The Shield of Ownership: The Limits of Market Sanctions Against Corruption*

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Abstract

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

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

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Breaking news often reveal transnational schemes of political corruption involving multinational firms and elites. The 2016 and 2021 Panama and Pandora Papers unveiled corruption, tax evasion, and money laundering implicating large corporations and famous politicians. The Brazilian "Lava Jato" anti-corruption investigation resulted in the conviction of the CEO of Odebrecht (2016) and President Lula da Silva (2017, overturned in 2021). Further back, in 1975, the US Senate-lead Watergate investigation revealed that weapon producer Lockheed had bribed government officials in US allies, a scandal that caused the resignation or conviction of presidents and prime ministers in Italy and Japan. Political scientists have studied the consequences of similar corruption scandals for involved politicians (Cheng-Matsuno and Berliner, 2024; Doria Vilaça, Morucci and Paniagua, 2025; Ferraz and Finan, 2008; Kelly and Tilley, 2024) and implicated firms (Culpepper, Jung and Lee, 2024; Woll, 2023).

How can public regulators prevent domestic firms from engaging in political corruption abroad? Although corruption scandals induce demands for stricter anti-corruption regulations (St-Georges, Arel-Bundock, Blais and Mendoza Avina, 2023), authorities typically sanction foreign corporate corruption indirectly: they largely outsource it to financial markets. News about firms' misconduct abroad generate negative market responses (Breitinger and Bonardi, 2019). Markets thus sanction and discipline foreign corporate misbehaviors, in a way enforcing and reinforcing state policies (Morse, 2019). When settling allegations, then, regulators take into account that these sanctions exist, mitigating the severity of law-enforcement to avoid sentencing an enterprise to death (Garrett, 2014).

In this paper, I illustrate the limits of the strategy to outsource anti-corruption regulation to markets. I show that markets fail to meaningfully sanction corruption when multinationals are implicated in corruption *indirectly*, i.e. via a responsible subsidiary. Corporations typically use their fragmented legal structures across subsidiaries to further misconduct (Findley, Nielson and Sharman, 2015). Multinational companies (MNCs) can pay bribes abroad, and extract rents, using foreign subsidiaries (Malesky, Gueorguiev and Jensen, 2015; Zhu, 2017), which they can also purpose to launder money (Cooley and Sharman, 2017). I argue that fragmentation of ownership also insulates a company from negative financial consequences generated by news of misconduct, thus undermining the effectiveness of anti-corruption market discipline. Crucially, I show that this is not driven by investors' unawareness of corporate ties, but merely by their profit-seeking motivations.

If the parent company is directly involved in revelations of political corruption abroad, investors who own its stocks are concerned that the firm might generate lower profit as a result of law enforcement costs (monetary settlements, fines, and legal expenses) and negative publicity. Because dividend repayment to stockholders depends on profit, investors decide to sell their equities. Increase in the supply of stocks is also met by a shrinkage in demand, as prospective shareholders direct their purchases towards safer assets. The result is a reduction in price that causes the company to experience abnormal financial penalties, *i.e.* losses that it would not have faced, had corruption not been revealed.

If, instead, a company is implicated in corruption indirectly (namely, through a subsidiary) investors' expectations about repayment of dividends are not negatively impacted. In this case, legal costs and negative publicity affect the subsidiary's operations and not the parent company (if not indirectly). Under most corporate regulations (including the one that I study in this paper) the parent is liable for a subsidiary's misconduct only if it is demonstrable that it had control and knowledge of wrongdoings.[1] Subsidiary misconduct thus represents a much less likely case to translate into costs for the parent. As such, market penalties for the parent company do not materialize. Profit-seeking motivations thus prevent markets to perform their regulatory function when corporate ownership structures limit legal liability. Subsidiaries insulate parent companies from the market effects of law enforcement, thus preventing meaningful financial losses in the wake of breaking news reporting corruption.

I rely on an event study to test my argument. The design identifies the effect of unexpected events on companies' daily returns on stock prices by imputing synthetic counterfactual observations. Political scientists have used it to study the effect of such events as elections (Aklin, 2018), court rulings (Kucik and Pelc, 2016; Voeten, 2025), regulations (Wilf, 2016), industrial policy announcements (Bayer, Crippa and Genovese, 2025), and sanctions (Crippa, Kalyanpur and Newman, 2026). I adopt this design to study the heterogeneous effects generated by revelations of political corruption on stock returns of an involved parent company, depending on whether the company was implicated directly or indirectly (*i.e.*, through a subsidiary). In other words, I study how the involved entity's position in the ownership structure moderates the size of the corruption sanction that financial markets impose.

I apply this design in the case of revelations of violations of the US anti-bribery law (the Foreign Corrupt Practices Act, FCPA). I construct a novel dataset reporting the day that allegations that publicly-traded companies violated US anti-corruption were made public.[2] My dataset contains information on 217 distinct publicly-traded companies involved in 263 corruption revelations. For each event in the dataset, I code the position of the responsible entity in its corporate group. I combine this dataset with daily stock prices data for the parent company in the days preceding and following the release of

[1] Merve Bakırcı, "Parent-Subsidiary Liability Within the Scope of Foreign Corrupt Practices Act." *Erdem & Erdem*. February 2021. `https://www.erdem-erdem.av.tr/en/insights/parent-subsidiary-liability-within-the-scope-of-foreign-corrupt-practices-act`.

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

information.

I find that, when parent companies are directly implicated in political corruption, they suffer a statistically significant negative effect on stock prices in the immediate aftermath that allegations are made public. The average company experiences more than \$1 billion abnormal losses per day (in terms of market capitalization) for two trading days after the event. Even two weeks later, cumulative returns to the average company involved directly in corruption are still more than \$4.5 billion lower what could have been expected before the event. In such case of direct involvement in corrupt schemes, markets sanction corruption and reinforce state regulations, imposing strong and sustained penalties that stick to a company's financials. However, I detect no statistically significant effect on the price of the parent company's equities at all when a subsidiary is (allegedly) responsible for bribe payments. I offer several tests to rule out alternative explanations for such heterogeneous effect.

Results paint a cynical picture of regulatory failure. Fragmentation of ownership is not just a way to further and conceal financial misconduct (Findley, Nielson and Sharman, 2015) or to circumvent regulations (Chapman, Jensen, Malesky and Wolford, 2021). It is also a device that shields parent companies from resulting damage, when misconduct is revealed. 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 clear limitation of the regulatory strategy to leverage market discipline to sanction firms (Garrett, 2014). In particular, results show the possibility of moral hazard by firms, who can deliberately engage in illicit activities through their subsidiaries and claim plausible deniability while knowing that stock market penalties will be limited.

The paper advances three scholarly contributions. First, it contributes to the political science literature on anti-corruption policies. Existing studies have largely investigated policies against the demand-side of corruption: bribe-taking (Brierley, 2020; Buntaine and Daniels, 2020; Cheeseman and Peiffer, 2022; Gulzar, Rueda and Ruiz, 2022; Szakonyi, 2023). I contribute to the growing scholarship on policies against the supply-side of corruption: bribe-paying (Brazys and Kotsadam, 2020; Chapman, Jensen, Malesky and Wolford, 2021; Jensen and Malesky, 2018). This literature has illustrated that markets punish corruption but has struggled to distinguish whether corruption is costly in and of itself (due to its inefficiencies) or whether it can generate profitable rents, becoming costly only for its legal consequences (Malesky, Gueorguiev and Jensen, 2015; Rose-Ackerman, 2013). Whichever answer one offers to this important debate informs the extent to which we deem anti-corruption corporate regulations necessary. By holding constant corruption-induced inefficiencies and varying law enforcement costs, my comparison between cases of direct and indirect involvement in corporate corruption indicates that law-enforcement is a significant driver of corruption costs. Strong anti-corruption mandates might therefore be needed to ensure markets discipline corporate crime. More generally, my results suggest that a virtuous circle might exist between state and market discipline: investors help to sanction firms as long as there are regulatory costs, but they stop when such a burden is removed (Crippa, Malesky and Picci, 2025).

Beyond the literature on political corruption, the paper questions whether market sanctions alone can substitute state-based regulations. Political science has long discussed whether state regulations or market mechanisms are more effective at holding global firms accountable (Johns, Pelc and Wellhausen, 2019; Morse, 2022; Ruggie, 2002; Strange, 1996; Vogel, 2008). State regulation can be effective when harmonized under international agreements (Crippa, 2025; Jensen and Malesky, 2018) but states are often reluctant to delegate sovereign regulatory powers to international bodies (Green and Colgan, 2013; Simmons, 2010). Market sanctions could offer a solution, generating pressure on companies and prompting them to self-regulate, complementing or even substituting for state action (Malhotra, Monin and Tomz, 2019; Thrall, 2021). Markets would thus name-and-shame corporations, the same way as civil society does (Acemoglu and Robinson, 2020; Fukuyama, 2016; Simmons, 2000). I show that this logic fails when corporate wrongdoers are successfully insulated by networks of legally distinct entities (as illustrated by Findley, Nielson and Sharman, 2015, 2025).

Finally, the paper contributes to an international relations literature that looks at information and reputation as powerful devices to ensure compliance with international regimes (Simmons, 1998, 2000; Weisiger and Yarhi-Milo, 2015). With its market effects, corporate reputation is presumed to induce respect of international regimes (Ruggie, 2018) when companies are directly responsible for compliance (Findley, Nielson and Sharman, 2015; Jensen and Malesky, 2018). I show that this expectation might be disappointed. Investors' behaviors appear to be elastic to negative publicity, but definitely inelastic when involvement into bad news is delegated to other entities within a corporate group. In this case, corporate ownership seems to shield the parent company's reputation.

These conclusions have a timely policy implication. The current US administration has taken a strong de-regulatory stance *vis-à-vis* corporate (criminal) activity, including corruption,[3] claiming that

[3] In February 2025, President Trump decided to pause and revise enforcement of the FCPA (Crippa, Malesky and Picci, 2025). The Department of Justice (DOJ)'s June 2025 revised FCPA enforcement guidelines offer leniency (and even declination of enforcement) to companies who will self-disclose corruption (see "FCPA Is Back in Style!" *Eversheds Shuterland*. August 15, 2025: https://www.eversheds-sutherland.com/en/czech-republic/insights/fcpa-is-back-in-style). This aligns with the new DOJ voluntary self-disclosure policy against corporate crime at large (see "9-47.120 - Criminal Division Corporate Enforcement and Voluntary Self-Disclosure Policy." *Department of Justice*. May 12, 2025: https://www.justice.gov/d9/2025-05/revised_corporate_enforcement_policy_-_2025.05.11_-_final_with_flowchart_0.pdf).

reducing the regulatory burden for American business would promote US economic interests.[4] Such deregulatory trend risks diffusing to Trump's "admirers" in other countries (Gilardi, 2010). In certain conservative circles, it could resurrect the hope for market discipline to substitute for state regulation (Allison, 2014). My results question the extent to which such lenient approach to corporate crime will generate any significant financial penalties. If states leave it to markets to discipline illicit corporate behavior, misconduct risks going unsanctioned and undeterred.

How Subsidiary Ownership Insulates Firms from Corruption Penalties

Research has shown that corruption in foreign markets creates costs for multinationals (Cuervo-Cazurra, 2006; Jain, Kuvvet and Pagano, 2017) but a significant debate remains about the causes of such costs. On the one hand, foreign corrupt deals are uncertain and virtually impossible to enforce for a multinational. This introduces inefficiencies that increase costs to MNCs (Lambsdorff, 2007; Rose-Ackerman, 2013). On the other hand, corrupt dealings can offer monopolistic positions to multinationals in foreign markets, excluding domestic or foreign competitors and offering rent opportunities (Malesky, Gueorguiev and Jensen, 2015; Zhu, 2017). The cost of foreign corruption for multinationals would thus accrue from its legal consequences at home (Kaufmann and Vicente, 2011; Karpoff, Lee and Martin, 2017). Foreign corruption would thus be deterred only insofar as MNCs face a significant risk of legal repercussions at home for such overseas misconduct.[5] Although they are theoretically distinct (and support different judgments on the need for strong anti-corruption mandates), empirical research has found it hard to distinguish inefficiencies *vs* legal costs (Crippa, Malesky and Picci, 2025).

