The Shield of Ownership.
The Limits of Market Sanctions Against Corporate Crime*

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Abstract

Regulating multinational companies is complicated by companies' complex ownership structures. To induce regulation and prevent corporate criminal activity, states often leverage market responses to the announcement of criminal investigations which generate negative penalties for involved companies, working as a deterrent against financial crime. That is, state authorities outsource part of the regulatory process to financial markets. However, corporations use their fragmented operations across subsidiaries and shell companies to conceal financial misconduct. It is unclear whether fragmented ownership conceals misconduct to investors too. I argue that markets' regulatory function is moderated by the ownership relation between the parent and the implicated entity. Investors penalize a parent company when it is directly responsible of crime. Instead, penalties decrease if the responsible company is a subsidiary. I test this argument studying corporate bribery. I leverage unexpected revelations of corporate corruption by 217 firms to estimate effects of scandals on stock prices of the parent company. I retrieve causal estimates by imputing synthetic counterfactual daily stock returns. When the parent is directly involved in a scandal, I calculate an average loss of more than $1 billion in capitalization on the two days following a scandal. This effect cumulates to more than $4.5 billion losses still detected 20 days after the event. However, the effect is null when the company is involved via a subsidiary. Findings indicate a regulatory failure. Companies can fragment corporate ownership not just to conceal criminal transactions or evade regulations, but also to protect themselves from market responses when misconduct becomes public.

Keywords: Multinational companies; Financial crime; Corporate regulation; Event-analysis

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1 Introduction

Breaking news often report examples of corporate misconduct to the public. In 2016 and 2021, the Panama and Pandora Papers unveiled international schemes of corporate tax evasion and money laundering. The 2015 "Dieselgate" scandal revealed German car manufacturer Volkswagen had falsified some of its vehicles' true CO2 emission levels. Corporate criminal scandals are no recent phenomenon, though. In 1975, a Senate-lead Watergate investigation discovered that the US weapon producer Lockheed had been paying bribes to government officials in West Germany, Italy, Japan, Saudi Arabia, and The Netherlands since at least the 1950s in order to sell aircrafts to US allies.

The public demands strong regulatory measures to prevent similar corporate misconduct (Culpepper et al., 2022). But how are companies to be regulated? Political science discusses whether state-based or "civil" regulations (Vogel, 2008) – such as market-based or self-regulatory mechanisms – are more effective. Evidence is mixed. Internationally coordinated state-based regulations have been successful (Jensen and Malesky, 2018) but often struggle to rule over complex structures of multinational companies (MNCs). Firms can use layers of subsidiaries or global supply chains to further nefarious transactions like bribery (Malesky et al., 2015), money laundering (Cooley and Sharman, 2017), or to violate labor rights (Mosley, 2017). States' reluctance to delegate sovereign powers to international bodies (Green and Colgan, 2013) and the existence of collective action problems (Simmons, 2010) hinder capacity to rule over such structures. Moreover, the very cross-border organizations used by firms to further financial crime can be purposed to evade criminal laws (Arel-Bundock, 2017; Chapman et al., 2020; Findley et al., 2015). As per "civil" regulations, MNCs' self-regulatory initiatives could satisfy public demands for corporate standards (Kolcava et al., 2021) and they could even substitute state-based actions (Malhotra et al., 2019). However, a fierce debate raises questions on whether self-regulation (Berliner and Prakash, 2015; Thrall, 2021) and market-based mechanisms (Bayer and Aklin, 2020; Green, 2021) effectively prevent corporate misconduct.

In practice, regulation embodies a mix of internationally coordinated state-led initiatives and market-based responses. States enforce corporate criminal laws – often through the assistance of partner countries (Kaczmarek and Newman, 2011). In doing so, they put pressure on prosecuted companies' stock prices (Alexander, 1999): law enforcement generates negative expectations of returns for investors holding shares of corporate criminals (Morse, 2019). Markets thus name-and-shame corporations, behaving as civil society actors (Acemoglu and Robinson, 2020; Fukuyama, 2016). At the same time, public prosecutors expect such penalties and, in an attempt to avoid declaring a "death sentence" on a company, compromise with corporations and mitigate the severity of enforcement actions (Garrett, 2014). That is, effectively states outsource part of the regulatory process to market mechanisms.

Under what conditions does state enforcement of corporate regulations generate negative market effects on involved companies? In this paper I set to answer this question. I build on previous evidence that

markets penalize companies for involvement into negative corporate social responsibility news (Kreitmeir et al., 2020; Krüger, 2015) and blacklisting (Morse, 2022). I complement these studies by investigating the market effect of state-lead enforcement actions against corporate crime. In particular, I shed light on a yet unstudied aspect of states' outsourcing of regulation to markets: whether financial markets impose penalties on companies for misconduct that has been committed through subsidiaries. This is an important question given that corporate criminals typically commit and conceal nefarious transactions from public authorities precisely by fragmenting them across layers of controlled entities and shell companies (Sharman, 2010). We do not know whether such fragmentation also conceals criminal conduct from the eyes of investors.

I argue that, in fact, fragmentation of ownership insulates a company from negative financial consequences generated by law enforcement actions. Ultimately, this is due to the profit-seeking rationale of investors' behaviors. When state authorities enforce corporate criminal regulations against a company, investors who own its stocks are concerned that the firm might generate lower profit as a result of law enforcement costs – such as monetary settlements, fines, and legal costs – and negative publicity (Sampath et al., 2018). Because periodic payment of dividends depends on profit, all else equal stockholders decide to sell their equities. Increase in the supply of stocks is also met by a shrinkage in demand, as perspective shareholders direct their purchases towards safer assets. The result is a reduction in price that causes the company to experience financial losses that it would not have faced, had law-enforcement not taken place. In this case, market-imposed penalties complement state-lead regulatory initiatives.

However, this negative financial effect materializes only if the parent company is directly involved in a law enforcement action. If, instead, a company is involved in a corporate criminal action indirectly, *i.e.* through a subsidiary, investors' expectations about repayment of dividends are not negatively impacted. In this case, legal costs and negative publicity affect the subsidiary's operations and do not concern the parent company but indirectly. Markets therefore struggle to perform their regulatory function when corporate structures obscure ownership. Subsidiaries screen corporate ownership and insulate parent companies from the market effects of law enforcement, thus preventing meaningful financial losses in the wake of breaking news reporting criminal misconduct.

I rely on an event analysis design to test my argument. The design identifies the effect of unexpected events on companies' daily stock prices, by imputing synthetic counterfactual observations. I follow Wilf (2016) and rely on a machine-learning procedure to estimate precise counterfactuals. I adopt this design to study the heterogeneous effects generated by sudden information about corporate criminal violations on stocks of a parent company, depending on whether the company was involved directly or indirectly – *i.e.*, through a subsidiary. In other words, I study how the involved entity's position in the ownership chain moderates the regulatory function exercised by financial markets.

I apply this design in the case of allegations for violations of US anti-bribery regulations. I construct

a novel dataset reporting the day allegations that publicly-traded companies violated US anti-corruption regulations hit the market for the first time. I first web-scrape information on anti-bribery investigations to select companies alleged to violate US anti-corruption law. This yields information on 217 distinct companies involved in 263 corruption scandals. I also code the position of the responsible entity in its corporate group for each event in the dataset. Finally, I obtain daily stock prices data for the parent company in the days preceding and following the release of information.

I find that, when parent companies are directly involved in an anti-corruption investigation, they suffer a statistically significant negative effect on stock prices in the immediate aftermath information is made public. I estimate that the average company lost more than $1 billion per day in terms of market capitalization for two trading days. The effect size is remarkably similar to that of comparable negative news estimated by previous studies (see Kreitmeir et al., 2020). Even more than two weeks after the event, cumulative returns to companies involved directly in scandals still remain more than $4 billion lower what could have been expected had the event not occurred. That is, market responses do complement states' regulatory functions, by imposing strong and sustained penalties that stick to a company's reputation when it is involved directly in a scandal. However, no statistically significant effect on the price of the parent company's equities is detected at all when a subsidiary is investigated for bribe payments.

Results paint a cynical picture of regulatory failure. Fragmentation of ownership cannot be only used to further and conceal financial crime (Sharman, 2010). Nor it is only a way to arbitrage regulations aimed at preventing it (Chapman et al., 2020). It is also a device that insulates parent companies from resulting damage, if misconduct is made public. Even though subsidiaries often engage in financial misconduct far from the parent's oversight – in fact, against its management (Alexander and Cohen, 1999) –, results indicate a clear limitation of state strategy to leverage market responses for regulatory purposes.

The paper calls into question the extent to which market-based mechanisms can complement and potentially substitute for formal state action in important aspects of the regulation of private transnational actors satisfactorily. It thus speaks to a vast literature on relations between public authorities and privates in the construction and regulation of global interdependence (see work as diverse as Green and Colgan, 2013; Johns et al., 2019; Morse, 2022; Ruggie, 2002; Strange, 1996). Specifically, results question whether negative information affects reputation to induce compliance of private actors with international regimes. International relations theory looks at reputation as a powerful device to ensure compliance with international regimes (Simmons, 1998, 2000; Weisiger and Yarhi-Milo, 2015). Since reputation is crucial for explaining private economic decisions too (Garriga, 2016), it is straightforward to expect markets' opinion of companies could also induce respect of international norms (Ruggie, 2018) when companies are directly responsible for compliance or defection (Baradaran et al., 2012; Findley et al.,

2015; Jensen and Malesky, 2018; Morse, 2019). I show that this expectation might be disappointed. Investors' behaviors appear to be elastic to negative publicity, but definitely inelastic when involvement into bad news is successfully hidden inside a corporate group. In this case, corporate ownership works as a shield for the parent company's reputation.

Implications of this grim conclusion travel towards various areas where respect of international norms relies on informal market responses. In the realm of green regulation, for instance, the US Securities and Exchange Commission has reportedly considered mandating US-listed companies to disclose their environmental impact. Importantly, companies would have to disclose emissions along supply and ownership chains[1]. The expectation is that investors would use this information to punish polluting companies and reward virtuous ones. My findings question whether, in this and similar cases, investors will use information on behaviors occurring deep inside a corporate group to perform any regulatory function.

2 How subsidiaries mitigate regulatory market penalties

State enforcement of corporate criminal regulation faces a range of obstacles, such as the arbitrage of jurisdictions that firms can exercise when operating across borders (Arel-Bundock, 2017; Chapman et al., 2020; Findley et al., 2015), the need for economic network centrality and international cooperation in order to rule over multinational corporate wrongdoing (Crippa, 2021; Kaczmarek and Newman, 2011; Kalyanpur and Newman, 2019), and political goals of the executive[2] (Gilbert and Sharman, 2016; Tomashevskiy, 2021).

Public authorities *de facto* attempt to deter companies from corporate wrongdoing by generating market pressure and leveraging economic responses to (the announcement of) regulatory actions (Morse, 2019). A company suffers significant financial losses when its reputation is damaged (Breitinger and Bonardi, 2019), because the market price of its equities reflects investors' current expectations of future dividend repayments (Fama, 1970). Corporations who trade their shares on stock exchanges indeed divide profits ("dividends") among their shareholders on a rolling basis – usually, quarterly. Investors sell and purchase shares as a function of their expected pay-off from dividend repayment. All else equal, negative publicity resulting from socially irresponsible corporate behavior has been shown to undermine such expectation and lead to financial losses (Capelle-Blancard and Petit, 2019; Krüger, 2015). Kreitmeir et al. (2020) estimate that companies in natural resource extraction suffer a loss of about 100 million US dollars following unexpected news of human right violations.

In the case of financial crime, it is argued, negative reputational effects are compounded by strictly material concerns. Stockholders restructure their portfolios out of concerns about potentially poor future

[1] See "The S.E.C. Moves Closer to Enacting a Sweeping Climate Disclosure Rule", *The New York Times*: https://www.nytimes.com/2022/03/21/business/sec-climate-disclosure-rule.html.

[2] See "Trump Administration Spares Corporate Wrongdoers Billions in Penalties", *The New York Times*: https://www.nytimes.com/2018/11/03/us/trump-sec-doj-corporate-penalties.html

economic performances. Financial crime introduces rents and uncertainty that weaken prospects of profits (Ades and Di Tella, 1999; Lambsdorff, 2007). In extreme cases, corporate fraud can even be deliberately exercised at the expense of investors[3]. Moreover, news of criminal investigation create expectations of legal costs, including fines and monetary settlement with public authorities (Garrett, 2011). As a result of these pressures it is estimated that, out of every dollar lost by a company for a case of financial fraud, only 0.20$ come from penalties imposed by regulators. The remaining 0.80$ is due to consequences on involved companies' financial prices (Sampath et al., 2018).

It thus seems like shareholders could complement state-based regulation by performing a function of scrutiny similar to the one that stakeholders and civil society actors carry out (Acemoglu and Robinson, 2020; Fukuyama, 2016). Similarly to consumers' "buycott" actions (Endres and Panagopoulos, 2017), investors' market responses could deter firms from misbehaving and potentially substitute states' weak enforcement of formal regulations (Kreitmeir et al., 2020).

However, companies commit crime by splitting illicit transactions across deep layers of subsidiaries and anonymous shell companies. Similar structures can be used to pay bribes, finance terrorism, and launder illicit finance (Findley et al., 2015; Malesky et al., 2015; Shelley, 2014). Corporate criminal structures can be extremely complex and investors can have little knowledge of them. A company sitting at the top of a corporate group (the "parent" company) can own, directly or indirectly, shares of hundreds of subsidiaries. Degrees of ownership can also vary. A parent company can wholly-own a subsidiary, or it can be its majoritarian owner (*i.e.*, the company owning the largest percentage of shares), or a minority shareholder. Mergers and acquisitions further complicate these networks. Finally, companies can structure their operations in ways that are more complex than traditional horizontal or vertical integrations, for instance creating joint ventures.

