The Shield of Ownership: The Limits of Market Sanctions Against Corporate Misconduct[*]

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Abstract

Companies' complex ownership structures complicate the regulation of multinationals and regulators often rely on financial markets to generate penalties for companies responsible of corporate malfeasance. But does fragmented ownership also obviate the financial consequences of corporate misconduct? I argue that investors only penalize a parent company when it is directly responsible for wrongdoing. By contrast, subsidiary misconduct results in no such market penalties for parent companies. To test the observable implications of the argument, I leverage unexpected revelations of corporate corruption on large, market-traded firms in an event study design for causal inference. When the parent company is directly involved in a scandal, it incurs an average loss of more than $1 billion in capitalization in the two days following the corruption revelations. After 20 days, this effect cumulates to an average of more than $4.5 billion in losses. However, on average, parent companies do not incur any stock price losses when regulators only identify their subsidiary as guilty of corruption. The findings suggest regulatory failure, because companies can protect themselves from market responses to misconduct by engaging in moral hazard via complex ownership structures.

Keywords: Multinational companies; Corporate misconduct; Corporate regulation; Event study

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1 Introduction

Breaking news often report examples of corporate malfeasance. In 2016 and 2021, the Panama and Pandora Papers unveiled international schemes of corporate tax evasion and money laundering. The 2015 "Dieselgate" scandal revealed car manufacturer Volkswagen had falsified some of its vehicles' true CO2 emission levels. Corporate wrongdoing is no recent phenomenon, though. In 1975, the US Senate-lead Watergate investigation discovered that the weapon producer Lockheed had been bribing government officials in US allies (West Germany, Italy, Japan, Saudi Arabia, and The Netherlands) since at least the 1950s in order to sell aircrafts. How can similar corporate malfeasance be prevented?

Companies' complex ownership structures complicate states' regulation and prevention of corporate wrongdoings. Firms can use layers of subsidiaries or global supply chains to further nefarious transactions like bribery (Malesky, Gueorguiev and Jensen, 2015), money laundering (Cooley and Sharman, 2017), or to violate labor rights (Mosley, 2017). Moreover, companies can purpose the very structures used to further corporate wrongdoing to evade state laws (Arel-Bundock, 2017; Chapman, Jensen, Malesky and Wolford, 2021; Findley, Nielson and Sharman, 2015). As a result, public demands for regulatory measures that address corporate misconduct (Culpepper, Jung and Lee, 2022) are often not met.

Effectively, state regulators often sanction corporate malfeasance indirectly: by leveraging negative market responses that follow news about investigations into firms' misconduct. Similarly to the "market enforcement" mechanism in the context of blacklisting (Morse, 2019), stock markets respond negatively to news on companies' wrongdoing (Breitinger and Bonardi, 2019). In the context of corporate regulation, such market penalties complement those imposed by states: following authorities' announcement of legal actions, traded firms suffer negative stock market shocks that effectively sanction misconduct (Sampath, Gardberg and Rahman, 2018). In turn, regulators mitigate the severity of their law-enforcement action, compromising with corporate wrongdoers in order to avoid imposing a "death sentence" on an enterprise (Garrett, 2014). However, corporate wrongdoers fragment structures across subsidiaries to conceal misconduct (Sharman, 2010). It is unclear whether such fragmentation also conceals misconduct from the eyes of investors, thus countering market-imposed sanctions. Under what conditions do markets generate penalties against corporate malfeasance?

I argue that fragmentation of ownership insulates a company from negative financial consequences generated by news of misconduct. This is due to investors' profit-seeking rationale. When state authorities enforce corporate 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. Because the 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 prospective shareholders direct their purchases towards safer assets. The result is a reduction in price that causes the company to experience *abnormal* financial penalties, *i.e.*

losses that it would not have faced, had law-enforcement not taken place.

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 legal action indirectly—*e.g.*, 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 misconduct.

I rely on an event study to test my argument. The design identifies the effect of unexpected events on companies' daily stock prices, by imputing synthetic counterfactual observations. I rely on a machine-learning procedure to estimate precise counterfactuals (see Wilf, 2016). I adopt this design to study the heterogeneous effects generated by sudden information about corporate wrongdoings 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 size of sanctions imposed by financial markets.

I apply this design in the case of state investigations for violations of the US anti-bribery law. I construct a novel dataset reporting the day state authorities first alleged that publicly-traded companies violated US anti-corruption[1] regulations. My dataset contains information on 217 distinct publicly-traded companies involved in 263 corruption scandals. For each event in the dataset, I code the position of the responsible entity in its corporate group. I combine this dataset with 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. The average company experiences more than $1 billion abnormal losses per day (in terms of market capitalization) for two trading days after the event. Even two weeks later, cumulative returns to the average company involved directly in scandals are still more than $4.5 billion lower what could have been expected before the event. That is, market responses do complement states' regulatory functions, impose strong and sustained penalties, and stick to a company's financials 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 only be used to further and conceal financial misconduct (Findley, Nielson and Sharman, 2015). Nor it is just

[1]In the article I use the terms "corruption" and "bribery" (as well as "anti-corruption" and "anti-bribery") indistinctly. This is because the type of corruption that I focus on—corporate corruption—is typically a transaction where a company offers a bribe payment to a public official. The law that I empirically study sanctions precisely this type of transactions. However, I caution the reader that, in other contexts, the meanings of bribery and corruption might not overlap completely.

a way to arbitrage regulations aimed at preventing it (Chapman, Jensen, Malesky and Wolford, 2021). 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 the regulatory strategy to leverage market responses as a fine (Garrett, 2014). In particular, results show the possibility of moral hazard by firms, who can deliberately engage in illicit activities through their subsidiaries and claim plausible deniability while knowing that stock markets' penalties will be limited.

The paper advances three scholarly contributions. First, it contributes to the vast literature in political science about anti-corruption policies. For the largest part, this literature has studied policies targeted at the demand-side of corruption—*e.g.*, bribe-taking—such as limits to campaign contributions (Gulzar, Rueda and Ruiz, 2022), anti-corruption campaigns (Cheeseman and Peiffer, 2022), or oversight of bureaucrats (Brierley, 2020; Buntaine and Daniels, 2020). Only a smaller (albeit growing) literature has investigated anti-corruption policies targeting the supply-side of the phenomenon—*e.g.*, bribe-paying—typically focusing on grand corruption (see Brazys and Kotsadam, 2020; Chapman, Jensen, Malesky and Wolford, 2021; Jensen and Malesky, 2018). I offer to this growing body of research a study that, first, maps the extent to which markets sanction companies' supply of corruption in international business.

Second, the paper questions whether market sanctions align with and complement state-based corporate regulations. Political science has long discussed whether state regulations or market mechanisms are more effective at holding global firms accountable (Johns, Pelc and Wellhausen, 2019; Morse, 2022; Ruggie, 2002; Strange, 1996; Vogel, 2008). Regulation of corporations can be effective when harmonized under international agreements (Crippa, 2023; Jensen and Malesky, 2018) but states' regulatory capacity is hindered by their reluctance to delegate sovereign powers to international bodies (Green and Colgan, 2013) and by collective action problems (Simmons, 2010). Market sanctions could generate significant pressure on companies (Morse, 2019), prompting them to self-regulatory initiatives that complement (Thrall, 2021) or even substitute (Malhotra, Monin and Tomz, 2019) for state action. Markets would thus name-and-shame corporations, the same way as civil society does (Acemoglu and Robinson, 2020; Fukuyama, 2016; Simmons, 2000). I show that this logic fails when corporate wrongdoers successfully outsource misconduct to separate entities.

Finally, the paper contributes to an international relations literature that looks at information and reputation as powerful devices to ensure compliance with international regimes (Simmons, 1998, 2000; Weisiger and Yarhi-Milo, 2015). Corporate reputation is presumed to induce respect of international norms (Ruggie, 2018) when companies are directly responsible for compliance or defection (Findley, Nielson and Sharman, 2015; Jensen and Malesky, 2018). 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 outsourced within 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 enforcement of corporate norms heavily relies on market mechanisms. For instance, the US Securities and Exchange Commission will soon mandate US-listed companies to disclose their environmental impact, including emissions along supply and ownership chains.[2] My findings question whether, in this and similar cases, investors will use information on behaviors occurring deep inside a corporate group to punish corporate wrongdoing.