The grand part of the legal costs that MNCs face for foreign corruption at home is not imposed explicitly by law-enforcement authorities: rather, it stems from market responses to corruption news (Sampath, Gardberg and Rahman, 2018). Negative news can create financial losses for a company (Breitinger and Bonardi, 2019). At any given time, the price of a firm's share reflects all information available to investors about its profitability (Fama, 1970). Profitability depends on expectations of periodic dividend repayment of profits and future stock value. Expectations of lower (higher) profitability lead investors to sell (purchase) stocks, causing the share price to depreciate (appreciate). Information

[4] See "DOJ White Collar Enforcement Six Months into the Trump Administration." *Covington*. Summer 2025: https://www.cov.com/en/news-and-insights/insights/2025/08/doj-white-collar-enforcement-six-months-into-the-trump-administration.

[5] Of course, MNCs might also risk legal repercussions *abroad*, wherever corruption occurs. However, countries where MNCs pay bribes often have very little to no anti-corruption enforcement, which significantly limits the deterrence of such legal instruments (Svensson, 2005). Weak anti-corruption enforcement in such contexts partly spurred the US first, and OECD countries then, to adopt policies to prosecute foreign bribery by their own firms (Picci, 2024).

from corporate misconduct can cause financial losses if it leads investors to expect a lower stock price or smaller dividends (Capelle-Blancard and Petit, 2019). For instance, reputational damage following news of human rights violations might cost companies in natural resource extraction about 100 million US dollars in losses (Kreitmeir, Lane and Raschky, 2024).

In the case of news of political corruption, this negative effect is caused by material and reputational concerns. Material concerns about poor future economic performances lead stockholders to restructure their portfolios. What causes such material concerns? Again, existing research points to both drivers presented above. First, corruption can introduce uncertainty that weakens prospects of profits (Lambsdorff, 2007). Second, news of legal actions create expectations of regulatory costs, such as fines and monetary settlement (Garrett, 2014). Investors might also be concerned that corruption information weakens the firm's reputation. Expecting negative market reactions to the same pressures, they might attempt to anticipate and mitigate losses, deciding to sell shares and thus initiating a self-fulfilling prophecy.

However, companies often commit wrongdoing by splitting illicit transactions across complex layers of subsidiaries and anonymous shell companies. Similar structures facilitate bribery (Findley, Nielson and Sharman, 2015). Corporate legal structures can be extremely complex. A company sitting at the top of a corporate group (the "parent" company) can own, directly or indirectly, shares of hundreds of subsidiaries. Degrees of ownership can also vary. A parent company can wholly-own a subsidiary, it can be its majoritarian owner (*i.e.*, the company owning the largest percentage of shares), or a minority shareholder. Mergers and acquisitions further complicate these networks.

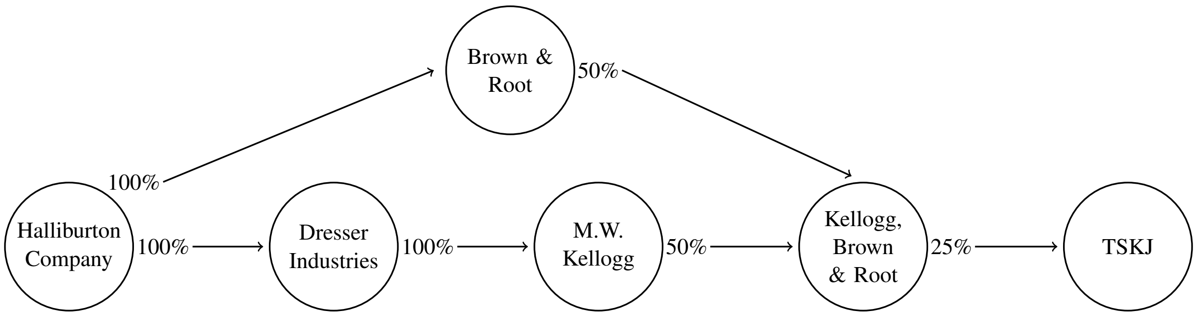


Figure 1: Halliburton Company's stakes in the TSKJ joint venture. Circles represent companies, arrows indicate ownership relations, and percentages represent degrees of ownership.

Figure 1 offers a real example by reconstructing the stakes held by the US extractive company Halliburton in a consortium called TSKJ, a Portuguese oil services joint venture. The company was formed in 1994 by the French Technip S.A., the Italian Snamprogetti B.V. (incorporated in The Netherlands), the US Kellogg Brown & Root (KBR), and the Japanese JGC Corporation. Each company owned 25% of TSKJ's shares. Halliburton held stakes in the consortium indirectly since 1998, when it acquired

Dresser Industries and formed KBR by joining its subsidiary Brown & Root with Dresser's subsidiary M.W. Kellogg. Similar structures are ideal for furthering nefarious transactions. TSKJ itself became infamous for funnelling hundreds of million US dollars in bribes to Nigerian public officials until at least 2004 in order to secure contracts for providing the infrastructure necessary to extract and refine liquified gas on Bonny Island, in the Niger Delta region (Lacey, 2006).

I claim that the opacity of these corporate structures is not only ideal to conceal wrongdoing to public prosecutors: it also gets in the way of the process by which markets would discipline and sanction malfeasance such as corruption. When a firm is directly implicated in illicit conduct, markets impose sanctions by the mechanism indicated above, because investors are concerned about future profitability. When, instead, a firm is not directly implicated in misconduct (but through a subsidiary), market penalties do not materialize. Subsidiary ownership does not cause corruption to be any more efficient. Whether a subsidiary or a parent engages in bribery, corrupt contracts are difficult to enforce and generate costs (Lambsdorff, 2007). However, subsidiary ownership reduces the risk of legal consequences at home and investors respond to such lower risk.

Market penalties do not materialize against a parent company, for illicit behavior of a subsidiary, because subsidiary ownership allows the parent to claim plausible deniability for the misconduct. Under most corporate regulations, a parent is liable for its subsidiaries' misconduct only if it is demonstrable that it controls them *and* has knowledge of the misbehavior.[6] Proving that a parent has knowledge of a subsidiary's misconduct is a very complex matter. As a result, authorities typically do not claim parent liability at the onset of a case. In fact, investigation on subsidiaries' misconduct often end up not involving the parent *at all*, if not as a provider of evidence, precisely on such grounds of plausible deniability (Garrett, 2014). Anecdotes show that, in the wake of breaking news about a subsidiary's misconduct, parents often signal their distance from the implicated company and offer cooperation with the authorities to swiftly resolve the matter.[7] These risk-mitigation strategies reassure shareholders about parents' extraneousness to the wrongdoings.

[6]For the case of the anti-corruption policy studied here, see Merve Bakırcı, "Parent-Subsidiary Liability Within the Scope of Foreign Corrupt Practices Act." *Erdem & Erdem*. February 2021. https://www.erdem-erdem.av.tr/en/insights/parent-subsidiary-liability-within-the-scope-of-foreign-corrupt-practices-act.

[7]For instance, between 2007 and 2009 the Italian oil services firm Saipem SpA was investigated by American authorities for violating the US anti-bribery policy, under suspicion that the firm had secured contracts in Algeria by offering $215 million in bribes to public officials close to the then Minister of Finance—See: https://www.reuters.com/article/eni-algeria-idUSL5N0BBAUX20130211. The Italian oil major ENI SpA retained around 43% of the total shares of Saipem at the time. When the scandal broke out, ENI distanced itself from its subsidiary by issuing an immediate press release where it underscored the independence of Saipem and it offered US authorities full cooperation—See: https://www.eni.com/it-IT/media/comunicati-stampa/2013/02/eni-dichiara-lestraneita-di-amministratori-e-dirigenti-dalle-vicende-indagate-sulle-attivita-di-saipem-in-algeria.html?lnkfrm=asknow.

Such plausible deniability reduces greatly the extent to which subsidiary misconduct undermines the profit expectations of the parent company's shareholders. Because, legally, the parent's and subsidiary's activities are distinct, investors rationally assess expected profits of the parent not to be at stake. Until otherwise proved, investors conclude, the parent is overall conducting business operations above board. As such, investors fail to update their pricing of the parent's stocks as a consequence of corporate wrongdoing through a subsidiary. This could appear like an efficient attribution of responsibility from a regulatory perspective: investors would impose sanctions on companies only when they bear direct responsibility on the alleged misconduct. It is nevertheless very concerning, given that fragmented ownership is pivotal to further financial wrongdoing (Sharman, 2010) and that news of misconduct by a subsidiary at a minimum imply that compliance programs by the parent company are not working as they should (Demsetz and Lehn, 1985).

More cynically, some investors might even take information about political corruption in foreign markets as a positive signal that a multinational is managing to secure positions by means of bribing that would allow it to extract rents (Malesky, Gueorguiev and Jensen, 2015; Zhu, 2017). From the point of view of the rents it offers, corruption can be welfare-enhancing for investors because it increases prospects of profits, hence dividends. In the case of similar transactions shareholders might evaluate that, if a firm is to pay bribes to secure such rents, it better do so via a subsidiary so as to remain protected from legal actions.

I thus expect market discipline to break down when corruption involves a parent company only indirectly, due to investors' profit-seeking rationale. It is not obvious that market sanctions should stop at the subsidiary level, especially given evidence that political risk can spread through investment networks (Crippa, Kalyanpur and Newman, 2026). If such spillovers fail to appear, this would suggest that corporate ownership structures effectively shield parent firms from their subsidiaries' corruption.

An alternative explanation is that corporate ownership does not create *plausible deniability*, as I claim, but rather *information scarcity* that gets in the way of market discipline and insulates parent companies from negative market responses to their subsidiaries' misconduct. Subsidiary ownership can cause two forms of information scarcity. First, subsidiary incorporation can generate *ownership misinformation* between the parent company and its shareholders. Put plainly, investors might not know that a firm is a subsidiary of a certain parent. Second, subsidiary incorporation might generate *misconduct misinformation*. Market actors who process, synthesize, and communicate firms' risk exposure to misconduct might fail at providing such information to markets in case of subsidiary responsibility. Here, I briefly present these alternative mechanisms. I provide evidence against both at the end of the empirics section.

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

Second, parent firms might be insulated from a subsidiary due to a regulatory failure of the market actors who are responsible for providing investors with information about firms' risk exposure to misconduct. Investors have to process vast amount of information to guide their investment decisions. They often rely on index providers to synthesize information (Cormier and Naqvi, 2023), instead of consuming it directly. Information about companies' exposure to the risk of misconduct is offered by index providers that measure firms' exposure to the risk of poor governance—for instance, the G pillar of indexes measuring firms' environmental, social, and governance (ESG) rating. If these index providers fail at updating parent firms' rating following a subsidiary's violation, investors might not acquire the information they need in order to impose market penalties. This regulatory failure would be akin to that of credit rating agencies in the context of the 2007 financial crisis (Mullard, 2012).

I argue, instead, that subsidiary ownership insulates a parent company from corruption financial penalties not because of lack of information, but because the parent can claim plausible deniability to mitigate legal enforcement costs. To buttress such a claim, I provide evidence against these alternative mechanisms that would cause ownership to insulate the parent. To sum up, I expect that parent company's stock prices suffer from news of law enforcement when a firm is directly implicated in political corruption. Conversely, a company involved in corruption only indirectly—*i.e.*, through an owned subsidiary—will not suffer penalties on stock markets. When compared to a case of direct involvement, corporate ownership therefore *insulates* the parent company from the financial consequences of corruption.

The Case: Violations of the US FCPA

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

The DOJ and the SEC investigate into alleged FCPA violations by a company when information on potential misconduct emerges.[8] However, very rarely companies alleged of FCPA violations go to court. The long time frame of trials would expose companies to prolonged negative publicity. In order to minimize such damage, companies usually settle allegations with prosecutors out of court, through non-prosecution agreements (NPAs) or deferred prosecution agreements (DPAs).[9]

Usually, agencies communicate to the public about investigations through press releases[10] only after allegations have been settled. At this point, however, companies themselves have typically already long disclosed corruption information. Under 1930s US law regulating securities, US-traded companies must disclose any information of material relevance for investors. This includes SEC or DOJ investigations into alleged FCPA misconduct. Companies disclose such information to investors by filing reports to the SEC itself which, since 1993, must be submitted on the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system,[11] a public platform designed to facilitate information flow from companies

[8]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. See: https://www.justice.gov/d9/pages/attachments/2020/08/20/fcpa-guide-2020_print-full-downloadable.pdf.