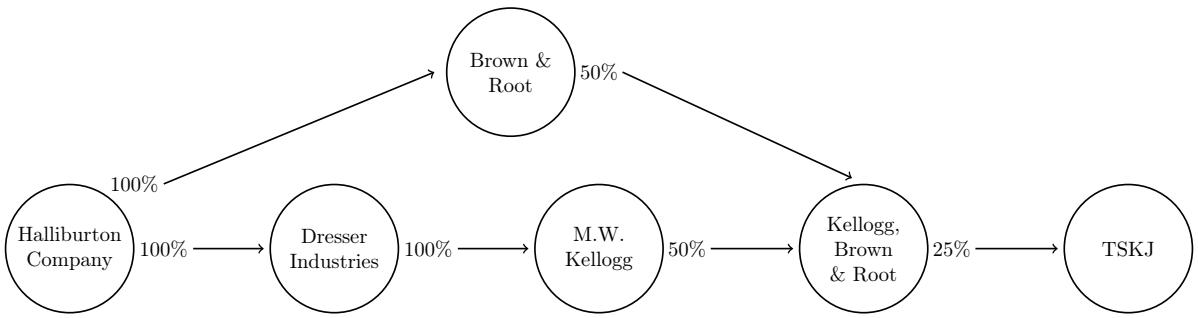


Figure 1: Halliburton Company's stakes in the TSKJ joint venture. Circles represent companies, arrows indicate ownership relations, and percentages represent degrees of ownership.

Figure 1 offers a (rather simple) real example by reconstructing the stakes held by the US extractive company Halliburton in a consortium called TSKJ, a joint venture in the oil services industry registered in Madeira, Portugal. The company was formed by the French Technip S.A., the Italian Snamprogetti B.V.

[3]E.g., Centennial Technologies Inc. defrauded its investors of an estimated figure between $150 and $376 million between 1994 and 1996. See "Jail and $150 Million Restitution for Fraud", *The New York Times*: https://www.nytimes.com/2000/05/18/business/jail-and-150-million-restitution-for-fraud.html.

(incorporated in The Netherlands), the American Kellogg Brown & Root (KBR), and the Japanese JGC Corporation. Each company owned 25% of TSKJ's shares. Halliburton held stakes in the consortium indirectly ever since 1998, when it acquired Dresser Industries and formed KBR by joining its subsidiary Brown & Root with Dresser's subsidiary M.W. Kellogg. Similar fragmented structures are ideal for furthering criminal transactions. For instance, TSKJ became infamous for funnelling hundreds of million US dollars in bribes to Nigerian public officials between 1995 and 2004 in order to secure contracts for extracting and refining liquified gas on Bonny Island, in the Niger Delta region (Lacey, 2006).

I claim that the opacity of these corporate structures is not only ideal to conceal criminal behaviors to the eyes of public prosecutors. It also gets in the way of the regulatory function performed by markets. Investors might be unaware that a company they own equity of – say, Halliburton – owns any stakes in another company involved in criminal conduct – say, the TSKJ consortium. Therefore, they might fail at negatively updating their expected payoffs when the subsidiary is involved in a law enforcement case. That is, adverse selection between a parent company and its shareholders insulates the firm from negative financial effects deriving from corporate crime involving its subsidiaries. Moreover, even if shareholders of a firm are aware of corporate ownership linkages with a subsidiary under criminal investigation, they might rationally evaluate that their expected profits are not directly at stake because of the misconduct. Subsidiaries are distinct legal entities from their parents. They can be subject to investigations that do not involve the parent at all[4]. They might be themselves publicly traded and have their own stockholders who dividends should be repaid to. Under such conditions, investors might evaluate that the activity of a subsidiary is distinct from that of its parent[5].

Based on this logic, I distinguish three possible ways a parent company can be involved in investigations for criminal misconduct. Figure 2 sketches this conceptual framework. In a first scenario (left panel), public authorities directly investigate the parent company for alleged violations of criminal laws. I call this scenario one of *direct involvement* of the parent in a scandal. Here, the parent company's stock prices suffer from news of law enforcement because (prospective) shareholders negatively update their expectations of profits. Stock prices of the firm devaluate, causing it to experience losses it would not otherwise have experienced, had the scandal not emerged.

Alternatively, a company can be involved in a scandal only indirectly, *i.e.* through a subsidiary part

[4] Anecdotes suggest that, in the wake of news about a subsidiary's misconduct, parents tend to signal their distance from it, perhaps as a way to reassure their own shareholders. For instance, between 2007 and 2009 the Italian oil services firm Saipem SpA was investigated by American authorities for violating the US anti-bribery policy, under suspicion that the firm had secured contracts in Algeria by offering $215 million in bribes to public officials close to the then Minister of Finance – See: https://www.reuters.com/article/eni-algeria-idUSL5N0BBAUX20130211. The Italian oil major ENI SpA retained around 43% of the total shares of Saipem at the time. As the scandal unfolded, ENI distanced itself from its subsidiary by issuing an immediate press release where it underscored the independence of Saipem and it offered US authorities full cooperation – See: https://www.eni.com/it-IT/media/comunicati-stampa/2013/02/eni-dichiara-lestraneita-di-amministratori-e-dirigenti-dalle-vicende-indagate-sulle-attivita-di-saipem-in-algeria.html?lnkfrm=asknow

[5] This could appear like an efficient attribution of responsibility from a regulatory perspective: investors would impose sanctions on companies only when they bear direct responsibility on the alleged misconduct. It is nevertheless concerning given that fragmented ownership is pivotal to further financial crime (Sharman, 2010) and that news of fraud at a minimum imply inefficiency of compliance programs the parent company should implement (Demsetz and Lehn, 1985).

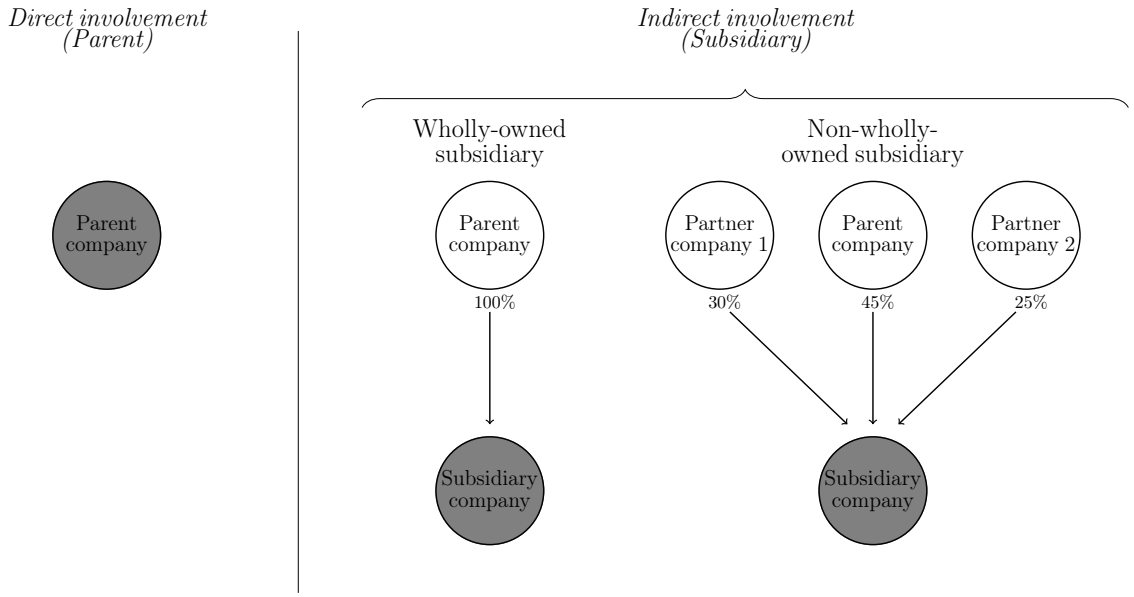


Figure 2: Three ways a parent company can be involved in a corporate criminal scandal along its ownership chain: directly, through a wholly-owned subsidiary, or through a non-wholly-owned subsidiary. Circles represent companies, grey circles represent companies investigated for violating corporate criminal regulations, arrows indicate ownership relations, and percentages represent degrees of ownership.

of its corporate group. In this case, the parent is not under investigation. In turn, two possible scenarios exist here (Figure 2, right panel). First, authorities investigate potential violations by a wholly-owned subsidiary (second scenario). Second, authorities can investigate a subsidiary that the parent is simply the majoritarian owner of (third scenario). I claim that the severity of the financial damage decreases with the distance of the involved entity from the parent company. Misconduct by a more integrated subsidiary (as in scenario 2) poses more serious threats to the parent's profitability than one which is more loosely connected to the corporate group, because full ownership might imply control over illicit conduct (Alexander and Cohen, 1999) and can more easily end up involving the parent in legal actions (Garrett, 2011). In the third scenario, instead, the parent firm does not even fully own the subsidiary found in breach of financial regulations. The parent company is less penalized because linkages between the subsidiary and the parent are weaker. The financial consequences are therefore less severe. Investors might update their reputation of the subsidiary and its stock prices might suffer as a result – if the subsidiary is in turn publicly traded. However, prices of the parent company should not be affected. When compared to a case of direct involvement, corporate ownership therefore *insulates* the parent company from a scandal.

3 The case: violations of the US anti-bribery law

I test my argument in the case of violations of the US anti-corruption law. The Foreign Corrupt Practices Act (FCPA) is a 1977 law adopted by the US Congress to prohibit bribe payments by multinational

corporations to foreign public officials in the conduct of business overseas. The Act is considered among the strongest corporate criminal regulations (Brewster, 2014). It is applied by the Department of Justice (DOJ) – in charge of its criminal enforcement – and by the Securities and Exchange Commission (SEC) – tasked with civil enforcement. Although the FCPA is an American regulation, the DOJ and the SEC have effectively become the watchdogs of the *global* anti-bribery regime. These agencies provide a very broad interpretation of the extraterritorial provisions included in the Act since 1997 (Crippa, 2021; Garrett, 2011; Kaczmarek and Newman, 2011). As a result, the FCPA *de facto* applies against misconduct from any US company *and* any non-US company trading on US stock markets[6] or else furthering a bribe payment using US means such as dollars, US mail, American bank accounts, and even email passing through internet servers located on US soil (Leibold, 2014; Tomashevskiy, 2021).

The DOJ or the SEC (or both) open a file on investigations into alleged FCPA violations by a company when information on potential misconduct emerges[7]. However, very rarely companies alleged of FCPA violations go to court. The long time frame of trials would expose companies to prolonged negative publicity on financial markets. In order to minimize such damage, companies usually settle allegations with prosecutors out of court, through non-prosecution agreements (NPAs) or deferred prosecution agreements (DPAs)[8].

Usually, agencies communicate to the public about investigations through press releases[9] only after allegations have been settled and companies agreed to pay their fines. Instead, information that similar investigations are ongoing is usually released by companies themselves before the final outcome. Under 1930s US law regulating securities, companies must disclose any information of material relevance for investors. This includes SEC or DOJ investigations into alleged FCPA misconduct. Companies disclose such information to investors by filing reports to the SEC itself which, since 1993, must be submitted on the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system[10], a public platform designed to facilitate information flow from companies to (prospective) investors.

Three reasons make the case ideal for comparing the effects of unexpected news about corporate criminal behavior occurring at different levels of a company structure. First, news that US agencies are investigating a company's alleged violation of the FCPA are released in a rather consistent scheme. Information is typically released by companies themselves before press releases by public agencies. Moreover, information is disclosed by filing mandatory SEC forms which are available to the general public of investors. Similar arrangements are not in place in other legal systems. By focusing on violations of

[6] This condition applies also to foreign companies trading American Depositary Receipts (ADRs).

[7] Information that a company along the ownership chain is engaging in corrupt behavior can emerge from different sources. For instance, the DOJ and the SEC can retrieve evidence of misconduct from their own investigations, whistleblowers, investigative reports, or voluntary disclosure from the involved firm following internal inspections.

[8] These solutions entail admission of guilt from the company, payment of fines commensurate to the misconduct, pledges to cooperate with authorities on future investigations, and agreements to undertake corporate reform to prevent future misconduct (Garrett, 2011).

[9] See press releases from the DOJ (https://www.justice.gov/criminal-fraud/enforcement-actions) and SEC (https://www.sec.gov/enforce/sec-enforcement-actions-fcpa-cases) databases.

[10] See: https://www.sec.gov/edgar.

the US FCPA I can therefore study the effect of unexpected news on financial markets while holding constant heterogeneity that pertains to different legal arrangements.

A second reason justifies the case choice. Whereas selections of companies into the group of those involved in cases of corporate corruption is likely endogenous to their reputation, the timing information is released can be considered plausibly exogenous. The case can then be used as a plausible natural experiment to study market responses to companies' misconduct. Often, companies are forced to release press statements or to file SEC forms informing investors about upcoming investigative reports on alleged involvement into cases of corruption[11]. Other times, anti-bribery investigation by US agencies forces companies to delay periodic SEC filings and to submit notes unveiling allegations of corporate corruption[12]. Even when companies disclose about investigation into periodic reports, investors and market analysts cannot expect involvement of the firm into FCPA investigations.

Finally, anti-bribery represents a least-likely case for the claim that financial markets fail to impose penalties on parents for subsidiaries' misconduct. News about anti-corruption investigations should concern investors regardless of where misconduct takes place in the company's operations, because they signal that the corporate group operates inefficiently. Imagine a competition among firms for public procurement. When the subsidiary of a company bribes a public officer in order to beat competitors, it adds unnecessary fees to operative cost. Corrupt contracts also involve terms that cannot be legally enforced (Treisman, 2007). Bribe-payers must rely on the public official's given word that another firm will not be awarded the contract instead (Lambsdorff, 2007). If negative effects were not detected in this case, then corporate misconduct by subsidiaries would be unlikely to bear consequences for parents in cases where the material damage of misbehavior is less clear from the parent company's perspective.

4 Data

In order to test my argument, I first obtain information on cases of alleged corporate corruption investigated by US agencies against publicly traded companies. I retrieve this information drawing from the dataset on anti-bribery prosecution in Crippa (2021). The dataset is obtained by scraping information reported in text documents from the TRACE Compendium[13], an open repository of 841 text documents summarising events of cross-border corporate corruption in violation of the international anti-bribery regime, and related law enforcement actions.

From this dataset, I keep only investigations initiated by US agencies (SEC or DOJ) under terms of the

[11] For example, on March 19, 2013 Microsoft was forced to release a blog statement to comment on allegations made by the Wall Street Journal about possible involvement into corrupt activities abroad. See blog post at: https://blogs.microsoft.com/on-the-issues/2013/03/19/our-commitment-to-compliance/.