2 How subsidiary incorporation moderates market penalties

A company suffers significant financial losses when it is involved in negative news (Breitinger and Bonardi, 2019). The market price of its equities reflects investors' current expectations on the profitability of holding shares (Fama, 1970). Corporations who trade their shares on stock exchanges divide profits ("dividends") among their shareholders on a rolling basis—usually, quarterly. Investors sell and purchase shares as a function of expectations of dividend repayment and stock price movements themselves. All else equal, negative publicity resulting from socially irresponsible corporate behavior undermines such expectation and leads to financial losses (Capelle-Blancard and Petit, 2019). For instance, Kreitmeir, Lane and Raschky (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 corporate wrongdoing, it is argued, negative reputational effects are compounded by strictly material concerns. When public authorities announce investigations on a companies' alleged misconduct, stockholders restructure their portfolios out of concerns about potentially poor future economic performances. Financial misconduct can introduce uncertainty that weakens prospects of profits (Ades and Di Tella, 1999; Alexander, 1999; Lambsdorff, 2007). Moreover, news of legal actions create expectations of costs, including fines and monetary settlement with public authorities (Garrett, 2014). In extreme cases, corporate fraud can even be deliberately exercised at the expense of investors.[3] As a result of these pressures it is estimated that only 20% of every dollar lost by a company for financial fraud comes from penalties imposed by regulators while the rest is due to stock price movements (Sampath, Gardberg and Rahman, 2018).

Effectively, shareholders thus complement state-based regulation by performing a function of scrutiny similar to that of civil society actors (Acemoglu and Robinson, 2020; Fukuyama, 2016). Similarly to consumers' "buycott" actions (Endres and Panagopoulos, 2017), investors' market choices could deter firms from misbehaving. Public authorities, on the other hand, *de facto* delegate part of the process of sanctioning corporate wrongdoing to markets themselves (Garrett, 2014).

[2] 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.

[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.

However, companies commit wrongdoing by splitting illicit transactions across complex layers of subsidiaries and anonymous shell companies. Similar structures can be used to pay bribes, finance terrorism, and launder illicit finance (Findley, Nielson and Sharman, 2015; Malesky, Gueorguiev and Jensen, 2015; Shelley, 2014). Corporate structures can be extremely complex. 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, 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, as in joint ventures.

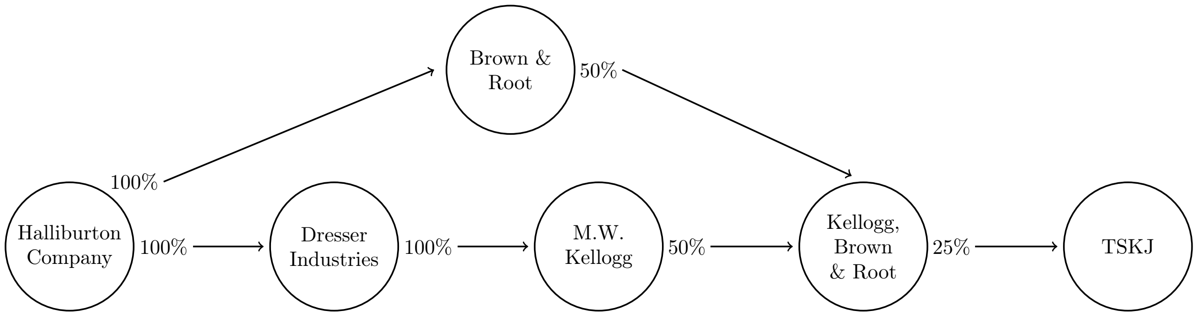


Figure 1: Halliburton Company's stakes in the TSKJ joint venture. Circles represent companies, arrows indicate ownership relations, and percentages represent degrees of ownership.

Figure 1 offers a real example by reconstructing the stakes held by the US extractive company Halliburton in a consortium called TSKJ, a Portuguese joint venture in the oil services industry. The company was formed by the French Technip S.A., the Italian Snamprogetti B.V. (incorporated in The Netherlands), the US Kellogg Brown & Root (KBR), and the Japanese JGC Corporation. Each company owned 25% of TSKJ's shares. Halliburton held stakes in the consortium indirectly 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 nefarious transactions: TSKJ itself became infamous for funnelling hundreds of million US dollars in bribes to Nigerian public officials until at least 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 wrongdoing to public prosecutors: it also gets in the way of the process by which markets sanction malfeasance. I identify three possible rationales that lead subsidiary incorporation to moderate the size of market sanctions against corporate wrongdoing. First, subsidiary incorporation can generate information asymmetry between the corporate wrongdoer and its shareholders. Second, subsidiary incorporation can enable parents to claim plausible deniability for the misconduct. Third, subsidiary incorporation can appear like an efficient way to manage illicit activities in the eyes of shareholders.

First, subsidiary incorporation can create information asymmetry between investors and the corporate group involved in wrongdoings. Investors might be unaware that a company they own equity of—say, Halliburton—owns any stakes in another company involved in misconduct—say, the TSKJ consortium. Therefore, they might fail at negatively updating their expected payoffs when the subsidiary is involved in a law enforcement case. This is consistent with evidence showing that investors often have short time horizons and limited information to conduct investment choices. Under such conditions, they exhibit "bounded rationality" and rely on available information to make sound enough investment decisions (Brooks, Cunha and Mosley, 2015). Under conditions of information scarcity, adverse selection between a parent company and its shareholders thus insulates the firm from negative financial effects deriving from corporate misconduct involving its subsidiaries.

Alternatively, shareholders of a parent firm might be aware of ownership linkages with a subsidiary under investigation, but they might believe the parent can plausibly deny implication in the misconduct. As a result, investors might rationally evaluate that their expected profits are not directly at stake because the parent is overall conducting business operations above board. Anecdotes indicate that this might be happening. In the wake of breaking news about a subsidiary's misconduct, parents often signal their distance from it as a way to reassure stakeholders about their innocence.[4] Because subsidiaries are distinct legal entities from parents, they can be subject to investigations that end up not involving the parent at all (Garrett, 2014). In some instances, they might be themselves publicly traded and have their own shareholders who dividends should be repaid to. Under such conditions, investors might evaluate that the activities of a subsidiary are distinct from those of its parent and fail to update their pricing of the parent's stocks as a consequence of corporate wrongdoing through a subsidiary.[5]

Finally, investors might know about the linkages between a misbehaving subsidiary and its parent company *and* they might find the parent's denial of responsibility implausible. Yet, they might not update their pricing of the parent's stocks because they evaluate that conducting illicit activities via a subsidiary is the most profitable way to do so. Corporate corruption offers perhaps the most intuitive example of this logic. Bribery can represent a viable way for firms to access foreign markets, obtain public contracts, and even derive rents that could not be extracted otherwise (Malesky, Gueorguiev and Jensen, 2015; Zhu, 2017). From this point of view, corruption can be welfare-enhancing for investors because it increases prospects of profits, hence dividends. In the case of similar transactions shareholders

[4]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 wrongdoing (Sharman, 2010) and that news of fraud at a minimum imply inefficiency of compliance programs the parent company should implement (Demsetz and Lehn, 1985).

might evaluate that, if a firm is to pay bribes, it better do so via a subsidiary so as to remain protected from legal actions.

Based on this logic, I expect that parent company's stock prices suffer from news of law enforcement when a firm is directly implicated in the wrongdoing. Conversely, a company involved in a scandal only indirectly—*i.e.*, through a subsidiary—will not suffer penalties on stock markets. When compared to a case of direct involvement, corporate ownership therefore *insulates* the parent company from the financial consequences of 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 overseas business. The Act is 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; Kaczmarek and Newman, 2011; Leibold, 2014). As a result, the FCPA *de facto* applies against misconduct from any US company *and* any non-US company trading on US stock markets 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.[6] However, very rarely companies alleged of FCPA violations go to court. The long time frame of trials would expose companies to prolonged negative publicity. 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).[7]

Usually, agencies communicate to the public about investigations through press releases[8] only after allegations have been settled. Information that FCPA investigations have opened is usually first 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 investiga-

[6] 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.

[7] 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, 2014).

[8] 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.

tions 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,[9] a public platform designed to facilitate information flow from companies to (prospective) investors. In the vast majority of cases, firms inform their investors about FCPA investigations by filing SEC disclosure forms.[10]

Two reasons make the case ideal for comparing the effects of unexpected news about corporate misbehavior 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. The US system thus incorporates a consistent flow of information from the firm to the market. Similar arrangements are not in place in other legal systems. By focusing on violations of 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]

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. To retrieve this information I draw on the dataset on anti-bribery cases 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

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

[10]As an example, see this summary of FCPA investigation disclosure produced by the FCPA blog, an FCPA-related forum published by corporate lawyers and anti-corruption practitioners: https://fcpablog.com/2018/09/12/how-are-fcpa-investigations-disclosed/.

[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.

summarising events of cross-border corporate corruption in violation of the international anti-bribery regime, and related law enforcement actions. It includes the universe of FCPA enforcement actions or investigations so far.

From this dataset, I keep only investigations initiated by US agencies (SEC or DOJ) under terms of the 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. This excludes just 8 events from 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. I measure whether each company found in violation of the US anti-corruption law is the corporate group's global ultimate parent ($Indirect = 0$), or a subsidiary[15] ($Indirect = 1$). 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 ($Indirect = 0$).

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.[16] 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 263 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.[17] 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] 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).