[9]These solutions entail admission of guilt from the company, payment of fines commensurate to the misconduct, pledges to cooperate with authorities on future investigations, and agreements to undertake corporate reform to prevent future misconduct (Garrett, 2014).

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

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

to (prospective) investors. In virtually all cases, investors learn about FCPA investigations from firms' own SEC disclosure forms.

Two reasons make the case ideal for comparing the effects of unexpected news about corporate misbehavior occurring at different levels of a company structure. First, news that US agencies are investigating a company's alleged violation of the FCPA are released in a rather consistent scheme. Information is typically released by companies themselves before press releases by public agencies. Moreover, information is disclosed by filing mandatory SEC forms which are available to the general public of investors. The US system thus incorporates a consistent flow of information from the firm to the market. Similar arrangements are not in place in other legal systems. By focusing on violations of the US FCPA I can therefore study the effect of unexpected news on financial markets while holding constant heterogeneity that pertains to different legal arrangements.

A second reason justifies the case choice. Whereas selections of companies into the group of those involved in cases of corporate corruption is likely endogenous to their market pricing or reputation, the *timing* information is released can be considered plausibly exogenous. The case can then be used as a plausible natural experiment to study market responses to companies' misconduct. Often, companies are forced to release press statements or to file SEC forms informing investors about upcoming investigative reports on alleged involvement into cases of corruption.[12] Other times, anti-bribery investigation by US agencies forces companies to delay periodic SEC filings and to submit notes unveiling allegations of corporate corruption.[13]

Of course, it is possible that firms choose the timing to release negative information strategically so as to minimize market penalties. Financial research has shown seasonal patterns in investors' attention to market-relevant news, for instance showing weaker market responses in the proximity of stock market holidays or Mondays (Wang, Li and Erickson, 1997; Hood and Lesseig, 2017). However, I note the following. If all firms (subsidiaries or parents) strategically announced corruption news at times of market inattention, any market effect of corruption information should be equally drawn towards the null, both for direct and indirect implications into corruption. Strategic selection of such information-release timing should thus bias against finding any market effect *and* equally mute differences in effect

[12]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/.

[13]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.

between direct and indirect involvement. A real threat to inference would emerge if parent companies strategically chose to release information *about indirect involvement into corruption* at times of lower market attention. Against this possibility, in the Appendix I show that cases of direct and indirect implications into corruption do not differ substantively in terms of the timing of information disclosure (Table B.1).

Data

I build a dataset to test my argument starting from information on cases of alleged corporate corruption investigated by US agencies against publicly traded companies. To retrieve this information I draw on data from Stanford's FCPA Clearinghouse[14] and consider reported FCPA investigations until 2021. Conveniently for my purposes, the FCPA Clearinghouse reports data on the very day that FCPA investigations were fist disclosed.

Because I am interested in studying the stock market effects of FCPA violations distinguishing between direct and indirect involvement, I manually reconstruct the ownership structure of each firm involved in a potential FCPA violation. I use Bureau Van Dijk's Orbis data to code the parent company (*i.e.* the corporate group's global ultimate owner) of each firm investigated for violating the FCPA.[15] Next, I keep only records relative to companies whose parent entity's stocks are publicly traded. Availability of stock price data further constrains my analysis to consider only events following the year 2002.[16] These limitations lead me to a final selection of 217 unique parent companies involved in 263 events of alleged violations of the US anti-corruption law.

I code which entity was involved in a corruption revelation, at the time the event was made public, along the corporate ownership chain. I measure whether each company found in violation of the US anti-corruption law is the corporate group's global ultimate parent (*Indirect* = 0), or a subsidiary[17] (*Indirect*

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

[15]Orbis reports detailed information on corporate ownership structures of companies. It also reports shareholder history, that allows to trace ownership structures at the time allegations of misconduct hit the market. I cross-check this information against a range of alternative sources. First, publicly available reports made by US authorities (SEC and DOJ). Second, extensive web searches to confirm the retrieved information. 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). Where Orbis information conflicts with alternative sources, I keep information available from reports by US authorities. Where this is not available, I rely on web searches.

[16]This forces me to discard 8 events where publicly traded firms (or their subsidiaries) were involved in FCPA violations. 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.

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

= 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. Out of the 263 events of corruption I consider, 143 (54%) involved the parent company directly, while 120 (46%) involved it through a subsidiary.

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

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

Table 1 provides a snapshot of my data. For each entry, a firm (*Violation entity*) is alleged to have violated the US FCPA by bribing public officials in a foreign market (*Violation country*).[18] I report the parent company of the involved entity (*Parent Company*), whether the parent was involved in corruption indirectly (*Indirect*), the symbol under which the parent company trades its securities (*Ticker*), and the date information on public investigation was first made public (*Investigation*).

The final data collection step concerns daily stock prices data. I retrieve all stock price and market indexes information from Refinitiv. I obtain data on the stock *Returns*: the percentage change in closing price of a stock at the end of a trading day, with respect to the same value relative to the previous trading day. Finally, I retrieve daily data on stock market indexes. This information serves to construct predictive covariates in the research design outlined in the next section. Given that companies in my dataset trade their equities on different exchanges, I retrieve daily percentage changes in values of 10 market-wide indicators.[19]

Research Design

I adopt an event-study research design to test my expectations that parents suffer financial penalties when they are directly implicated in a regulatory enforcement action, but not when they violate corporate laws through subsidiaries. This empirical strategy is used for estimating market effects of unexpected events

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

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

(Karpoff, Lee and Martin, 2008). Political scientists have used it to assess the effect of international institutions and regulations (Gray, 2009; Wilf, 2016), communications (Genovese, 2021), elections (Aklin, 2018), sanctions (Bayer, Crippa and Genovese, 2025; Crippa, Kalyanpur and Newman, 2026), and international rulings (Bechtel and Schneider, 2010; Kucik and Pelc, 2016; Voeten, 2025).

The design imputes daily synthetic counterfactual *Returns* to each company around an event. It then measures the difference between observed and synthetic counterfactual observations on the day of the event and after, thus estimating an average treatment effect on the treated (ATT) companies' stock prices. To achieve that, one divides daily stock price observations for each company in two time-windows. First, an "estimation window," predating the event (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 corruption event, my event windows start 30 days before and end 30 days after the event (61 days per event). The estimation window of each company begins 210 days before the beginning of its event window.[20]

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

For each event, I model *Returns* employing only the most predictive market-wide indexes, selected via a LASSO procedure. This is done to return more precise counterfactuals over those that would be imputed with models that include all market-wide indexes—such as using ordinary least squares (OLS). The LASSO associates a set of non-negative weights $\mathbf{w_e}$ to each index, which results in the lowest residual sum of squares, hence in the most predictive model (Tibshirani, 1996). Previous event studies have shown its improved performance over OLS market models (Wilf, 2016). In my LASSO estimation, I adopt a five-fold cross-validation procedure for learning the set of most predictive weights specific to each individual event e. I then employ these weights to determine how covariates enter Equation 1 for that specific event. Each model thus represents the best feasible predictor of a company's stock *Returns*, before the unexpected event took place.

$$Returns_{et} = \alpha_e + \mathbf{X_t'} \mathbf{w_e} \hat{\beta}_e + \varepsilon_{et} \;\mid\; t_0 \leq t < t_1 \tag{1}$$

Figure A.1 in appendix shows that the procedure effectively omits least-predictive indicators, whereas it includes more frequently market-wide indexes with higher predictive power. This results in market

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

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

Next, I use estimated market-model coefficients $\mathbf{w_e}\hat{\beta}_e$ to predict daily *Returns* to each company in the event window. Equation 2 represents this second step. *Expected Returns* are effectively counterfactual *Returns* to a company, absent news of malfeasance: they are estimated based on information available before news of misconduct were unexpectedly released. I also compute daily *Cumulative Observed* and *Cumulative Expected Returns* by summing, respectively, daily *Returns* and *Expected Returns* relative to a specific event.

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

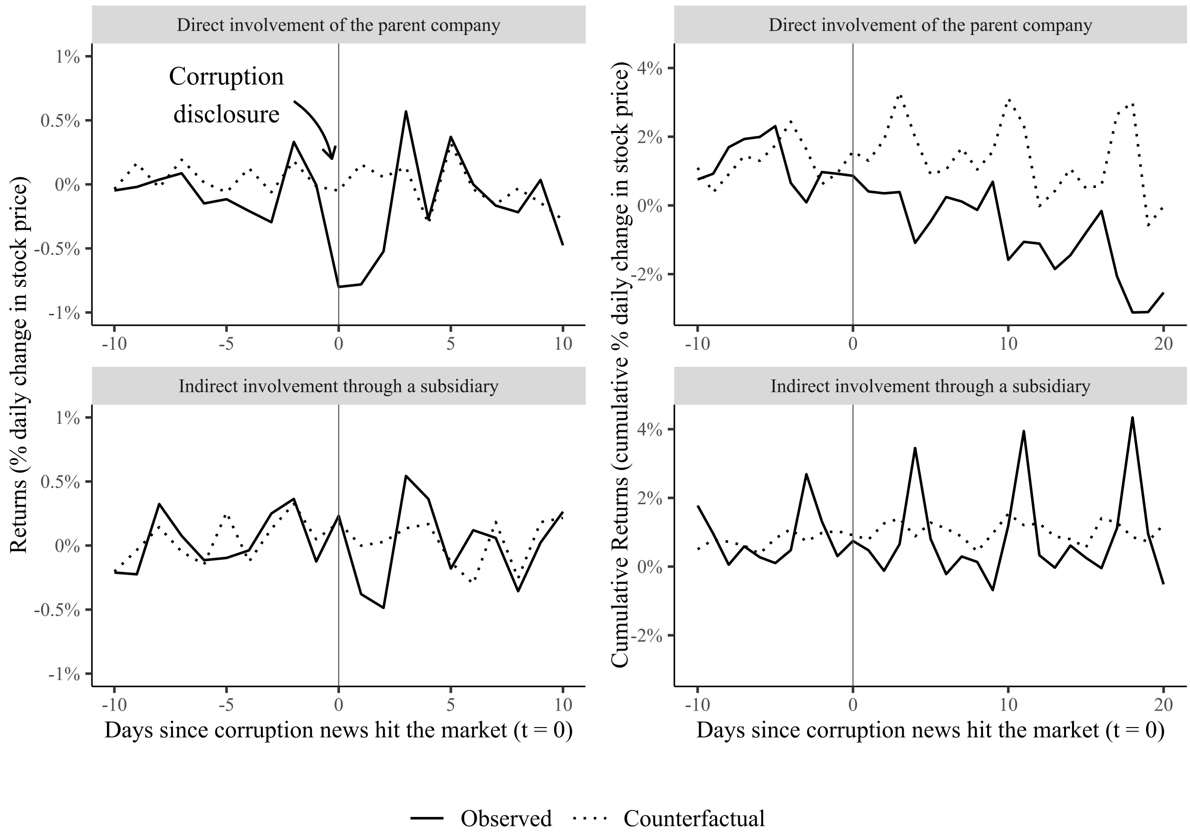
Figure 2 plots the daily average observed and counterfactual *Returns* (left panels) and *Cumulative Returns* (right panels) in the 10 days before and after the corruption disclosure, distinguishing between cases of direct (top panels) and indirect involvement in corruption through a subsidiary (bottom panels). It provides initial evidence in support of my argument. Pre-treatment differences between observed and counterfactual (*Cumulative*) *Returns* are small, which indicates the good out-of-sample predictive performance of the LASSO. The top-left panels shows that observed *Returns* are on average lower than counterfactuals at the closing of the very day news of investigations are released (and consistently so in the following 48 hours) when parent companies are implicated in corruption directly. After that, *Returns* do not seem to depart from their counterfactuals, but *Cumulative Returns* are significantly smaller than expected ones even until 20 days after the event (top-right panel). Corruption seems to impose a long-lasting penalty on firms' profits. For cases of indirect involvement (bottom panel), however, observed *Returns* and *Cumulative Returns* to the parent company are not significantly lower than their counterfactuals after the event.