[12] For example, on June 14, 2017 the US-based financial provider World Acceptance Corporation (WAC) announced its investors that it would be unable to file a periodic SEC report on time due to potential misconduct by its wholly-owned Mexican subsidiary WAC de Mexico. See the Notification of Late Filing, filed on that day and entirely dedicated to this alleged corrupt event, at: https://www.sec.gov/Archives/edgar/data/108385/000010838517000019/wrld_6-15x17xform12bx25.htm.

[13] See: https://www.traceinternational.org/resources-compendium.

Foreign Corrupt Practices Act. This initial selection leads me to 372 companies involved in 478 violations of the US anti-corruption policy in total. The dataset reports the parent entity (*i.e.* the corporate group's global ultimate owner) for each company involved in an event of anti-corruption violation (326 parent companies in total). I retrieve information on whether each of these parent companies publicly trades its stocks on any exchange. I rely on Bureau Van Dijk's Orbis data to retrieve this information. I keep only records relative to companies whose parent entity's stocks are publicly traded. Finally, availability of stock price data further constrains my analysis to consider only events following the year 2002. I thus select 8 events out of the group to be studied[14] . This leads me to a final selection of 217 unique companies involved in 263 events of investigation for violating the US anti-corruption law.

Next, I code which entity was involved in a scandal of corruption, at the time the event was made public, along the corporate ownership chain. First, I measure whether each company found in violation of the US anti-corruption law is the corporate group's global ultimate parent ($Subsidiary = 0$), or a subsidiary[15] ($Subsidiary = 1$). This variable allows me to study whether differences exist among direct or indirect involvement. If a company is involved in a case both directly and through a subsidiary, I consider it as a case of direct involvement ($Subsidiary = 0$). Next, I disentangle indirect involvement by coding the *degree* of ownership. I record whether the parent company is directly involved in a scandal ($Ownership = 0$). If not, I measure whether it wholly owns the subsidiary responsible for the event ($Ownership = 1$), or whether it is only the majoritarian owner of shares ($Ownership = 2$)[16].

I mainly employ Orbis data to obtain corporate ownership information. Orbis reports detailed information on corporate ownership structures of companies. It also reports shareholder history, that allows to trace ownership structures at the time allegations of misconduct hit the market. I cross-check this information against a range of alternative sources. First, publicly available reports made by US authorities (SEC and DOJ). Second, extensive web searches to confirm the retrieved information[17]. Where Orbis information conflicts with alternative sources, I keep information available from reports by US authorities. Where this is not available, I rely on web searches. Out of the 267 events of corruption I consider, 143 (54%) involved the parent company directly, while 120 (46%) involved it through a subsidiary.

The next step consists in coding, for each FCPA enforcement, the very day information was made public. I employ the Foreign Corrupt Practices Act Clearinghouse (FCPAC) datasets hosted by Stanford University[18]. The FCPAC draws on compulsory company reports from EDGAR, press releases from

[14] Cases excluded are: (1) a 1994 case involving Allied Products Corp; (2) a 2002 case involving Baker Hughes Co; (3) a 2000 case involving Bellsouth Corp; (4) a 2002 case involving Halliburton Co; (5) a 2002 case involving Monsanto; (6) a 1995 case involving Triton Energy Corp; (7) a 2002 case involving Syncor International Corp; and (8) a 2002 case involving Xerox Holdings Corp.

[15] For the sake of simplicity, I do not distinguish between direct and indirect ownership.

[16] This three-level indicator for the degree of ownership is unfortunately a forced choice, because available data on corporate ownership is not precise enough to allow the use of a continuous indicator for corporate ownership.

[17] For this final check I employ datasets from leaked offshore corporate documents (*e.g.:* ICIJ Offshore Leaks Database, OCCRP reports), NGO information (*e.g.:* the UN Global Compact program), and private information providers on company data (Bloomberg, Dun & Bradstreet, and Crunchbase).

[18] See: https://fcpa.stanford.edu.

government agencies, and newspaper articles to establish the earliest date news of a US FCPA case were made public. I manually search through the FCPAC database for each instance of FCPA violation selected from above and code the date information was first released.

Table 1: US anti-corruption policy violations: Sample of data

Parent company	Violation entity	Subsidiary	Ownership	Ticker	Violation country	Investigation
BHP Billiton	BHP Billiton	0	0	BHP	China	2010-09-21
ENI SpA	ENI SpA	0	0	E	Lybia	2013-05-03
ENI SpA	Snamprogetti B.V.	1	1	E	Nigeria	2004-10-05
ENI SpA	SAIPEM SpA	1	2	E	Algeria	2014-04-10
Raytheon Company	Thales-Raytheon Systems Company LLC	1	2	RTN	Middle East	2020-02-12
Royal Dutch Shell PLC	Royal Dutch Shell PLC	0	0	SHEL	Nigeria	2008-03-17
Royal Dutch Shell PLC	Shell Nigeria EPCO LTD	1	1	SHEL	Nigeria	2016-03-10
Novo Nordisk A/S	Novo Nordisk A/S	0	0	NVO	Iraq	2006-02-06
...	...	...	...	...	...	...

Table 1 provides a snapshot of my data. For each entry, a firm (*Violation entity*) is alleged to have violated the US FCPA by bribing public officials in a foreign market (*Violation country*)[19]. I report the parent company of the involved entity (*Parent Company*), whether the parent was involved in the scandal indirectly (*Subsidiary*), the type of indirect involvement (*Ownership*), the symbol under which the parent company trades its securities (*Ticker*), and the date information on public investigation was first made public (*Investigation*).

The final data collection step concerns daily stock prices data. I retrieve all stock price and market indexes information from Refinitiv Workspace. I obtain data on the stock *Returns*: the percentage change in closing price of a stock at the end of a trading day, with respect to the same value relative to the previous trading day. Finally, I retrieve daily data on stock market indexes. This information serves to construct predictive covariates in the research design outlined in the next section. Given that companies in my dataset trade their equities on different exchanges, I retrieve daily percentage changes in values of 10 market-wide indicators[20].

5 Research design

My argument states that regulatory penalties for a parent are imposed by financial market actors. I claim that the effect is conditional on the degree of integration of the involved entity in the parent's ownership chain. Parent companies should suffer less severe negative reputational effects on their stock prices when they are involved in violations of the US anti-corruption law through owned subsidiaries.

I adopt an event-study research design to test these expectations and estimate the effects of interest. This empirical strategy is widely used for estimating market reputational effects of unexpected events

[19] In a minority of cases, neither agencies nor the involved company disclose the specific country where bribery occurred. Often companies just declare the geographic region of misconduct (see the Middle Eastern Raytheon case in Table 1). In other cases, no location is specified at all.

[20] I obtain price history of: S&P 500 Index (SPX), NASDAQ Composite Index (IXIC), NYSE Composite Index (NYA), NASDAQ 100 Index (NDX), Shanghai SE Composite Index (SSEC), the Financial Times Stock Exchange 100 Index (FTSE), Euronext 100 Index (N100), Shenzhen SE Composite Index (SZSC), TSX-Toronto Stock Exchange 300 Composite Index (GSPTSE), and the Deutsche Boerse DAX Index (GDAXI).

(Karpoff et al., 2008). It has been recently adopted by political economists to assess the effect of international institutions and regulations (Gray, 2009; Wilf, 2016), political communications (Genovese, 2021), elections (Aklin, 2018), and international rulings (Kucik and Pelc, 2016).

The design imputes daily synthetic counterfactual *Returns* to each company around an event of interest. It then measures the difference between observed and synthetic counterfactual observations on the day of an event of interest, thus estimating an average treatment effect on the treated (ATT) companies' stock prices. In order to achieve that, I divide daily stock price observations for each company in two time-windows. First, an *estimation window*, predating the unexpected event of interest (from t_0 to t_1). Next, an *event window*, centred around the event whose effect is to be estimated (from t_1 to t_2). For each of the events of corruption, I define an *event window* that starts 30 days before the event and ends 30 days after the event (total length is 61 days per event). The *estimation window* of each company begins 210 days before the beginning of its *event window*[21].

In the *estimation window*, I estimate one market-model for each event by explaining the parent company's *Returns* using market-wide indexes. Equation 1 summarizes this step. Daily observed *Returns* for each parent company involved in an event e, within the *estimation window* ($t_0 \leq t < t_1$), are modelled as a function of the matrix of company-invariant market-wide indexes listed in the previous section ($\mathbf{X_t}$).

My matrix of covariates includes 10 market-wide indexes that are not necessarily relevant to explain returns for a given company. Estimating Equation 1 with ordinary least squares (OLS) would result in noisy predictions with large variance, thus returning imprecise counterfactuals and potentially reducing precision of my estimates. I adopt a least absolute shrinkage and selection operator (LASSO) procedure for selecting the most predictive indexes in this matrix for each company. The LASSO is a covariate-selection algorithm that effectively associates sets of non-negative weights to each variable in the matrix of covariates $\mathbf{X'_t}$. It then selects the single set of weights $\boldsymbol{w_e}$ that results in the lowest residual sum of squares, hence in the most predictive model (Tibshirani, 1996). Effectively, it sacrifices non-predictive covariates (multiplying them by a 0 weight) to reduce the variance of an OLS estimation. Previous event-analysis designs have shown its improved performance over OLS market models. Each market model thus represents the best feasible predictor of a company's stock return, before the unexpected event e took place.

$$Returns_{et} = \alpha_e + \mathbf{X'_t}\boldsymbol{w_e}\boldsymbol{\beta_e} + \varepsilon_{et} \;\mid\; t_0 \leq t < t_1 \tag{1}$$

In my LASSO estimation procedure, I adopt a cross-validation procedure for selecting the set of most predictive weights for each individual event e involving a company. I employ 5 folds for each event and select the single vector of weights $\boldsymbol{w_e}$ that minimizes the mean cross-validated error across the sets of weights considered. I then employ this set of weights to determine how covariates enter Equation 1 for

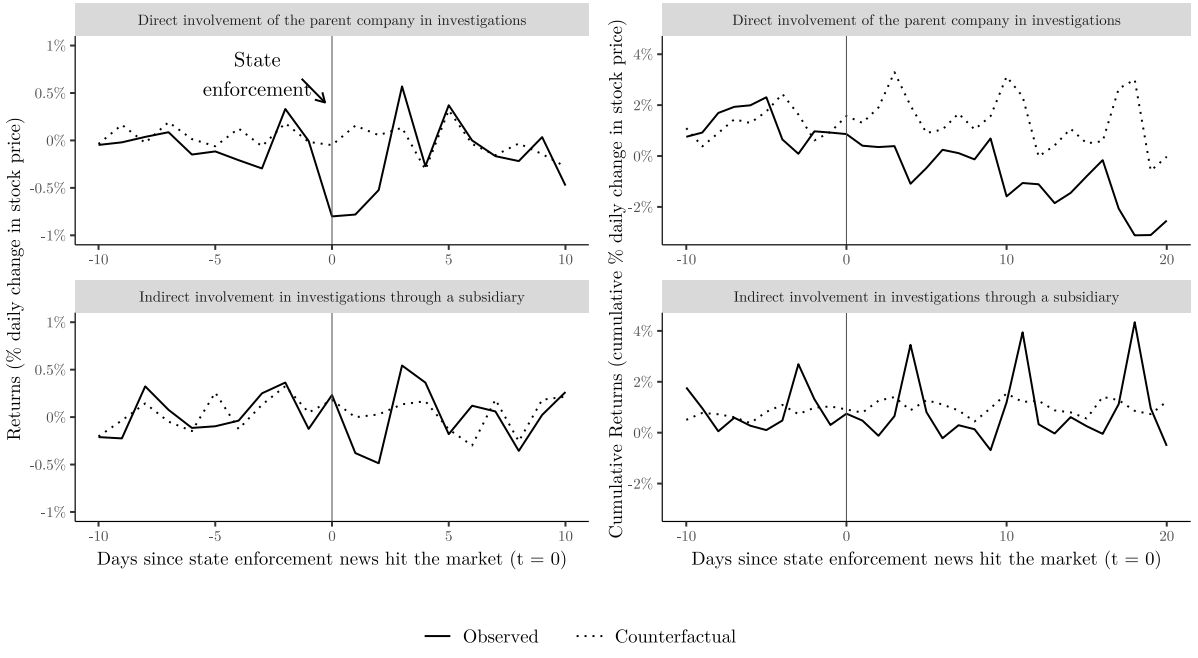
[21] In a robustness test, I show results are not dependent on the arbitrary choice of *event window* length (Tables C.2, C.3, and C.1).

that specific event. Figure A.1 in appendix shows that the procedure effectively omits least-predictive indicators, whereas it includes more frequently market-wide indexes with higher predictive power. This results in market models with high in-sample predictive performances. All models result in Root Mean Squared Errors[22] (RMSEs) with values smaller than 0.20, with the bulk of models yielding a value of just 0.10. Models perform well also from the R-squared perspective: the majority explain at least half of the variation of *Returns* in the *estimation window* (Figure A.2).

Once vectors of coefficients α_e and $\boldsymbol{\beta_e}$ have been estimated using *estimation window* data, I use $\hat{\alpha}_e$ and $\hat{\boldsymbol{\beta}}_{\boldsymbol{e}}$ to predict daily stock prices to each company in the *event window* (from t_1 to t_2). Equation 2 represents this second step. Effectively, I thus impute daily synthetic counterfactual stock prices to each company in the *event window*, based on models estimated in Equation 1. $E[Returns|\mathbf{X}]$ represents the expectable returns to a company in the *event window*, based on information available just before unexpected news of corporate criminal investigations were released. I take it as a measure of counterfactual *Returns* to a company, had event e not occurred. I also compute daily *Cumulative Observed* and *Cumulative Expected Returns* by summing, respectively, daily *Returns* and $E[Returns|\mathbf{X}]$ relative to a specific event.

$$E[Returns_{et}|\mathbf{X_t}] = \hat{\alpha}_e + \mathbf{X_t'}\boldsymbol{w_e}\hat{\boldsymbol{\beta}}_e \;\mid\; t_1 \leq t \leq t_2 \tag{2}$$

Figure 3: Average observed and counterfactual *Returns* (left panels) and *Cumulative Returns* (right panels) in the days before and after the release of corruption news, disaggregated by type of involvement. Top panels present direct involvement of a parent company, bottom panels report involvement through a subsidiary



[22] For each event e, the RMSE is computed as: $RMSE_e = \sqrt{\sum_t (\hat{y}_t - y_t)^2 / N_e}$ where y_t and $\hat{y}_t$ are daily observed and predicted values of *Returns* and N_e is the number of observations relative to a given event. The normalized version is calculated to allow comparison (any normalized RMSE ranges between 0 and 1). For each event e: $Normal\ RMSE_e = RMSE_e / [max_e(y_t) - min_e(y_t)]$.