[17] 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**	**Indirect**	**Ticker**	**Violation country**	**Investigation**
BHP Billiton	BHP Billiton	0	BHP	China	2010-09-21
ENI SpA	ENI SpA	0	E	Lybia	2013-05-03
ENI SpA	Snamprogetti B.V.	1	E	Nigeria	2004-10-05
ENI SpA	SAIPEM SpA	1	E	Algeria	2014-04-10
Raytheon Company	Thales-Raytheon Systems Company LLC	1	RTN	Middle East	2020-02-12
Royal Dutch Shell PLC	Royal Dutch Shell PLC	0	SHEL	Nigeria	2008-03-17
Royal Dutch Shell PLC	Shell Nigeria EPCO LTD	1	SHEL	Nigeria	2016-03-10
Novo Nordisk A/S	Novo Nordisk A/S	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*).[18] I report the parent company of the involved entity (*Parent Company*), whether the parent was involved in the scandal indirectly (*Indirect*), 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. 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.[19]

5 Research design

I adopt an event-study research design to test my expectations that parents suffer financial penalties when they are directly implicated in a regulatory enforcement action, but not when they violate corporate laws through subsidiaries. This empirical strategy is used for estimating market effects of unexpected events (Karpoff, Lee and Martin, 2008). Political scientists have used it to assess the effect of international institutions and regulations (Gray, 2009; Wilf, 2016), communications (Genovese, 2021), elections (Aklin, 2018), and international rulings (Bechtel and Schneider, 2010; Kucik and Pelc, 2016).

The design imputes daily synthetic counterfactual *Returns* to each company around an event of

[18] 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.

[19] 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).

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 (61 days per event). The estimation window of each company begins 210 days before the beginning of its event window.[20]

In the estimation window, I estimate one market-model for each event that explains 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 ($\mathbf{X_t}$).

For each event, I model *Returns* employing only the most predictive market-wide indexes, selected via a LASSO procedure. This is done to return more precise counterfactuals over those that would be imputed with models that include all market-wide indexes—such as using ordinary least squares (OLS). The LASSO associates sets of non-negative weights to each variable in the matrix of covariates. 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). Previous event studies have shown its improved performance over OLS market models (Wilf, 2016). In my LASSO estimation, I adopt a five-fold cross-validation procedure for learning the set of most predictive weights specific to each individual event e. I then employ these weights to determine how covariates enter Equation 1 for that specific event. Each model thus represents the best feasible predictor of a company's stock *Returns*, before the unexpected event took place.

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

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[21] (RMSEs) with values smaller than 0.20, with the bulk of models yielding a value of just 0.10. Models perform well also in terms of R-squared: the majority explain at least half of the variation of *Returns* in the estimation window (Figure A.2).

Next, I use market-model coefficients estimated in the estimation window to predict daily *Returns*

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

[21] 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: $Normal\ RMSE_e = RMSE_e / [max_e(y_t) - min_e(y_t)]$.

to each company in the event window (from t_1 to t_2). Equation 2 represents this second step. *Expected Returns* are effectively counterfactual *Returns* to a company, absent news of malfeasance: they are estimated based on information available before news of misconduct were unexpectedly released. I also compute daily *Cumulative Observed* and *Cumulative Expected Returns* by summing, respectively, daily *Returns* and *Expected Returns* relative to a specific event.

$$\textit{Expected Returns} = \hat{\alpha}_e + \mathbf{X_t'} \boldsymbol{w_e} \hat{\boldsymbol{\beta}}_e \;\mid\; t_1 \leq t \leq t_2 \tag{2}$$

Figure 2: 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

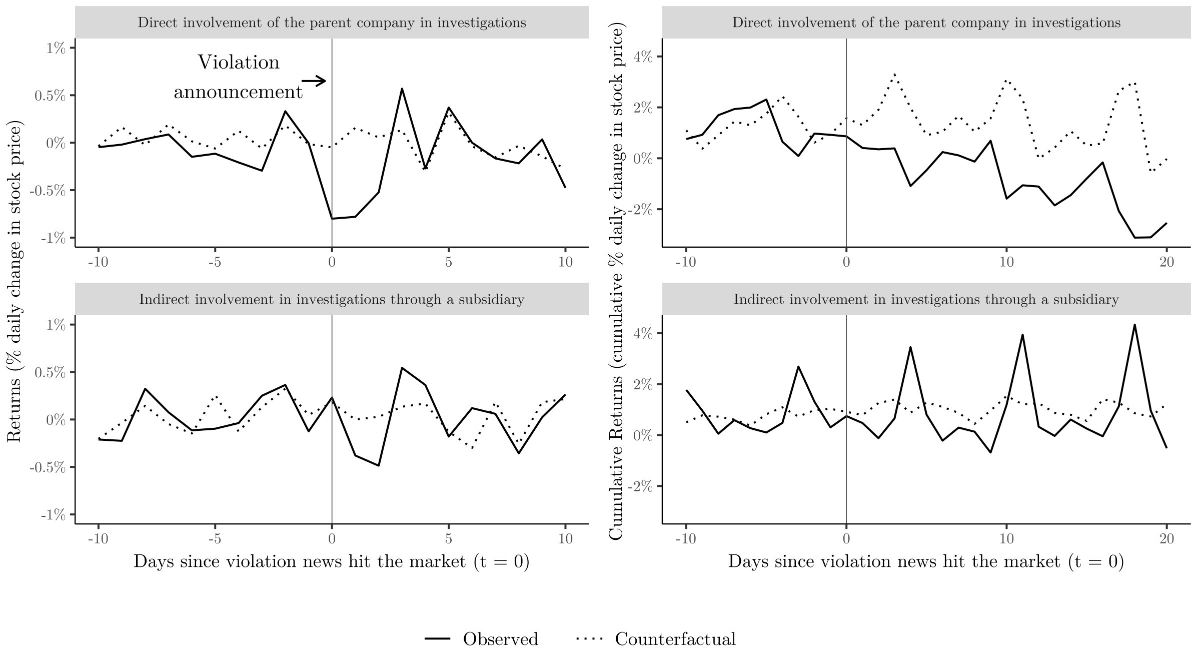


Figure 2 plots the daily average observed and counterfactual *Returns* (left panels) and *Cumulative Returns* (right panels) in the days before and after the *Investigation*, distinguishing between cases of direct involvement in investigations (top panels) from those where involvement happened through 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 (*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 study that includes a categorical variable for the number of days separating each observation from the day of the *Investigation* (0) using day -1 as a baseline. Pre-*Investigation* coefficients quantify average pre-treatment differences in daily values of the dependent variable from the baseline day. Post-*Investigation* coefficients, instead, quantify differences in daily (*Cumulative*) *Abnormal Returns* following the event, thus returning dynamic ATT estimates. The models include an event-fixed effect to completely remove between-event heterogeneity and are estimated using OLS. All standard errors are clustered at the parent company-level.

I perform my event study after subsetting my sample between cases of direct ($Indirect = 0$) and indirect involvement in FCPA investigations ($Indirect = 1$). Thus, I identify the effect of FCPA investigations on involved parent companies' stock prices in the two scenarios. But how comparable are these cases? In appendix I show that, at least when looking at 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 features like the number of foreign countries involved, the size of the parent, the value of its stocks, the level of corruption of the violation country (Table B.1), or the distribution of headquarter countries and industries (Figures B.1 and B.2).

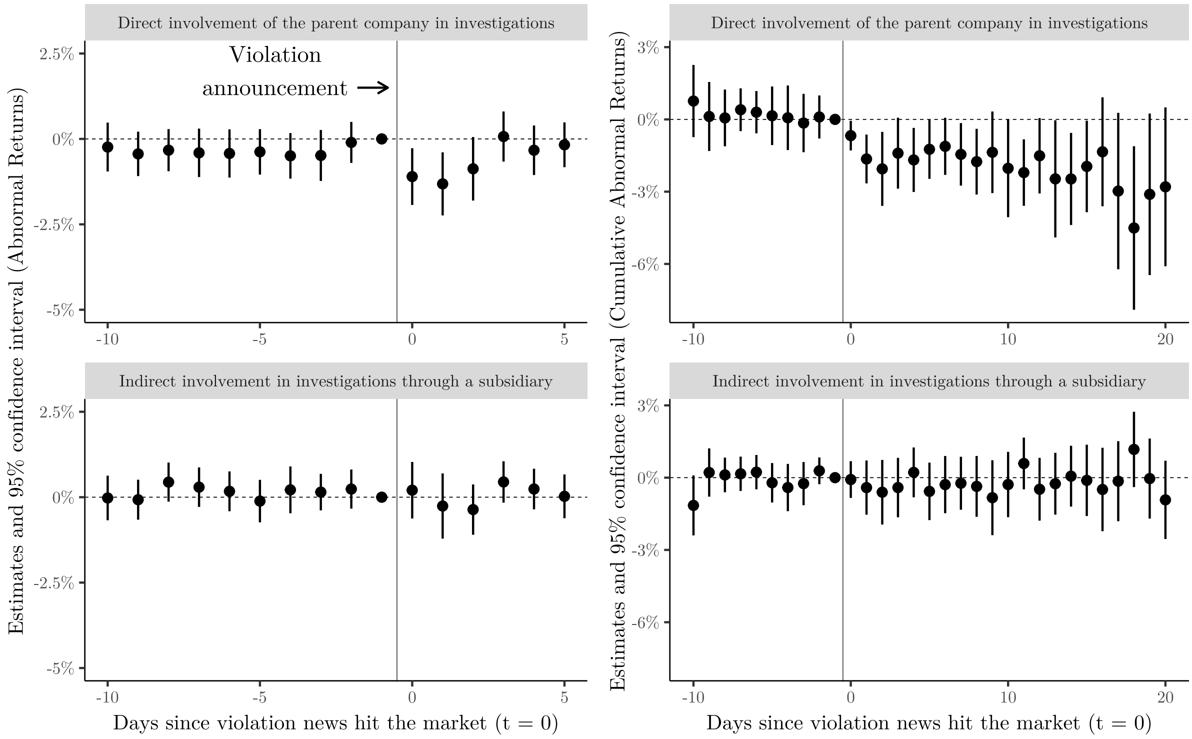
6 Results

Figure 3 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*).[22] 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*.