Using these variables, I can finally compute my dependent variables. My main dependent variable is *Abnormal Returns*: the difference between observed and expected *Returns* in the event window. I follow common practice[22] and introduce a buffer of 20 days between the end of the estimation window

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

[22] I show that my results are robust to this choice in Appendix.

Figure 2: Average observed and counterfactual *Returns* (left panels) and *Cumulative Returns* (right panels) in the days before and after the release of corruption news, disaggregated by type of involvement. Top panels present direct involvement of a parent company, bottom panels report involvement through a subsidiary



and the beginning of the event window, to ensure no anticipation effect. Thus, effectively my analysis spans over the days [-10, +10]. I also compute *Cumulative Abnormal Returns*: the difference between observed and expected *Cumulative Returns* over the days [-10, +30].[23] *Abnormal Returns* and *Cumulative Abnormal Returns* quantify unexpected changes in stock returns after corruption. Positive (negative) values indicate changes in stock prices that exceed (fall behind) what market models expected based on information available before the event.

I model these dependent variables in an event study that includes a categorical variable for the number of days separating each observation from the day of *Corruption* (0) using day -1 as a baseline, as in Equation 3. Y_{et} expresses (*Cumulative*) *Abnormal Returns*. Coefficients until day -1 quantify average pre-treatment differences in daily values of the dependent variable from the baseline day. Later coefficients, instead, quantify differences in daily (*Cumulative*) *Abnormal Returns* following the event, thus returning dynamic ATT estimates. The models include an event-fixed effect to completely remove

[23] *Cumulative Abnormal Returns* are thus studied in an asymmetric window. I show robustness to this choice in Appendix.

between-event heterogeneity and are estimated using OLS. All standard errors are clustered at the parent company-level.

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

I perform my event study after subsetting my sample between cases of direct and indirect involvement in corruption. Thus, I identify the effect of corruption revelations on involved parent companies' stock prices in the two scenarios. But how comparable are these cases? In appendix I show that, at least when looking at observable covariates, events of direct and indirect involvement of the parent company in foreign bribery are comparable. These scenarios do not seem to differ significantly across features like the number of foreign countries involved in the bribe exchange, the size of the parent, the value of its stocks, the level of corruption of the country where bribery occurs, the timing of information disclosure (Table B.1), or the distribution of headquarter countries and industries (Figures B.1 and B.2).

Results

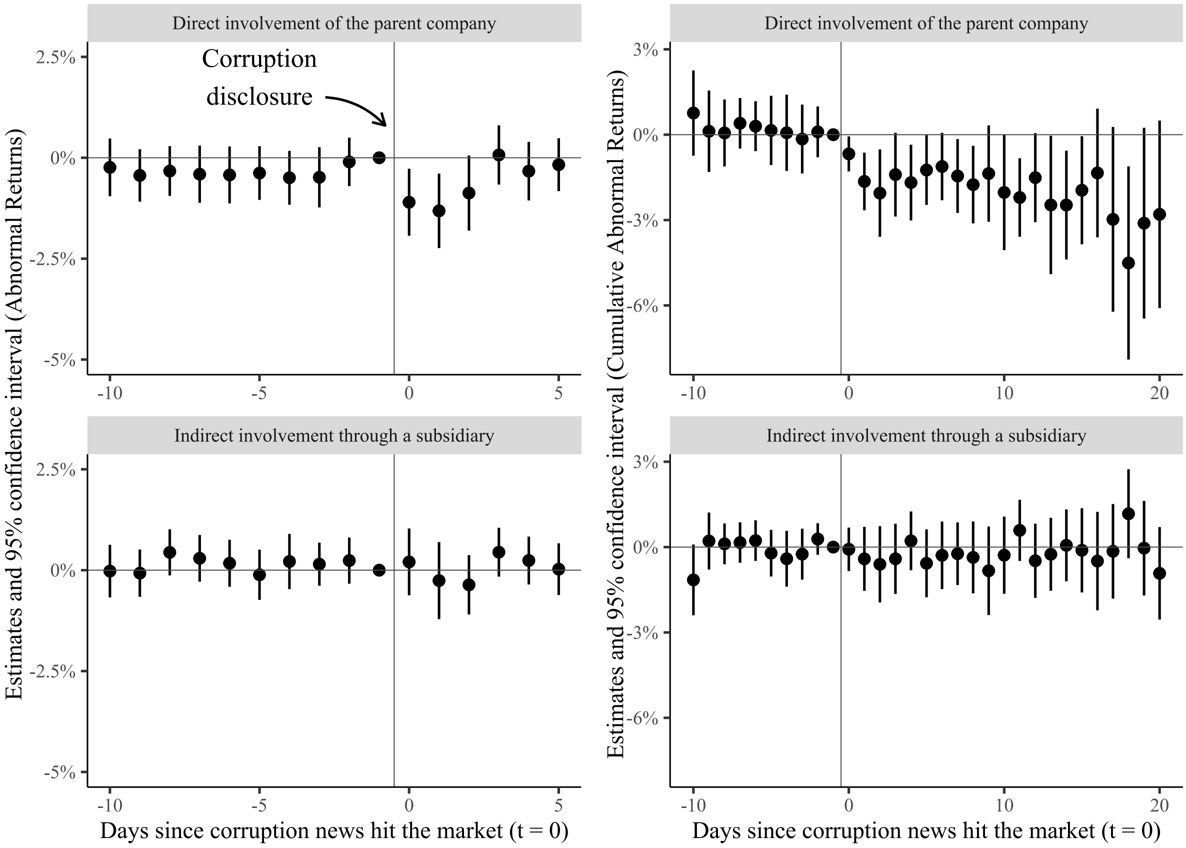
Figure 3 presents results relative to the 10 days before the event and 10 days after the event (20 post-event days in the case of *Cumulative Abnormal Returns*).[24] Top panels report events of direct involvement in investigations, whereas bottom panels report events where the parent company was involved in an FCPA case only indirectly, via a subsidiary. Left panels study *Abnormal Returns* whereas right panels study *Cumulative Abnormal Returns*.

In all panels, I observe no significant pre-treatment trend, which reassures on the internal validity of post-treatment estimates. *Abnormal Returns* to the parent company's stocks drop in value on the trading day of the *Investigation* and the following (days 0 and 1) when a company is directly involved in an investigation (top-left panel). The effect is also almost significant on day 2 ($p = 0.065$). On each of these days, companies' stocks on average closed their trading at a price about 1% lower than what they did on the day before the event. After that, the effect is re-absorbed, consistently with the market efficiency hypothesis (Fama, 1970). I observe no significant effect for cases of indirect involvement at all (bottom-left panel), where post-treatment point-estimates are small and statistically insignificant.

Do short-term *Abnormal Returns* penalties for direct implication cumulate substantively? The top-right panel shows a consistently significant and negative *Cumulative Abnormal Returns* effect on the days after information hit the market, detected even after almost 20 days from the event. Still on day

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

Figure 3: Event study of *Abnormal* and *Cumulative Abnormal Returns* in the 20 days around the publication of corruption news, conditional on direct or indirect implication of the parent company



18, involved companies experience 4.5% lower *Cumulative Abnormal Returns* than what could have been expected before corruption revelation. Instead, I find no significant cumulative loss on the parent company's stock returns when the firm is implicated in corruption through a subsidiary (bottom-right panel).

How sizeable are direct-involvement penalties? The average company involved directly in an investigation traded at about $72.5 per share on the day before the FCPA investigation was revealed. A 1% loss on days 0 and 1 from revelations of an FCPA enforcement means that such company lost about $0.73 per share due to the unexpected information for two days. To estimate how this loss translates in terms of market capitalization, I retrieve from Orbis information on the number of outstanding shares traded by each parent company at the end of the month before each event considered. The average company in my data traded more than 1.5 billion shares, for a market capitalization of almost $105 billion before enforcement. A daily loss of $0.73 per share amounts to almost $1.2 billion in losses each of the two days. With a similar logic, when looking at cumulative effects, on day 18 the average company had lost more than $4.5 billion with respect to pre-event capitalization.

Is the effect detected for direct involvement statistically different from the null-effect relative to indirect involvement? In order to answer this question, I estimate linear models of *Abnormal Returns* where I interact a binary treatment variable taking value 1 solely on the day of corruption revelations (0 otherwise) with the *Indirect* indicator. Table 2 reports my main results (models 1–4). I also model *Cumulative Abnormal Returns* (models 5–7) in a similar manner, although here the binary treatment takes value 1 on the entire period after corruption revelations (to account for the cumulated effect). I being by introducing only the interaction variables. Next, I introduce: a one-day lag of the dependent variable (not for *Cumulative Abnormal Returns*, given that the lag would be mechanically related to the dependent variable), a year-fixed effect to account for year-specific heterogeneity in FCPA enforcement action, and an event-fixed effect to absorb all between-event heterogeneity.[25]

Table 2: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.744*	-0.845**	-0.848**	-0.768**			
	(0.303)	(0.300)	(0.299)	(0.294)			
Post-corruption					-2.161*	-2.200*	-2.144*
					(0.888)	(0.878)	(0.851)
Corruption × Indirect	0.848+	0.949*	0.945*	0.860+			
	(0.452)	(0.454)	(0.452)	(0.445)			
Post-corruption × Indirect					1.858+	1.907+	2.004*
					(1.065)	(1.060)	(1.010)
Indirect	0.080	0.100	0.081		-0.089	-0.282	
	(0.083)	(0.086)	(0.080)		(1.049)	(1.208)	
Abnormal Returns (t–1)		-0.021	-0.034	-0.108**			
		(0.035)	(0.032)	(0.033)			
(Intercept)	-0.117+	-0.124+			0.083		
	(0.066)	(0.067)			(0.773)		
Num.Obs.	3606	3494	3494	3494	5607	5607	5607
R2	0.004	0.006	0.021	0.112	0.005	0.085	0.825
R2 Adj.	0.003	0.005	0.015	0.039	0.004	0.082	0.816
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +20]	[-10, +20]	[-10, +20]

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

Across all models, the coefficient of *Corruption* indicates an average 0.8% abnormal loss in stock value for cases of direct involvement in investigations ($Indirect = 0$). The effect is distinguishable from zero at a 0.05 conventional level of significance. The interaction term $Corruption \times Indirect$ is positive and similar in magnitude to the previous one: this indicates that the negative effect of corruption on stock markets is completely absorbed when a company is involved through a subsidiary. Such moderation is

[25] *Indirect* is event-invariant so naturally the un-interacted constitutive term gets excluded from this model due to perfect collinearity.

statistically significant at a 0.05 level. Similarly, the market effect of *Corruption* cumulates substantively to a 2% loss for cases of direct implication in corruption (significant at 0.05 level), an effect that gets completely muted in cases of indirect implication (as indicated by the positive and statistically significant interaction term of models 5–7, of about the same size).