Figure 3 plots the daily average observed and counterfactual *Returns* (left panels) and *Cumulative Returns* (right panels) in the days before and after the *Investigation*, distinguishing between cases of direct involvement in investigations (top panels) from those where involvement happened through a subsidiary (bottom panels). It provides initial evidence in support of my argument. Pre-treatment differences between observed and counterfactual (*Cumulative*) *Returns* are small. This indicates the lack of a pre-treatment difference among the two groups and reassures on the out-of-sample predictive performance of the LASSO. The top-left panels shows that observed *Returns* are on average lower than counterfactuals at the closing of the very day news of investigations are released (and consistently so in the following 48 hours) when parent companies are involved directly in a scandal. After that, *Returns* do not seem to depart from their counterfactuals. However, observed *Cumulative Returns* are significantly smaller than expected ones even until 20 days after the event (top-right panel), indicating that investigations seem to impose a long-lasting penalty on firms' profits. For cases of indirect involvement (bottom panel), instead, observed *Returns* and *Cumulative Returns* to the parent company are not significantly lower than their counterfactuals after the event.

Using these variables, I can finally compute my dependent variables. My main dependent variable is the difference between observed and expected *Returns* in the *event window*. I call this difference *Abnormal Returns*. I also compute *Cumulative Abnormal Returns* as the difference between observed and expected *Cumulative Returns*. *Abnormal Returns* and *Cumulative Abnormal Returns* quantify unexpected changes in stock returns following an event. Positive (negative) values indicate changes in stock prices that exceed (fall behind) what market models expected based on information available before the event.

I model these dependent variables in a standard event design that includes Day_t, a categorical variable measuring the number of days separating from the day of the *Investigation* (t_e) using day -1 as a baseline category. Equation 3 summarizes this step when I study the dependent variable *Abnormal Returns*, though the model takes the same form for *Cumulative Abnormal Returns*. Parameters γ_t quantify average differences in daily values of the dependent variable from the baseline day $(t_e - 1)$ until day $t_e - 2$. They are included as a placebo test against pre-treatment trends. Parameters δ_t, instead, estimate average post-treatment differences in daily (*Cumulative*) *Abnormal Returns* from the day before the event $(t_e - 1)$, thus returning ATT estimates. The model includes an event-fixed effect to completely remove between-event heterogeneity.

$$Abnormal\ Returns_{et} = \sum_{t=t_1}^{t_e-2} \gamma_t Day_t + \sum_{t=t_e}^{t_2} \delta_t Day_t + \phi_e + \varepsilon_{et} \tag{3}$$

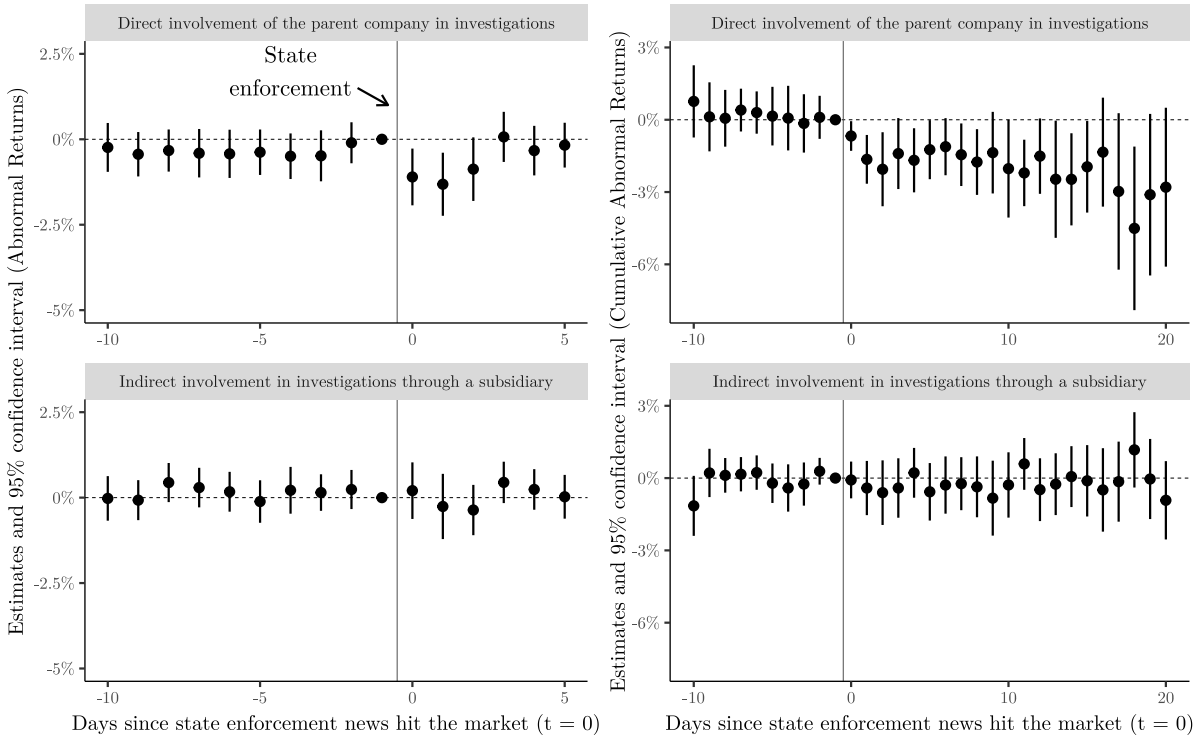
I estimate models in equation 3 after subsetting my sample between cases of direct ($Subsidiary = 0$) and indirect involvement in FCPA investigations ($Subsidiary = 1$). Thus, I aim at identifying the effect of FCPA investigations on involved parent companies' stock prices between scenarios of direct and indirect

involvement. But how comparable are these scenarios? Are firms or corruption events heterogeneous depending on whether involvement in investigations is direct or not? In appendix I show that, at least when looking at a range of relevant observable covariates, events of direct and indirect involvement of the parent company in investigations are comparable. These scenarios do not seem to differ significantly across pre-treatment observable features like the number of foreign countries involved, the size of the parent company, the value of its stocks, the level of corruption of the host country (Table B.1), or the distribution of headquarter countries and industries (Figures B.1 and B.2).

6 Results

I estimate equation 3 using OLS. Standard errors in all analyses are clustered at the parent company-level, to account for likely correlation between observations relative to the same firm. Figure 4 presents results relative to the 10 days before the event and 10 days after the event (20 post-event days in the case of *Cumulative Abnormal Returns*)[23]. Top panels report events of direct involvement in investigations, whereas bottom panels report events where the parent company was involved in an FCPA case only indirectly, via a subsidiary. Left panels study *Abnormal Returns* whereas right panels study *Cumulative Abnormal Returns*.

Figure 4: Event-analysis of *Abnormal* and *Cumulative Abnormal Returns* in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the investigations



[23]Full event-window results are omitted for ease of reading and are presented in appendix (Figure C.1).

In all cases, I observe no significant pre-treatment trend, which reassures on the internal validity of post-treatment estimates. *Abnormal Returns* to the parent company's stocks drop in value on the trading day of the *Investigation* and the following (days 0 and 1) when a company is directly involved in an investigation (top-left panel). On both days, companies' stocks on average closed their trading at a price about 1% lower than what they did on the day before the event. After that, the effect is re-absorbed, consistently with the market efficiency hypothesis (Fama, 1970). I observe no significant effect for cases of indirect involvement at all (bottom-left panel). Here, post-treatment point estimates are small and effects are never statistically significant.

Do penalties on days 0 and 1 in the top-left panel cumulate to any sustained loss? The top-right panel shows a consistently significant and negative *Cumulative Abnormal Returns* effect on the days after information hit the market, detected even after almost 20 days from the event. On day 18, involved companies experience average cumulative abnormal losses of about 4.5%. Instead, no significant cumulative loss on the parent company's stock returns is detected when the firm is involved in corporate criminal investigations through a subsidiary (bottom-right panel).

How sizeable are direct-involvement penalties? The average company involved directly in an investigation ($Subsidiary = 0$) traded at about \$72.5 per share on the day before the FCPA investigation was revealed. A 1% loss on days 0 and 1 from revelations of an FCPA enforcement means that such company lost about \$0.73 per share due to the unexpected information for two days. To estimate how this loss translates in terms of market capitalization, I retrieve from Orbis information on the number of outstanding shares traded by each parent company at the end of the month before each event considered. The average company in my data traded more than 1.5 billion shares, for a market capitalization of almost \$105 billion before enforcement. A daily loss of \$0.73 per share amounts to almost \$1.2 billion in losses each of the two days. With a similar logic, when looking at cumulative effects, on day 18 the average company had lost more than \$4.4 billion with respect to pre-event capitalization.

Is the effect detected for direct involvement statistically different from the null-effect relative to indirect involvement? In order to answer this question, I estimate a linear model of *Abnormal Returns* where I interact a binary treatment variable taking value 1 on the day of the investigation (0 otherwise) with the *Subsidiary* indicator. Table 2 reports my main results, which are relative to the full event window. In model 1, I introduce only my variables of interest. In following models, I introduce additional control variables. First, a one-day lag of the dependent variable to account for any anticipation effects. Next, a year-fixed effect to account for year-specific heterogeneity in FCPA enforcement action (Garrett, 2011) which might correlate with time-varying political motives of law-enforcement agencies (Tomashevskiy, 2021) and with stronger market reactions. Finally, specific events might have extreme resonance for reasons that are unrelated to my explanation. In model 4 I substitute year-fixed effect with event-fixed

effect to absorb all between-event heterogeneity in stockholders' response[24].

Table 2: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature

	(1)	(2)	(3)	(4)
Event	-0.828**	-0.939**	-0.942**	-0.919**
	(0.308)	(0.301)	(0.301)	(0.300)
Event × Subsidiary	0.914*	1.034*	1.027*	1.007*
	(0.455)	(0.455)	(0.455)	(0.454)
Subsidiary	0.015	0.022	0.003	
	(0.054)	(0.056)	(0.052)	
Abnormal Returns (t–1)		0.005	-0.0002	-0.027
		(0.034)	(0.033)	(0.034)
(Intercept)	-0.034	-0.034		
	(0.041)	(0.042)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	10455	9890	9890	9890
R2	0.001	0.002	0.007	0.035
R2 Adj.	0.001	0.001	0.005	0.008

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Across all models, the coefficient associated with the un-interacted *Event* variable is negative and distinguishable from zero at a 0.05 conventional level of significance. Point estimates of this coefficient return the same 1% average decrease in stock value for the parent company's stocks presented in Figure 4, for cases of direct involvement in investigations ($Subsidiary = 0$). The interaction term $Event \times Subsidiary$ has a positive coefficient. The estimate is also statistically significant at a 0.05 conventional level. This indicates that the negative effect of the scandal is moderated when a company is involved through a subsidiary and that such moderation is statistically significant.

I extensively test robustness of my results in appendix. First, I show that results are not driven by any single outlier – *e.g.*, a scandal with significantly negative impact (see Figures C.2 and C.3). Next, I show that findings are not driven by arbitrary choices followed in the procedure. I restrict *event windows* to the intervals: $[day\ -5\ , day\ 5]$, $[day\ -10\ , day\ 10]$, and $[day\ -10,\ day\ 0]$ to make sure I consider only data in the immediate proximity of the FCPA investigation (Tables C.1, C.2, and C.3). Next, I test robustness of results to the exclusion of events with imprecise market models from equation 1 – that is, yielding R-squared values smaller than 0.10 (Figure C.4 and Table C.4). To conclude, I show that similar findings can be obtained when leveraging different research designs. I exploit the exogenous timing of investigation and use days from the event as a treatment variable of observed *Returns* and *Cumulative Returns*. Results are consistent with my event study (Figure C.5 and Table C.5). I also use the number of days to the event as an instrument in a regression discontinuity in time design and I find consistent estimates with those presented (Section D). In appendix, I also dig deeper inside the null-effect relative

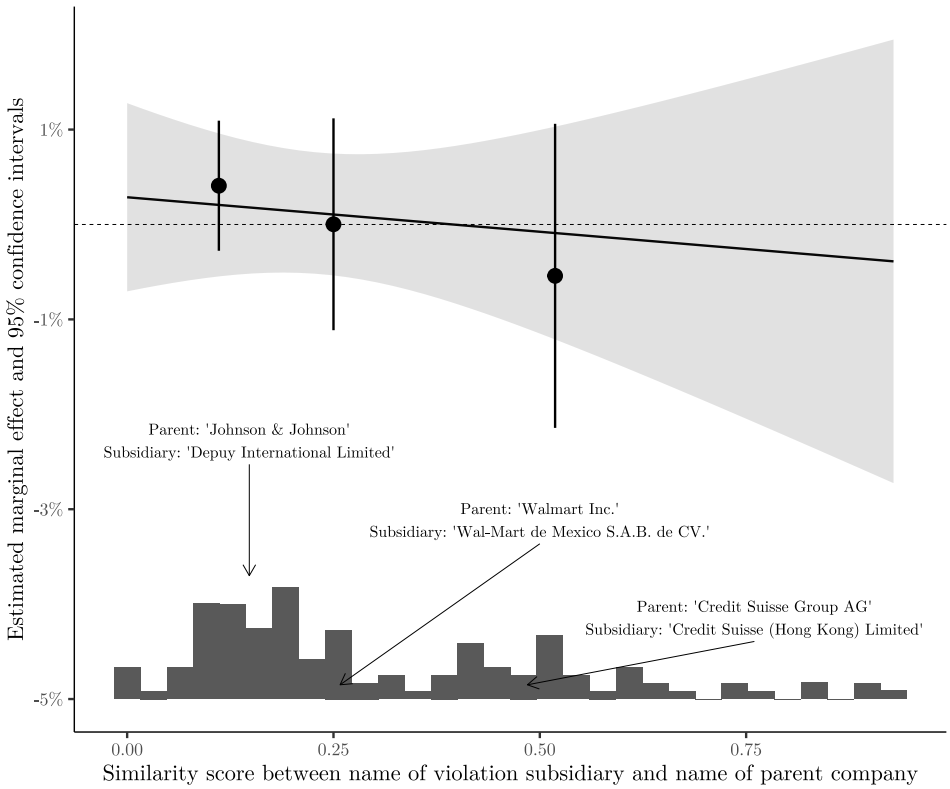
[24]Obviously, in this specification the coefficient for the event-invariant constitutive term of the interaction *Subsidiary* cannot be estimated due to perfect collinearity.

to indirect involvement. Consistently with my framework (Figure 2), I distinguish cases where the parent is the whole owner of the investigated subsidiary from those where it is only majoritarian owner. I find no effect in either of these categories (Section E).

