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

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Abstract

Research shows that, when news of corporate crime emerge, implicated companies' stock prices suffer. Investors thus perform a naming-and-shaming regulatory function against companies' misconduct. However, corporations conceal criminal transactions by fragmenting operations across subsidiaries and shell companies. It is unclear whether fragmented ownership conceals misconduct to investors too. I argue that markets' naming-and-shaming 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 214 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 a significant loss of about $132 million in capitalization on the day of a scandal. However, the effect is null when the company is involved via a subsidiary (whether wholly-owned or majority-owned). Findings indicate a regulatory failure. Fragmentation of corporate ownership can be used not only to conceal criminal transactions, but also to protect a firm's market reputation.

Keywords: Multinational companies; Financial crime; Corporate scandals; Reputation; Event-analysis

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1 Introduction

Preventing multinational corporations' financial crime is a complicated task for states. Multinational corporations (MNCs) operate across borders and jurisdictions with extremely complex legal structures articulated in networks of subsidiaries, joint ventures, and shell companies. The opacity of these networks is conducive to nefarious corporate transactions because it provides ways to conceal them (Sharman, 2010). Foreign subsidiaries can be purposed to engage in criminal activities like bribery in order to secure contracts (Alexander and Cohen, 1999; Malesky et al., 2015). In turn, fragmented ownership can be used to launder revenues from criminal transactions (Cooley and Sharman, 2017; Sharman and Chaikin, 2009). On top of this, companies' fragmented structures even offer them loopholes between regulations aimed at preventing financial crime. Such "regulatory arbitrage" makes it difficult for formal state-based legal means to hold complex corporate networks accountable for illicit transactions like bribery (Chapman et al., 2020), money laundering (Findley et al., 2015), or tax evasion (Arel-Bundock, 2017; Thrall, 2021).

How can states regulate such complex networks and prevent financial crime? Studies indicate that states find a helping-hand to hold large companies accountable for cross-border misconduct in the naming-and-shaming function performed by civil society actors (Acemoglu and Robinson, 2020; Fukuyama, 2016). This is often translated into the argument that financial markets can complement states' regulatory action (Alexander, 1999; Morse, 2022). News that state authorities are investigating alleged criminal behavior of a company should negatively affect its stock prices, thus turning into a sanction (Krüger, 2015). On the one hand markets would behave as a "global civil society" (Ruggie, 2018). On the other, regulators could *de facto* wage sanctions on companies by weaponizing investors' responses[1]. This informal regulatory function would complement institutionalized regulation – potentially even substitute for it, see Kreitmeir et al. (2020) – and deter companies against crime (Alexander and Arlen, 2018; Morse, 2019).

However, the limits of the regulatory power markets leverage through this mechanism are still unclear. Namely, it is not clear whether the naming-and-shaming function performed by markets targets the complexity of structures companies use to further criminal activity. Firms conceal criminal transactions from the eyes of public authorities by fragmenting corporate ownership across subsidiaries and shell companies (Sharman, 2010). We do not know whether fragmentation also conceals criminal conduct from the eyes of market actors, or else whether investors penalize a company for misconduct by its subsidiaries. Filling this gap allows to evaluate the extent to which informal market responses can effectively complement formal regulations against corporate misconduct.

In this paper I theorize that, in fact, fragmentation of ownership insulates a company from damages generated by news of criminal behavior. The literature on penalties for corporate law claims the

[1] This effect is consistent with the "weaponized interdependence" framework proposed by (Farrell and Newman, 2019), insofar as states leverage private economic networks to exercise coercive functions.

mechanism inducing penalties on financial markets is reputation-based (Alexander, 1999; Sampath et al., 2018). When a firm is publicly involved in an unexpected criminal event, investors who own its stocks are concerned prospects of future dividends might be damaged by negative publicity. They therefore decide to sell their equities. Increase in the supply of stocks is also met by a shrinkage in demand, as investors direct their purchases towards safer assets. The result is a reduction in price that causes the company to experience losses that it would not have experienced, had the scandal not emerged.

I claim that this effect depends on where, along the corporate ownership chain, the scandal occurred. It is negative if a parent company is directly involved in a scandal. In this case, the company's reputation is directly at stake and investors restructure their portfolios accordingly. However, the effect intensity decreases if the company is involved in a scandal indirectly, *i.e.* through a subsidiary. In this case, negative publicity does not concern the parent company directly. Markets will therefore struggle to perform their regulatory function when corporate structures obscure ownership. That is, subsidiary companies screen corporate ownership and insulate parent companies from news of a scandal, thereby preventing meaningful financial losses in the wake of breaking stories 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 draw on an original web-scraped dataset on anti-bribery investigations to select companies alleged to violate US anti-corruption law (Crippa, 2021). This yields information on 214 distinct companies involved in 264 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. This effect amounts to a loss of about 0.30$ per share for the median company in my data, totalling about $132 million losses per day in terms of market capitalization. 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 remain about $517 million lower what could be expected had the event not occurred.

That is, markets perform a regulatory function by imposing strong and sustained penalties that stick to a company's reputation when it is involved directly in a scandals. 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. I further disentangle the null-effect relative to indirect involvement and find that no effect is detected for indirect involvement when considering only wholly-owned subsidiaries or only majority-owned ones.

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, see Alexander and Cohen (1999) –, results indicate a 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 of the international economic architecture (see work as diverse as Johns et al., 2019; Morse, 2022; Ruggie, 2002; Strange, 1996). More broadly, 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; 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 hidden inside a corporate group. In this case, corporate ownership works as a shield for the parent company's reputation.

Policy 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[2]. 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]See: https://www.nytimes.com/2022/03/21/business/sec-climate-disclosure-rule.html.

2 Conceptual framework

2.1 Formal regulation of financial crime and informal penalties

Countries have laws in place to prohibit multinational companies under their jurisdiction from committing crime. These laws prosecute corporate criminal behavior such as corruption (Jensen and Malesky, 2018), money laundering (Sharman, 2010), financing of transnational terrorism (Shelley, 2014), or trade with sanctioned countries (Andreas, 2005; Putnam, 2009).

Although these laws include powerful provisions on paper, enforcing them is a challenging endeavor for regulators. The structure of multinational companies poses obstacles to an effective enforcement. Companies can fragment their operations in ways that offer loopholes across overlapping jurisdictions (Arel-Bundock, 2017). For instance, those subject to anti-corruption provisions can outsource bribery to unregulated partners (Chapman et al., 2020; Malesky et al., 2015). Such arbitrage couples with the usage of diluted ownership as a tool to further financial crime. For instance, bribes are often paid through anonymous shell corporations, which can also be purposed for laundering reasons (Findley et al., 2015). Corporate networks can thus be used both to further financial crime and to evade regulation.

A second order of obstacles to enforcement comes from the side of regulators. Prosecution can be subject to political goals of the executive (Gilbert and Sharman, 2016; Tomashevskiy, 2021). Moreover, since corporate crime typically takes place across borders (Cooley and Sharman, 2017), networks of cooperation with foreign authorities have to be established (Kaczmarek and Newman, 2011). Even powerful countries, that can rely on experienced regulators with extensive foreign support, need to leverage economic connections to exercise their regulatory authority (Crasnic et al., 2017; Crippa, 2021; Kalyanpur and Newman, 2019). The result is that, often, formal regulatory provisions remain empty letter (Findley et al., 2015; Garrett, 2011).

Deterrence against financial misconduct is often achieved by informal means instead. Regulators *de facto* leverage negative shocks on companies' stock prices following the release of information about criminal investigations (Alexander and Arlen, 2018). The market price of a company's financial assets reflects the current assessment by investors on future profitability of the firm and it is updated when information is revealed unexpectedly (Fama, 1970). The reputation of a company is part of such complex price evaluations. Information that a company engaged in misconduct negatively updates investors' opinion of it. Investors have been shown to re-structure their portfolios by over-selling shares of companies involved in adverse events, for fear that exposure to negative publicity undermines the value of future dividends. Large negative shocks on stock prices are observed when information on poor environmental, social, and governance performance emerges (Capelle-Blancard and Petit, 2019; Krüger, 2015). For instance, 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, the reputational negative effect is compounded by material concerns. Stock-holders restructure their portfolios out of concerns about potentially poor future 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 is deliberately exercised at the expense of investors[3]. Moreover, news of criminal investigation can create expectations of fines and monetary settlement with 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).

2.2 The shield of ownership: How to mitigate damage to reputation

It thus seems like investors could perform a regulatory function similar to that civil societies carry out (Acemoglu and Robinson, 2020; Fukuyama, 2016). As formal regulatory means struggle to hold companies accountable for corporate misbehavior, markets impose informal penalties that represent credible and sizeable sanctions. Morse (2019), for instance, finds significant market responses to information provided by international organizations like the Financial Action Task Force about financial criminal risk. Such market responses, then, could deter firms from misbehaving and potentially substitute states' weak enforcement of formal regulations (Kreitmeir et al., 2020).

The assumption behind this argument is that companies' corporate reputation is a function of business ethics along their entire operations (Vergin and Qoronfleh, 1998). However, companies' business structures can be extremely complex. A company sitting at the top of a corporate group (usually referred to as "parent") 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 (the company that owns 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.

As a result, even for the simplest corporate structures it is often difficult to establish what operations belong to a given corporate group. 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., Snamprogetti B.V. (of Italian origin but 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 indirect control over the consortium ever since 1998, when it acquired Dresser Industries and formed KBR by joining its subsidiary Brown & Root with Dresser's subsidiary

[3] E.g., Centennial Technologies Inc. defrauded its investors of an estimated figure between $150 and $376 million between 1994 and 1996: https://www.nytimes.com/2000/05/18/business/jail-and-150-million-restitution-for-fraud.html.

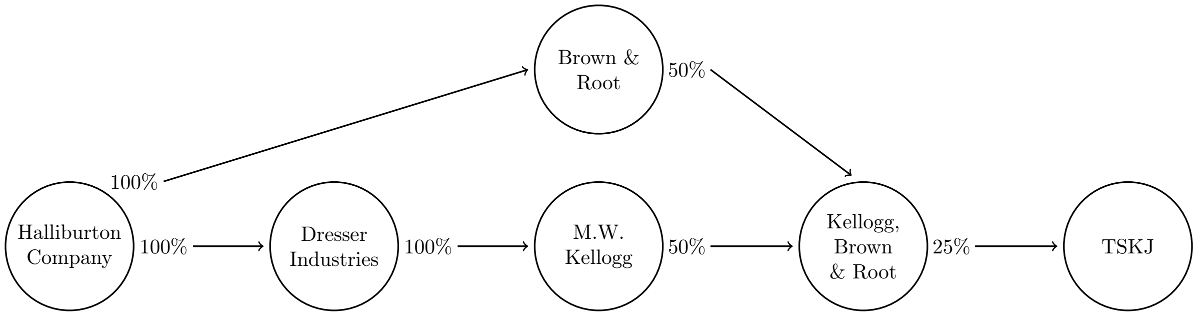


Figure 1: Example of corporate ownership structure: Halliburton Company's stakes in the TSKJ joint venture. Circles represent companies, arrows indicate ownership relations, and percentages represent degrees of ownership.

M.W. Kellogg. Similar fragmented structures are ideal for furthering illicit transactions. TSKJ became in fact (in-)famous for allegedly 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 makes it difficult for investors to assess which operations belong to a given corporate group. As a result, a company can use complex corporate structures to shield its reputation from that of subsidiaries in its ownership chain. Fragmented corporate ownership can thus be purposed to shield a company from informal penalties on financial markets, in case misconduct is revealed to the public.

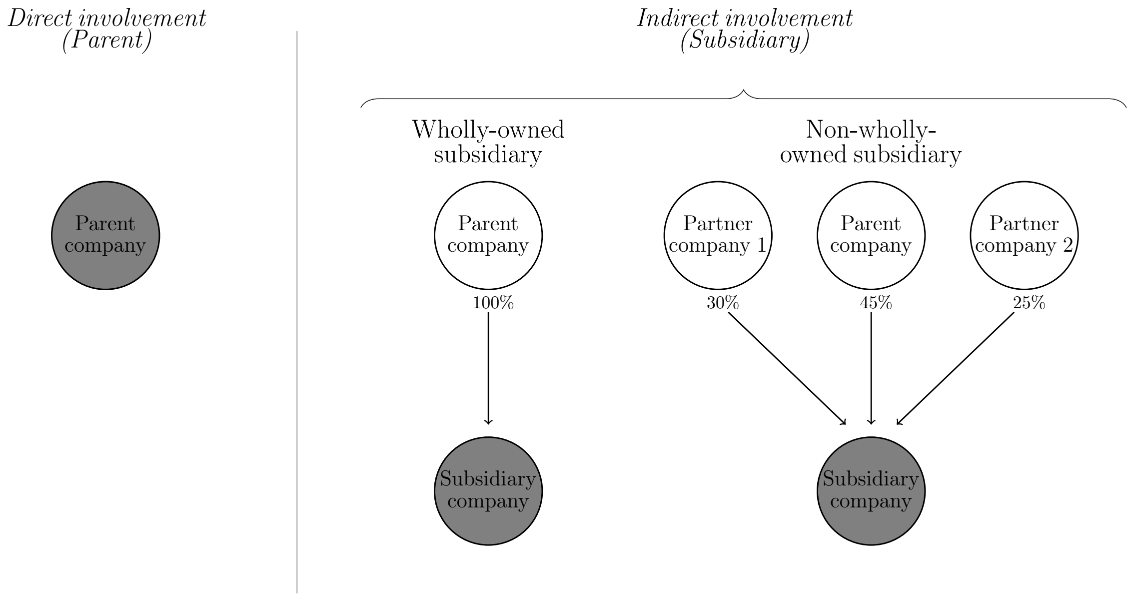


Figure 2: Three ways a parent company can be involved in a corporate criminal scandal along its ownership chain: directly, through a wholly-owned subsidiary, or through a non-wholly-owned subsidiary. Circles represent companies, grey circles represent companies investigated for violating corporate criminal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

A parent company can be involved in investigations for possible criminal misconduct in three possible ways. Figure 2 sketches this conceptual framework. In a first scenario, 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. Alternatively, a company can be involved in a scandal indirectly, *i.e.* through a subsidiary part of its corporate group. In turn, two possible scenarios exist here. The parent company can be involved in a scandal because authorities investigate potential violations by a wholly-owned subsidiary (second scenario). Alternatively, authorities can investigate a subsidiary that the parent is simply the majoritarian owner of (third scenario). In this conceptual framework, typologies of *indirect involvement* are therefore a function of degrees of integration of the involved entity in the parent company's corporate group.

I argue that the regulatory function performed by financial markets' reputational penalties is not equal across these three scenarios. When ownership of subsidiaries is diluted, so is control by the parent over their operations (Demsetz and Lehn, 1985), including criminal activity (Alexander and Cohen, 1999). The reputation of the parent company will therefore be less compromised, in the eyes of investors, when involvement is indirect. This could appear like an efficient attribution of responsibility from a regulatory perspective: investors would negatively update the standing of 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.