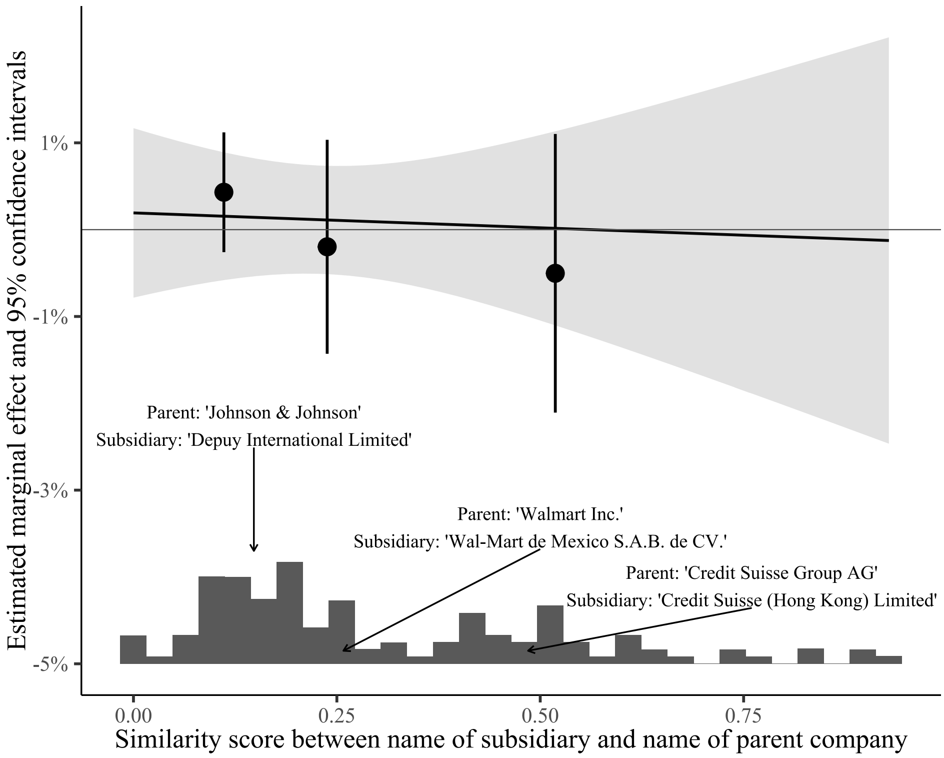
I extensively test robustness of my results in appendix. First, I show that results are not driven by any single outlier—*e.g.*, a corruption scandal with significantly negative impact. Next, I show that findings are not driven by arbitrary choices followed in the procedure. I show robustness to alternative event window lengths. Next, I test robustness of results to the exclusion of events with imprecise market models from equation 1—that is, yielding R-squared values smaller than 0.05. I also show robust results when using OLS-estimated market models, as opposed to LASSO ones. To conclude, I show that similar findings can be obtained even when adopting different research designs. First, I use the number of days to the event as an instrument in a regression discontinuity-in-time design and I find consistent estimates with those presented above. Second, I remove the market-model-imputed synthetic counterfactuals and study observed *Returns* and *Cumulative Returns* directly. Results are consistent with those presented here.

Ruling Out Alternative Mechanisms

I claim that investors buy into companies' plausible deniability, and thus fail to discipline companies when they are implicated in corruption through a subsidiary. But an alternative explanation would posit that such regulatory failure is entirely imputable to information scarcity, not plausible deniability. Investors might simply not possess enough information to sanction subsidiary corruption. In particular, investors might fail at sanctioning a parent because they are not aware that the company responsible for corruption is its subsidiary (that is, subsidiary incorporation obfuscates ownership structure information); or, investors might be aware of corporate ownership, but they might not receive information about corruption when a subsidiary is responsible for it (subsidiary ownership reduces information about misconduct). Here, I offer tests to rule out both possibilities.

To rule out the first alternative explanation, I leverage differences between the names of involved subsidiaries and those of parent companies. Cases of indirect involvement include subsidiaries with very different names from that of the parent. For instance, Depuy International LTD (wholly-owned by Johnson & Johnson). In similar cases, investors might not be necessarily aware of corporate ownership, when informed of corruption. But in other cases, 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. My intuition is thus that investors are likely aware of corporate ownership ties in cases of extreme name similarity. If subsidiary corruption failed at generating market sanctions in such cases, the regulatory failure is likely not imputable to asymmetry in information about corporate ownership relations.

Figure 4: Marginal effects of indirect involvement into FCPA investigations on the parent company's *Abnormal Returns*, conditional on the degree of similarity between the name of the subsidiary and that of the parent company.



I calculate a score representing the similarity[26] between the name of the parent and that of the subsidiary in cases of indirect involvement in corruption. I employ a metric for string similarity based on the Levenshtein distance,[27] which ranges from 0 (indicating extreme diversity between two strings) to 1 (indicating perfect equality). Next, I re-estimate my event-fixed effect model from Table 2, subsetting my sample for cases of indirect involvement only. I employ the name-similarity score as a moderating variable in the binning estimator proposed by Hainmueller, Mummolo and Xu (2019). Figure 4 reports results and presents three examples of pairs of names ending up in each of the three levels of the moderating variable. I observe no significant effect for any type of indirect involvement, even when the name of the subsidiary responsible for alleged corruption is as similar to that of the parent as "Credit Suisse

[26] In Appendix, I show robustness when simply using a hand-coded binary indicating whether the name of the parent is included in that of the subsidiary.

[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.

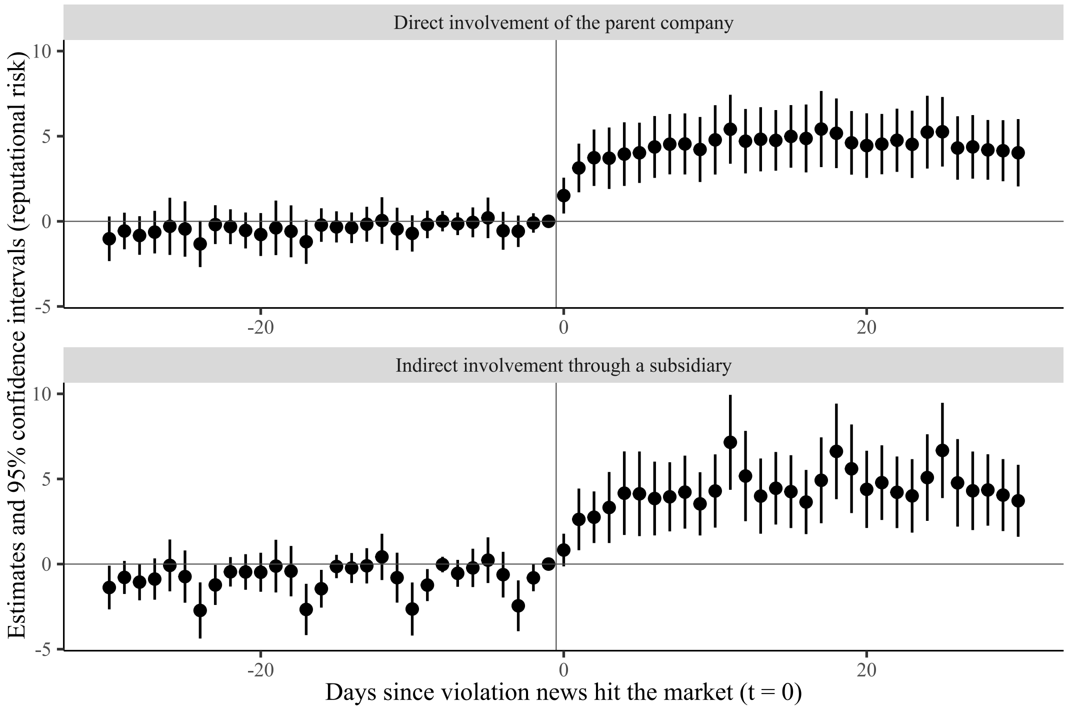
(Hong Kong) Limited" is to "Credit Suisse Group AG". This lends confidence against the hypothesis that the null-effect is driven by genuine ignorance on the side of investors about corporate ownership linkages. It suggests investors in fact fail to penalize parents for misconduct by their subsidiaries even when plausibly knowing about corporate ownership ties.

Perhaps, however, investors are aware of corporate ownership relations, they just do not sanction subsidiary corruption because they are not informed about it. Similarly to how credit rating agencies inform investors about the risk of default of certain assets, ESG index providers inform them about their reputational risk exposure to corporate social responsibility standards. These indexes provide useful benchmarks, given the large amount of information that investors need to process and the limited time for doing so. Perhaps, these providers fail at updating the reputational risk index of a parent firm after subsidiary corruption.

To test for this possibility, I collect information on the Reputational Risk Index (RRI) measured by RepRisk. RepRisk is a Swiss market analyst whose analytics are sourced by such market actors as FactSet, UBS, Dow Jones, and JP Morgan. By observing its RRI, I'm thus able to study the type of information that investors consume when deciding for or against certain stock purchases or sales. I retrieve information on the RRI for each parent company in my data, from 30 days before to 30 days after each corruption disclosure day. The RRI can range from a value of 0 (indicating no reputational risk) to 100 (maximum risk). RepRisk produces the index with a proprietary algorithm that processes and scores information on several corporate social responsibility "incidents" involving the parent company. In my sample, the average firm on the average pre-corruption day has an average RRI score of 21.37 (standard deviation: 18.71), indicating modest reputational risk.

I model the RRI in the same event study presented in Equation 3, distinguishing between cases of direct or indirect implication in corruption. Results are reported in Figure 5. Before corruption news, the RRI trends flat (safe for noisy patterns in case of subsidiary corruption). Contrary to the argument that investors are simply not informed about misconduct, post-corruption estimates clearly indicate that the reputational risk of a parent equally deteriorates after news of corruption, regardless of whether the parent is implicated directly or via a subsidiary. The effect is sizeable and comparable in both cases (about a +5 increase in RRI, which amounts to almost 27% of a standard deviation). The pieces of evidence presented in this section suggest that investors are provided with all the necessary information to sanction parent firms for corruption via a subsidiary. That they fail to do so, regardless of the information reaching them, suggests that investors buy into the parent's plausible deniability for corruption by a subsidiary.

Figure 5: Event study of the RRI in the 30 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the investigations



Conclusion

In this paper, I showed that companies can use subsidiary ownership as a shield against market sanctions for foreign corruption. I distinguished cases where a parent company is directly involved in foreign corruption and those where it is implicated indirectly—via an owned subsidiary. I claimed that markets impose penalties on a company after allegations of its *direct involvement* in corruption, due to concerns about the firm's profitability. However, the effect disappears when the company is implicated only indirectly.

I offered evidence from an original dataset on 263 investigations for alleged violations of the US anti-corruption criminal law (FCPA) in 217 distinct corporate groups. I retrieved data on the day information of misbehavior first hit the market and daily stock prices of the parent company sitting at the top of each corporate group. I also coded the relationship between the entity (allegedly) responsible for a violation and the parent. An event study showed that parents suffer a significant abnormal loss of about 1% to their stock returns on the two days following the release of information. This effect amounts to a daily loss of about $1 billion in market value for the average company and cumulates to more than $4.5 billion in losses even almost 20 days after the investigation. However, I provided evidence of no effect on the parent's stock price when involvement occurs through a subsidiary.

Results indicate a failure of market discipline that is relevant to the global governance literature. Investors do penalize companies for direct involvement in misconduct, similarly to evidence existing in the context of country blacklisting for money laundering (Morse, 2019). However, they do not seem to bite against parent companies for misconduct by entities down the line of their corporate groups. This is concerning because it shows that companies can fragment ownership to meet a cynical threefold goal: to further corporate wrongdoing (Findley, Nielson and Sharman, 2015), to evade regulations (Chapman, Jensen, Malesky and Wolford, 2021), and to minimize market sanctions.

More fundamentally, findings question the extent to which market discipline can substitute for formal state-based regulations. I illustrate the existence of a virtuous circle between state and market discipline: when states impose (or threaten) regulatory costs, markets impose sustained sanctions, potentially assisting formal regulation in disciplining misconduct. But this virtuous circle brakes when regulatory costs weaken, such as with subsidiary misconduct. This conclusion contributes to a long-lasting debate in political science on state-market relations (Ruggie, 2018; Strange, 1996) and on ensuring compliance of private actors with international norms (Findley, Nielson and Sharman, 2015; Jensen and Malesky, 2018). Future research on the matter could learn from these conclusions to study whether public regulators respond differently to different size of market responses against corporate misconduct, perhaps rebalancing the regulatory failure documented here. Furthermore, global governance scholars could study whether different forms of corporate integration (*e.g.* vertical vs horizontal integration, supply chains, joint ventures, or sub-contracting) insulate or expose parents to private regulatory responses by investors. Additionally, scholars of political economy could study whether wordings of negative news by companies in their communications of misbehavior affect markets differently.