In all panels, 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

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

Figure 3: Event study 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



trading day of the *Investigation* and the following (days 0 and 1) when a company is directly involved in an investigation (top-left panel). The effect is also almost significant on day 2 ($p = 0.065$). On each of these 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), where post-treatment point-estimates are small and statistically insignificant.

Do short-term penalties 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. Still on day 18, involved companies experience abnormal cumulative returns that were 4.5% lower what could have been expected before the scandal. Instead, no significant cumulative loss on the parent company's stock returns is detected when the firm is involved in investigations through a subsidiary (bottom-right panel).

How sizeable are direct-involvement penalties? The average company involved directly in an investigation 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 $4.5 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 solely on the day of the investigation (0 otherwise) with the *Indirect* indicator. Table 2 reports my main results. In model 1, I introduce only my variables of interest. In following models, I progressively introduce additional control variables: a one-day lag of the dependent variable, a year-fixed effect to account for year-specific heterogeneity in FCPA enforcement action, and an event-fixed effect to absorb all between-event heterogeneity.[23]

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 × Indirect	0.914*	1.034*	1.027*	1.007*
	(0.455)	(0.455)	(0.455)	(0.454)
Indirect	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 of the *Event* variable indicates an average 1% abnormal loss in stock value for cases of direct involvement in investigations ($Indirect = 0$). The effect is distinguishable from zero at a 0.05 conventional level of significance. The interaction term $Event \times Indirect$ is positive and similar in magnitude to the previous one: this indicates that the negative effect of the scandal is completely absorbed when a company is involved through a subsidiary. Such moderation is statistically significant at a 0.05 level.

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 D.2 and D.3). Next, I show that findings are not driven by arbitrary choices followed in the procedure. I restrict event windows

[23]Obviously, in the specification that includes event fixed effect, the coefficient for the event-invariant constitutive term of the interaction *Indirect* cannot be estimated due to perfect collinearity.

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 D.1, D.2, and D.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 D.4 and Table D.4). To conclude, I show that similar findings can be obtained even when adopting different research designs. First, I 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 above (Section E). Second, I remove the market-model-imputed synthetic counterfactuals and study observed *Returns* and *Cumulative Returns*. Results are consistent with those presented here (Figure F.3 and Table F.1).

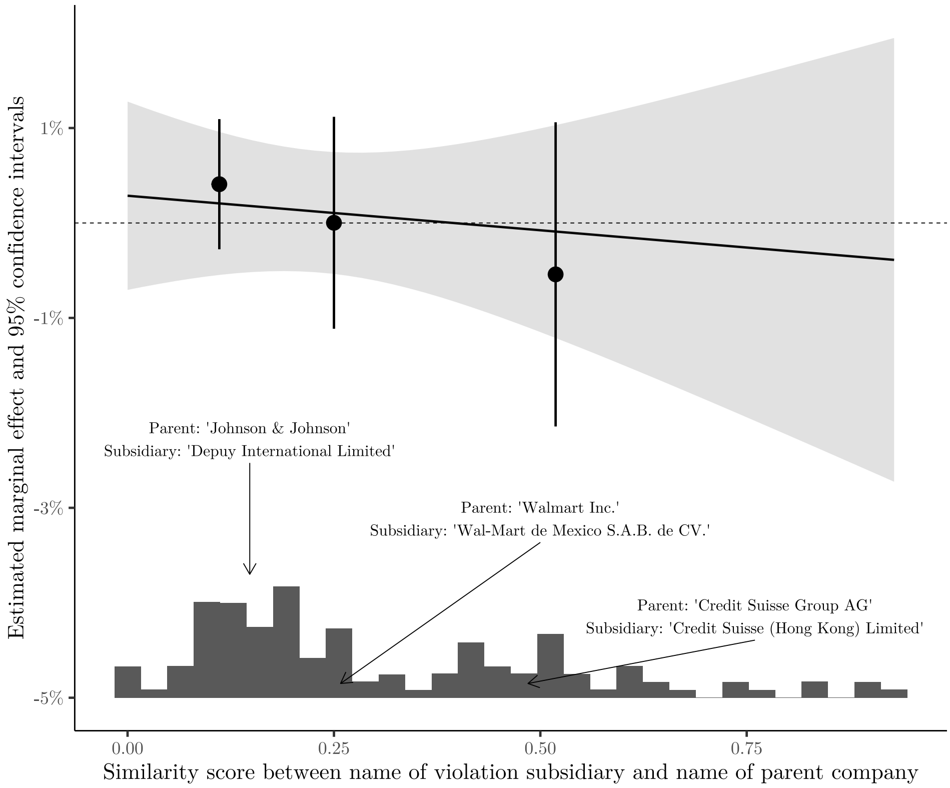
6.1 Name similarity

Are investors and market analysts ignorant of companies' corporate structures, or else do they buy into companies' plausible deniability? In order to provide evidence on the mechanism driving the null-effect, I leverage differences between the names of involved subsidiaries and those of parent companies. Cases of indirect involvement 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.

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,[24] 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 in the binning estimator proposed by Hainmueller, Mummolo and Xu (2019). Figure 4 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.

[24] 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.

Figure 4: 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.



7 Conclusion

Corporate wrongdoers can exploit their fragmented operations in order to conceal malfeasance (Cooley and Sharman, 2017) and evade regulations states cast to prohibit it (Arel-Bundock, 2017; Chapman, Jensen, Malesky and Wolford, 2021). This poses a real threat to an effective limitation of nefarious transactions and questions whether formal regulatory provisions bear any deterrence against corporate misconduct (Findley, Nielson and Sharman, 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, Lane and Raschky, 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 wrongdoers (Garrett, 2014). However, it is not clear whether markets penalize companies for misconduct happening down their ownership chains. This is a relevant gap because fragmented ownership can be purposed precisely to further financial wrongdoing (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 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 wrongdoing hits the markets, due to concerns about the firm's profitability. However, the effect disappears when the company is involved indirectly.

My empirical tests leveraged an original dataset on 263 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 study shows that parent companies suffer a significant abnormal loss of about 1% to their stock returns on the two days following the release of information. This effect amounts to a daily loss of about $1 billion in market value for the average company and cumulates to more than $4.5 billion 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 evidence (Morse, 2019), investors do not seem to bite against parent companies for misconduct 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 corporate wrongdoing (Findley, Nielson and Sharman, 2015), to evade regulations (Chapman, Jensen, Malesky and Wolford, 2021), and to minimize losses on equity markets. This has important implications for debates in governance beyond financial wrongdoing, 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 (Findley, Nielson and Sharman, 2015; 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 misconduct, 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, supply chains, joint ventures, or sub-contracting) 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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Supplementary Information

The Shield of Ownership: The Limits of Market Sanctions Against Corporate Misconducts

A LASSO: Estimation window descriptives

Figure A.1: Heatmap reporting the value of estimated coefficients relative to financial indicators (y-axis) as they enter each of the 263 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.