In the first scenario, the parent company's stock prices suffer from unexpected news of misconduct. When information that a company was directly involved in criminal activities hits the market, investors negatively update their beliefs on prospects of profitability for that firm. They update their company's reputation and fear exposure to negative publicity might undermine the value of future dividends. This has two joint effects. On the supply side of the stock market, investors who own shares sell their equities at an abnormal rate, by fear that their future price will be lower. Increase in the supply of stocks is also met by a shrinkage in demand, as the title is perceived to be less profitable. The result is price devaluation, causing the parent company to experience losses it would not otherwise have experienced, had the scandal not emerged.

However, with indirect involvement (second and third scenarios) reputational consequences are less severe. Here, the parent company is not directly involved in the scandal. The parent company still suffers from investors' material concerns. For instance, parent companies are often mandated fines and monetary settlements for misconduct by their subsidiaries (Garrett, 2011). However, the parent's reputation is not directly at stake. This results in weaker financial penalties.

In particular, I claim that the severity of the damage to reputation 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 reputation than one which is more loosely connected to the corporate group, because full ownership implies control over illicit conduct (Alexander and Cohen, 1999). In the third scenario, instead, the parent firm does not even fully own the subsidiary found in breach of financial regulations. The parent company's reputation 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[4] 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[5]. To take a real example, in March 2016 the DOJ and SEC opened up investigations into alleged bribery by Shell Nigeria Exploration and Production Co LTD, wholly-owned subsidiary of the Royal Dutch Shell PLC, in connection with the award of rights to drill the Nigerian offshore oil block OPL 245[6]. Very rarely companies alleged of FCPA violations go to court. The long time frame of trials would expose companies to prolonged reputational losses 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)[7]. For

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

[5]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.

[6]See: https://fcpa.stanford.edu/investigation.html?id=414.

[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, 2011).

instance, in April 2020 the Italian oil company ENI SpA entered into a similar agreement with the SEC to settle allegations of bribery in Algeria by SAIPEM, a subsidiary owned for 43% of shares by ENI[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 possible FCPA misconduct. Companies are mandated to 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 precisely to facilitate the flow of information from companies to (potential) investors. In both the Shell and the ENI cases, for instance, the companies informed investors as US authorities opened up an investigation (March 10, 2016 and April 10, 2014 respectively).

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 forms to the SEC itself, which are available to the general public of (potential) investors. Similar arrangements are in place in some other countries (*e.g.* in the UK through Companies House) but not in all 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 makes this case a good test for my argument. Whereas selections of companies into the group of those involved in cases of corporate corruption is likely endogenous to their reputation on the market, 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

[8]See: https://fcpa.stanford.edu/enforcement-action.html?id=796.

[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).

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

[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.

periodic reports, investors and market analysts cannot necessarily expect involvement of the firm into public 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 an official 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-paying companies must rely on the public official's given word that another firm will not be awarded the contract instead (Lambsdorff, 2007). Moreover, under FCPA terms a parent company is liable for misconduct by its subsidiaries (Lenczowski, 1979). This means that an FCPA violation by a subsidiary can result into material losses (in terms of fines and monetary settlements) for the parent too (Garrett, 2011). 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 corporate group's perspective.

4 Data

In order to test my argument, I require information on the date events of US investigation for corporate bribery were unexpectedly revealed to the public. I also need data on the corporate relations between the involved entity and its parent company. Finally, I need daily observations on stock prices relative to companies involved in violations of US anti-corruption policy and daily observations on stock market indices. In this section I detail the data collection procedure.

To obtain information on cases of corporate corruption, I first identify events of 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 database made of 841 text documents summarising events of cross-border corporate corruption in violation of the international anti-bribery regime, and related law enforcement actions.

Out of 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

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

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 214 unique companies involved in 264 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) at the time of the anti-corruption investigation. Second, extensive web searches to confirm the retrieved information[17]. Where Orbis information conflicts with these alternative sources, I keep information available from reports by US authorities. Where this is not available, I rely on web searches. Out of the 264 events of corruption I consider, 139 (53%) involved the parent company directly, while 125 (47%) involved it through a subsidiary. Out of these 125 events, 64 involved a wholly-owned subsidiary ($Ownership = 1$) and 61 involved a non-wholly-owned one ($Ownership = 2$).

The next step consists in coding, for each violation of the US anti-corruption law, the very day allegations were 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 government agencies, and newspaper articles to establish the earliest date news of a potential violation of the US FCPA by a company were made public. I manually search

[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 consider direct ownership and indirect ownership indistinctively.

[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.

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	Event
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 what my dataset looks like. For each entry, an entity (*Violation entity*) is alleged to have bribed public officials in a foreign market (*Violation country*)[19], thus violating the US FCPA. 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 ticker symbol under which the parent company trades its securities (*Ticker*), and the date information on the violation was first made public (*Event*).

The final data collection step concerns daily stock prices data. I retrieve all stock price and market indexes information from Refinitiv Workspace. Consistently with typical political-economy applications of the design (Aklin, 2018; Genovese, 2021; Kucik and Pelc, 2016; Wilf, 2016), I measure daily percentage *change* in stock prices with respect to the previous day – which I call *Returns*. This variable is measured as the percentage difference in closing price of a stock at the end of a trading day, with respect to the same value at the end of 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 the most important market indicators. I obtain price history of: S&P 500 Index (SPX), NASDAQ Composite Index (IXIC), NYSE Composite Index (NYA), NASDAQ 100 Index (NDX), Tokyo Stock Exchange REIT Index (TREIT), 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).

5 Research design

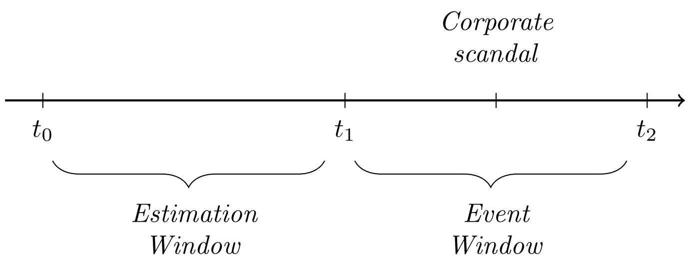
My argument states that regulatory penalties for a parent are imposed by financial market actors. I claim that the effect is conditional on the degree of integration of the involved entity in the parent's

[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.

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 in finance studies for estimating market-induced reputational effects of unexpected events on involved companies (Karpoff et al., 2008; Kreitmeir et al., 2020). 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). Here, I employ it to study the heterogeneous effect of state-mandated corporate regulations, as it is moderated by the involved entity's position in the corporate chain.

Figure 3: Event-study design: Time windows



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 as presented in Figure 3. 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 264 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*[20].

In the *estimation window*, I estimate one market-model relative to each event by explaining the involved company's *Returns* using market-wide indexes. Equation 1 summarizes this step. Daily observed *Returns* for each company i 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 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 into noisy predictions with large variance, thus returning imprecise counterfactuals and potentially re-

[20]In a robustness test, I show results are not dependent on the arbitrary choice of *event window* length, see Table C.1.

ducing precision of my identification. 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 superior predictive performance when compared to standard ordinary least square (OLS) market models[21] (Wilf, 2016). Thanks to the LASSO, each market model represents the best feasible predictor of a company i's stock prices 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 that specific event. Figure A.1 is a heatmap reporting coefficients $\boldsymbol{w_e}\hat{\boldsymbol{\beta}}_{\boldsymbol{e}}$ with whom indexes enter each of the 264 market models estimated using the LASSO. It also reports the percentage of models that include each of the 11 indexes. The procedure effectively omits non-predictive indicators (represented by white cells). Minor indexes are sacrificed and tend to appear less frequently (*e.g.* Euronext 100 Index enters only 36.4% of the models). More predictive and relevant indexes tend to be more frequently included in the models (*e.g.* the NYSE Composite is included in 59.1% of the models).

The LASSO results in market models with very satisfactory in-sample predictive performances. Figure A.2 reports the distribution of the Normalized Root Mean Squared Error (RMSE) and the R-squared for the 264 market models in the estimation window[22]. All models result in 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.*

Once vectors of coefficients α_e and $\boldsymbol{\beta_e}$ have been estimated following this procedure, 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. This phase is effectively imputing daily synthetic counterfactual stock prices to each company in the *event window*, based on models estimated in Equation 1. $\widehat{Returns}$ thus represents the best expectable returns to a company in the *event window*, based on

[21] In a robustness test, I replicate my results using OLS (thus effectively employing all market-wide indexes for all companies) and verify results hold, although estimation becomes less precise. See Section E.

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

information available before unexpected news of corporate crime were released. I take it as a measure of counterfactual *Returns* to a company, had event e not occurred.

$$\widehat{Returns}_{et} = \hat{\alpha}_e + \boldsymbol{w_e}\mathbf{X_t'}\hat{\boldsymbol{\beta}}_e \mid t_1 \leq t \leq t_2 \tag{2}$$

Next, I calculate the deviation of observed *Returns* from imputed counterfactual $\widehat{Returns}$ in the *event window*. I call this difference *Abnormal Returns*:

$$Abnormal\ Returns_{et} = Returns_{et} - \widehat{Returns}_{et} \mid t_1 \leq t \leq t_2 \tag{3}$$

Abnormal Returns represents my dependent variable. Positive (negative) *Abnormal Returns* indicate changes in stock prices that exceed (fall behind) what market models expected based on information available before event e took place.

Figure 4: Example of the synthetic counterfactual imputation procedure: allegation of FCPA violation by OSI Systems, Inc. in February 2018

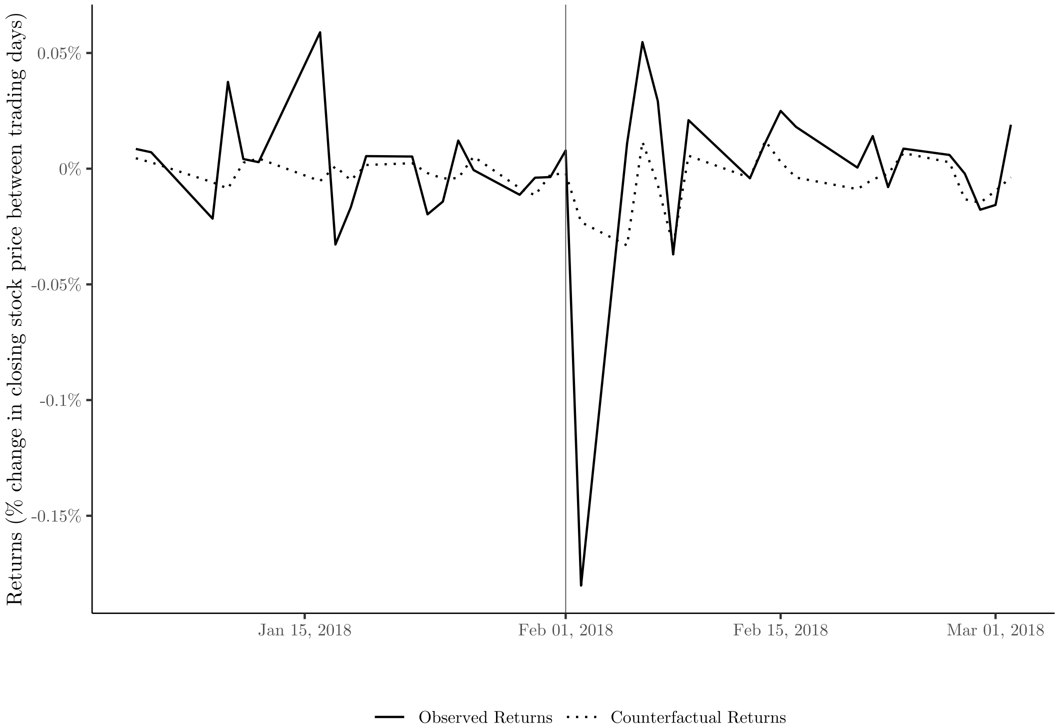


Figure 4 exemplifies the procedure by drawing on a real case. On February 1, 2018 the California-based defense technology manufacturer OSI Systems, Inc. (trading on NASDAQ as OSIS) announced its investors that the SEC and the DOJ had opened investigations into potential violations of the FCPA in an unspecified foreign country[23]. As this case exemplifies, in the pre-treatment period the LASSO procedure retrieves estimates of synthetic counterfactuals that closely approximate observed *Returns*. On

[23] See record on the FCPAC: https://fcpa.stanford.edu/investigation.html?id=380.

the day following the announcement of allegations, my synthetic counterfactual imputation estimates an abnormal loss in *Returns* of about 0.16%, indicating that investors responded to this event and the company realized *Returns* that were substantively below what market indicators would have expected. The company was trading at about $66.1 per share before the event. Thus, the value of OSIS lost about $10.6 per share. This equals a loss in market capitalization of more than $201 million, given that the company had about 19 million outstanding shares. The company was then dismissed of all allegations: in a press release on June 5, 2019 it announced that the DOJ and the SEC had closed any investigation on the matter and would not be taking any further actions[24].