6.1 Name similarity

What drives this null-effect for the case of indirect involvement? Are investors and market analysts ignorant of companies' corporate structures, or else do they not impose a penalty on a parent company's stocks out of profit reasons? In order to provide evidence on this mechanism, I propose one last empirical test. I leverage differences between the names of involved subsidiaries and those of parent companies to understand whether cases of indirect involvement in which ownership is obvious lead to any significant effect. Cases of indirect involvement can include subsidiaries with very different names from that of the parent. For instance, Depuy International LTD (wholly-owned by Johnson & Johnson). In similar cases, investors might not be necessarily aware of true corporate ownership when informed of a corruption scandal. Alternatively, the name of a subsidiary can be very similar to that of the parent, often even incorporating it – as in the case of Wal-Mart de Mexico, owned by Walmart Inc.

Figure 5: Marginal effects of indirect involvement into FCPA investigations on the parent company's *Abnormal Returns*, conditional on the degree of similarity between the name of the subsidiary and that of the parent company.



I leverage these differences and calculate a score representing the similarity between the name of the

parent and that of the subsidiary in case of indirect involvement in a scandal. I employ a metric for string similarity based on the Levenshtein distance[25], which ranges from 0 (indicating extreme diversity between two strings) to 1 (indicating perfect equality). Next, I re-estimate my event-fixed effect model from Table 2, subsetting my sample for cases of indirect involvement only. I employ the name-similarity score as a moderating variable. To this aim, I employ the binning estimator proposed by Hainmueller et al. (2019), which does not force the moderating effect to be linear. Figure 5 reports results and presents three examples of pairs of names ending up in each of the three levels of the moderating variable. I observe no significant effect for any type of indirect involvement, even when the name of the subsidiary responsible for alleged corruption is as similar to that of the parent as "Credit Suisse (Hong Kong) Limited" is to "Credit Suisse Group AG". This lends confidence against the hypothesis that the null-effect is driven by genuine ignorance on the side of investors about corporate ownership linkages. It suggests investors in fact fail to penalize parents for misconduct by their subsidiaries out of expectations that involvement of a subsidiary will not negatively affect profits.

7 Conclusion

Multinational companies can exploit their fragmented ownership chains in order to conceal financial crime (Cooley and Sharman, 2017) and evade regulations states cast to prohibit misconduct (Arel-Bundock, 2017; Chapman et al., 2020). This poses a real threat to an effective limitation of nefarious transactions and questions whether formal regulatory provisions bear any deterrence against corporate crime (Baradaran et al., 2012; Findley et al., 2015). It is often argued that formal state-based legal tools can find an unexpected regulatory helping-hand from markets (Morse, 2022). Investors would behave as a "global civil society" (Fukuyama, 2016; Ruggie, 2018) by "buycotting" companies' stock prices when information on corporate misconduct emerges (Alexander, 1999; Kreitmeir et al., 2020). Public authorities *de facto* outsource part of the regulatory process to such market responses, mitigating their sanctions to avoid declaring a "death sentence" on corporate criminals (Garrett, 2011). However, it is not clear whether markets penalize companies for misconduct happening down their ownership chains. The gap is relevant because fragmented ownership can be purposed precisely to further financial crime (Sharman, 2010).

In this paper, I argued that companies can fragment their ownership as a shield against informal penalties imposed by financial markets when information on public criminal investigation emerges. My conceptual framework distinguishes cases where a parent company is directly involved in a scandal and those where involvement happens indirectly – that is, via an owned subsidiary. I claim that markets impose penalties on a company when unexpected allegations of its *direct involvement* in a crime hits

[25] The Levenshtein distance $L(a, b)$ is defined as the minimum number of modifications that are necessary in order to turn the word a into the word b. The metric I employ is a similarity score calculated as $1 - \frac{L}{M}$, where M is the number of characters for the longest of the two strings.

the markets, due to concerns about the firm's profitability. However, the effect is diminished when the company is involved indirectly. In particular, I claimed the effect declines in size as the degree of integration of the responsible entity in the corporate group decreases, because with diluted ownership comes reduced control by the parent (Alexander and Cohen, 1999).

My empirical tests leveraged an original dataset on 267 investigations for alleged violation of the US anti-corruption criminal law (FCPA) in 217 distinct corporate groups. I retrieved data on the day information of misbehavior first hit the market and daily stock prices of the parent company sitting at the top of each corporate group. I also coded the relationship between the entity (allegedly) responsible for a violation and the parent company. An event-analysis design shows that parent companies suffer a significant abnormal loss of about 0.01% to their stock returns on the two days following the release of information. This effect amounts to a daily loss of about $150 million in market value for the median company and cumulates to more than $500 million in losses even almost 20 days after the investigation. However, I show evidence of no effect on the parent company's stock prices when involvement occurs through a subsidiary.

Results indicate a failure of the supposed regulatory function performed by markets that is of interest to the international governance literature. Although I provide evidence that markets do penalize companies for direct involvement in misconduct, consistently with important previous studies (Morse, 2019), investors do not seem to bite against parent companies for crime conducted by entities down the line of their corporate groups. This is concerning because it shows that companies can strategically fragment ownership to meet a cynical threefold goal: to further financial crime (Findley et al., 2015), to evade regulations (Chapman et al., 2020), and to minimize losses on equity markets. This has important implications for debates in governance beyond financial crime, for instance in environmental regulation.

More fundamentally, findings question the extent to which markets are a viable complement (or substitute) for formal state-based regulations, a conclusion that contributes to a long-lasting debate in political science on state-market relations (Ruggie, 2018; Strange, 1996) and on ensuring compliance of private actors with international norms (Baradaran et al., 2012; Jensen and Malesky, 2018). Future research on the matter could learn from these conclusions to study whether public regulators respond differently to different size of market responses against corporate crime, perhaps rebalancing the regulatory failure documented here. Furthermore, global governance scholars could study whether different forms of corporate integration (*e.g.* vertical vs horizontal integration, joint ventures, and licensing) insulate or expose parents to private regulatory responses by investors. Additionally, scholars of political economy could study whether wordings of negative news by companies in their communications of misbehavior affect markets differently.

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Appendix

The Shield of Ownership. The Limits of Markets' Regulatory Function Against Financial Crime

A Estimation procedure

Figure A.1: Heatmap reporting the value of estimated coefficients relative to financial indicators (y-axis) as they enter each of the 267 market models from the *estimation window* (x-axis) when using the LASSO procedure. The plot shows in white indexes that are excluded from a market model and colors cells according to the size of the estimated coefficient (multiplied by the LASSO weight). A percentage is also reported indicating the share of models each index is included in.

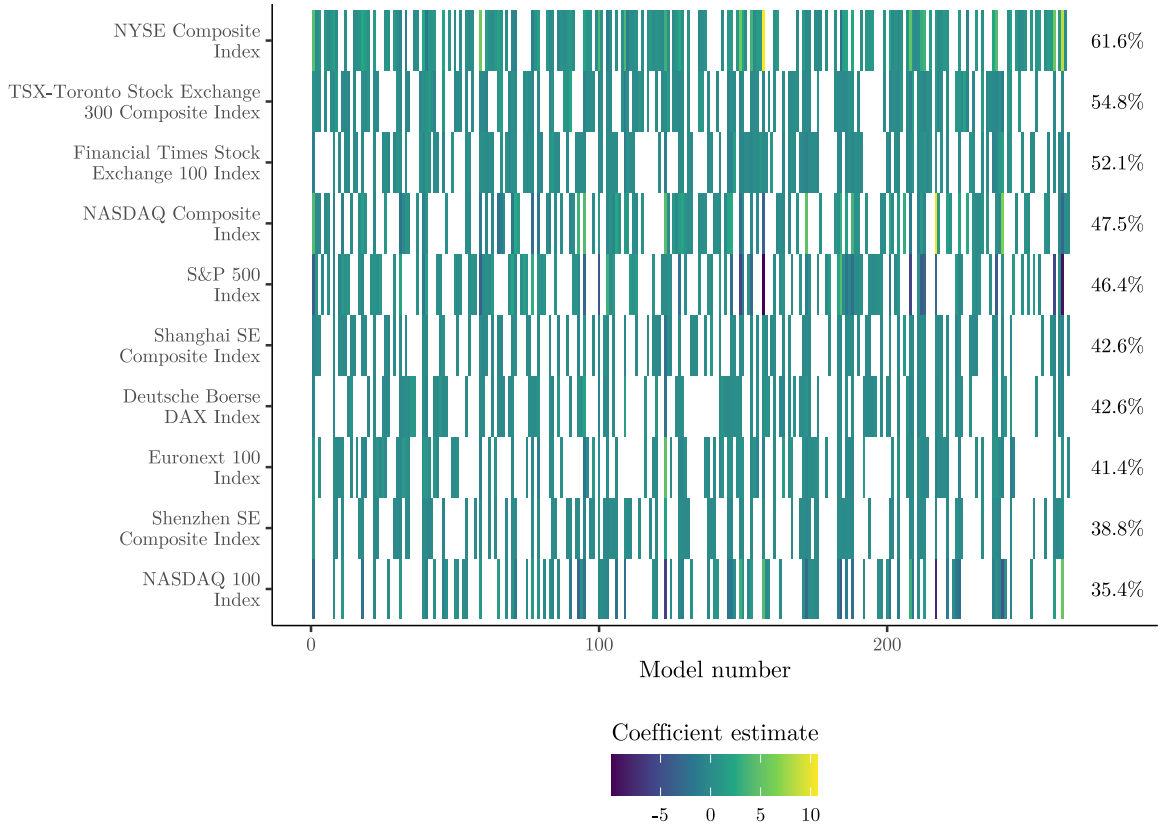
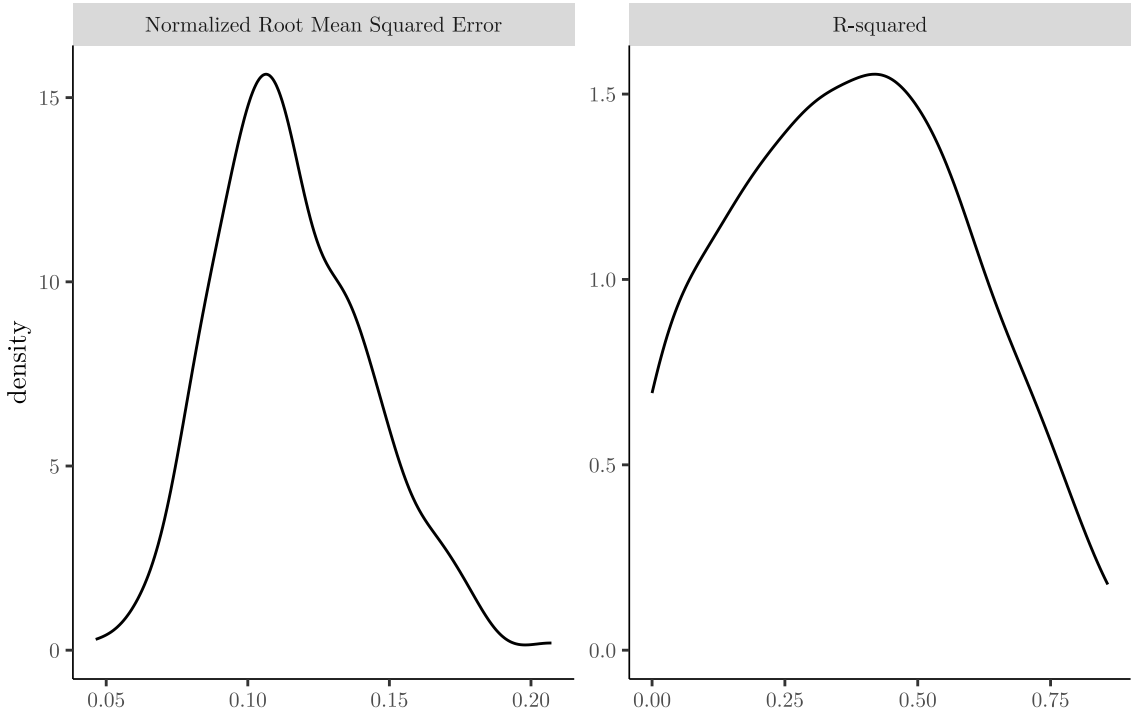


Figure A.2: Distribution of the normalized Root Mean Squared Error (RMSE) and of the R-squared yielded by the 267 market models estimated using the LASSO procedure.



B Descriptives

B.1 Balance in observable covariates across types of involvement

I retrieve information on characteristics of each parent company involved in an event e to evaluate whether events of direct and indirect involvement in investigations are comparable. All information is retrieved from the Orbis Corporate Ownership database. For each company involved in an event e I collect time-varying information. First, I measure the number of outstanding shares traded by each company at the end of the month before each event. Second, I measure market capitalization (computed as number of outstanding shares times closing price) on the day before each event for each company. Next, I retrieve information on the companies' revenues, asset value, and number of employees at the end of the solar year before each event. Finally, I retrieve relevant variables relative to the alleged corruption event. I measure the number of *Violation countries* for each event (meaning, the number of foreign countries where each company was alleged to have violated the FCPA). I also measure the level of corruption of the host country where a scandal occurs, as reported by the V-DEM country corruption estimate (*Host country corruption*). Where corruption allegedly took place across multiple host countries, I take the mean of their V-DEM country corruption estimate. I then compute simple difference in means for these variables based on events where involvement was direct ($Subsidiary = 0$) and those where it was indirect ($Subsidiary = 1$).

