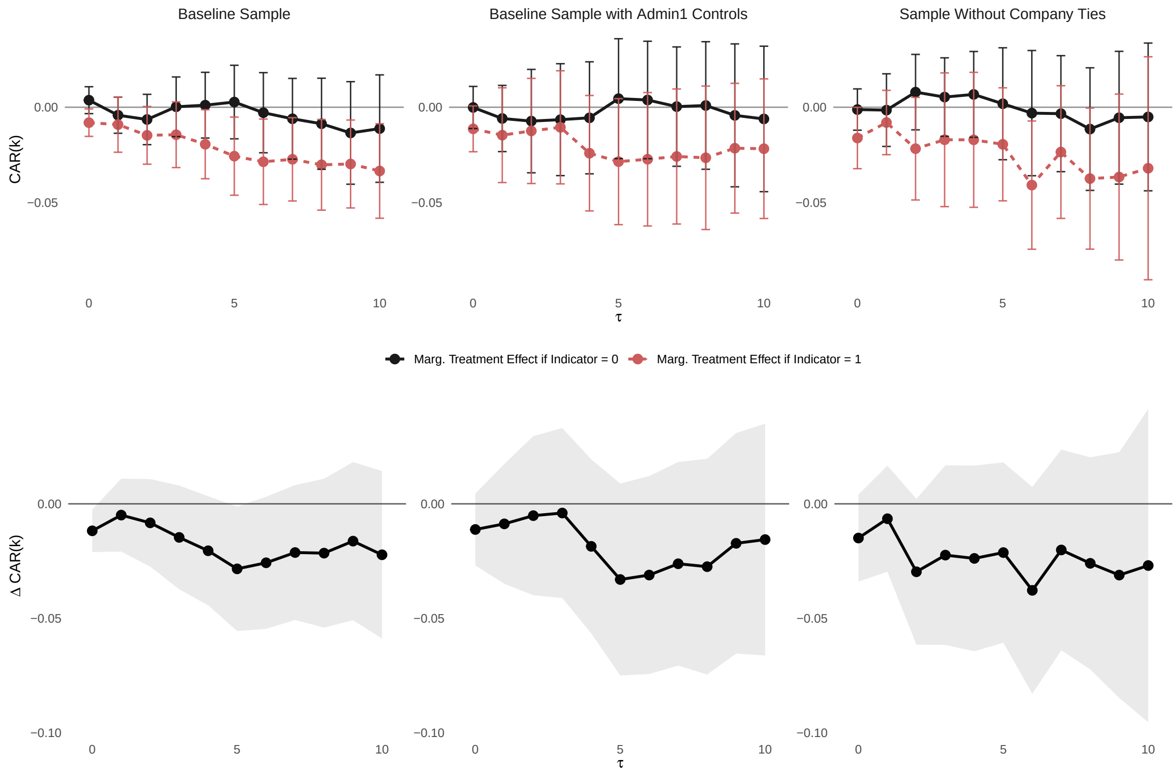


Figure C.7: The Influence of Oversight - EITI Membership

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. Columns present regression specifications with the assassination indicator interacted the indicator variable displayed in the column header. 95% confidence intervals using robust standard errors clustered on the event-level correspond to the error bars in the top panel and the ribbon in the bottom panel

D Additional Tables

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

			p-value			
	Mean	SD	Normality	BMP	ADJ-BMP	GRANK
CAR0to0	-0.0002	0.0024	0.934	0.465	0.498	0.632
CAR0to1	-0.0054	0.0035	0.118	0.081	0.106	0.348
CAR0to2	-0.0047	0.0043	0.270	0.145	0.176	0.255
CAR0to3	-0.0035	0.0049	0.480	0.172	0.205	0.047
CAR0to4	-0.0038	0.0055	0.496	0.136	0.166	0.061
CAR0to5	-0.0040	0.0061	0.511	0.175	0.209	0.114
CAR0to6	-0.0062	0.0066	0.347	0.089	0.115	0.069
CAR0to7	-0.0092	0.0070	0.190	0.048	0.066	0.043
CAR0to8	-0.0110	0.0075	0.139	0.038	0.054	0.027
CAR0to9	-0.0153	0.0079	0.052	0.018	0.028	0.004
CAR0to10	-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 (6) and (7) 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.1.

Table D.2: Private Information and Pre-Trends

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

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CAR0to0	-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)
CAR0to1	-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)
CAR0to2	-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)
CAR0to3	-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)
CAR0to4	-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)
CAR0to5	-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)
CAR0to6	-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)
CAR0to7	-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)
CAR0to8	-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)
CAR0to9	-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)
CAR0to10	-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. Standard errors in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

Table D.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)
CAR0to0	-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)
CAR0to1	-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)
CAR0to2	-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)
CAR0to3	-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)
CAR0to4	-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)
CAR0to5	-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)
CAR0to6	-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)
CAR0to7	-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)
CAR0to8	-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)
CAR0to9	-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)
CAR0to10	-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. Standard errors in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CAR0to0	-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)
CAR0to1	-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)
CAR0to2	-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)
CAR0to3	-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)
CAR0to4	-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)
CAR0to5	-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)
CAR0to6	-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)
CAR0to7	-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)
CAR0to8	-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)
CAR0to9	-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)
CAR0to10	-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. Standard errors in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CAR0to0	-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)
CAR0to1	-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)
CAR0to2	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)
CAR0to3	-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)
CAR0to4	-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)
CAR0to5	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)
CAR0to6	-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)
CAR0to7	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)
CAR0to8	-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)
CAR0to9	-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)
CAR0to10	-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. Standard errors in parentheses: *p<0.1, ** p<0.5, *** p<0.01.

Table D.7: The Effect of Assassination Events on Institutional Investor Holdings - Baseline Event Study Companies Only

		Institutional Investor Type						
Dep. Variable:	IO	Banks	Insurance Comp.	Investment Comp.	Investment Advisors	Pesion Funds & Endowment	Hedge Funds	Top 5
Assassination	0.0001	0.0000	-0.0001	-0.0011	0.0054	-0.0005	-0.0036***	0.0061
	(0.0050)	(0.0001)	(0.0001)	(0.0018)	(0.0045)	(0.0006)	(0.0011)	(0.0043)
Size and Leverage	X	X	X	X	X	X	X	X
Company FE	X	X	X	X	X	X	X	X
Company-specific quadratic time trend	X	X	X	X	X	X	X	X
R-squared	0.717	0.516	0.591	0.601	0.660	0.615	0.410	0.523
Observations	3042	3042	3042	3042	3042	3042	3042	3042
Mean	0.2575	0.0003	0.0006	0.0573	0.1658	0.0137	0.0197	0.1098

Notes: Robust standard errors clustered at the company-level in parentheses: *p<0.1, ** p<0.05, *** p<0.01.

Table D.8: The Relationship between Assassinations and the Channge in Tax Revenue Shares

	Dependent Variable: Δ Tax Share				
	(1)	(2)	(3)	(4)	(5)
Assassination	0.012	0.011	0.012	0.013	0.021
	(0.023)	(0.023)	(0.024)	(0.025)	(0.050)
Country FE		X	X		
Year FE			X		
Country $\times$ Year FE				X	
Company $\times$ Country FE					X
R-squared	0.001	0.002	0.011	0.053	0.002
Observations	784	784	784	784	784

Notes: Robust standard errors in parentheses: *p<0.1, ** p<0.05, *** p<0.01.