In order to average similar effects across different events, I estimate the model reported in Equation 4 using data from the *event window*. The model explains the *Abnormal Returns* measure computed in Equation 3. It includes a binary treatment variable *Event* taking value 1 on the day the corporate criminal scandal e was made public, and 0 otherwise. Parameter δ represents the estimand: an average treatment effect of the treated (ATT) companies' stock prices on the day information on potential corruption was released, retrieved by discounting observed returns from changes in stock prices that were expectable had the event never taken place. It measures by how much observed *Returns* diverge from counterfactual $\widehat{Returns}$, on average, on the day the unexpected event took place.

$$Abnormal\ Returns_{et} = \gamma + \delta Event_{et} + u_{et} \ \mid \ t_1 \leq t \leq t_2 \tag{4}$$

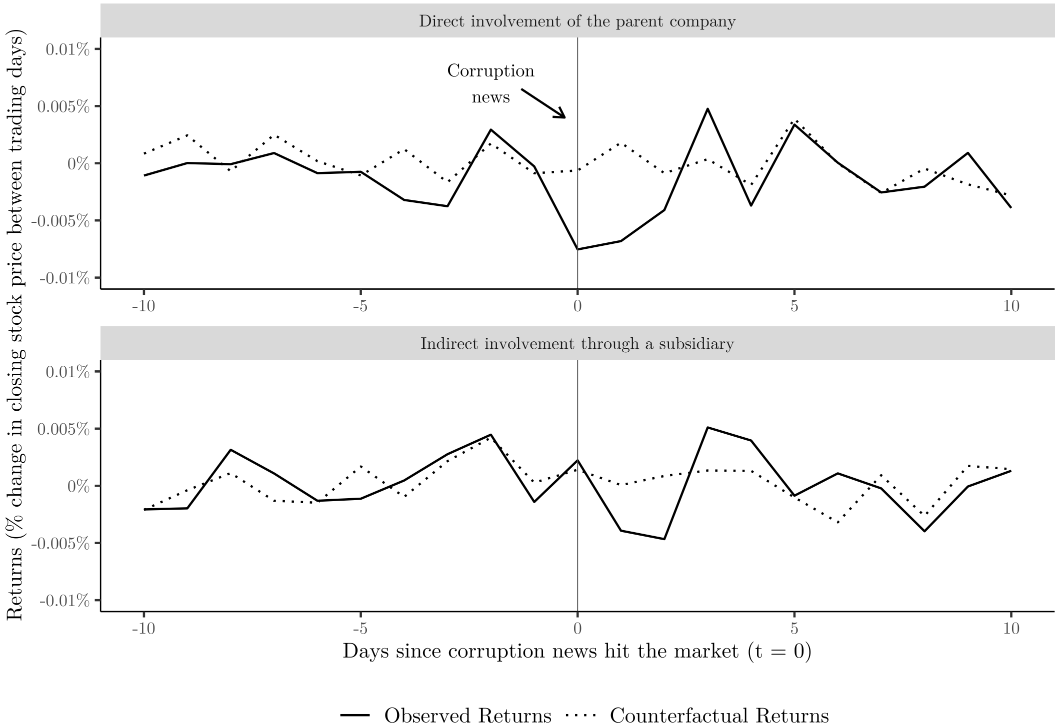
To test my argument on the moderating effect of indirect involvement, I apply this design and condition the effect of *Event* on the position of the involved entity in the ownership chain. First, I compare direct and indirect involvement in a corruption scandal. I estimate Equation 4 after interacting the *Event* treatment variable with my *Subsidiary* variable, as in Equation 5. In a later test, I substitute the *Subsidiary* moderator with my indicator measuring the degree of ownership ($Ownership$). I expect parameter δ to be negative: when a parent company is directly involved in a corporate corruption scandal, its stock returns should be negatively affected. I expect coefficient ϕ to be positive instead: involvement through a *Subsidiary* should moderate the negative impact of a scandal, and decrease its magnitude.

$$Abnormal\ Returns_{et} = \gamma + \delta Event_{et} + \phi\ Event_{et} \times Subsidiary_e + \theta\ Subsidiary_e + u_{et} \tag{5}$$

Figure 5 shows the daily average observed and counterfactual *Returns* in the 10 days before and after the *Event*, distinguishing between cases where the parent company was directly involved in a scandal ($Subsidiary = 0$, top panel) from those where involvement happened through a subsidiary ($Subsidiary = 1$, bottom panel). In both panels, pre-treatment average observed *Returns* are well approximated by average counterfactuals. This indicates the lack of a pre-treatment difference among

[24] See press statement: https://fcpa.stanford.edu/fcpac/releases/4000/003168.pdf.

Figure 5: Average *Returns* and $\widehat{Returns}$ in the 10 days before and after the release of corruption news, disaggregated by type of involvement. Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary



the two groups. The top panel shows that observed *Returns* are on average lower than counterfactuals at the closing of the very day corruption news are released (and consistently so in the following two days) when parent companies are involved directly in a scandal. After that, *Returns* do not seem to depart from their counterfactuals. The picture offered by the bottom panel provides an initial confirmation of expectations from my argument: observed *Returns* to the parent company are lower than their counterfactuals in the two days following release of corruption news when a subsidiary is involved in a scandal, but this gap appears smaller in size than what is observed in the top panel. After that, trends between observed and counterfactual data get closer again.

My design aims at identifying the effect of the *Event* on involved parent companies' stock prices in these different scenarios. But how comparable are different types of involvement? Is it possible that firms or corruption events are heterogeneous depending on whether involvement is direct or not? In appendix I show that, at least when looking at a range of observables, events of direct ($Subsidiary = 0$) or indirect involvement ($Subsidiary = 1$) of the parent company 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 company, the value of its stocks (Table B.1), or the distribution of headquarter countries and industries (Figures B.1 and B.2).

Figure B.4 shows average *Abnormal Returns* (that is, the difference between the solid and the dotted lines from Figure 5) and 95% confidence intervals in the entire 30 days before and after the *Event*. It confirms the interpretation drawn from Figure 5 and provides further evidence in favor of my expectation. Next section presents econometric results to assess the size and significance of these descriptives.

6 Results

I estimate Equation 5 using OLS. I cluster all standard errors at the parent company-level, to account for likely correlation between observations relative to the same firm. Table 2 reports my main results. In model 1, I introduce only my variables of interest. In following models, I introduce controls to remove potential sources of endogeneity. First, I introduce a one-day lag of the dependent variable to account for any anticipation effects on the market. Next, I account for year-specific heterogeneity. Research has vastly documented variation in the number of FCPA cases over time (Garrett, 2011) and the dependence of prosecutors' activity on political motives that might also be time-varying (Tomashevskiy, 2021). My data confirm the observation that the intensity of investigation changes with time (Figure B.3). If specific years see a stronger reaction by markets for unobservable reasons unrelated to my causal story, it is possible that my estimates are endogenous. I therefore introduce a year-fixed effect in model 3 (considering the year of the *Event* day), so as to only focus on explaining within-year variation and removing these potential sources of endogeneity. Finally, specific events might have extreme resonance for reasons that are unrelated to my causal story. In model 4 I substitute year-fixed effect with event-fixed effect to absorb all between-event heterogeneity in stockholders' response.

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

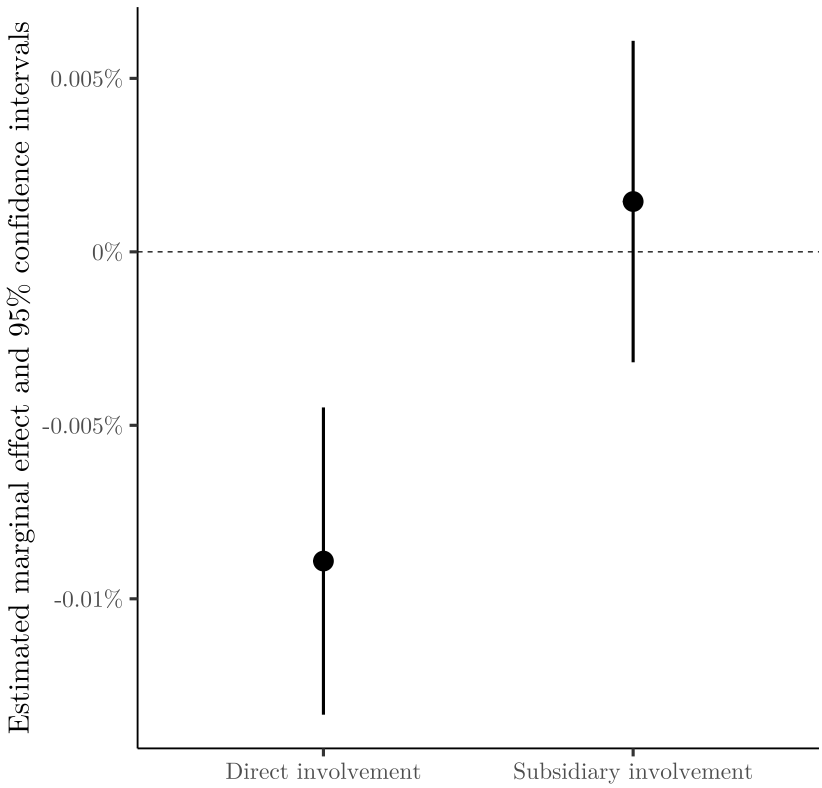
	(1)	(2)	(3)	(4)
Event	-0.009**	-0.011***	-0.011***	-0.010***
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Subsidiary	0.010*	0.012*	0.012*	0.011***
	(0.005)	(0.005)	(0.005)	(0.003)
Subsidiary	0.000	0.000	0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.006)
Abnormal Returns (t−1)		-0.021	-0.026	-0.052***
		(0.033)	(0.032)	(0.010)
(Intercept)	0.000	0.000	0.004	0.001
	(0.000)	(0.000)	(0.003)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	10351	9670	9670	9670
R2	0.002	0.003	0.008	0.037
R2 Adj.	0.001	0.006	0.006	0.010
F	5.319	6.279	3.600	1.376

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

Across all models, results are in line with my expectation. The coefficient associated with the un-interacted *Event* variable is negative and distinguishable from zero at a 0.05 conventional level of significance. Its point estimate ranges from -0.009 to -0.011. Recall that this coefficient indicates the effect of news of corruption (*Event*) on stock prices to the parent company, when the company is directly involved in a scandal ($Subsidiary = 0$). In this scenario, thus, I estimate a 0.01% average decrease in value for the parent company's stocks on the very day news of corporate corruption hits the market, with respect to the previous day. The interaction term $Event \times Subsidiary$ has a positive coefficient, as expected. 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.

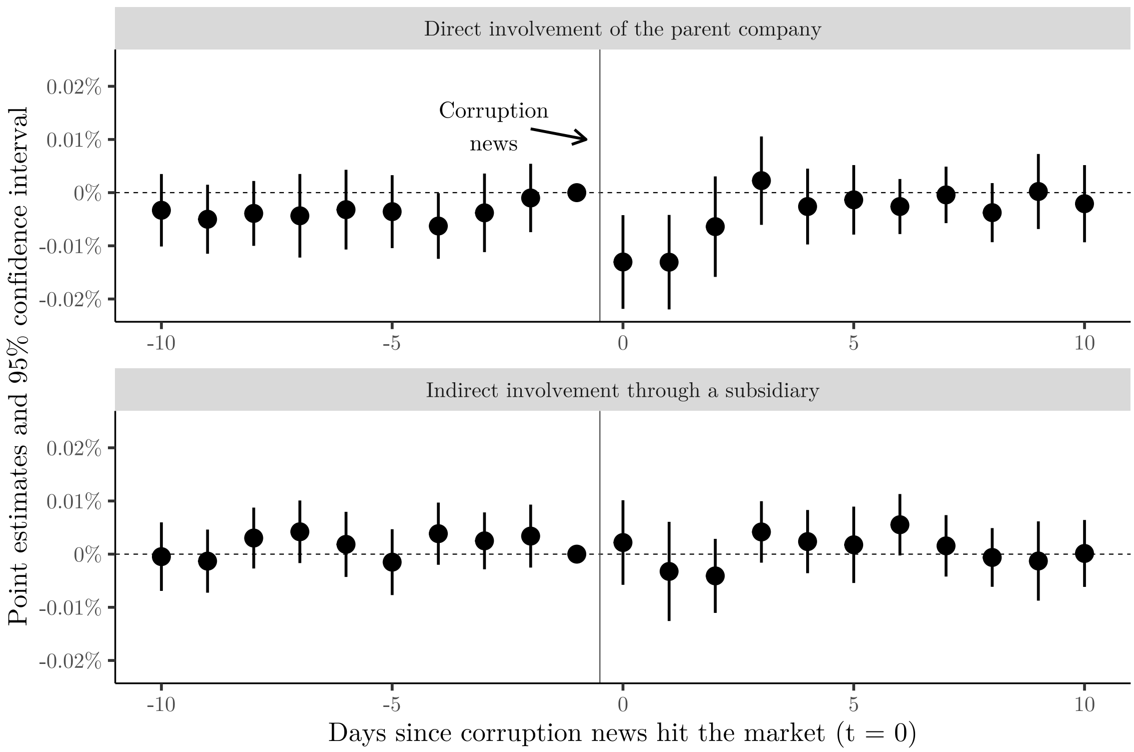
In order to appreciate the size of the moderating effect, I calculate marginal effects of the *Event* on *Abnormal Returns* in the two scenarios of direct and indirect involvement. I present results in Figure 6 drawing from model 1 in Table 2. Results relative to the other models are similar and available upon request. I observe a statistically significant and negative effect of about -0.01% when the company is involved directly in the scandal. However, the effect is positive in size and statistically insignificant when the company is involved in a scandal only through a subsidiary. This confirms my argument that subsidiary ownership insulates the parent company from a corporate criminal scandal.

Figure 6: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly or through a subsidiary. Results from model 1 in Table 2



How severe are the estimated negative effects of the scandal on the parent company's stocks? The median[25] parent company involved directly in an event of corruption ($Subsidiary = 0$) traded at about \$30.26 per share the day before the *Event*. A -0.01% abnormal loss on the day corruption was made public means that such company lost about \$0.30 in price of its shares due to the unexpected corruption information. 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 median company in my data traded 440,519,000 shares, for a market capitalization of about \$13 billion the day before information was released. A loss of \$0.30 per share on the day corruption news hit the market amounts to about \$132 million in losses. This average effect is remarkably similar to that estimated in case of other negative corporate social responsibility events (Kreitmeir et al., 2020).

Figure 7: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal



How do penalties evolve over time? In order to consider this, I study in more details the narrow time window that goes from ten days before the event until ten days after the event[26]. I perform a fully-fledged event-analysis by including a categorical variable relative to each trading day around the publication of corruption news. I employ the day before the event is made public (day -1) as a baseline category for this variable. I subset my sample according to whether the parent company is directly ($Subsidiary = 0$) or indirectly involved in a scandal ($Subsidiary = 1$). I include one-day lag and event fixed-effect for

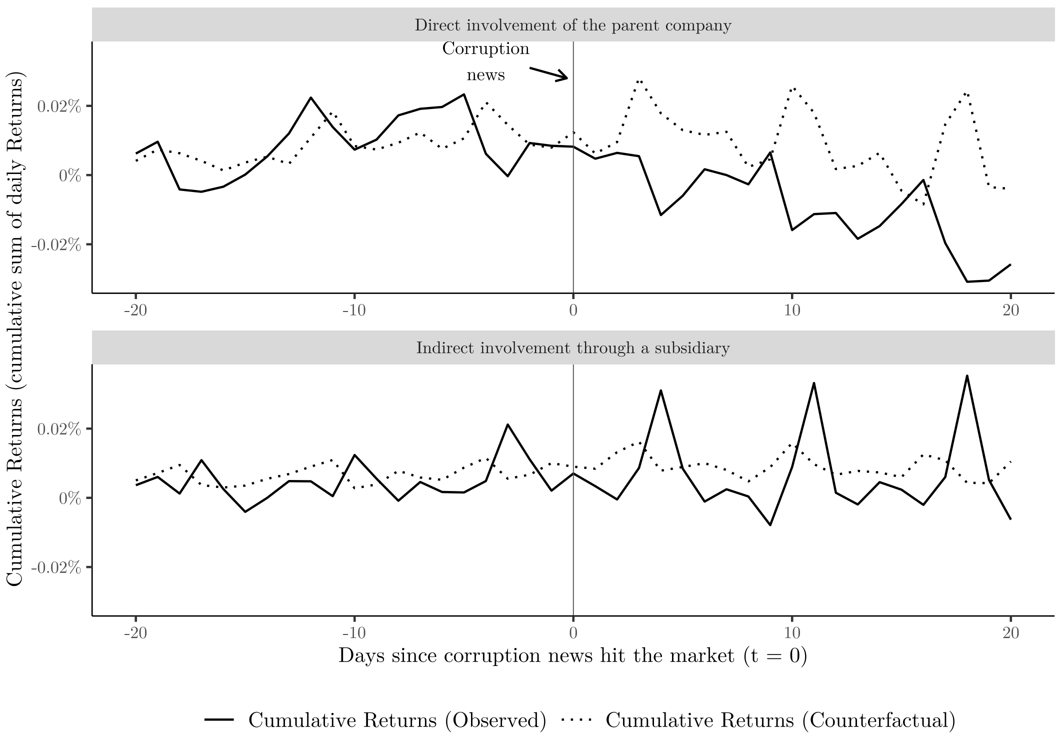
[25] I consider the median company, instead of the average company, to account for outliers in the distribution of closing prices. When considering averages, losses amount to about \$0.72 per share on the day of the *Event*, over a pre-event mean value of \$71.6 per share. Estimated losses to the average company amount to about \$1 billion in market capitalization.