Table B.1 reports summary statistics for these covariates across these two groups. It shows reassuring evidence that the two groups are balanced with respect at least to these important pre-treatment observable characteristics. All differences in their average values across the two groups are statistically insignificant with large p-values. The signs of the differences, moreover, are mixed and not implying any consistent imbalance. For instance, companies involved directly tend to have larger market capitalization ($50.20 *vs* $43.79 billion) and are larger by assets ($124.67 *vs* $87.69 billion) but they tend to be smaller by revenues ($27.16 *vs* $29.61 billion) and number of employees (56.43 *vs* 84.35 thousands). The only exception is represented by the level of corruption of the host markets involved in the scandals, as measured by the VDEM index. Cases of indirect involvement are, on average, slightly *less* corrupt than cases of direct involvement. However, this difference is marginal (it corresponds to less than one third of a standard deviation of this variable). Moreover, it is in the *opposite* sign that one would expect to observe if MNCs were strategically outsourcing corruption in most severe locations to subsidiaries. In Figures B.1 and B.2, I show that the two groups are also balanced with respect to time-invariant characteristics including the headquarter country and the industry of activity – according to the 3-digits North American Industry Classification System (NAICS-3).

Table B.1: Balance in covariates relative to events with direct involvement ($Subsidiary = 0$) and with indirect involvement ($Subsidiary = 1$). Pre-treatment covariates only

	Direct involvement (N=139)		Indirect involvement (N=125)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Parent Outstanding Shares (billions)	1.50	2.90	1.40	2.18	-0.10	0.76
Parent Market Capitalization (billion USD)	50.20	83.46	43.79	60.86	-6.41	0.51
Parent Revenue (billion USD)	27.16	47.79	29.61	57.14	2.45	0.72
Parent Assets (billion USD)	124.67	392.41	87.69	262.53	-36.98	0.38
Parent No. Employees (thousands)	56.43	76.74	84.35	222.90	27.92	0.22
Number of violation countries	2.03	2.11	1.80	2.09	-0.23	0.40
Host country corruption (VDEM)	-0.43	0.99	-0.15	1.06	0.28	0.04

Figure B.1: Proportion of events involving companies by headquarter country, across cases of direct ($Subsidiary = 0$) and indirect involvement ($Subsidiary = 1$).

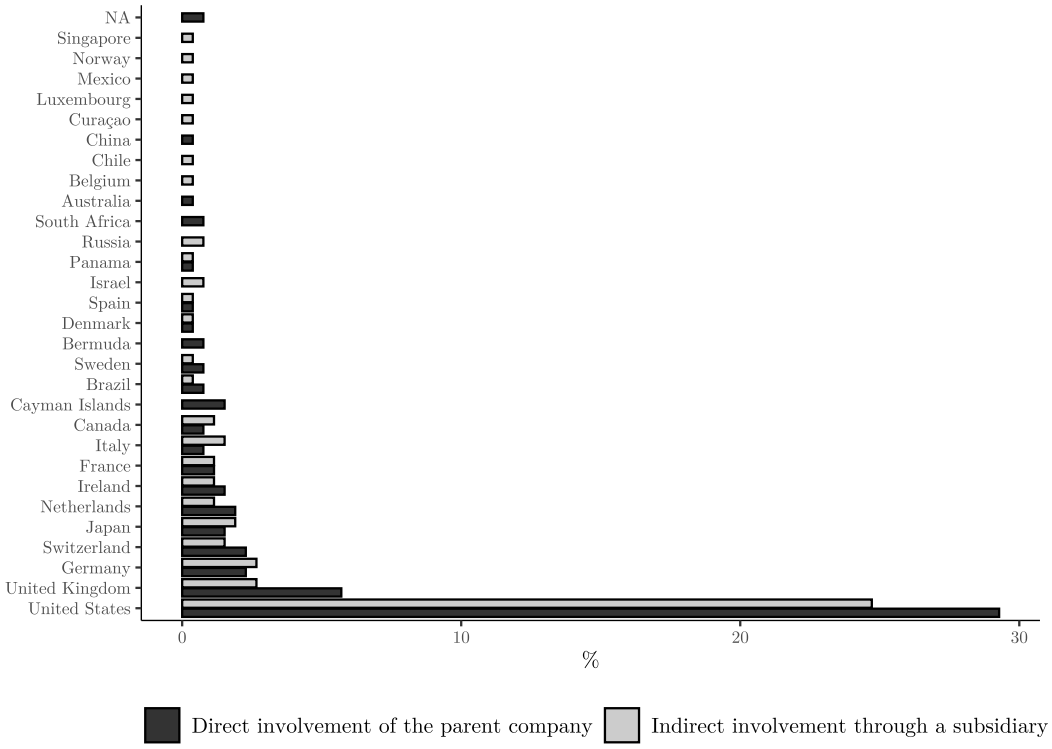
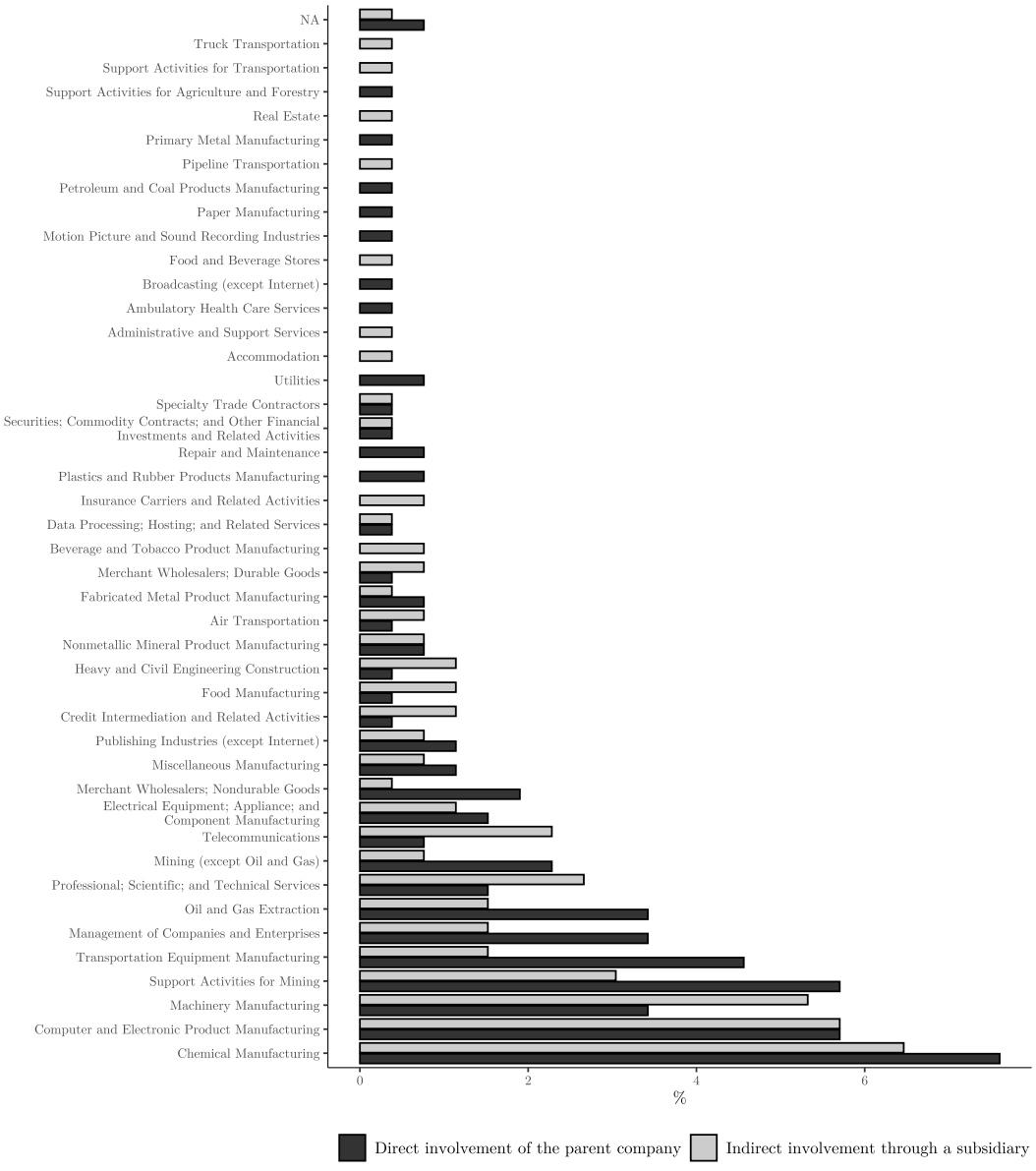


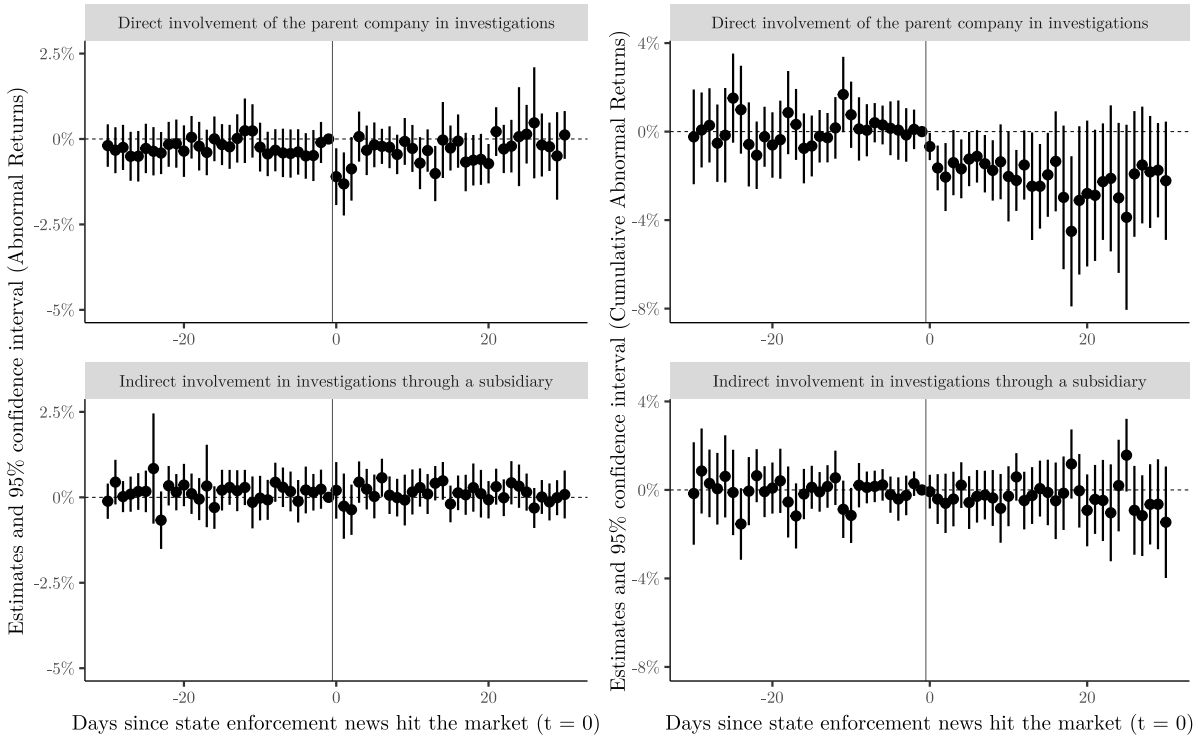
Figure B.2: Proportion of events involving companies by NAICS-3 code, across cases of direct ($Subsidiary = 0$) and indirect involvement ($Subsidiary = 1$).



C Analysis: LASSO-estimated synthetic counterfactuals

C.1 Event analysis disclosure

Figure C.1: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full *event window* results



C.2 Abnormal Returns: Robustness tests

C.2.1 Exclusion of outliers

I perform extensive robustness tests on my findings. First, I rule out that results are driven by any single outlier (a scandal with significantly negative impact, or a particularly "bad" firm) in my data. I replicate my event analysis from Figure 4 adopting a jackknife approach. I estimate the model multiple time, each time leaving one different event out of the model. I report point estimates and confidence intervals in Figure C.2 (alongside full-sample estimates for comparison). Second, I re-estimate the full model from Table 2 following the same leave-one-out approach. Figure C.3 reports estimated coefficients for the un-interacted *Investigation* term and the interaction term *Investigation* × *Subsidiary*, alongside their 95% confidence intervals.

Figure C.2: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full *event window*. Plot reports point estimates and 95% confidence intervals obtained when excluding one event at the time from the dataset. Solid lines represent point estimates. Dotted lines represent lower and upper bounds of the confidence intervals. Grey lines represent estimates obtained when leaving one event out whereas black lines report full sample estimates for comparison.