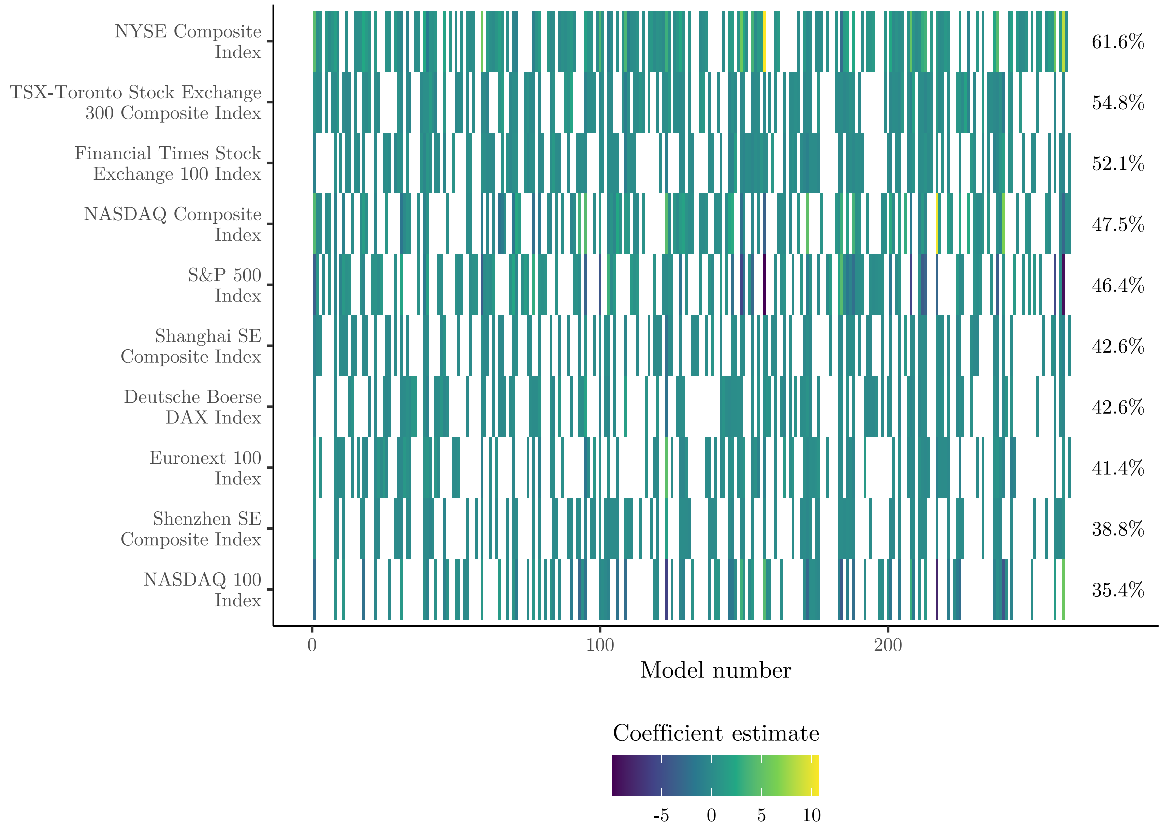
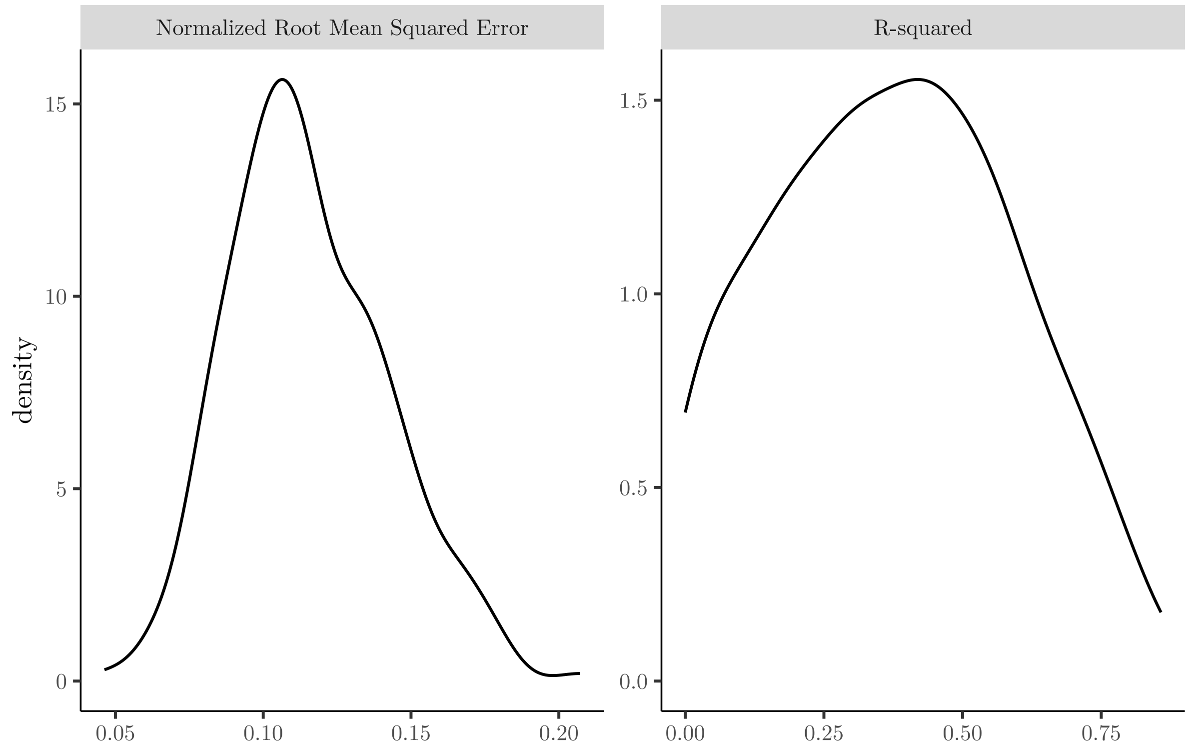


Figure A.2: Distribution of the normalized Root Mean Squared Error (RMSE) and of the R-squared yielded by the 263 market models estimated using the LASSO procedure.



B FCPA cases 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 ($Indirect = 0$) and those where it was indirect ($Indirect = 1$).

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 ($Indirect = 0$) and with indirect involvement ($Indirect = 1$). Pre-treatment covariates only

	Direct involvement (N=143)		Indirect involvement (N=120)			
	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 ($Indirect = 0$) and indirect involvement ($Indirect = 1$).