[26] I report results including the entire *event window* in appendix (Figures C.2 and C.3)

consistency with the most complete model of Table 2. Results are reported in Figure 7. Similar results are obtained when omitting all these additional terms (Figure C.1). All standard errors are clustered at the parent company-level.

The top panel reports results relative to events that involve the parent company directly. I observe no significant differences in trading value when comparing days before news of corporate corruption hit the market and the baseline day (*i.e.* day -1). These are good news from an identification perspective: they suggest the lack of an anticipation effect and of pre-treatment trends that could confound the estimation. Rather, I observe an immediate drop in value for the stocks to the parent company on the trading day of the *Event* and the following (days 0 and 1). On both days, companies' stocks on average closed their trading at about 0.01% value less than what they did on the day before the event. After that, the effect is re-absorbed. On and after day 2, trading is on average not statistically different from what was observed on day -1 anymore. The bottom panel reports the same analysis of the parent company's stock returns when looking at indirect involvement – that is, when a subsidiary is involved in an event of corruption. Again, no pre-treatment significant differences are observed. However, post-treatment point estimates are all smaller than in the top panel. Moreover, effects are not significant in the aftermath of the *Event*.

Figure 8: Average *Cumulative Observed Returns* and *Cumulative Expected Returns* in the 20 days around the publication of corruption news, disaggregated by type of involvement. Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary



That markets quickly recover from losses to companies' *Returns* is consistent with the efficient market hypothesis (Fama, 1970). But do penalties suffered in the first two days after the *Event* cumulate to any sustained loss? How would cumulative returns have looked like, had corruption news not hit the market? To address this question, I follow previous event-studies (Aklin, 2018; Wilf, 2016) and calculate *Cumulative Observed* and *Cumulative Expected Returns* by summing, respectively, daily *Returns* and $\widehat{Returns}$ relative to a specific event. Figure 8 plots the average trends of these variables distinguishing between cases of direct and indirect involvement. It shows that, over the entire period that follows the release of corruption news, counterfactual *Cumulative Expected Returns* to a company's stocks (counterfactual) are higher than *Observed* ones (factual), when involvement in a scandal is direct. When considering events of direct involvement in a scandal, observable *Cumulative Returns* are significantly below their counterfactuals even 20 days after the event. In appendix I propose an event-analysis analogous to that in Figure 7 to estimate this cumulative effect. I find that companies involved directly in a corporate corruption scandal experience cumulative losses in the order of 0.04%, equalling about $517 million in capitalization, detectable with precision at least up to 18 days following the release of corruption news (Figure C.4). No clear difference between observed and counterfactuals is detected, instead, for indirect involvement.

I report extensive robustness tests on my findings in appendix. First, I show that results are not driven by any single outlier – a scandal with significantly negative impact – (see Figures C.5 and C.6). Next, I address the potential concern that results are driven by arbitrary choices followed in the procedure. I restrict *event windows* to the intervals: $[day - 10\,, day\,10]$ and $[day - 10,\ day\,0]$, to make sure I consider only data in the immediate proximity of the *Event* (Tables C.1 and C.2). Then, I exclude from the analysis any event associated with an imprecise estimation from Equation 1 (Table C.3, Figures C.7 and C.8). Finally, I replicate my entire analysis substituting LASSO synthetic counterfactuals with OLS ones and verify results do not hinge on the chosen procedure to impute counterfactuals (Section E).

6.1 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.

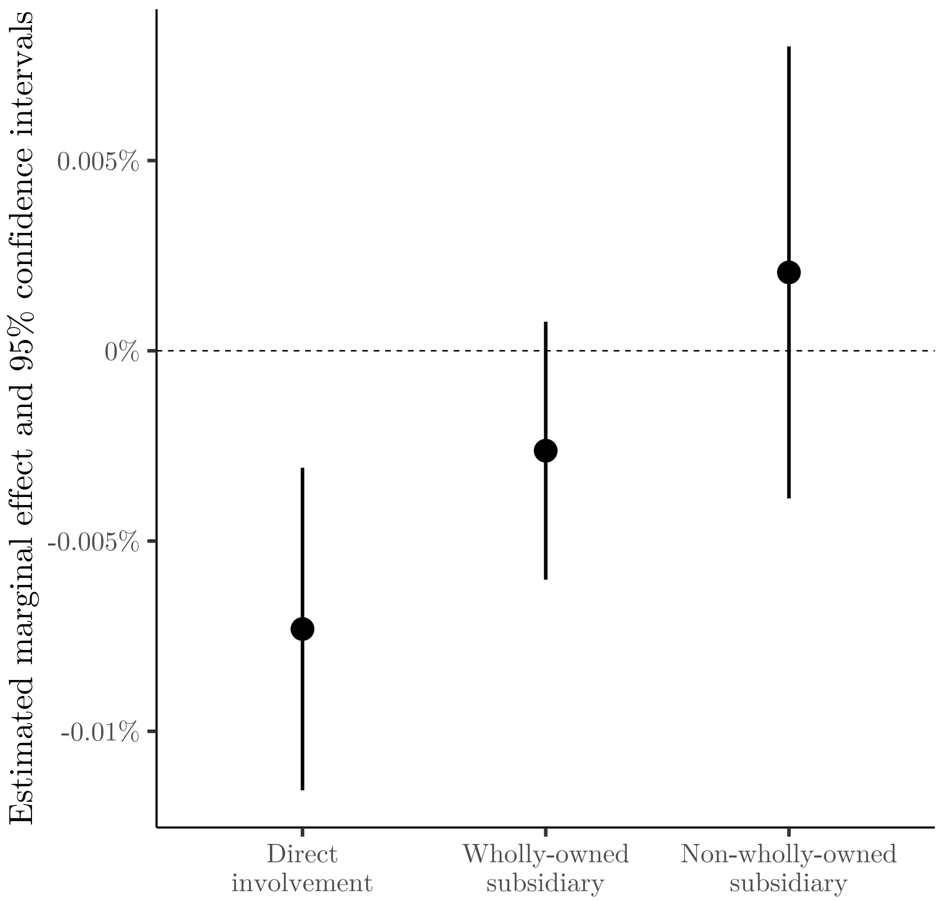
I re-estimate models in Table 2 and report the marginal effects from the most complete model in Figure 9. Full results are in Table D.1. I observe a negative effect for involvement through a wholly-owned subsidiary, smaller in size than when the parent company is involved directly in a scandal and just falling short of the conventional 0.05 level. However, this effect is not robust to alternative specifications. For instance, in a further test I run a more flexible specification that uses a categorical measure of *Ownership*. This is equivalent to the binning estimator proposed by Hainmueller et al. (2019), because it does not force the moderating effect to be linear. Resulting marginal effects from Figure D.1 show no significant effect on the parent company's stocks even for involvement in a corruption scandal through a wholly-owned subsidiary (full results in Table D.2). No significant effect at all is detected for involvement through a majority-owned subsidiary.

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 cynically choose to ignore a company's involvement in a scandal through a subsidiary? 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) or Armor Holdings Inc. (wholly-owned by BAE Systems). In these 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., or of Novartis Korea LTD, owned by Novartis AG.

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[27], which ranges from 0 (indicating extreme diversity between

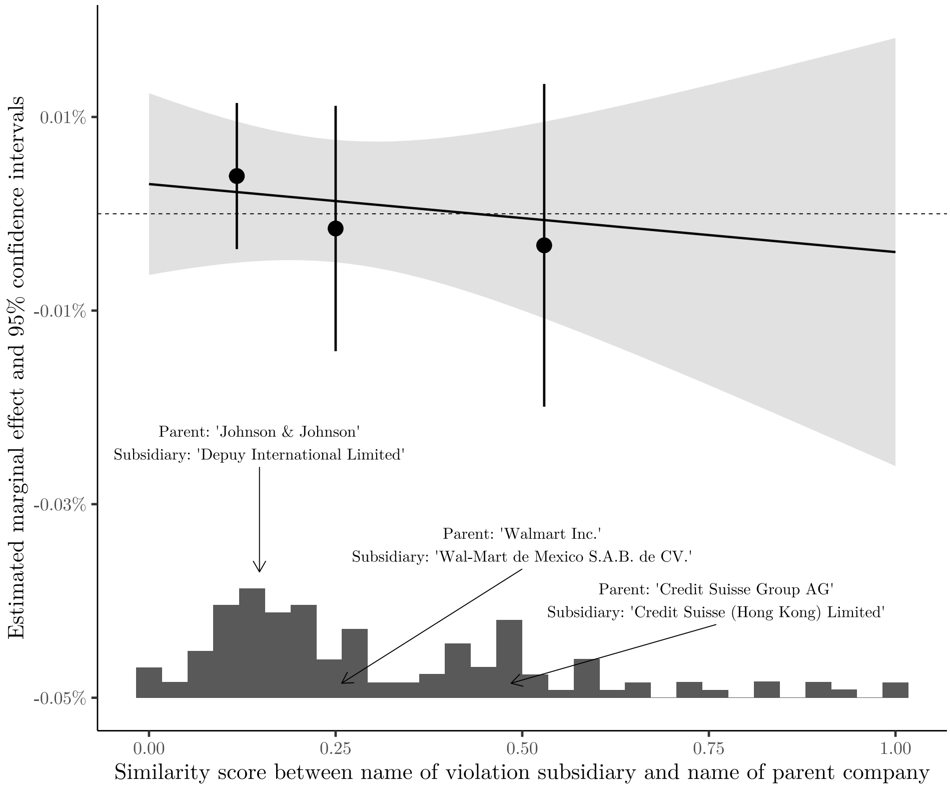
[27] The Levenshtein distance $L(a, b)$ is defined as the minimum number of modifications that are necessary in order to turn the word a into the word b. The metric I employ is a similarity score calculated as $1 - \frac{L}{M}$, where M is the number of characters for the longest of the two strings.

Figure 9: Marginal effect 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 model 1 in Table D.1



two strings) and 1 (indicating perfect equality). Next, I re-estimate my event-fixed effect model from Table 2, subsetting my sample to cases of indirect involvement only. In this specification, I employ this newly computed 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 10 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 true corporate structures. It suggests investors are in fact cynically avoiding to penalize parents for misconduct by their subsidiaries.

Figure 10: Marginal effects of indirect involvement into corporate corruption scandals 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

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 penalizing companies' stock prices when information on corporate misconduct emerges (Alexander, 1999; Kreitmeir et al., 2020). Authorities would then be able to *de facto* wage sanctions by leveraging the effect that investigations for misconduct bear on companies' asset prices (Alexander and Arlen, 2018; Farrell and Newman, 2019). 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 misconduct 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 the markets, due to concerns that negative publicity undermines 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 264 investigations for alleged violation of the US anti-corruption criminal law (FCPA) in 214 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. I used this dataset in an event-analysis design that imputes synthetic counterfactuals around the day unexpected information first hit the markets. My results show that parent companies suffer a significant abnormal loss of about 0.01% to their stock returns on the very day following the release of information. This effect amounts to a loss of about $132 million for the median company. However, I show evidence of no effect on the parent company's stock prices when involvement occurs through a subsidiary.

Results indicate a clear 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 (Kreitmeir et al., 2020; 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 and how 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 264 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.

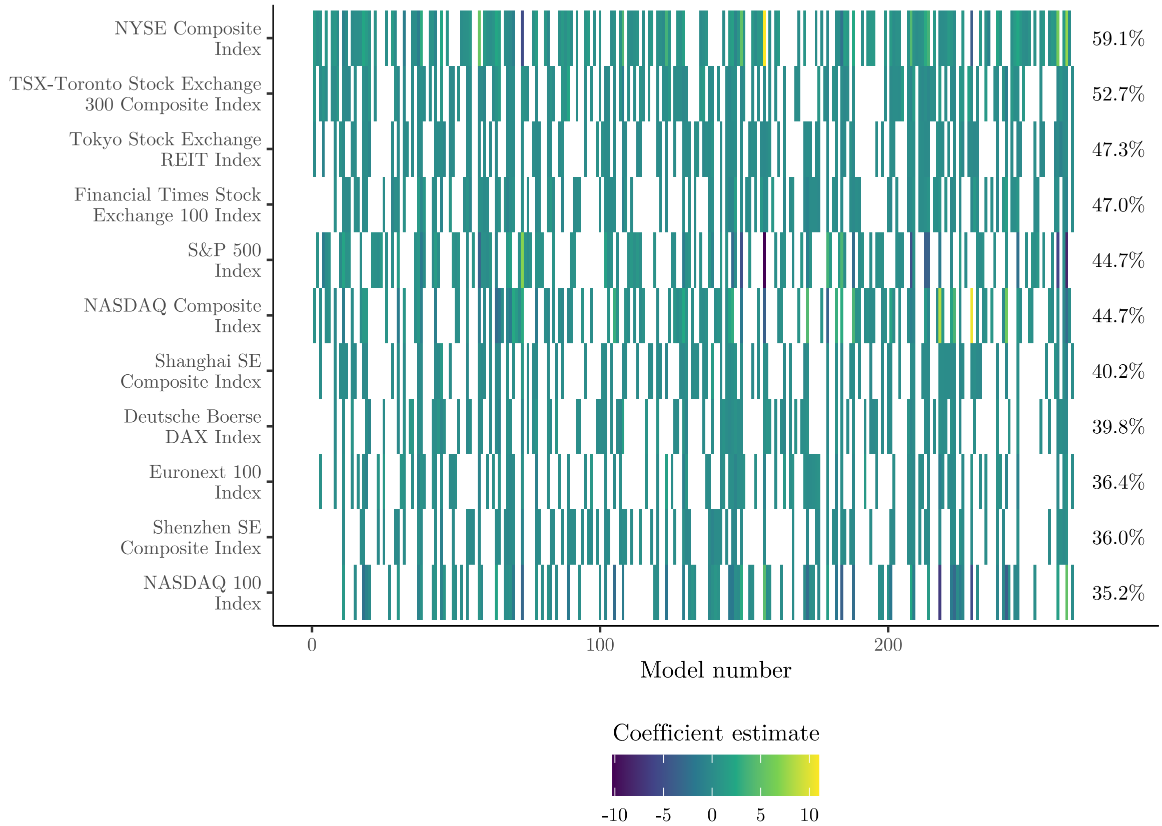
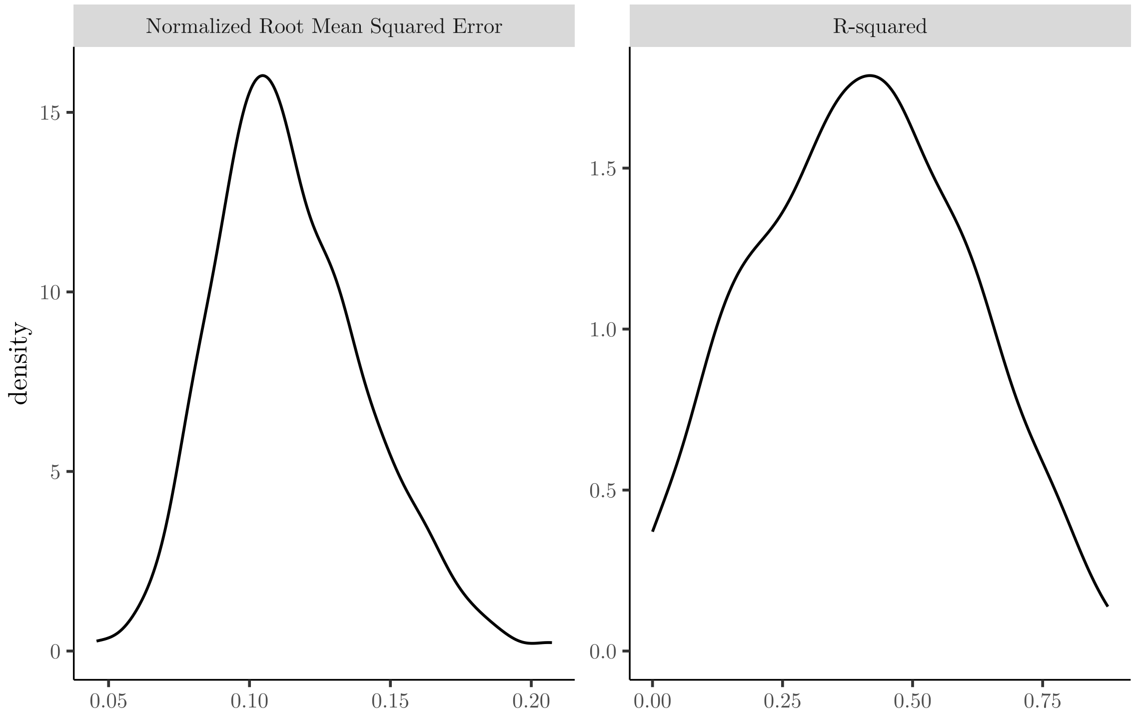