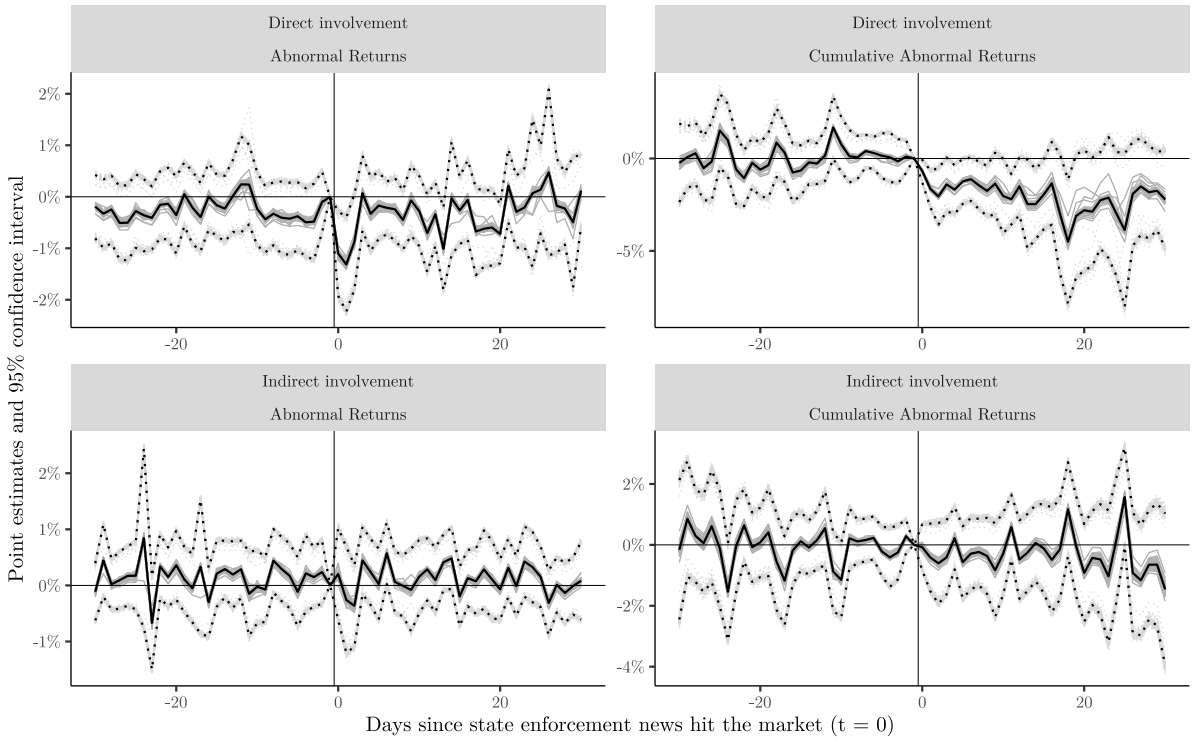
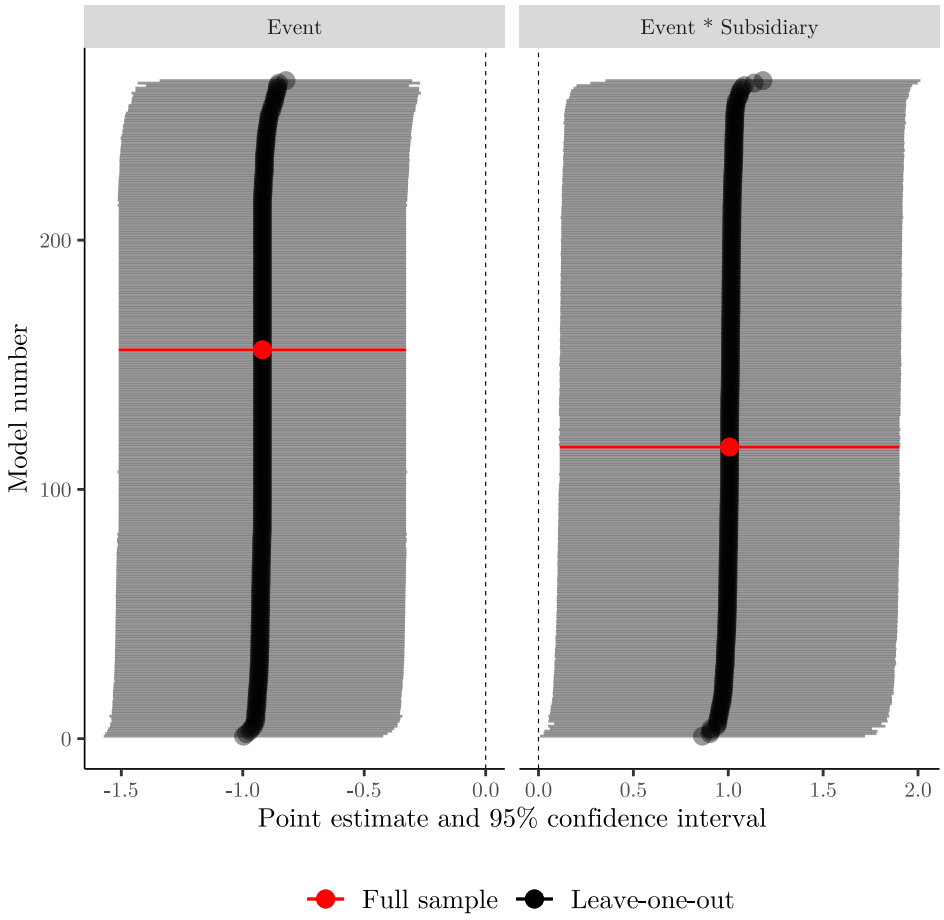


Figure C.3: Replication of model 4 from Table 2, leaving one event out of the dataset at a time. Point estimates and 95% confidence intervals reported refer to the un-interacted *Investigation* term and to the interaction term *Investigation* × *Subsidiary*. Red coefficients represent full-sample estimates from the main text.



C.2.2 Alternative window sizes

Next, I address the potential concern that results are driven by arbitrary choices followed in the procedure. I replicate the entire analysis restricting my *event window* to the 5-days before and 5-days after the *Investigation*. This verifies results do not hinge on my arbitrary choice for the length of the time window. Results in Table C.1 from the same sparse and full models of table 2 are consistent with my expectations. In a further test, I restrict *event window* data to the interval $[day-10, day\,10]$ and $[day-10,\ day\,0]$, to show robustness of results against alternative window sizes. Results are consistent with earlier findings (Tables C.2 and C.3). Notice that these tables also include models using two different measures for involvement: *Ownership* and a categorical variable for whether involvement occurs through a majority or wholly-owned subsidiary.

Table C.1: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature. Event window data limited to 5 days before - 5 days after the Event

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.670*	-0.619*	-0.502	-0.480	-0.670*	-0.619*
	(0.326)	(0.307)	(0.316)	(0.297)	(0.326)	(0.307)
Event × Subsidiary	0.842+	0.714				
	(0.477)	(0.453)				
Event × Ownership			0.320	0.282		
			(0.288)	(0.276)		
Event × Wholly-owned Subsidiary					1.250*	1.027+
					(0.593)	(0.554)
Event × Non-wholly-owned Subsidiary					0.376	0.349
					(0.581)	(0.560)
Subsidiary	0.087					
	(0.140)					
Ownership			0.102			
			(0.088)			
Wholly-owned Subsidiary					-0.054	
					(0.158)	
Non-wholly-owned Subsidiary					0.250	
					(0.179)	
Abnormal Returns (t–1)		-0.193***		-0.193***		-0.193***
		(0.036)		(0.036)		(0.035)
(Intercept)	-0.191+		-0.219*		-0.191+	
	(0.106)		(0.100)		(0.106)	
Event FE		Yes		Yes		Yes
Num.Obs.	1754	1703	1754	1703	1754	1703
R2	0.006	0.220	0.004	0.218	0.008	0.221
R2 Adj.	0.004	0.076	0.003	0.075	0.005	0.077

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table C.2: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Event window data limited to 10 days before - 10 days after the Event

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.744*	-0.768**	-0.597*	-0.626*	-0.744*	-0.768**
	(0.303)	(0.294)	(0.295)	(0.287)	(0.303)	(0.294)
Event × Subsidiary	0.848+	0.860+				
	(0.452)	(0.445)				
Event × Ownership			0.357	0.378		
			(0.270)	(0.271)		
Event × Wholly-owned Subsidiary					1.168*	1.139*
					(0.571)	(0.554)
Event × Non-wholly-owned Subsidiary					0.482	0.533
					(0.546)	(0.550)
Subsidiary	0.080					
	(0.083)					
Ownership			0.066			
			(0.052)			
Wholly-owned Subsidiary					0.027	
					(0.093)	
Non-wholly-owned Subsidiary					0.144	
					(0.105)	
Abnormal Returns (t–1)		-0.108**		-0.109***		-0.108**
		(0.033)		(0.033)		(0.033)
(Intercept)	-0.117+		-0.124*		-0.117+	
	(0.066)		(0.062)		(0.066)	
Event FE		Yes		Yes		Yes
Num.Obs.	3606	3494	3606	3494	3606	3494
R2	0.004	0.112	0.003	0.111	0.005	0.112
R2 Adj.	0.003	0.039	0.002	0.038	0.003	0.039

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table C.3: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Event window data limited to 10 days before the Event and the event day

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.789*	-0.777**	-0.654*	-0.652*	-0.789*	-0.777**
	(0.309)	(0.294)	(0.300)	(0.287)	(0.309)	(0.294)
Event × Subsidiary	0.854+	0.818+				
	(0.455)	(0.450)				
Event × Ownership			0.378	0.375		
			(0.270)	(0.271)		
Event × Wholly-owned Subsidiary					1.125+	1.040+
					(0.578)	(0.569)
Event × Non-wholly-owned Subsidiary					0.546	0.553
					(0.542)	(0.552)
Subsidiary	0.075					
	(0.091)					
Ownership			0.045			
			(0.055)			
Wholly-owned Subsidiary					0.070	
					(0.105)	
Non-wholly-owned Subsidiary					0.081	
					(0.108)	
Abnormal Returns (t–1)		-0.164***		-0.165***		-0.164***
		(0.032)		(0.032)		(0.032)
(Intercept)	-0.072		-0.067		-0.072	
	(0.072)		(0.068)		(0.072)	
Event FE		Yes		Yes		Yes
Num.Obs.	1932	1870	1932	1870	1932	1870
R2	0.009	0.186	0.006	0.185	0.009	0.187
R2 Adj.	0.007	0.052	0.005	0.050	0.007	0.052

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C.2.3 Exclusion of events with imprecise imputed counterfactuals

In a following test, I verify results do not hinge on the inclusion of events for which the imputation of synthetic counterfactual was imprecise. I exclude from the analysis any event with market model from Equation 1 yielding an R-squared lower than 0.10. This restricts the analysis to a subset of 189 companies involved in 235 events. I replicate my entire analysis and verify results are consistent (Figures C.4 and Table C.4). The event analysis results in noisier estimates, but overall results are in line with previously presented ones.

Figure C.4: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full *event window*. Plot reports point estimates and 95% confidence intervals obtained when excluding firms with imprecise counterfactual estimation

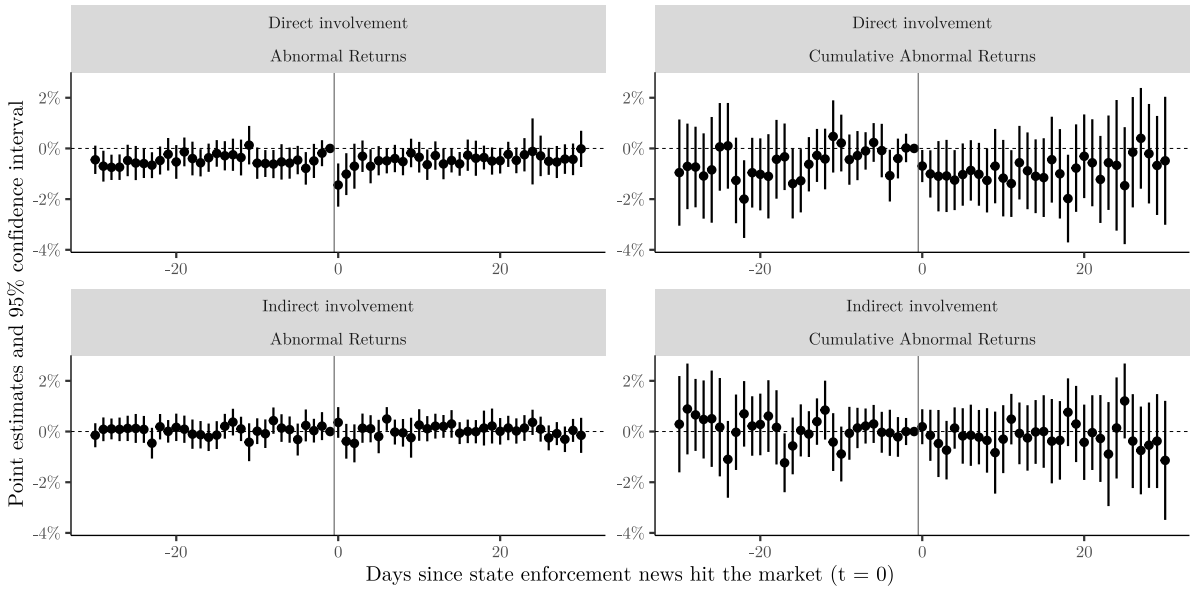


Table C.4: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature. Event window data limited to events with precise counterfactual imputation

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-1.000**	-0.960**	-0.899**	-0.855**	-1.000**	-0.960**
	(0.324)	(0.340)	(0.312)	(0.326)	(0.324)	(0.340)
Event × Subsidiary	1.324**	1.274**				
	(0.434)	(0.450)				
Event × Ownership			0.768**	0.732**		
			(0.233)	(0.243)		
Event × Wholly-owned Subsidiary					1.302*	1.272*
					(0.566)	(0.580)
Event × Non-wholly-owned Subsidiary					1.353**	1.278**
					(0.432)	(0.452)
Subsidiary	-0.063					
	(0.046)					
Ownership			-0.036			
			(0.027)			
Wholly-owned Subsidiary					-0.065	
					(0.058)	
Non-wholly-owned Subsidiary					-0.061	
					(0.053)	
Abnormal Returns (t–1)		-0.006		-0.006		-0.006
		(0.020)		(0.020)		(0.020)
(Intercept)	0.028		0.022		0.028	
	(0.033)		(0.032)		(0.033)	
Event FE		Yes		Yes		Yes
Num.Obs.	9090	8588	9090	8588	9090	8588
R2	0.004	0.039	0.003	0.038	0.004	0.039
R2 Adj.	0.003	0.012	0.003	0.012	0.003	0.012

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C.3 Non-synthetic counterfactual event analysis

Finally, I replicate my analysis when studying *Returns* and *Cumulative Returns* – that is, without discounting synthetic counterfactuals from stock price returns. Results are reported in Figure C.5 and Table C.5.