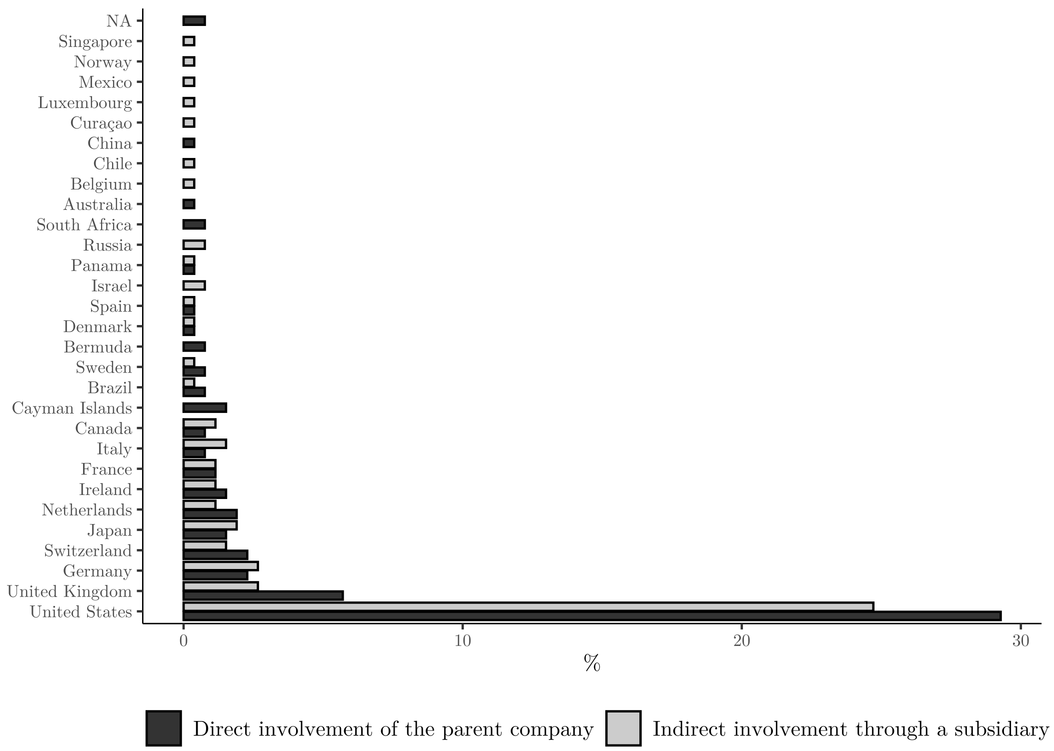
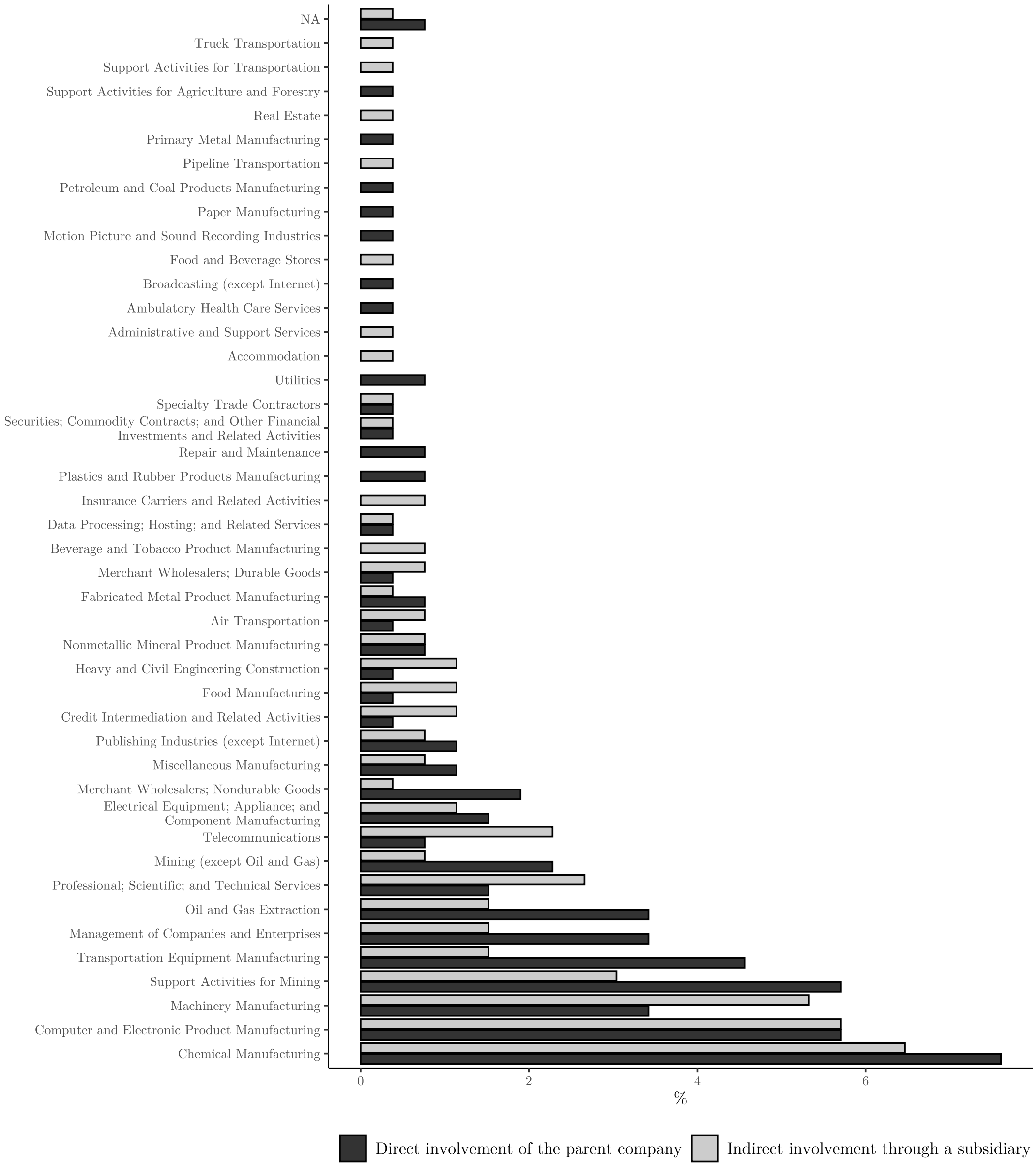
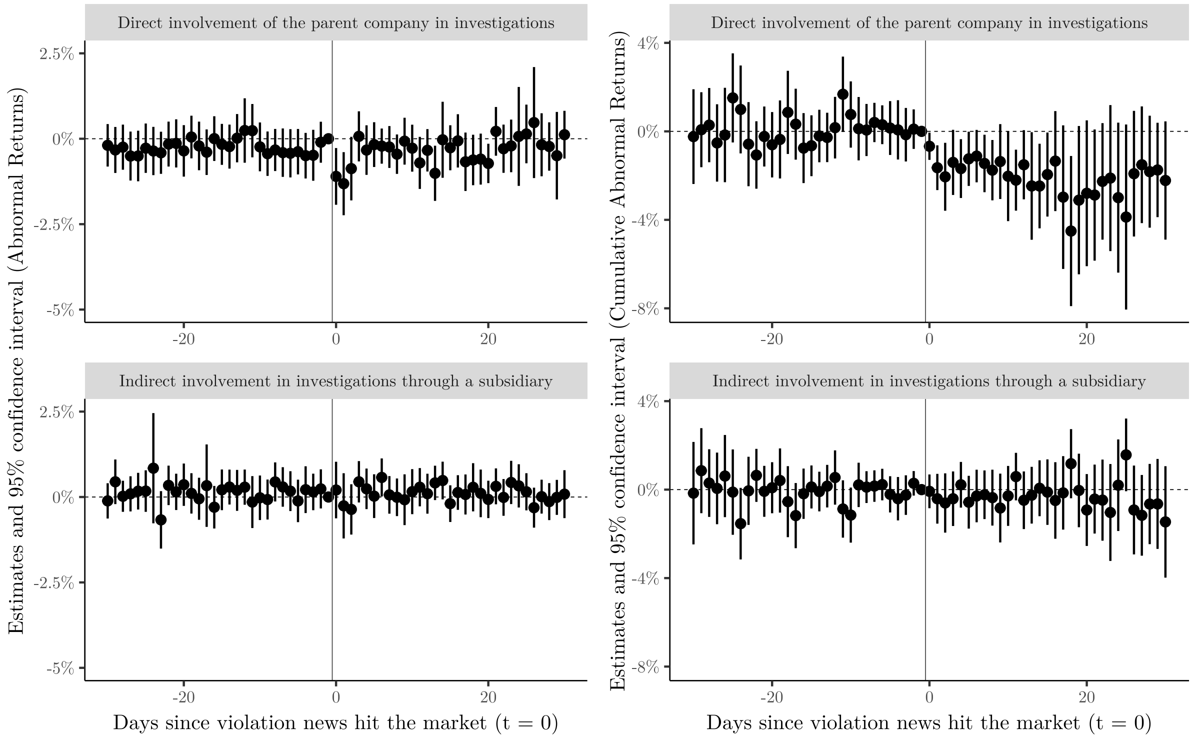


Figure B.2: Proportion of events involving companies by NAICS-3 code, across cases of direct ($Indirect = 0$) and indirect involvement ($Indirect = 1$).



C Event study: full disclosure of results

Figure C.1: Event study 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



D Event study: Robustness tests

D.1 Leave-one-out test

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 study from Figure 3 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 D.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 D.3 reports estimated coefficients for the un-interacted $Investigation$ term and the interaction term $Investigation \times Indirect$, alongside their 95% confidence intervals.

Figure D.2: Event study 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.