Figure A.2: Distribution of the normalized Root Mean Squared Error (RMSE) and of the R-squared yielded by the 264 market models estimated using the LASSO procedure.



B Descriptives

B.1 Balance in observable covariates across types of involvement

My research design identifies how the effect of the *Event* on each company's stock *Returns* changes according to *Subsidiary* (and *Ownership*). The design imputes synthetic counterfactuals to retrieve what stock *Returns* would have looked like in the absence of the *Event*. But how comparable are scenarios distinguished by my indicators for corporate ownership? Do firms involved in scandals directly (left-hand side of Figure 2) differ fundamentally from those involved in scandals indirectly (right-hand side)? If so, any difference in the identified effect might be due to this fundamental heterogeneity rather than to the mode of involvement in a scandal (direct *vs* indirect).

I retrieve information on characteristics of each parent company involved in an event e to evaluate this. 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 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 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 (\$50.31 *vs* \$43.99 billion) and are larger by assets (\$119.48 *vs* \$93.76 billion) but they tend to be smaller by revenues (\$27.15 *vs* \$30.74 billion) and number of employees (56.45 *vs* 84 thousands). 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)	1.52	2.92	1.49	2.45	-0.04	0.92
Parent Market Capitalization (billion USD)	50.31	84.01	43.99	59.70	-6.31	0.51
Parent Revenue (billion USD)	27.15	48.15	30.74	57.44	3.59	0.59
Parent Assets (billion USD)	119.48	389.14	93.76	268.01	-25.72	0.54
Parent No. Employees (thousands)	56.45	77.31	84.00	217.97	27.55	0.21
Number of violation countries	2.04	2.14	1.82	2.08	-0.22	0.42

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

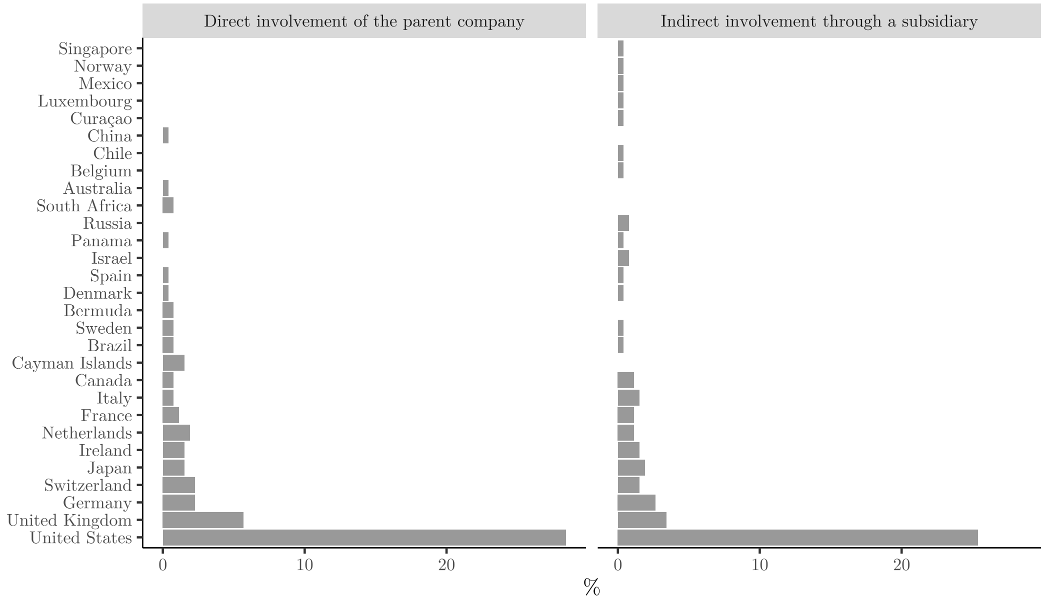


Figure B.2: Proportion of events involving companies by NAICS-3 code, across cases of direct ($Subsidiary = 0$) and indirect involvement ($Subsidiary = 1$).

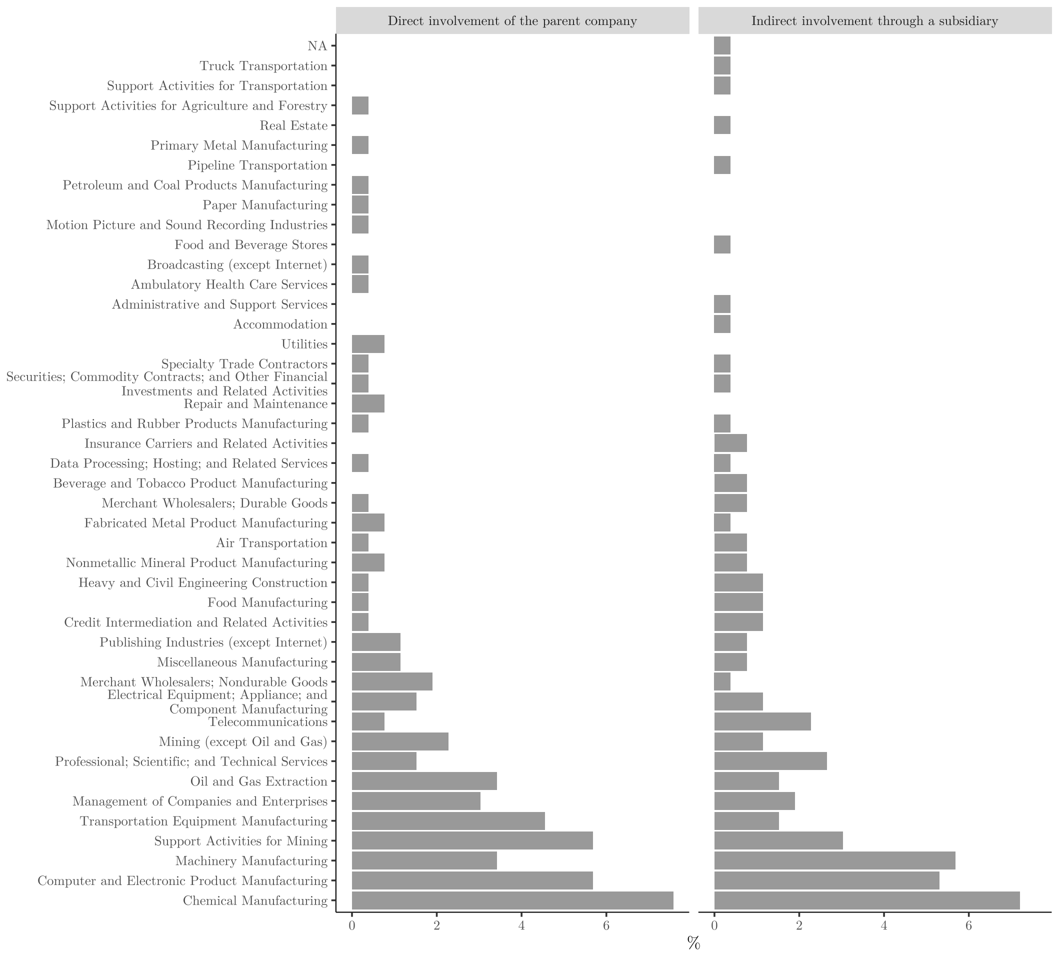


Figure B.3: Distribution of the 264 events of FCPA violation in the dataset, by date of release of news

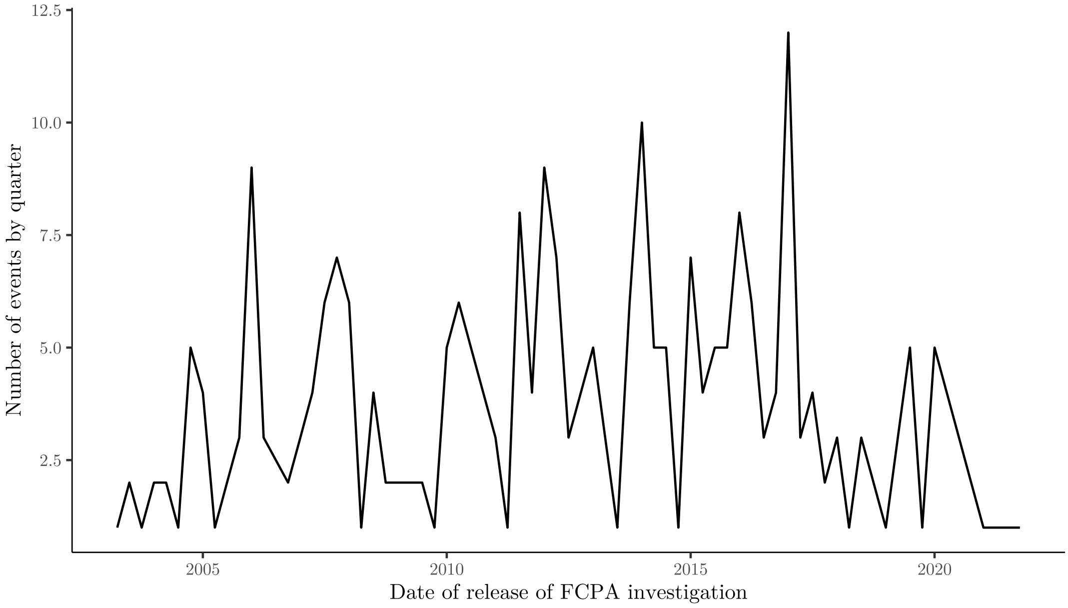
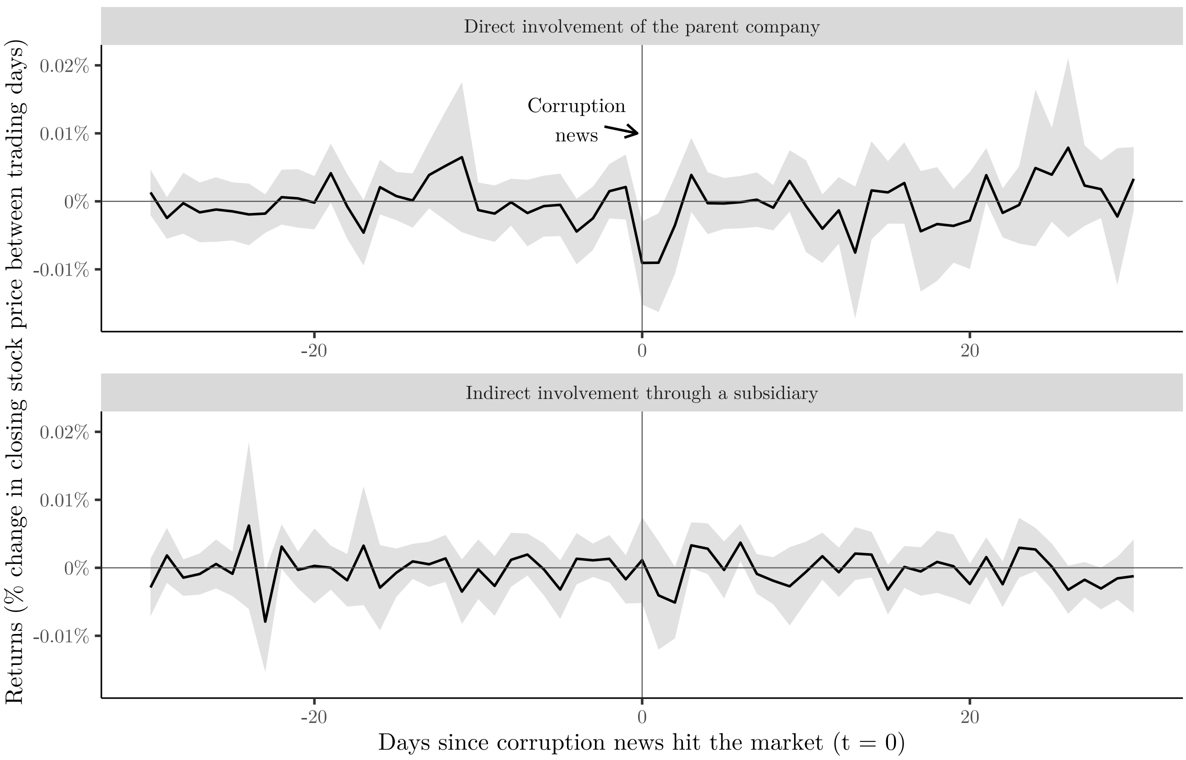