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Supplementary Information

The Shield of Ownership: The Limits of Market Sanctions Against Corruption

Contents

A Estimation window descriptives

Figure A.1: Heatmap reporting the value of estimated coefficients relative to financial indicators (y-axis) as they enter each of the 263 market models from the estimation window (x-axis) when using the LASSO procedure. The plot shows in white indexes that are excluded from a market model and colors cells according to the size of the estimated coefficient (multiplied by the LASSO weight). A percentage is also reported indicating the share of models each index is included in.

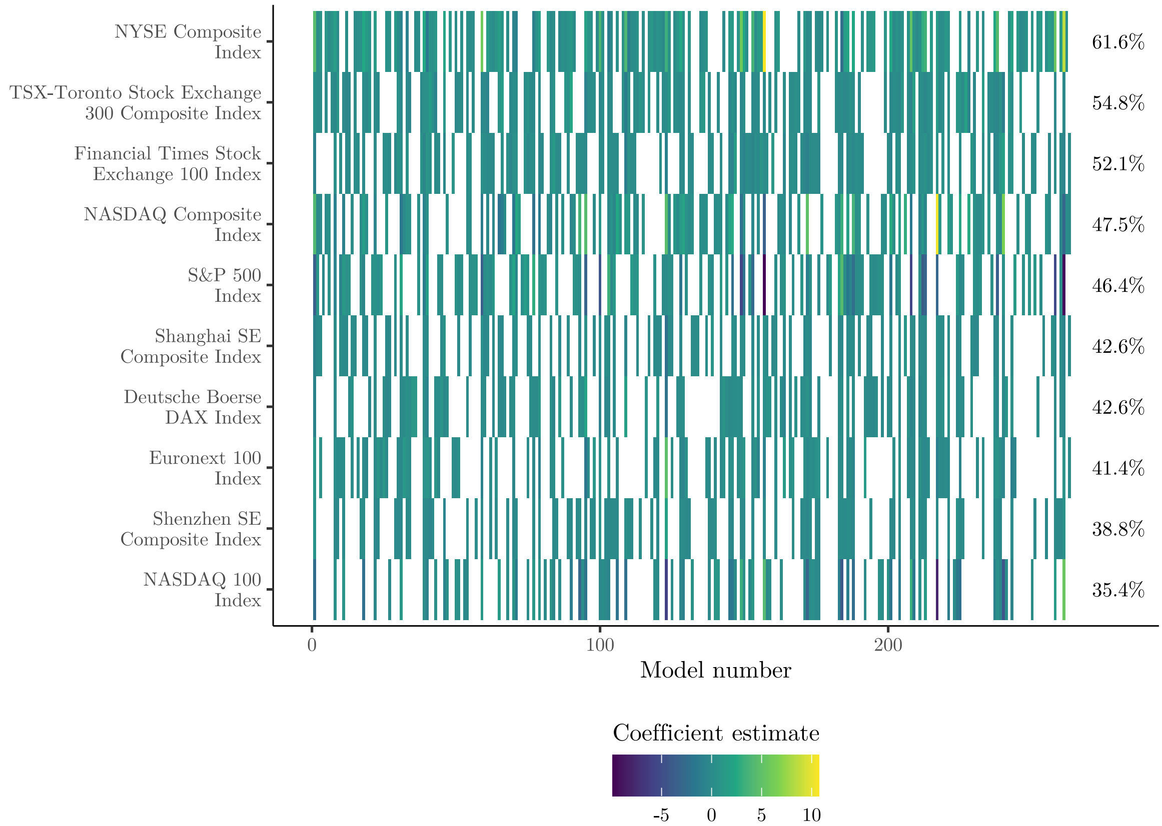
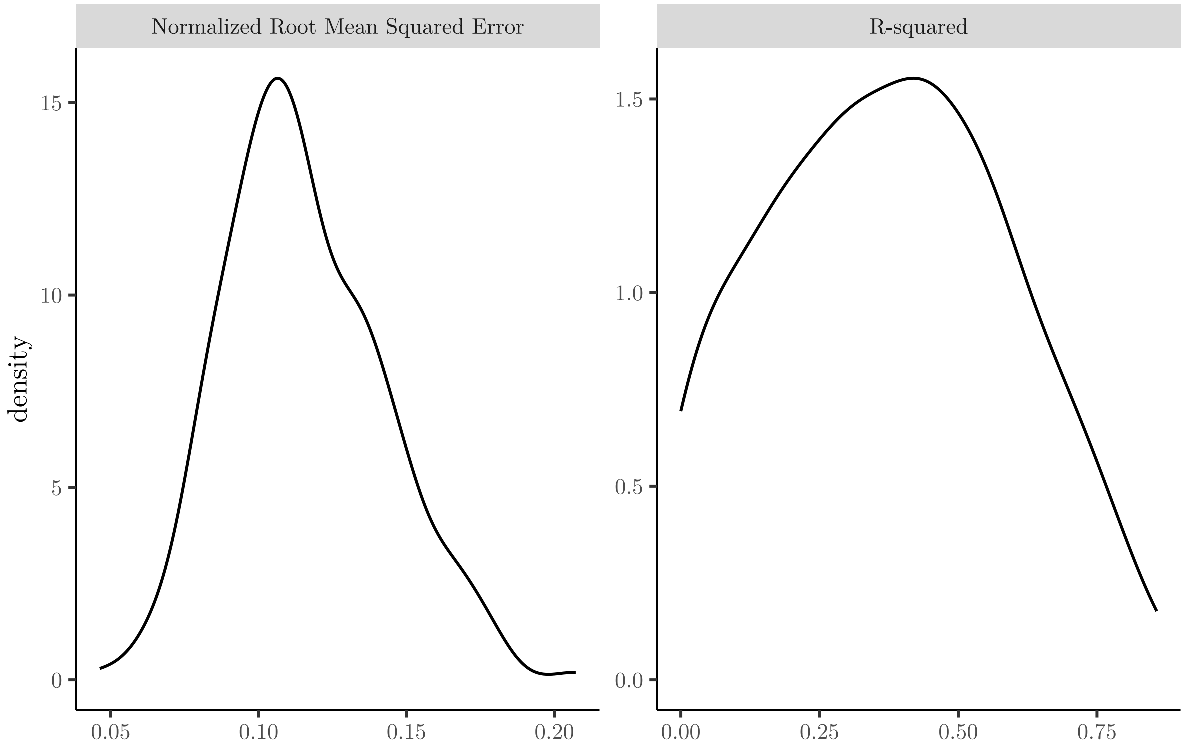


Figure A.2: Distribution of the normalized Root Mean Squared Error (RMSE) and of the R-squared yielded by the 263 market models estimated using the LASSO procedure.



B Balance in covariates across types of corruption involvement

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

I then compute simple difference in means for all 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 observable characteristics. Differences in the average values across the two groups are statistically insignificant with large p-values. The only exception is represented by the level of corruption of the host markets where bribery occurs, as measured by the VDEM index. Cases of indirect involvement are, on average, slightly *less* corrupt than cases of direct involvement. However, this difference is in the *opposite* sign that one would expect to observe if MNCs were strategically outsourcing corruption in most severe locations to subsidiaries.

In general, the signs of the differences-in-means for these variables are mixed and not implying any consistent imbalance. For instance, companies involved directly tend to have larger market capitalization (\$50.20 *vs* \$43.79 billion) and are larger by assets (\$124.67 *vs* \$87.69 billion) but they tend to be smaller by revenues (\$27.16 *vs* \$29.61 billion) and number of employees (56.43 *vs* 84.35 thousands). As per the disclosure timing, I notice that there seems not to be a strategic timing selection for cases of direct and indirect involvement: averages indicate a uniform and comparable distribution of timing in either case. In either case, corruption is disclosed around the fourth day of the week (Wednesday) and around the middle of the year (June). The average case takes place in 2012.

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=143)		Indirect involvement (N=120)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Parent Outstanding Shares (billions)	1.50	2.90	1.40	2.18	-0.10	0.76
Parent Market Capitalization (billion USD)	50.20	83.46	43.79	60.86	-6.41	0.51
Parent Revenue (billion USD)	27.16	47.79	29.61	57.14	2.45	0.72
Parent Assets (billion USD)	124.67	392.41	87.69	262.53	-36.98	0.38
Parent No. Employees (thousands)	56.43	76.74	84.35	222.90	27.92	0.22
Number of corruption countries	2.03	2.11	1.80	2.09	-0.23	0.40
Host country corruption (VDEM)	-0.43	0.99	-0.15	1.06	0.28	0.04
Day of disclosure (DOW)	4.12	1.36	3.85	1.35	-0.27	0.11
Month of disclosure	6.20	3.35	5.56	3.42	-0.64	0.13
Year of disclosure	2012.20	4.46	2011.77	4.69	-0.43	0.45