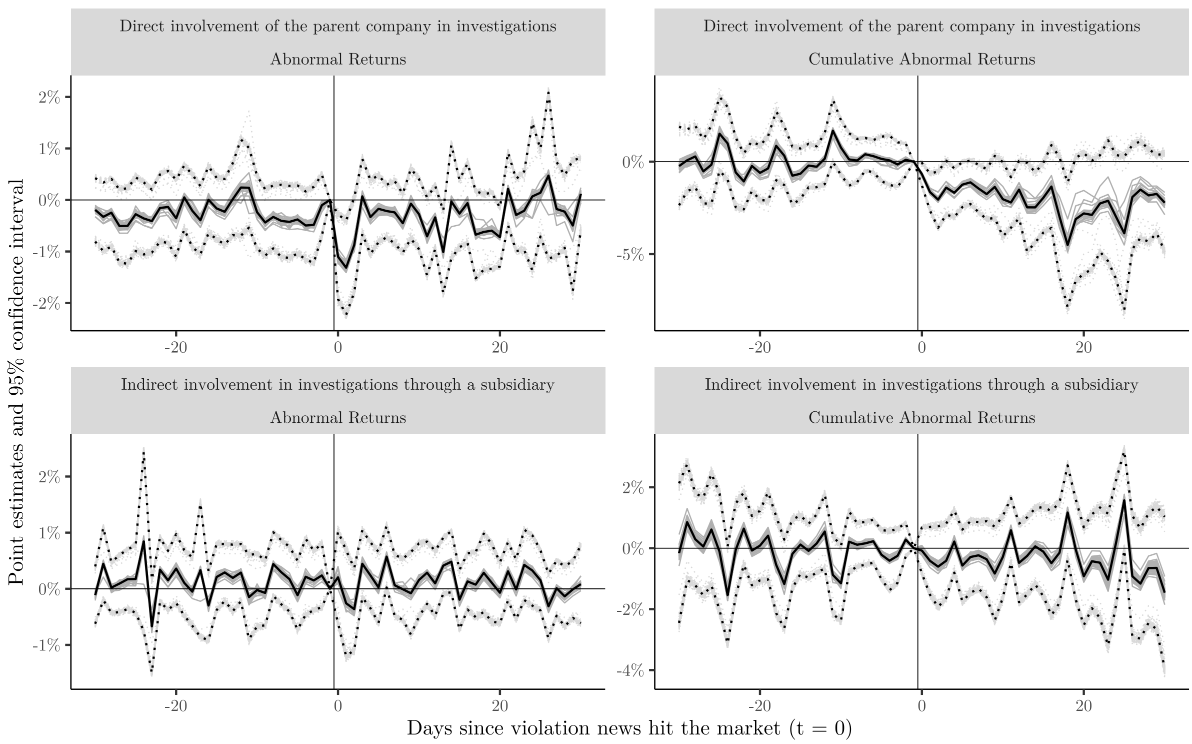
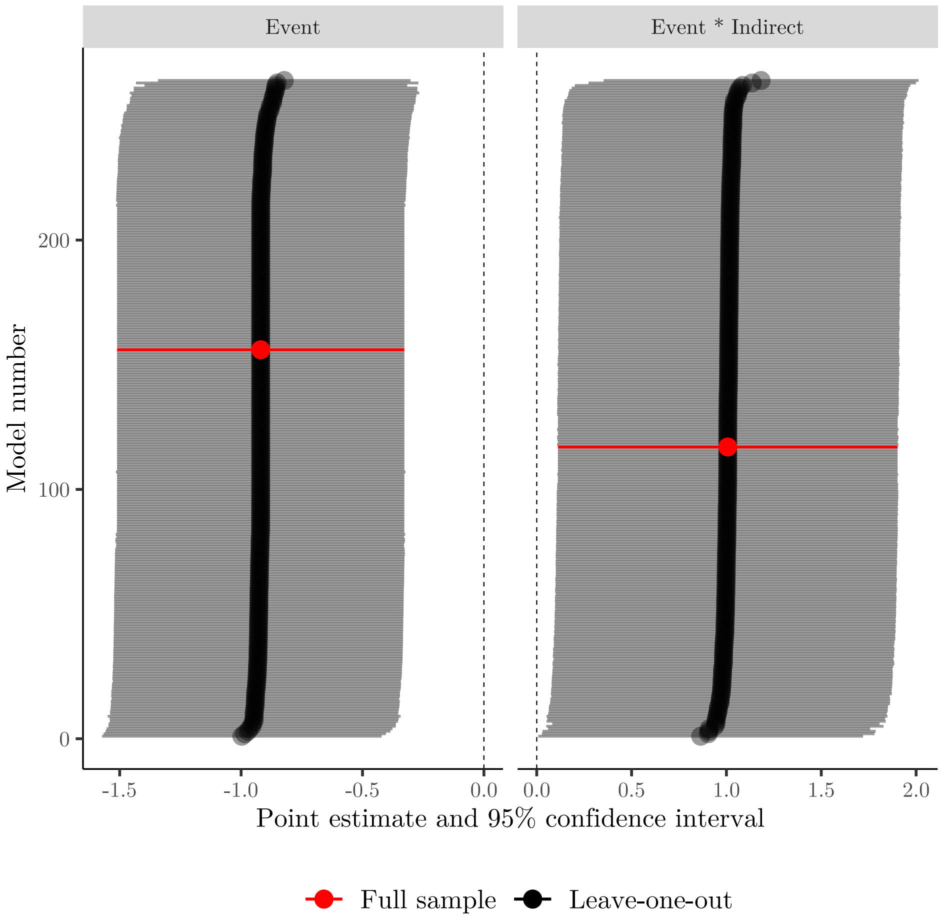


Figure D.3: Replication of model 4 from Table 2, leaving one event out of the dataset at a time. Point estimates and 95% confidence intervals reported refer to the un-interacted *Investigation* term and to the interaction term $Investigation \times Indirect$. Red coefficients represent full-sample estimates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



D.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 D.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 D.2 and D.3).

Table D.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)
Event	-0.670*	-0.780*	-0.774*	-0.619*
	(0.326)	(0.319)	(0.318)	(0.307)
Event × Indirect	0.842+	0.946*	0.941*	0.714
	(0.477)	(0.477)	(0.475)	(0.453)
Indirect	0.087	0.109	0.111	
	(0.140)	(0.141)	(0.138)	
Abnormal Returns (t–1)		0.0003	-0.020	-0.193***
		(0.047)	(0.044)	(0.036)
(Intercept)	-0.191+	-0.192+		
	(0.106)	(0.105)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	1754	1703	1703	1703
R2	0.006	0.008	0.029	0.220
R2 Adj.	0.004	0.006	0.016	0.076

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

Table D.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)
Event	-0.744*	-0.845**	-0.848**	-0.768**
	(0.303)	(0.300)	(0.299)	(0.294)
Event × Indirect	0.848+	0.949*	0.945*	0.860+
	(0.452)	(0.454)	(0.452)	(0.445)
Indirect	0.080	0.100	0.081	
	(0.083)	(0.086)	(0.080)	
Abnormal Returns (t–1)		-0.021	-0.034	-0.108**
		(0.035)	(0.032)	(0.033)
(Intercept)	-0.117+	-0.124+		
	(0.066)	(0.067)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	3606	3494	3494	3494
R2	0.004	0.006	0.021	0.112
R2 Adj.	0.003	0.005	0.015	0.039

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

Table D.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)
Event	-0.789*	-0.903**	-0.909**	-0.777**
	(0.309)	(0.301)	(0.302)	(0.294)
Event × Indirect	0.854+	0.963*	0.966*	0.818+
	(0.455)	(0.452)	(0.452)	(0.450)
Indirect	0.075	0.078	0.045	
	(0.091)	(0.095)	(0.098)	
Abnormal Returns (t–1)		-0.054	-0.066*	-0.164***
		(0.035)	(0.032)	(0.032)
(Intercept)	-0.072	-0.063		
	(0.072)	(0.075)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	1932	1870	1870	1870
R2	0.009	0.014	0.030	0.186
R2 Adj.	0.007	0.012	0.018	0.052

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

D.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 187 companies involved in 229 events. I replicate my entire analysis and verify results are consistent (Figures D.4 and Table D.4). The event study results in noisier estimates, but overall results are in line with previously presented ones.

Figure D.4: Event study 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

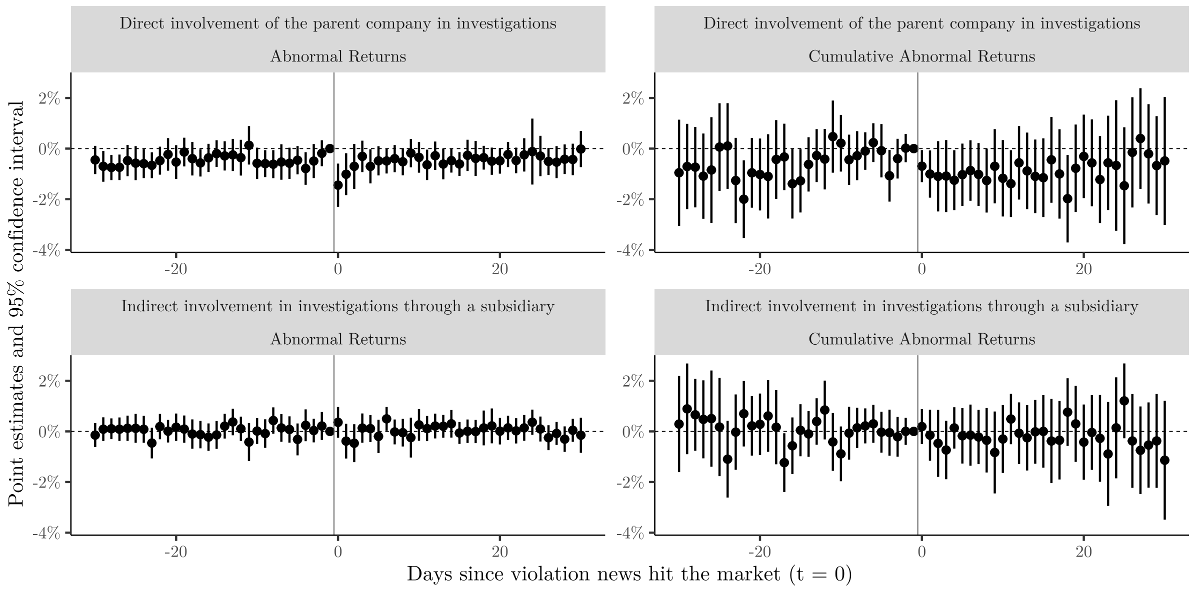


Table D.4: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature. Event window data limited to events with precise counterfactual imputation