Figure B.4: Average *Abnormal Returns* in the 30 days before and after the release of corruption news, disaggregated by type of involvement. Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary. 95% confidence intervals around the sample mean are reported



C Analysis: LASSO-estimated synthetic counterfactuals

Figure C.1: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Sparse model

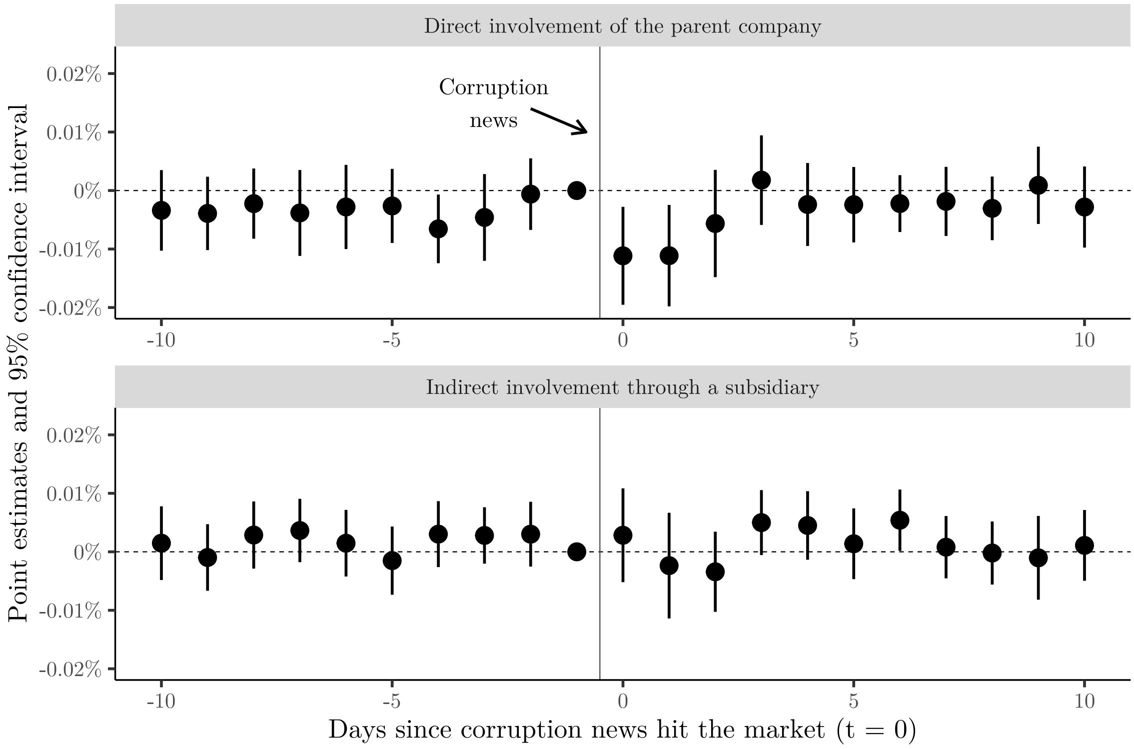


Figure C.2: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full model. Full *event window*

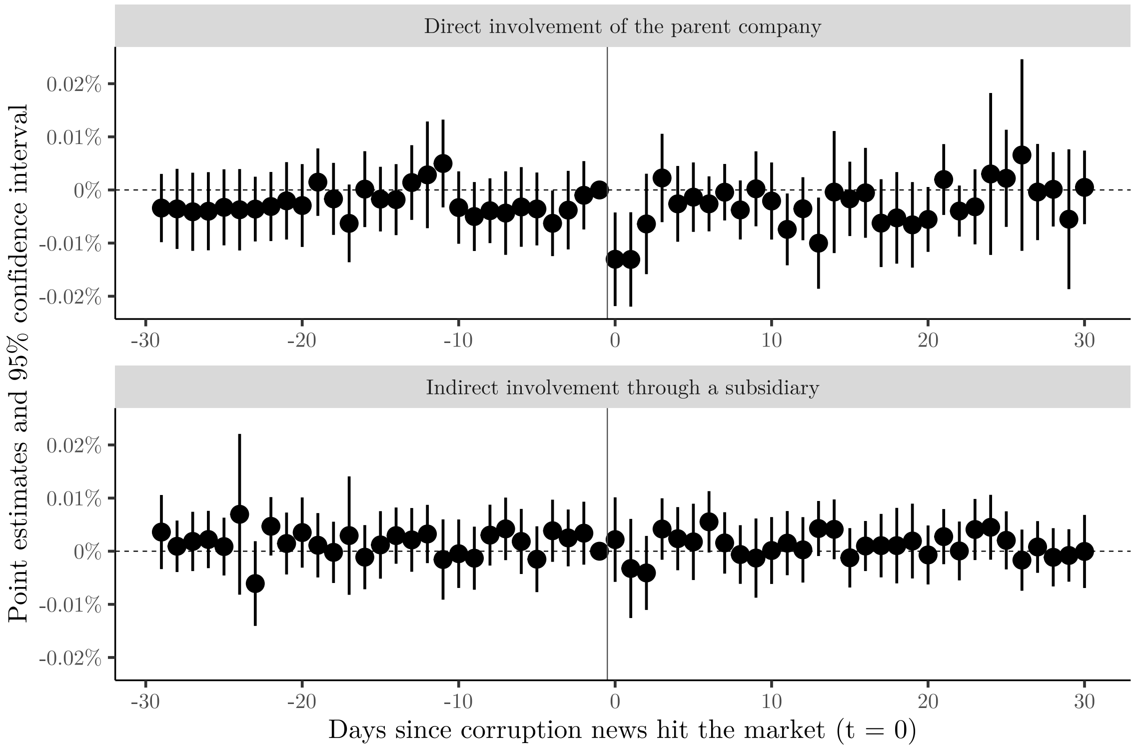
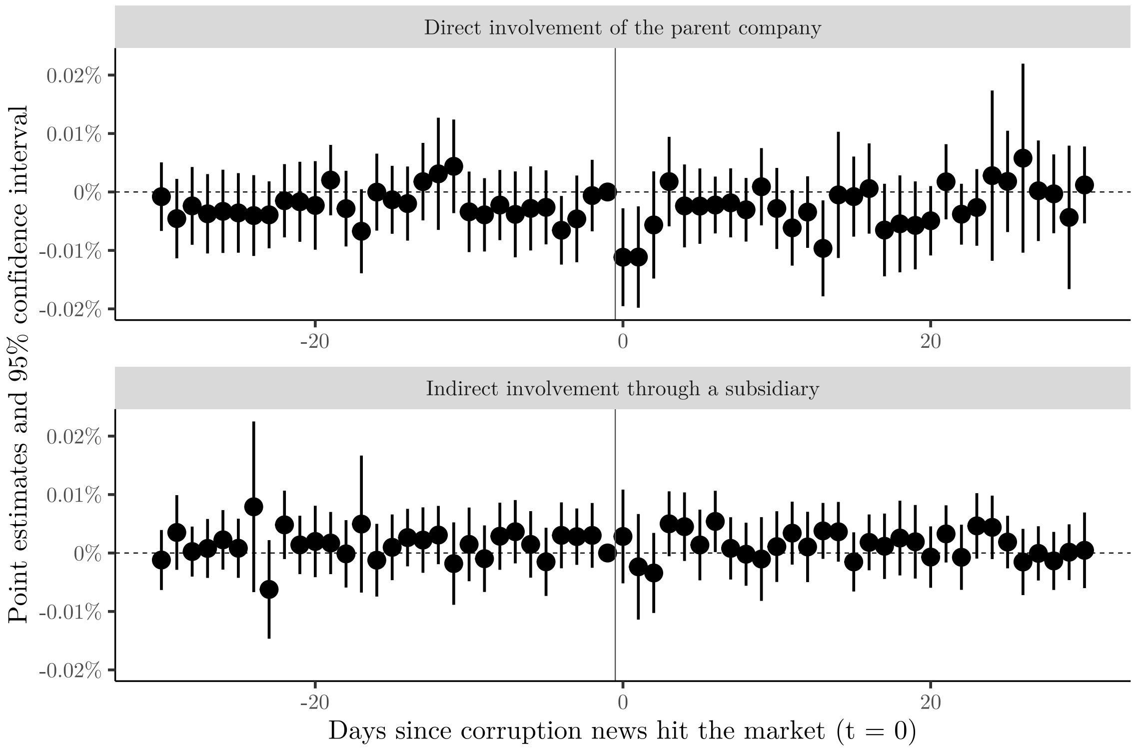


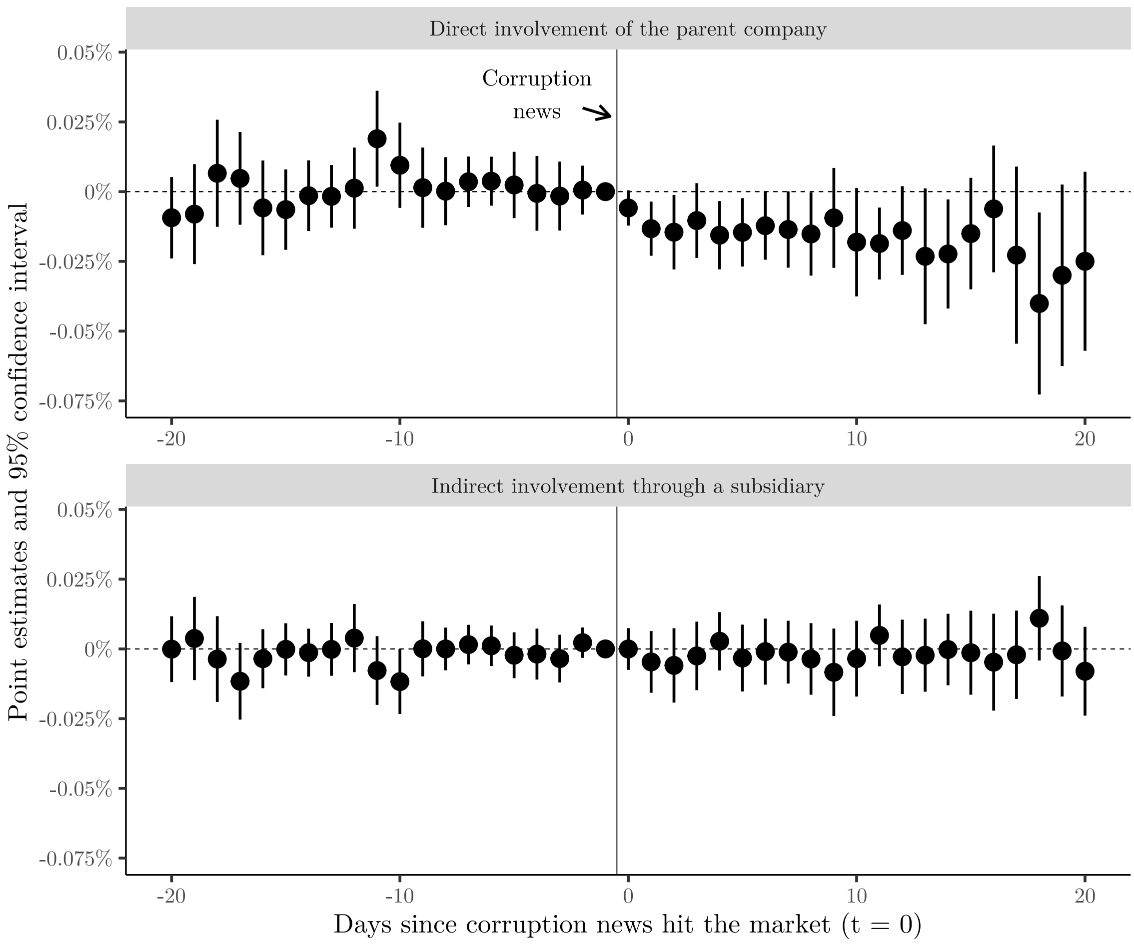
Figure C.3: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Sparse model. Full *event window*



C.1 Cumulative Abnormal Returns: analysis

To quantify the cumulative effect of the *Event*, I model *Cumulative Abnormal Returns*, defined as the daily difference between *Observed* and *Expected Cumulative Returns* in the same fully-fledged event analysis proposed in Figure 7. Models include a categorical indicator for each trading day around the *Event* and an event-fixed effect. Standard errors are clustered at the company-level. Estimated daily coefficients are reported in Figure C.4. In either typology of involvement, there seems to be no clear trend in the dependent variable before the event. In the case of direct involvement, post-event *Cumulative Abnormal Returns* are significantly lower than those of day -1 until at least day 18 after the event. On this day, observed cumulative returns are 0.04% lower what was expectable based on market models. When considering pre-event trading of the median company, this amounts to a loss in market capitalization of almost $517 million, when compared to the pre-event value, detectable more than two weeks after the event. Instead, no significant effect is ever detected for instances of indirect involvement.

Figure C.4: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal



C.2 Abnormal Returns: Robustness tests

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 7 multiple times, each time leaving one different event out of the model. I report point estimates and confidence intervals in Figure C.5 (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.6 reports estimated coefficients for the un-interacted *Event* term and the interaction term *Event* × *Subsidiary*, alongside their 95% confidence intervals.

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 model. 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.