Figure C.5: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full *event window* without discounting synthetic counterfactuals. Plot reports point estimates and 95% confidence intervals

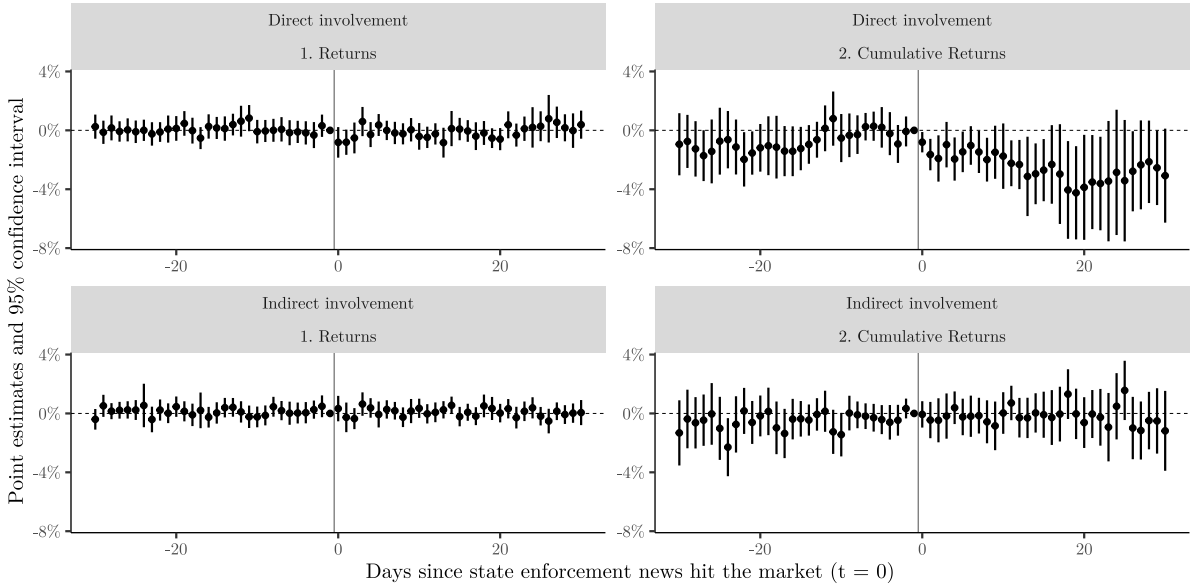


Table C.5: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature. Non-discounted Returns

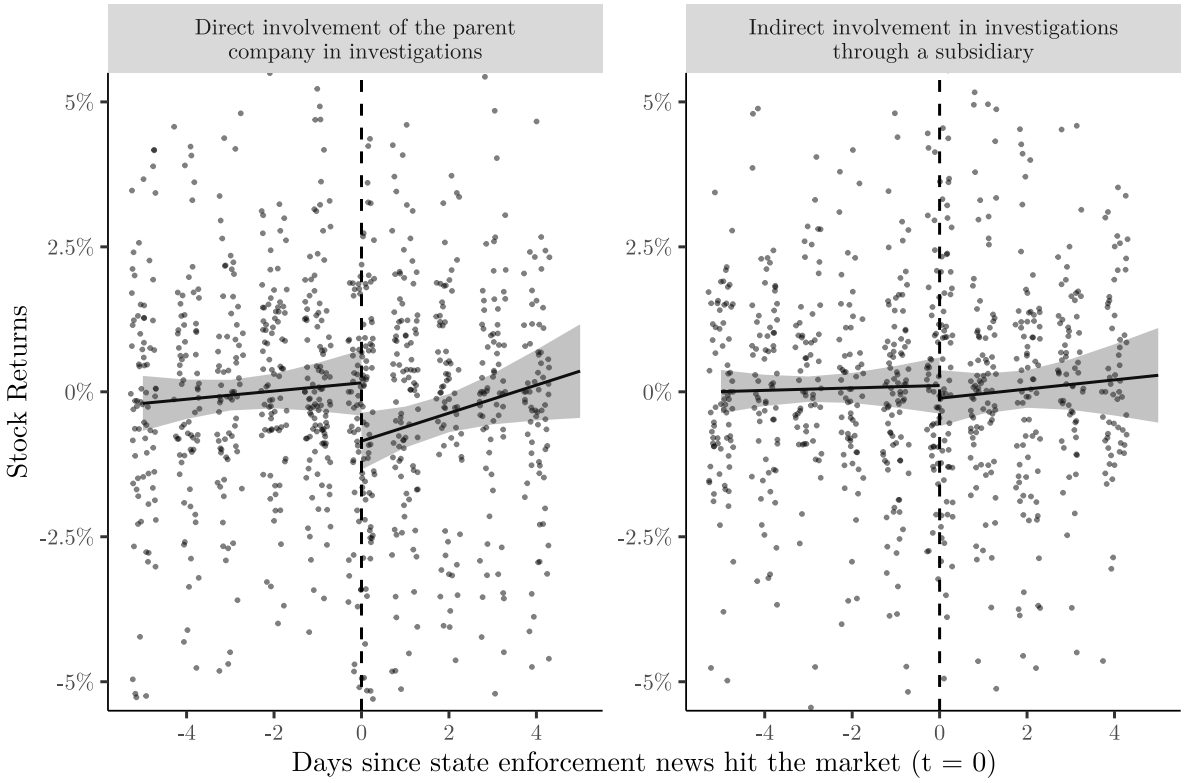
	(1)	(2)	(3)	(4)
Event	-0.803*	-0.795*	-0.797*	-0.805*
	(0.340)	(0.340)	(0.339)	(0.337)
Event × Subsidiary	1.033*	1.020*	1.021*	1.022*
	(0.482)	(0.483)	(0.483)	(0.479)
Subsidiary	-0.0003	0.012	-0.00003	
	(0.054)	(0.055)	(0.047)	
Returns (t–1)		-0.009	-0.014	-0.034
		(0.026)	(0.025)	(0.026)
(Intercept)	0.002	-0.007		
	(0.042)	(0.042)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	11124	10852	10852	10852
R2	0.001	0.001	0.007	0.026
R2 Adj.	0.0007	0.0007	0.004	0.002

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

D Time to enforcement as an instrument

In this section I show that similar results can be estimated when adopting yet another identification strategy. Instead of relying on a synthetic counterfactual of stock prices, here I use time to the day of state enforcement as an instrument for firms' *Returns*. Assuming that the timing of news of an enforcement is exogenous (an assumption I defended in the main text), we can estimate the effect of a scandal by comparing *Returns* to companies right before and right after news broke out, by taking a sufficiently small window around the day news hit the public. Effectively, this is analogous to estimating a regression discontinuity design where the running variable is represented by the "days from the enforcement news". The procedure is exemplified in Figure D.1. The figure plots *Returns* to each company based on the distance from the news of enforcement in a window of 5 days before - 5 days after the event. Similarly to earlier visualizations, the first panel shows cases of direct involvement in news, the second cases of indirect involvement. I introduce a linear model on each side of the discontinuity represented by day 0 (the day news hit the public). The local average treatment effect (LATE) of enforcement news on firms' *Returns* (by each type of involvement) can be estimated by taking the distance between the intercepts of the two linear models with the vertical line at day 0.

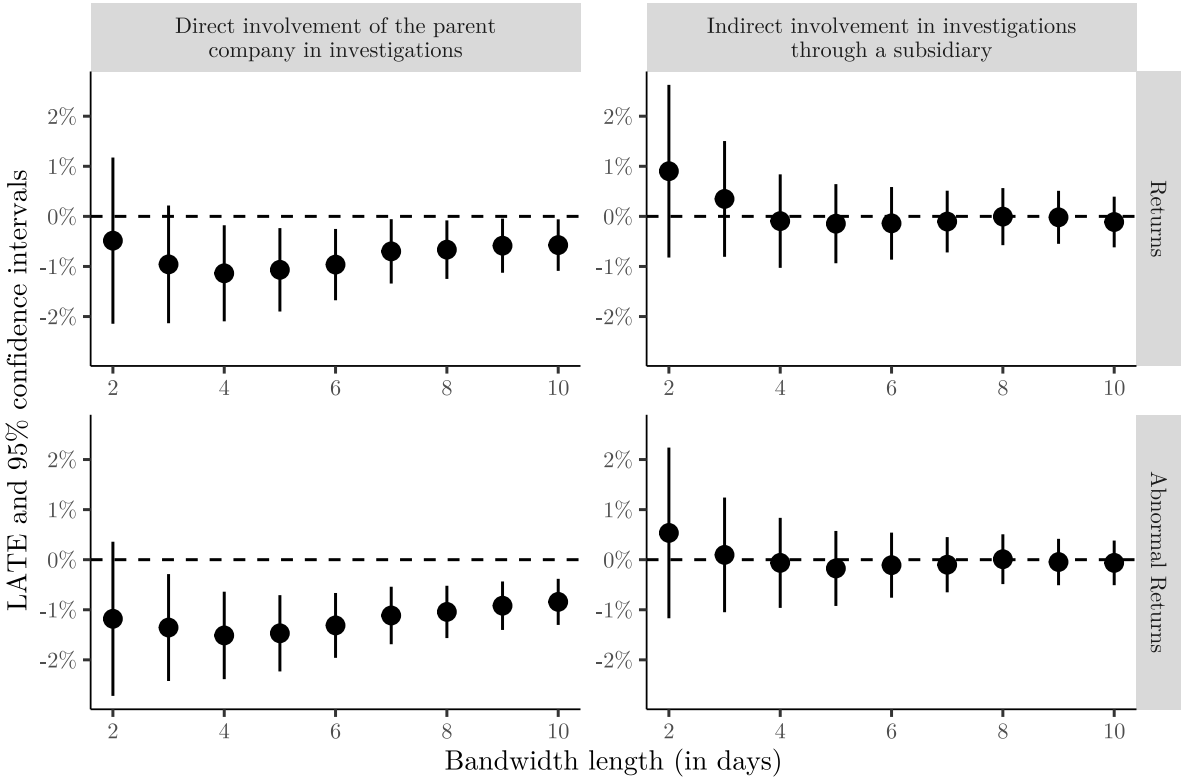
Figure D.1: Regression discontinuity design when using time to the news of enforcement as a running variable. Example of application when adopting a bandwidth of 5 days before the event and 5 days after the event



I estimate LATEs for cases of direct and indirect involvement by means of a simple regression discontinuity design where linear models with varying slopes are fitted to both sides of the discontinuity. In Figure D.2 I present estimates obtained when varying the size of the bandwidth (meaning, the number of days before and after the event) from 2 to 10. The top two panels present estimates obtained when studying *Returns* of companies' stocks in cases of direct (left) and indirect (right) involvement in news of FCPA enforcement. Across bandwidths (except for bandwidths 2 and 3), estimates are negative and statistically distinguishable from zero at a 0.05 level of significance for cases of direct involvement. Some estimates borderline statistical significance but overall the evidence indicates a reduction of about 1% in stock returns for cases of direct involvement. Instead, I find no significant effect for cases of indirect involvement. At the bottom of the figure, I replicate the procedure but I study *Abnormal Returns* to these companies. I intend this as a dependent variable capturing firms' stock returns "cleaned" from broader market trends. When I do so, estimates for cases of direct involvement are negative and precisely

estimated, with estimates of size consistent with the previous ones. Instead, cases of indirect involvement are smaller and never statistically significant.

Figure D.2: Regression discontinuity design when using time to the news of enforcement as a running variable. All estimated LATEs when adopting bandwidths from 2 to 10 days before and after the event and when studying *Returns* or *Abnormal Returns*



E Typologies of indirect involvement

My results show that parent companies are not imposed any penalty when they are involved in a corruption scandal through a subsidiary. Thus, corporate ownership can insulate a company from naming-and-shaming damage on financial markets resulting from unexpected information of criminal activity. This is evidence pointing to a concerning regulatory failure: a company can outsource illicit behavior to its subsidiaries so as to shield itself from the reputational damage documented in the direct involvement scenario. A potential objection with such interpretation is that companies do not necessarily hold control over all their subsidiaries. Markets perhaps refrain from penalizing a company for its subsidiaries' misconduct because the parent company cannot be held responsible for illicit behaviors of a subsidiary that it does not fully control.

I argue that this concern appears less relevant in the case of corruption, a type of criminal activity which introduces inefficiencies and should concern investors' profit prospects regardless of where it occurs along the ownership chain. However, I address this concern empirically by distinguishing between involvement of a company in an event of corruption through a wholly-owned subsidiary and a non-wholly owned one. Wholly-owned entities are more directly associated to the parent company, which holds direct control over their operations (Demsetz and Lehn, 1985), including criminal activities (Alexander and Cohen, 1999). Evidence that markets do not penalize a parent company for its subsidiaries' misconduct even in the case of whole ownership would buttress my claim of a regulatory failure. I substitute my *Subsidiary* moderator variable with the *Ownership* indicator presented above, to study how the moderating effect of corporate ownership varies across wholly-owned and non-wholly owned subsidiaries.

I re-estimate models in Table 2. Full results are in Table E.1. Consistently with previous evidence, I find that stronger degrees of corporate complexity mitigate the negative effect of corporate corruption scandals on stock markets. In a further test, I run a more flexible specification that uses a categorical measure of *Ownership*. Similarly to the binning estimator proposed by Hainmueller et al. (2019), this procedure does not force the moderating effect to be linear. Results are in Table E.2. Here, too, I find that subsidiary incorporation significantly mitigates the negative effect suffered by companies on stock market for direct implication in cases of corporate corruption (Figure E.1)

Table E.1: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Continuous ownership measure

	(1)	(2)	(3)	(4)
Event	-0.683*	-0.780**	-0.782**	-0.761*
	(0.299)	(0.295)	(0.295)	(0.294)
Event × Ownership	0.405	0.472+	0.465+	0.455
	(0.273)	(0.277)	(0.277)	(0.276)
Ownership	0.018	0.026	0.027	
	(0.034)	(0.035)	(0.035)	
Abnormal Returns (t–1)		0.005	-0.0004	-0.027
		(0.034)	(0.033)	(0.034)
(Intercept)	-0.039	-0.041		
	(0.039)	(0.040)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	10455	9890	9890	9890
R2	0.001	0.001	0.007	0.035
R2 Adj.	0.0007	0.001	0.005	0.008

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table E.2: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Discrete ownership measure

	(1)	(2)	(3)	(4)
Event	-0.828**	-0.939**	-0.942**	-0.919**
	(0.308)	(0.301)	(0.301)	(0.300)
Event × Wholly-owned Subsidiary	1.205*	1.320*	1.320*	1.299*
	(0.571)	(0.570)	(0.571)	(0.570)
Event × Non-wholly-owned Subsidiary	0.584	0.696	0.682	0.665
	(0.553)	(0.562)	(0.563)	(0.562)
Wholly-owned Subsidiary	-0.009	-0.012	-0.067	
	(0.061)	(0.063)	(0.051)	
Non-wholly-owned Subsidiary	0.043	0.062	0.081	
	(0.070)	(0.072)	(0.074)	
Abnormal Returns (t–1)		0.005	-0.0003	-0.027
		(0.034)	(0.033)	(0.034)
(Intercept)	-0.034	-0.034		
	(0.041)	(0.042)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	10455	9890	9890	9890
R2	0.002	0.002	0.008	0.035
R2 Adj.	0.001	0.001	0.005	0.009

+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Figure E.1: Marginal effects of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on the degree of ownership by the company of the subsidiary. Results from linear interaction models with continuous and categorical measures of ownership (models 4 in Tables E.1 and E.2)