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

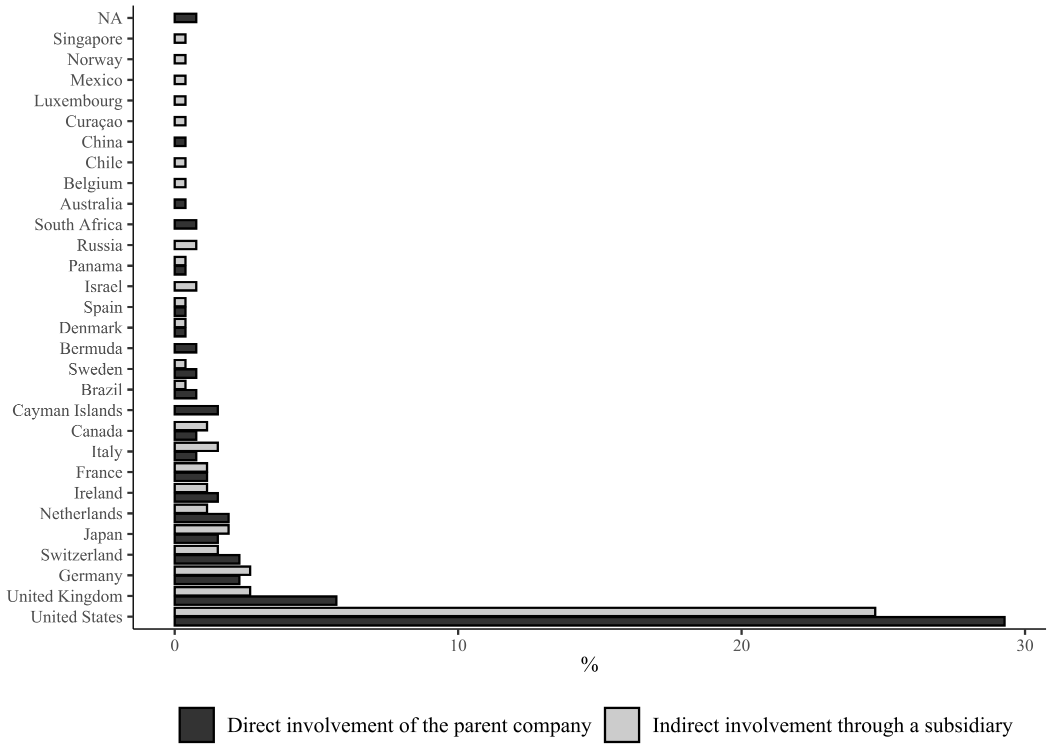
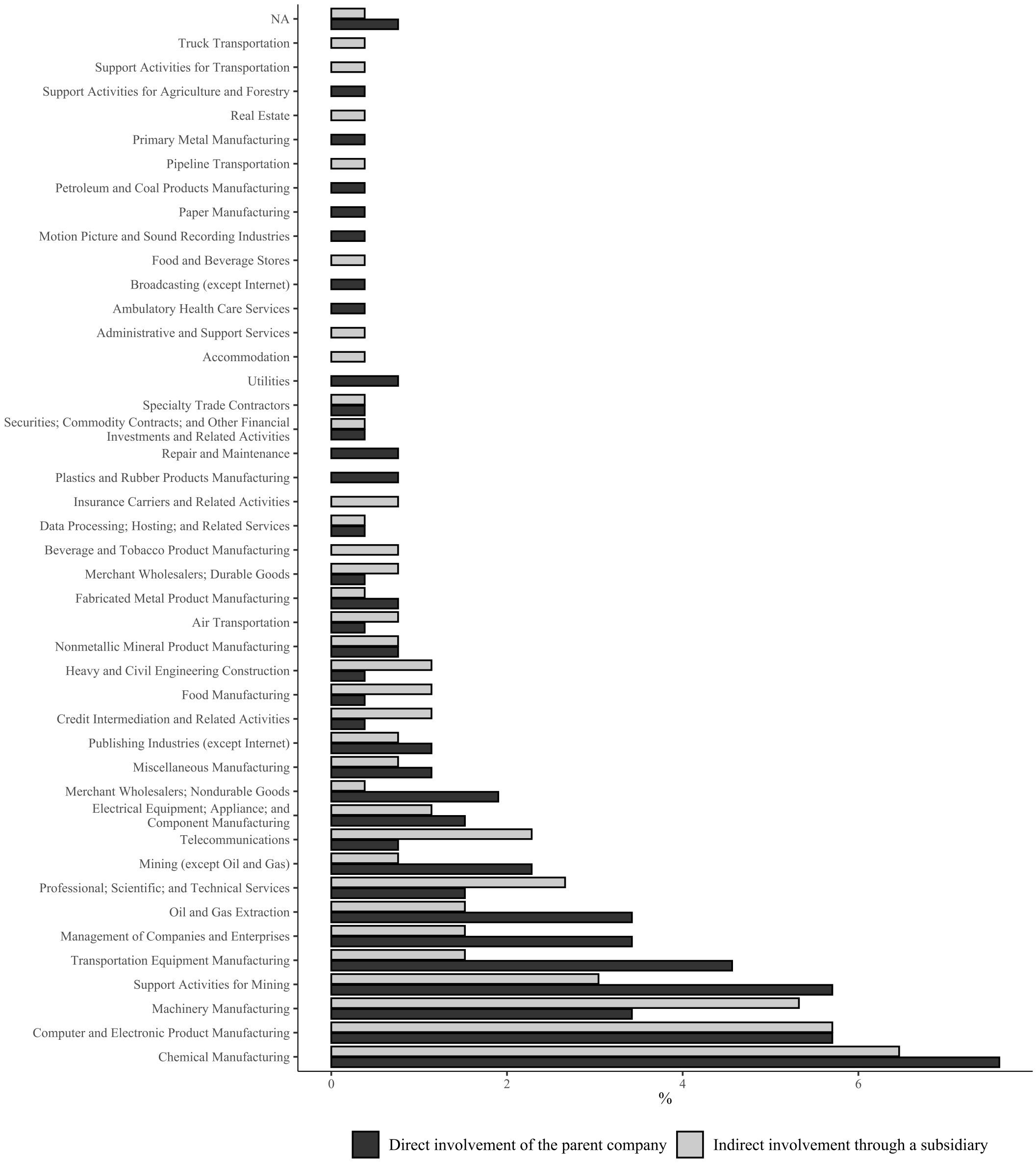
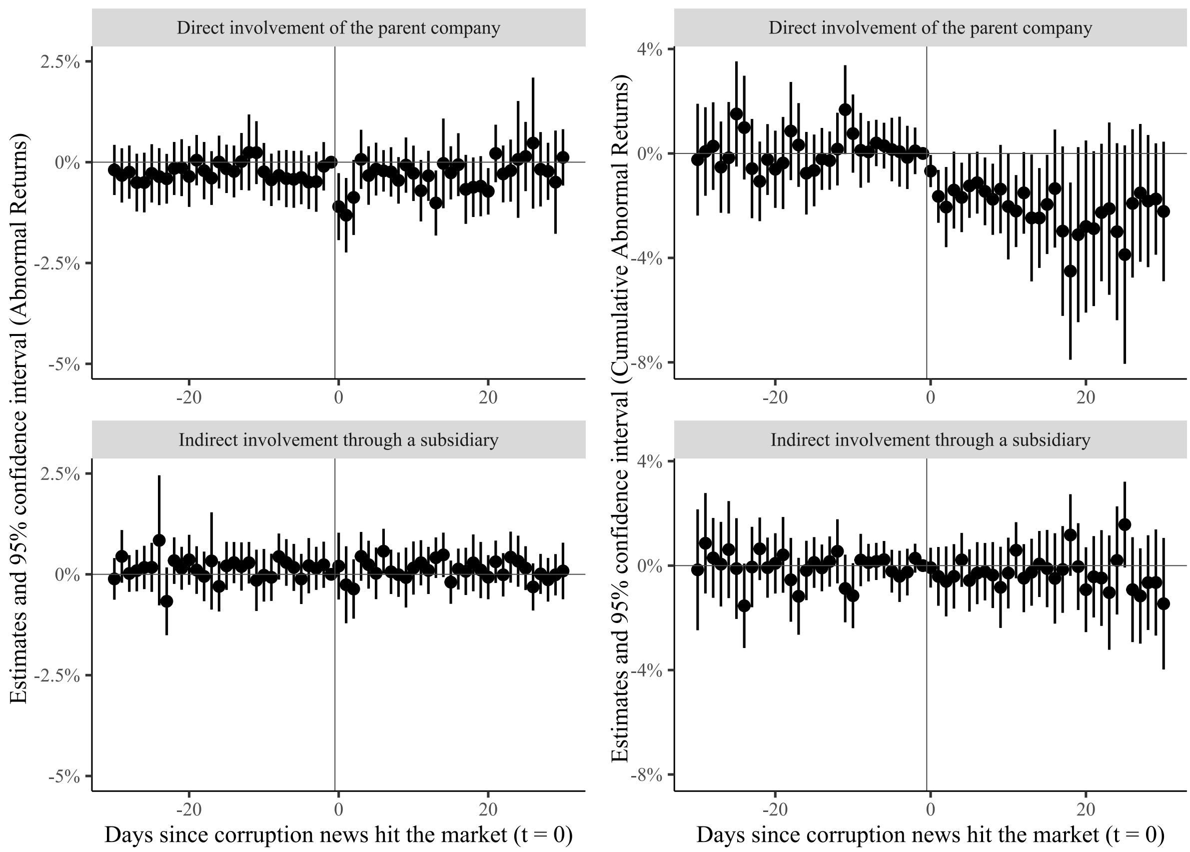


Figure B.2: Proportion of events involving companies by NAICS-3 code, across cases of direct ($Indirect = 0$) and indirect involvement ($Indirect = 1$).



C Event study: Full disclosure of results

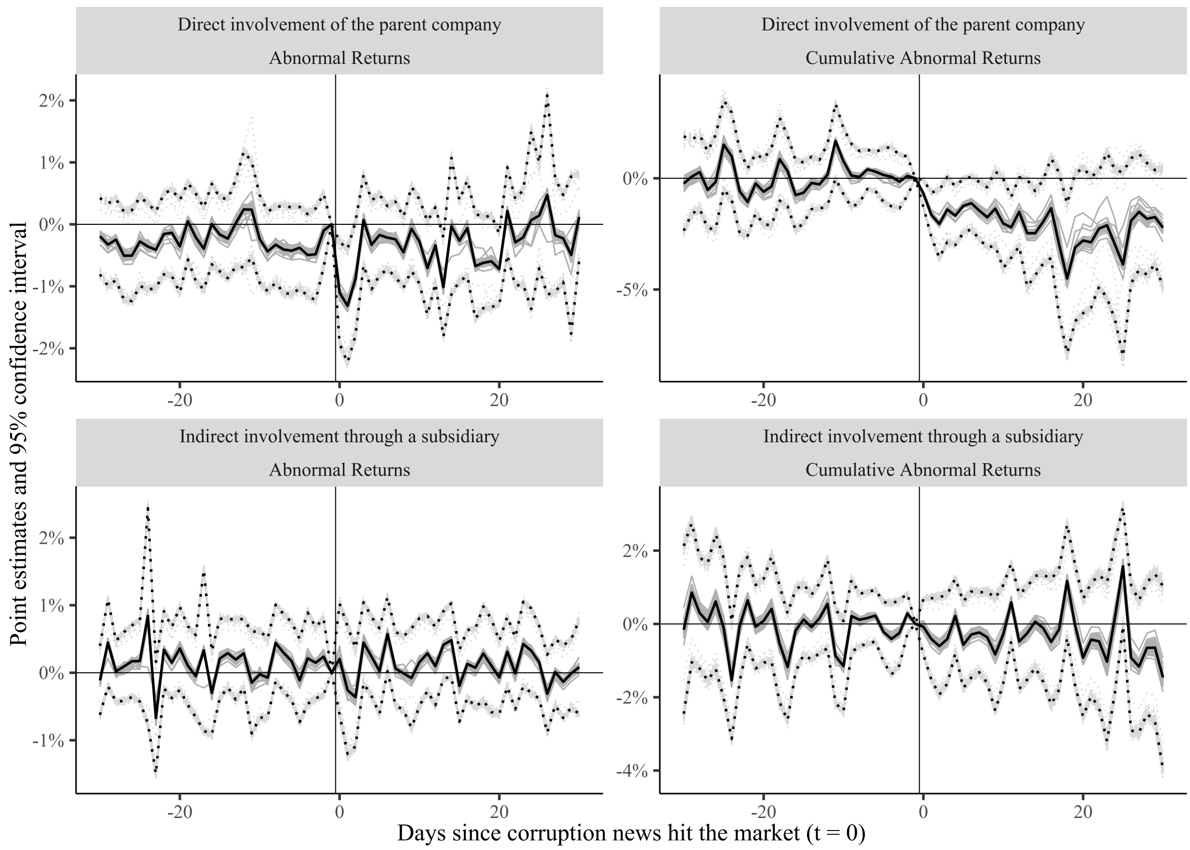
Figure C.1: Event study in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in corruption. Full event window results



D Event study: Leave-one-out

I rule out that results are driven by any single outlier (a corruption scandal with significantly negative impact, or a particularly "bad" firm) in my data. I replicate my event study from Figure 3 adopting a jackknife approach. I estimate the model multiple time, each time leaving one different event out of the model. I report point estimates and confidence intervals in Figure D.1 (alongside full-sample estimates for comparison).

Figure D.1: Event study in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in corruption. 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.



E Event study: Alternative window sizes

I replicate the analysis in Table 2 when changing my event window sizes, to verify that results do not hinge on my arbitrary length choice. In Table E.1 I consider the interval [-5, +5] for *Abnormal Returns* and the interval [-5, +10] for *Cumulative Abnormal Returns*. In Table E.2, I consider the full event windows [-30, +30] for both dependent variables. In my main results, *Abnormal Returns* are modelled in a shorter symmetric window, while *Cumulative Abnormal Returns* in a shorter asymmetric one. I swap this in Table E.3 by imposing a short asymmetric event window for *Abnormal Returns* ([-10, 0]) and a short symmetric one for *Cumulative Abnormal Returns* ([-10, +10]).