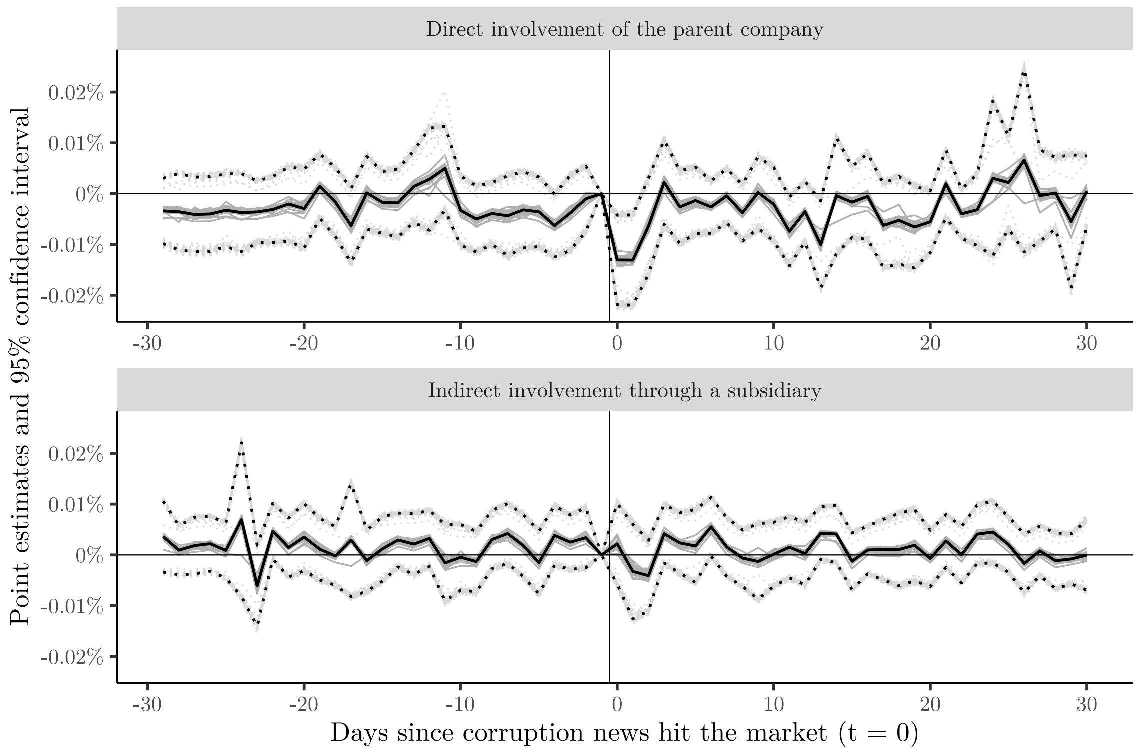
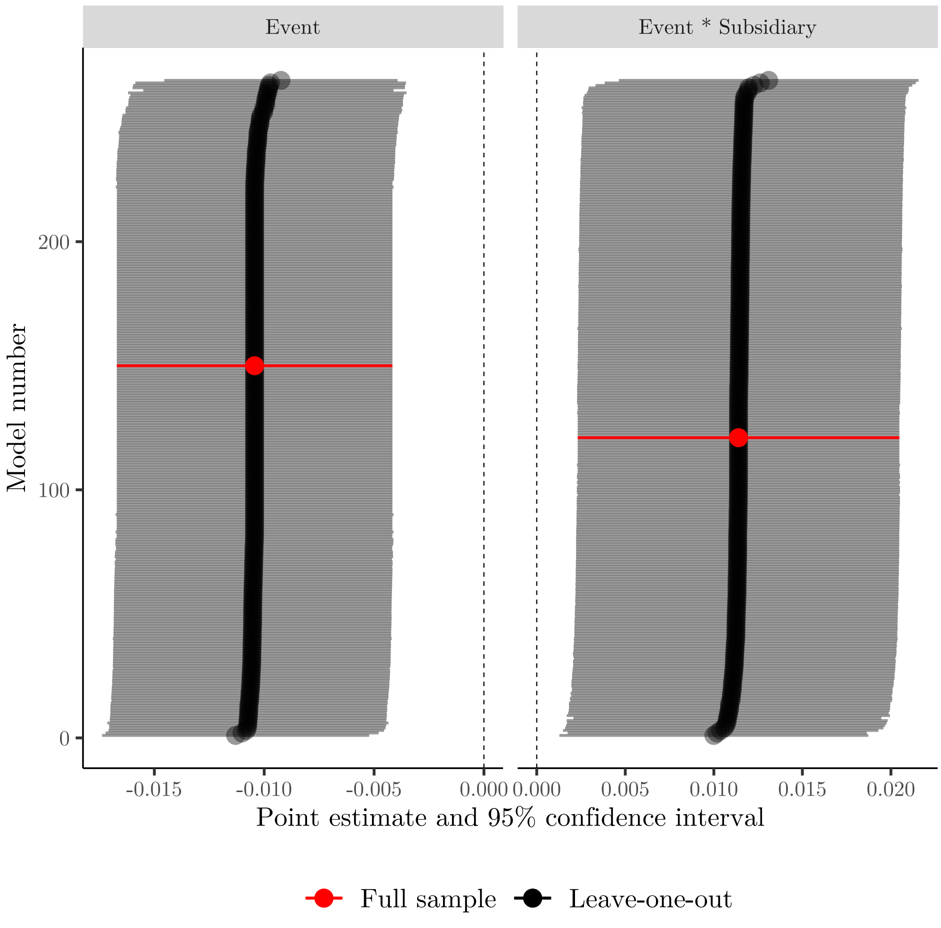


Figure C.6: Replication of model 4 from Table 2, leaving one event out of the dataset at a time. Point estimates and 95% confidence intervals reported refer to the un-interacted *Event* term and to the interaction term *Event* × *Subsidiary*. Red coefficients represent full-sample estimates from the main text.



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 10-days before and 10-days after the *Event*. This verifies results do not hinge on my arbitrary choice for the length of the time window, a conclusion that can also be drawn from the event analysis presented earlier (*e.g.* Figure 7). Results in Table C.1 from the same sparse and full models estimated above are consistent with my expectations. In a further test, I restrict *event window* data to the interval $[day - 10,\ day\, 0]$, to make sure my binary treatment variable only compares *Abnormal Returns* on the day of the *Event* to the immediate preceding days. Results are consistent with earlier findings (Table C.2).

Table C.1: 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	-0.008**	-0.009***	-0.006*	-0.007***	-0.008**	-0.009***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Event × Subsidiary	0.010*	0.011***				
	(0.004)	(0.003)				
Event × Ownership			0.004	0.005*		
			(0.003)	(0.002)		
Event × Wholly-owned Subsidiary					0.013*	0.014***
					(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary					0.006	0.007+
					(0.005)	(0.004)
Subsidiary	0.001	-0.015				
	(0.001)	(0.009)				
Ownership			0.000	-0.008		
			(0.000)	(0.005)		
Wholly-owned Subsidiary					0.000	-0.013
					(0.001)	(0.009)
Non-wholly-owned Subsidiary					0.001	-0.015
					(0.001)	(0.009)
Abnormal Returns (t–1)		-0.126***		-0.126***		-0.126***
		(0.017)		(0.017)		(0.017)
(Intercept)	-0.001	0.005	-0.001+	0.005	-0.001	0.005
	(0.001)	(0.007)	(0.001)	(0.007)	(0.001)	(0.007)
Event FE		Yes		Yes		Yes
Num.Obs.	3579	3422	3579	3422	3579	3422
R2	0.005	0.113	0.003	0.112	0.005	0.114
R2 Adj.	0.004	0.038	0.003	0.037	0.004	0.039
F	5.594	1.513	4.115	1.490	3.830	1.515

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

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 the Event and the event day

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.008**	-0.009***	-0.007*	-0.007***	-0.008**	-0.009***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Event × Subsidiary	0.009*	0.010**				
	(0.005)	(0.003)				
Event × Ownership			0.004	0.004*		
			(0.003)	(0.002)		
Event × Wholly-owned Subsidiary					0.012*	0.012**
					(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary					0.007	0.007+
					(0.005)	(0.004)
Subsidiary	0.001	-0.014				
	(0.001)	(0.012)				
Ownership			0.000	-0.007		
			(0.001)	(0.006)		
Wholly-owned Subsidiary					0.001	-0.015
					(0.001)	(0.012)
Non-wholly-owned Subsidiary					0.001	-0.014
					(0.001)	(0.012)
Abnormal Returns (t–1)		-0.169***		-0.170***		-0.169***
		(0.025)		(0.025)		(0.025)
(Intercept)	-0.001	0.006	-0.001	0.006	-0.001	0.006
	(0.001)	(0.009)	(0.001)	(0.009)	(0.001)	(0.009)
Event FE		Yes		Yes		Yes
Num.Obs.	1914	1822	1914	1822	1914	1822
R2	0.009	0.196	0.007	0.194	0.010	0.197
R2 Adj.	0.008	0.059	0.005	0.056	0.008	0.059
F	6.022	1.428	4.260	1.408	3.936	1.428

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

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 (Table C.3). I also report event-analysis estimates using a sparse and full model (Figures C.7 and C.8).

Table C.3: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Event window data limited to events with precise counterfactual estimation

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.009*	-0.011***	-0.008*	-0.010***	-0.009*	-0.011***
	(0.004)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)
Event × Subsidiary	0.013**	0.014***				
	(0.005)	(0.003)				
Event × Ownership			0.007**	0.008***		
			(0.003)	(0.002)		
Event × Wholly-owned Subsidiary					0.013*	0.014***
					(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary					0.013**	0.014***
					(0.005)	(0.004)
Subsidiary	-0.001+	-0.010*				
	(0.000)	(0.005)				
Ownership			0.000+	-0.010*		
			(0.000)	(0.005)		
Wholly-owned Subsidiary					-0.001	-0.010*
					(0.001)	(0.005)
Non-wholly-owned Subsidiary					-0.001	-0.001
					(0.001)	(0.005)
Abnormal Returns (t–1)		-0.017		-0.017		-0.017
		(0.011)		(0.011)		(0.011)
(Intercept)	0.000	0.001	0.000	0.001	0.000	0.001
	(0.000)	(0.004)	(0.000)	(0.004)	(0.000)	(0.004)
Event FE		Yes		Yes		Yes
Num.Obs.	9168	8542	9168	8542	9168	8542
R2	0.002	0.033	0.002	0.032	0.002	0.033
R2 Adj.	0.002	0.005	0.002	0.005	0.002	0.005
F	7.550	1.187	6.461	1.169	4.533	1.182

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

Figure C.7: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Data limited to events with precise counterfactual estimation. Full model

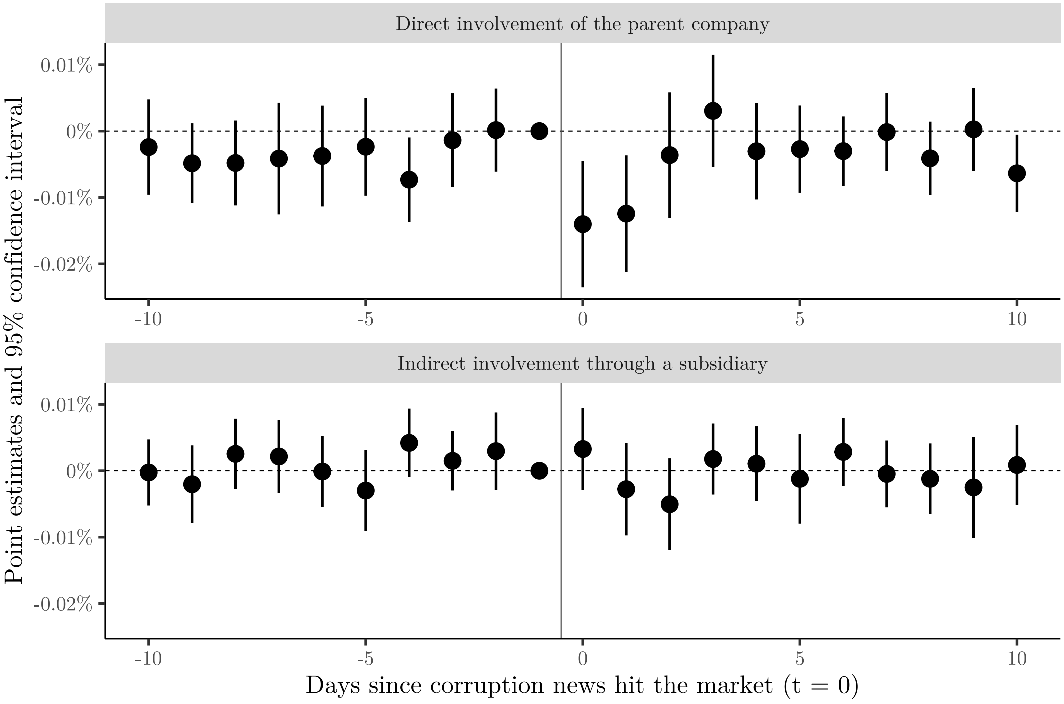
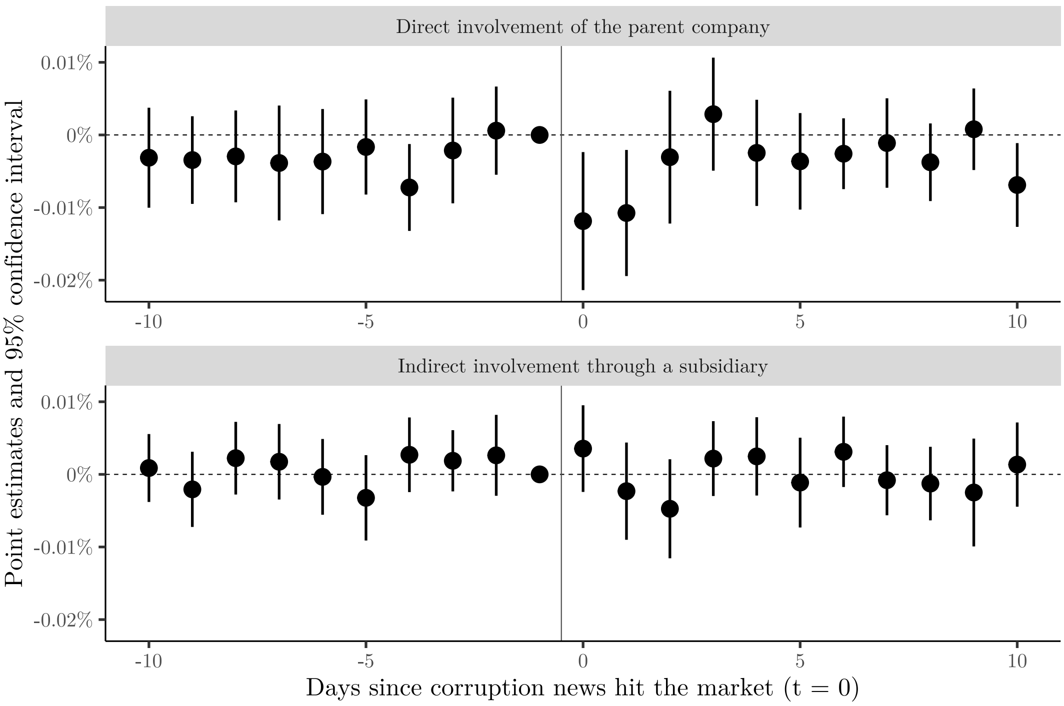


Figure C.8: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Data limited to events with precise counterfactual estimation. Sparse model



D Typologies of indirect involvement

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

	(1)	(2)	(3)	(4)
Event	-0.007*	-0.009**	-0.009**	-0.009***
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Ownership	0.005+	0.005+	0.005+	0.005**
	(0.003)	(0.003)	(0.003)	(0.002)
Ownership	0.000	0.000	0.000	-0.002
	(0.000)	(0.000)	(0.000)	(0.003)
Abnormal Returns (t–1)		-0.021	-0.026	-0.053***
		(0.033)	(0.032)	(0.010)
(Intercept)	0.000	0.000	0.004	0.001
	(0.000)	(0.000)	(0.003)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	10351	9670	9670	9670
R2	0.001	0.002	0.008	0.037
R2 Adj.	0.001	0.002	0.005	0.010
F	3.847	4.980	3.369	1.357