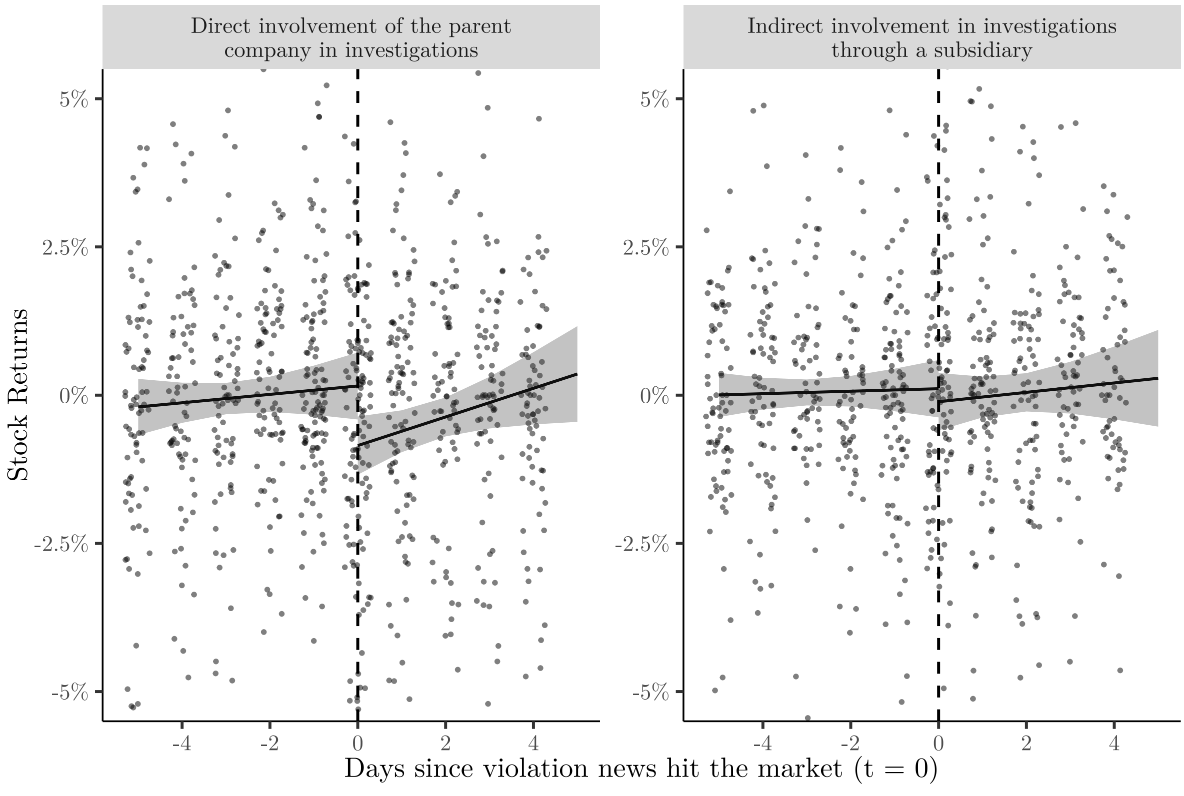
	(1)	(2)	(3)	(4)
Event	-1.000**	-0.983**	-0.983**	-0.960**
	(0.324)	(0.341)	(0.341)	(0.340)
Event × Indirect	1.324**	1.304**	1.299**	1.274**
	(0.434)	(0.450)	(0.450)	(0.450)
Indirect	-0.063	-0.062	-0.078+	
	(0.046)	(0.047)	(0.040)	
Abnormal Returns (t–1)		0.028	0.023	-0.006
		(0.018)	(0.019)	(0.020)
(Intercept)	0.028	0.037		
	(0.033)	(0.033)		
Year FE			Yes	
Event FE				Yes
Num.Obs.	9090	8588	8588	8588
R2	0.004	0.004	0.009	0.039
R2 Adj.	0.003	0.004	0.007	0.012

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

E Time to enforcement as an instrument

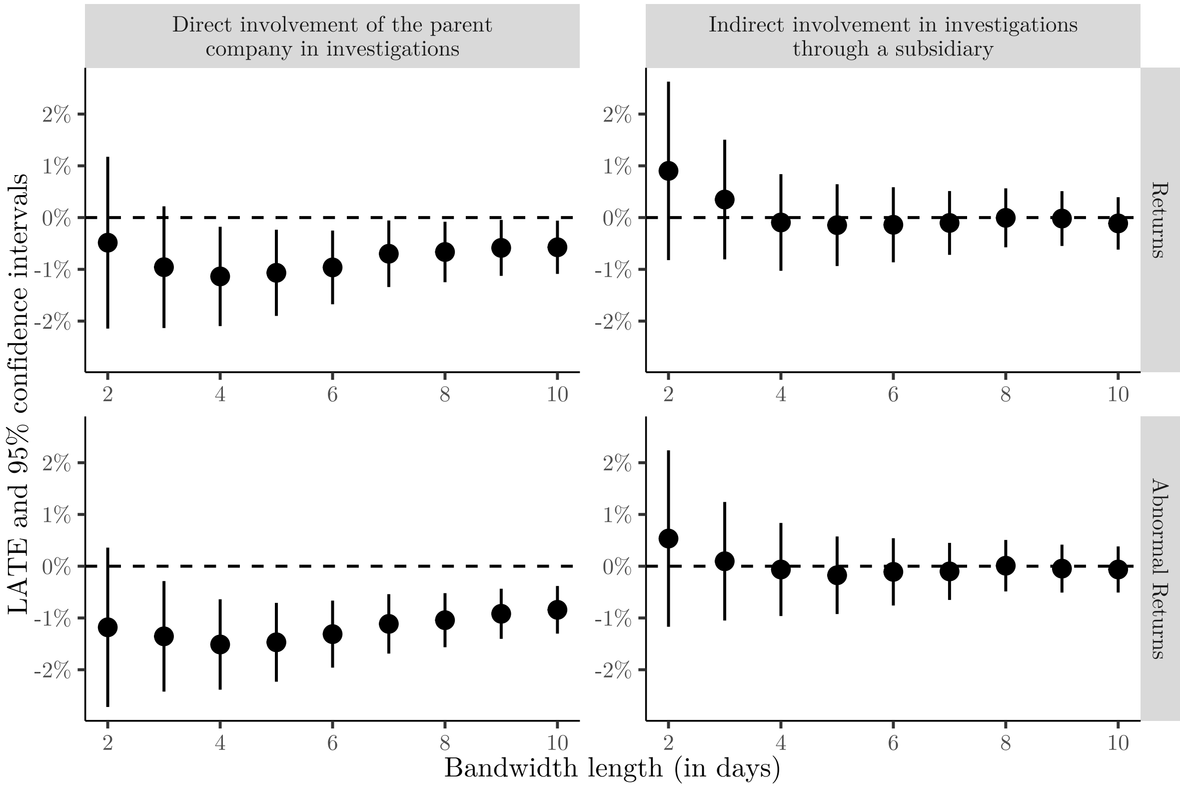
In this section I show that similar results can be estimated when adopting an entirely different identification strategy. Instead of relying on a synthetic counterfactual of stock prices, here I use time-to-the-day of FCPA *Investigation* as an instrument for firms' *Returns*. Assuming that the timing 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 E.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 E.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 E.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. In this test, 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 E.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*



F Non-synthetic counterfactual event study

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 F.3 and Table F.1.

Figure F.3: Event study 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

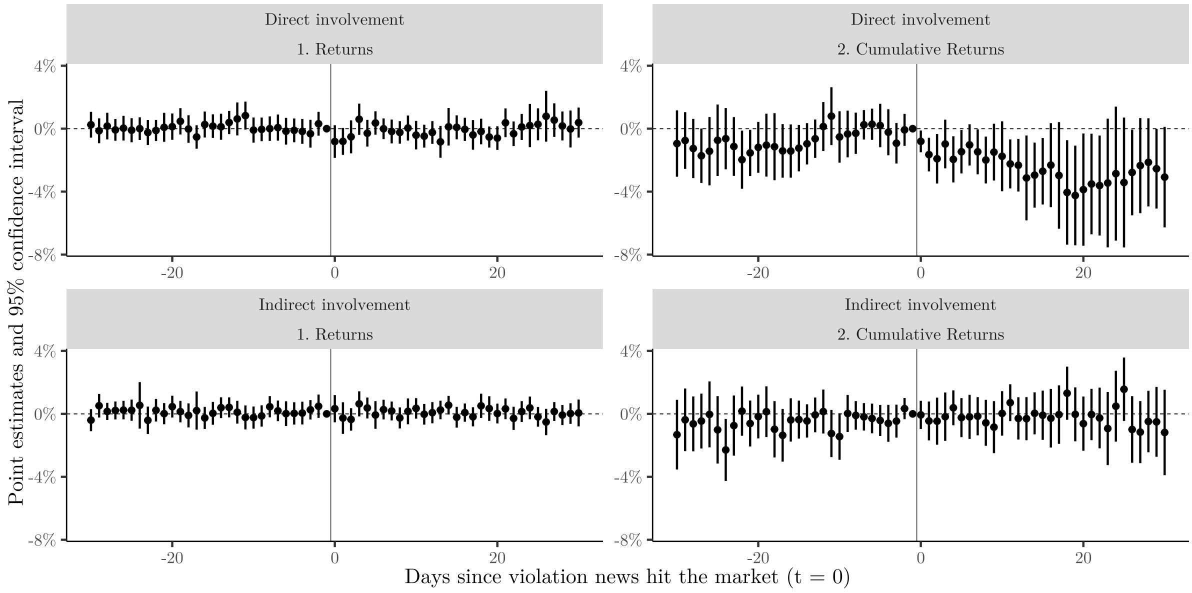


Table F.1: Heterogeneous effects of FCPA investigation on parent companies' stocks, conditional on involved entity nature. Non-discounted Returns

	(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