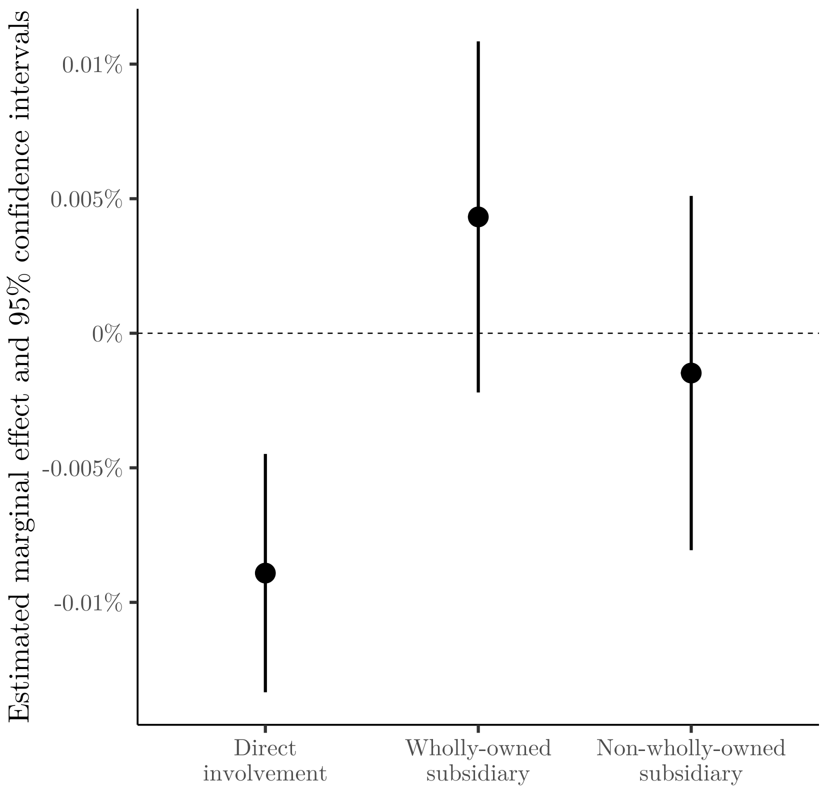
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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

	(1)	(2)	(3)	(4)
Event	-0.009**	-0.011***	-0.011***	-0.010***
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Wholly-owned Subsidiary	0.013*	0.015*	0.015*	0.014***
	(0.006)	(0.006)	(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary	0.007	0.009	0.008	0.009*
	(0.005)	(0.006)	(0.006)	(0.004)
Wholly-owned Subsidiary	0.000	0.000	-0.001	-0.011+
	(0.001)	(0.001)	(0.001)	(0.006)
Non-wholly-owned Subsidiary	0.000	0.000	0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.006)
Abnormal Returns (t–1)		-0.021	-0.026	-0.052***
		(0.033)	(0.032)	(0.010)
(Intercept)	0.000	0.000	0.004	0.001
	(0.000)	(0.000)	(0.003)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	10351	9670	9670	9670
R2	0.002	0.003	0.008	0.038
R2 Adj.	0.001	0.002	0.006	0.010
F	3.516	4.466	3.417	1.376

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

Figure D.1: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly, through a wholly-owned subsidiary, or through a majority-owned subsidiary. Results from sparse model of Table D.2



E Analysis: OLS-estimated synthetic counterfactuals

This section replicates the main results when substituting LASSO-estimated counterfactuals with OLS-estimated ones.

Table E.1: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. OLS-estimated synthetic counterfactuals

	(1)	(2)	(3)	(4)
Event	-0.008*	-0.009**	-0.009**	-0.009***
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Subsidiary	0.009+	0.010*	0.010*	0.010**
	(0.005)	(0.005)	(0.005)	(0.004)
Subsidiary	0.000	0.000	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.007)
Abnormal Returns (t–1)		-0.036	-0.041	-0.077***
		(0.034)	(0.034)	(0.011)
(Intercept)	0.000	0.000	0.004	0.001
	(0.000)	(0.000)	(0.004)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	9698	8681	8681	8681
R2	0.001	0.003	0.009	0.047
R2 Adj.	0.001	0.002	0.006	0.017
F	3.684	6.259	3.437	1.574

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

Figure E.1: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly or through a subsidiary. OLS-estimated synthetic counterfactuals. Results from model 1 of Table E.1

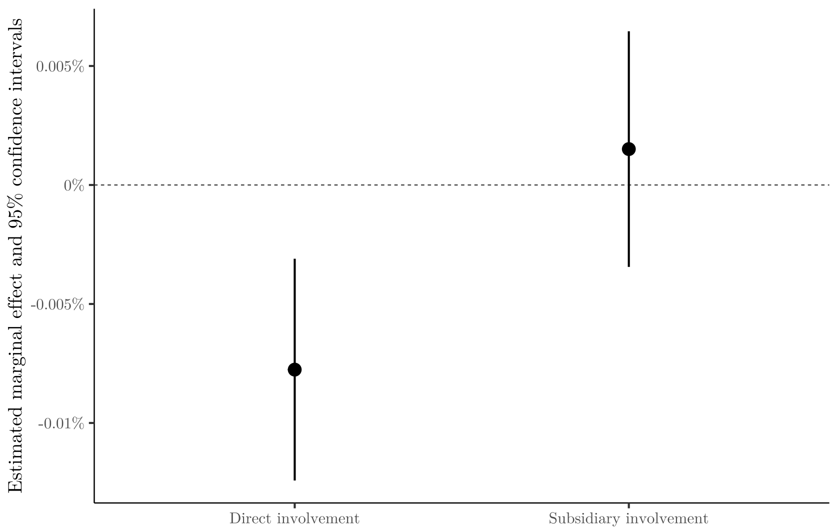


Figure E.2: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full model. Full *event window*. OLS-estimated synthetic counterfactuals

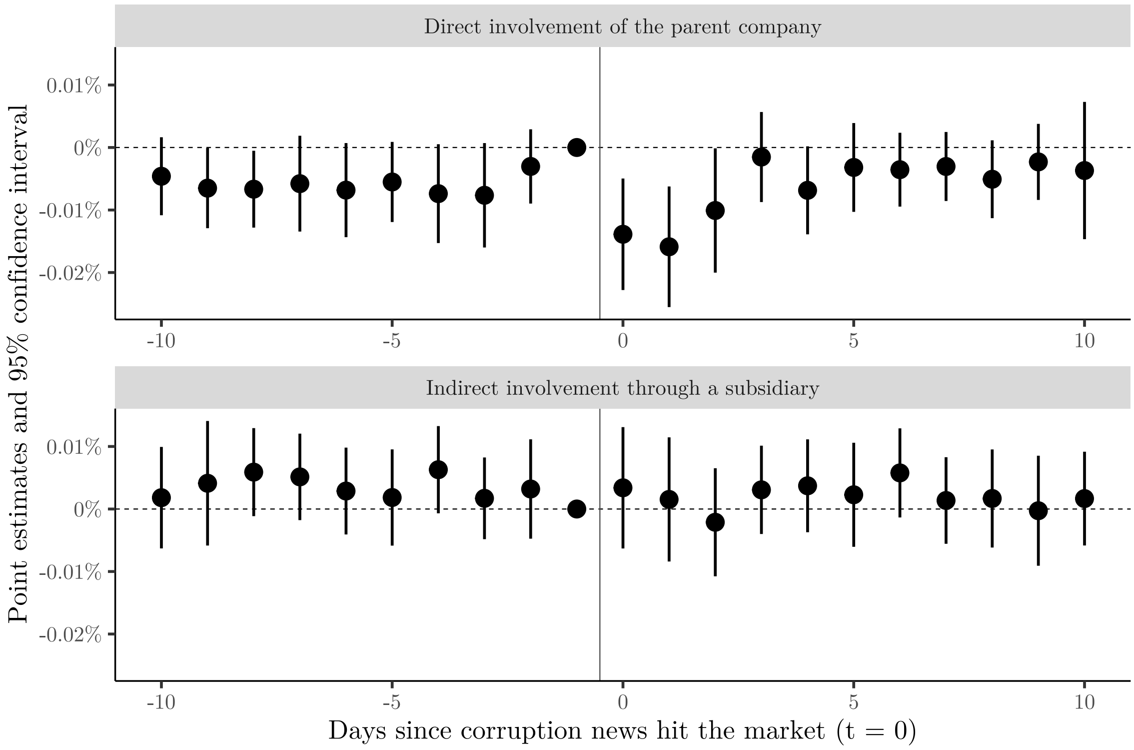


Figure E.3: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Sparse model. Full *event window*. OLS-estimated synthetic counterfactuals

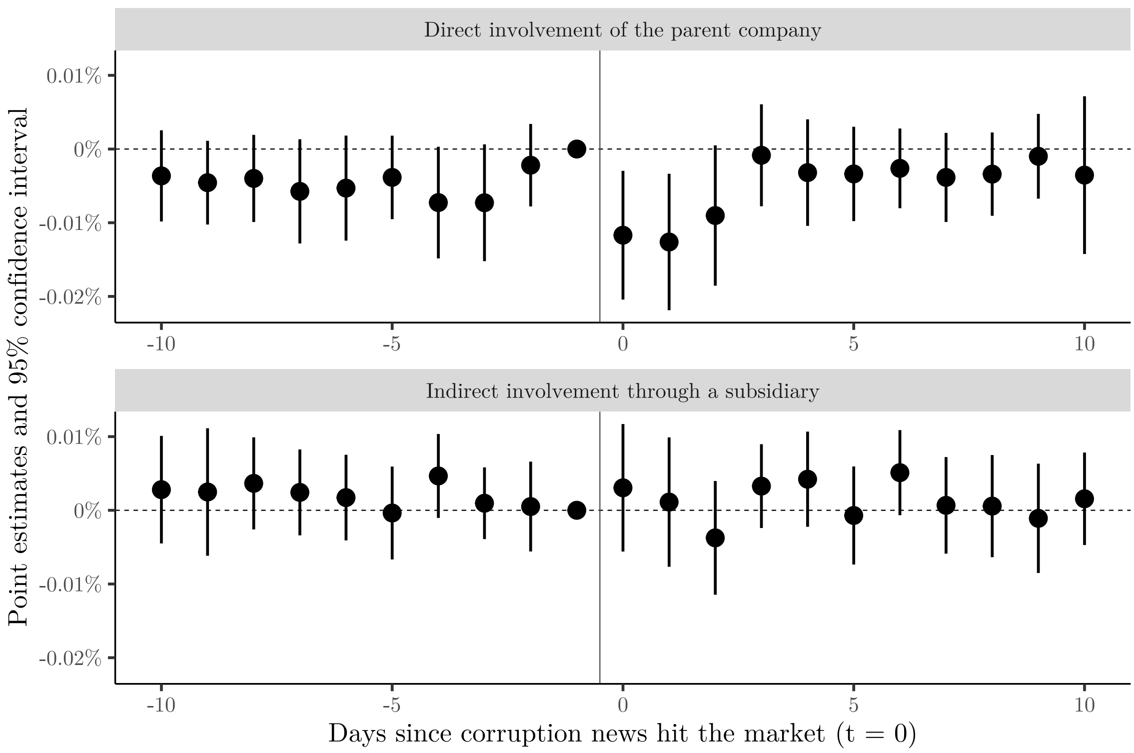


Table E.2: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Continuous *Ownership* measure. OLS-estimated synthetic counterfactuals.

	(1)	(2)	(3)	(4)
Event	-0.006+	-0.008*	-0.008*	-0.008**
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Ownership	0.004	0.005	0.005	0.005*
	(0.003)	(0.003)	(0.003)	(0.002)
Ownership	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.003)
Abnormal Returns (t–1)		-0.037	-0.041	-0.077***
		(0.034)	(0.034)	(0.011)
(Intercept)	0.000	0.000	0.004	0.000
	(0.000)	(0.000)	(0.004)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	9698	8681	8681	8681
R2	0.001	0.002	0.008	0.047
R2 Adj.	0.001	0.002	0.006	0.017
F	2.685	5.369	3.262	1.563

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

Figure E.4: Marginal effect 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 model 1 in Table E.2. OLS-estimated synthetic counterfactuals

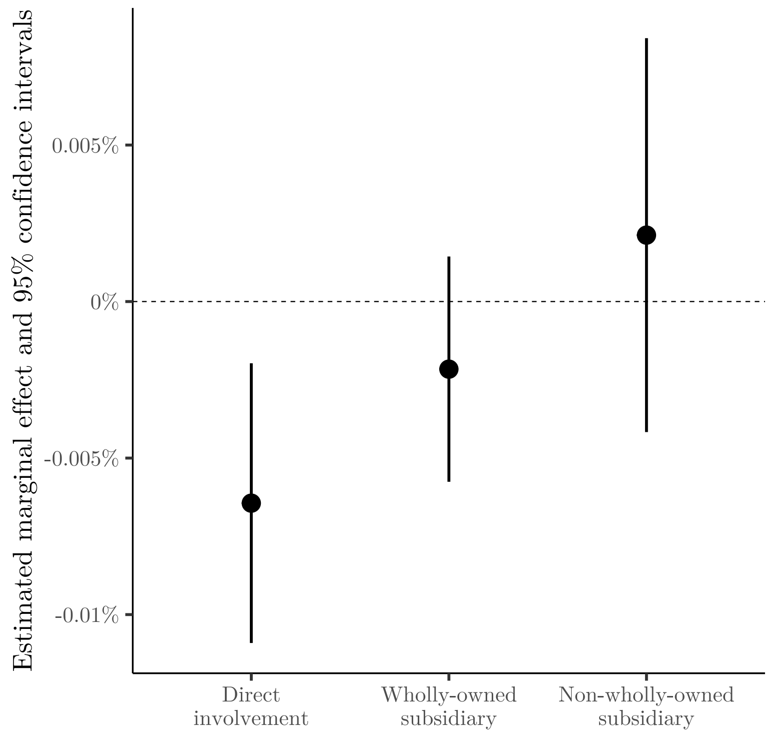


Table E.3: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Discrete *Ownership* measure. OLS-estimated synthetic counterfactuals

	(1)	(2)	(3)	(4)
Event	-0.008*	-0.009**	-0.009**	-0.009***
	(0.003)	(0.003)	(0.003)	(0.002)
Event × Wholly-owned Subsidiary	0.012+	0.013*	0.013*	0.012**
	(0.006)	(0.006)	(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary	0.007	0.008	0.007	0.008+
	(0.006)	(0.006)	(0.006)	(0.004)
Wholly-owned Subsidiary	-0.001	-0.001	-0.001+	-0.010+
	(0.001)	(0.001)	(0.001)	(0.006)
Non-wholly-owned Subsidiary	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.007)
Abnormal Returns (t–1)		-0.036	-0.041	-0.077***
		(0.034)	(0.034)	(0.011)
(Intercept)	0.000	0.000	0.004	0.001
	(0.000)	(0.000)	(0.004)	(0.004)
Year FE			Yes	
Event FE				Yes
Num.Obs.	9698	8681	8681	8681
R2	0.001	0.003	0.009	0.047
R2 Adj.	0.001	0.002	0.006	0.017
F	2.423	4.426	3.275	1.570

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

Figure E.5: Marginal effect 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 model 1 in Table E.3. OLS-estimated synthetic counterfactuals