Table E.1: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. Event window data start 5 days before the Event

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.670*	-0.780*	-0.774*	-0.619*			
	(0.326)	(0.319)	(0.318)	(0.307)			
Post-corruption					-1.196+	-1.304*	-1.421*
					(0.636)	(0.650)	(0.582)
Corruption × Indirect	0.842+	0.946*	0.941*	0.714			
	(0.477)	(0.477)	(0.475)	(0.453)			
Post-corruption × Indirect					0.767	0.897	1.250+
					(0.835)	(0.858)	(0.746)
Indirect	0.087	0.109	0.111		0.357	0.139	
	(0.140)	(0.141)	(0.138)		(1.164)	(1.277)	
Abnormal Returns (t–1)		0.000	-0.020	-0.193***			
		(0.047)	(0.044)	(0.036)			
(Intercept)	-0.191+	-0.192+			-0.413		
	(0.106)	(0.105)			(0.875)		
Num.Obs.	1754	1703	1703	1703	2861	2861	2861
R2	0.006	0.008	0.029	0.220	0.002	0.069	0.920
R2 Adj.	0.004	0.006	0.016	0.076	0.001	0.062	0.912
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-5, +5]	[-5, +5]	[-5, +5]	[-5, +5]	[-5, +10]	[-5, +10]	[-5, +10]

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

Table E.2: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. Event window data start 30 days before the Event

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.828**	-0.939**	-0.942**	-0.919**			
	(0.308)	(0.301)	(0.301)	(0.300)			
Post-corruption					-2.044+	-2.068+	-2.067+
					(1.123)	(1.126)	(1.125)
Corruption × Indirect	0.914*	1.034*	1.027*	1.007*			
	(0.455)	(0.455)	(0.455)	(0.454)			
Post-corruption × Indirect					1.706	1.729	1.735
					(1.389)	(1.393)	(1.382)
Indirect	0.015	0.022	0.003		0.006	-0.187	
	(0.054)	(0.056)	(0.052)		(0.703)	(0.880)	
Abnormal Returns (t–1)		0.005	-0.000	-0.027			
		(0.034)	(0.033)	(0.034)			
(Intercept)	-0.034	-0.034			-0.104		
	(0.041)	(0.042)			(0.505)		
Num.Obs.	10455	9890	9890	9890	11162	11162	11162
R2	0.001	0.002	0.007	0.035	0.004	0.072	0.644
R2 Adj.	0.001	0.001	0.005	0.008	0.004	0.070	0.636
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-30, +30]	[-30, +30]	[-30, +30]	[-30, +30]	[-30, +30]	[-30, +30]	[-30, +30]

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

Table E.3: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. Alter symmetry and asymmetry of windows

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.789*	-0.903**	-0.909**	-0.777**			
	(0.309)	(0.301)	(0.302)	(0.294)			
Post-corruption					-1.692*	-1.778*	-1.609**
					(0.724)	(0.720)	(0.615)
Corruption × Indirect	0.854+	0.963*	0.966*	0.818+			
	(0.455)	(0.452)	(0.452)	(0.450)			
Post-corruption × Indirect					1.213	1.263	1.352+
					(0.929)	(0.936)	(0.800)
Indirect	0.075	0.078	0.045		-0.089	-0.215	
	(0.091)	(0.095)	(0.098)		(1.049)	(1.157)	
Abnormal Returns (t–1)		-0.054	-0.066*	-0.164***			
		(0.035)	(0.032)	(0.032)			
(Intercept)	-0.072	-0.063			0.083		
	(0.072)	(0.075)			(0.773)		
Num.Obs.	1932	1870	1870	1870	3831	3831	3831
R2	0.009	0.014	0.030	0.186	0.003	0.059	0.883
R2 Adj.	0.007	0.012	0.018	0.052	0.003	0.054	0.874
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-10, 0]	[-10, 0]	[-10, 0]	[-10, 0]	[-10, +10]	[-10, +10]	[-10, +10]

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

F Event study: Exclude imprecise-imputed counterfactuals

In a following test, I verify results do not hinge on the inclusion of events for which the imputation of synthetic counterfactual was imprecise. I exclude from the analysis any event with market model from Equation 1 yielding an R-squared lower than 0.05. This restricts the analysis to a subset of 200 companies involved in 244 events. I replicate my entire analysis and verify results are consistent (Figures F.1 and Table F.1). Estimates for *Cumulative Abnormal Results* are noisier, but overall consistent with previously presented ones.

Figure F.1: Event study in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in corruption. Full event window. Plot reports point estimates and 95% confidence intervals obtained when excluding firms with imprecise counterfactual estimation

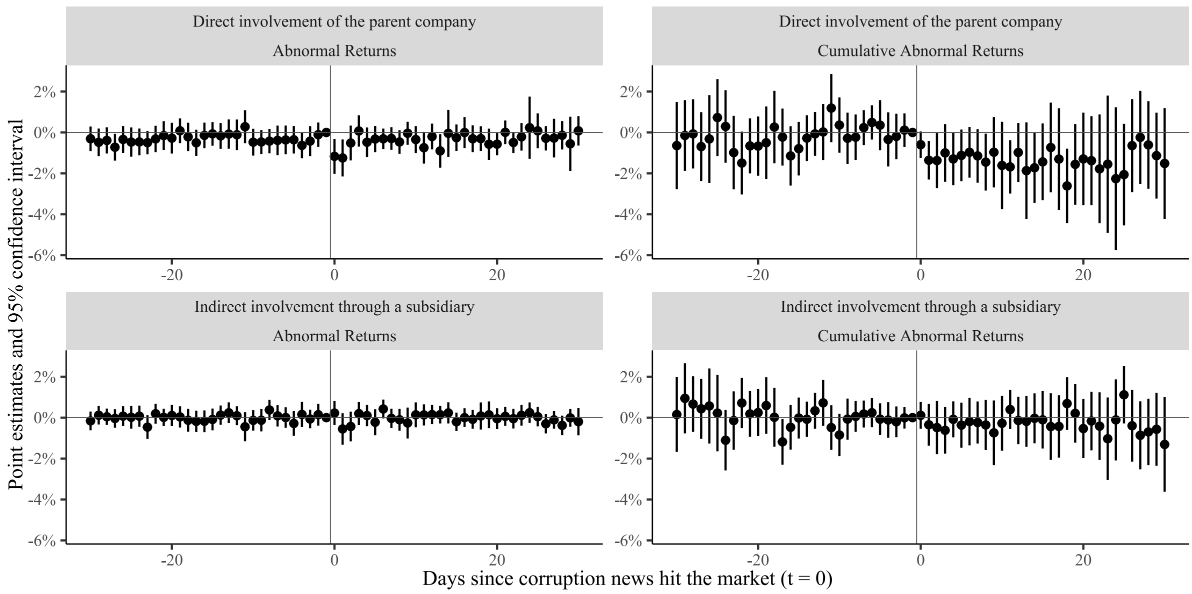


Table F.1: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. Event window data limited to events with precise counterfactual imputation

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.788*	-0.911**	-0.914**	-0.835**			
	(0.321)	(0.319)	(0.319)	(0.313)			
Post-corruption					-1.445+	-1.488+	-1.396*
					(0.779)	(0.758)	(0.698)
Corruption × Indirect	1.040*	1.178**	1.175**	1.119**			
	(0.424)	(0.425)	(0.424)	(0.422)			
Post-corruption × Indirect					1.164	1.209	1.271
					(0.945)	(0.932)	(0.858)
Indirect	0.026	0.024	0.023		-0.512	-0.631	
	(0.080)	(0.081)	(0.081)		(1.003)	(1.130)	
Abnormal Returns (t–1)		-0.008	-0.018	-0.096**			
		(0.035)	(0.031)	(0.032)			
(Intercept)	-0.071	-0.066			0.198		
	(0.064)	(0.063)			(0.790)		
Num.Obs.	3344	3243	3243	3243	5205	5205	5205
R2	0.005	0.007	0.019	0.109	0.002	0.073	0.860
R2 Adj.	0.004	0.006	0.013	0.036	0.001	0.070	0.853
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +20]	[-10, +20]	[-10, +20]

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

G Event study: OLS market models

I replicate my entire analysis when estimating market models (which are used to impute synthetic counterfactual *Returns* and *Cumulative Returns*) with OLS models featuring all market-wide indexes rather than the LASSO. Event-study results are in Figure G.1 while linear model results are in Table G.1. Estimates are consistent with those presented in the main text.

Figure G.1: Event study in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in corruption. Synthetic counterfactuals imputed with OLS-estimated market models

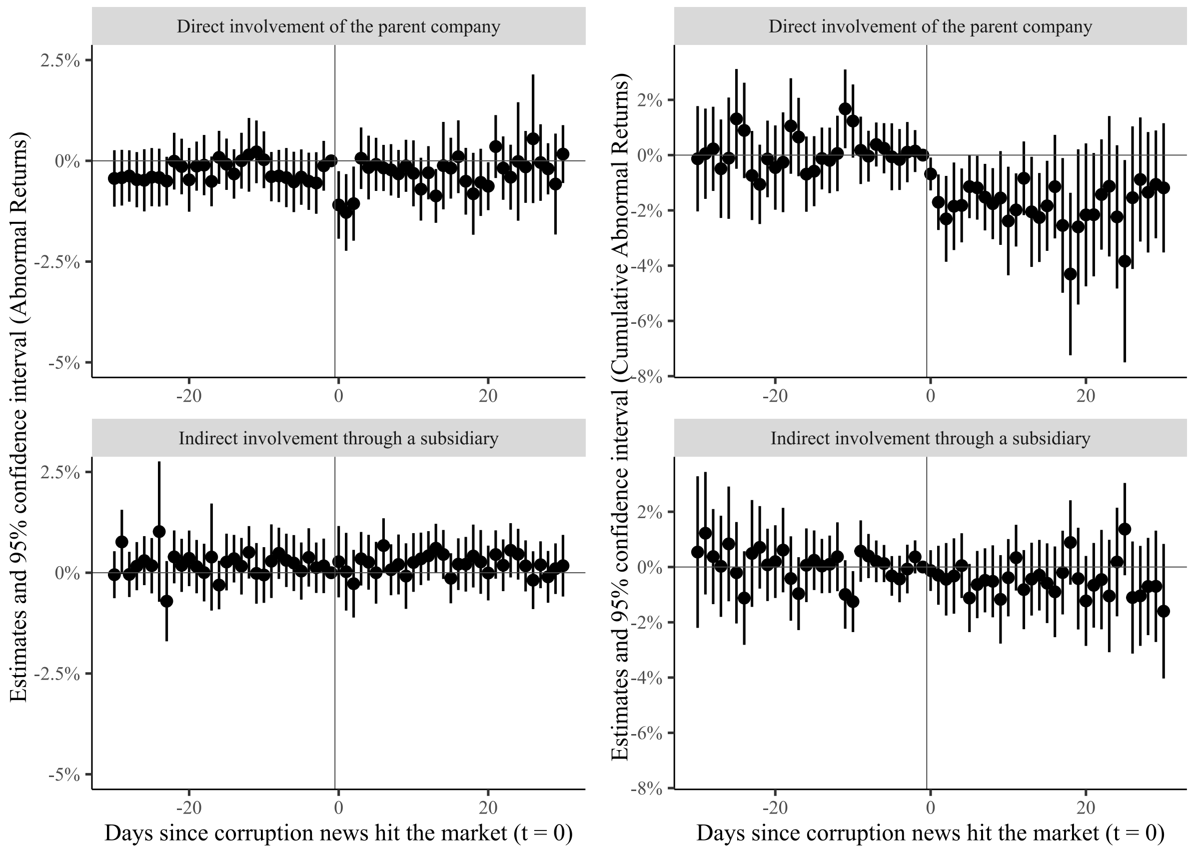


Table G.1: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. OLS-imputed synthetic counterfactuals

	Abnormal Returns				*Cumulative Abnormal Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.752*	-0.844**	-0.845**	-0.770*			
	(0.312)	(0.302)	(0.302)	(0.299)			
Post-corruption					-2.024**	-2.058**	-2.004**
					(0.736)	(0.733)	(0.700)
Corruption × Indirect	0.878+	0.983*	0.977*	0.845+			
	(0.455)	(0.462)	(0.461)	(0.452)			
Post-corruption × Indirect					1.495	1.533	1.591+
					(0.948)	(0.944)	(0.889)
Indirect	0.043	0.066	0.052		0.145	0.028	
	(0.086)	(0.087)	(0.083)		(1.092)	(1.198)	
Abnormal Returns (t–1)		-0.006	-0.015	-0.089**			
		(0.031)	(0.030)	(0.032)			
(Intercept)	-0.099	-0.115+			-0.287		
	(0.062)	(0.065)			(0.700)		
Num.Obs.	3472	3293	3293	3293	5607	5607	5607
R2	0.004	0.005	0.016	0.109	0.005	0.075	0.846
R2 Adj.	0.003	0.004	0.009	0.031	0.004	0.072	0.838
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +20]	[-10, +20]	[-10, +20]

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

H Event study: Non-synthetic counterfactuals

I replicate my analysis when studying *Returns* and *Cumulative Returns*—that is, without discounting synthetic counterfactuals from stock price returns. Results are reported in Figure H.1 and Table H.1.

Figure H.1: Event study in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in corruption. Full event window without discounting synthetic counterfactuals. Plot reports point estimates and 95% confidence intervals

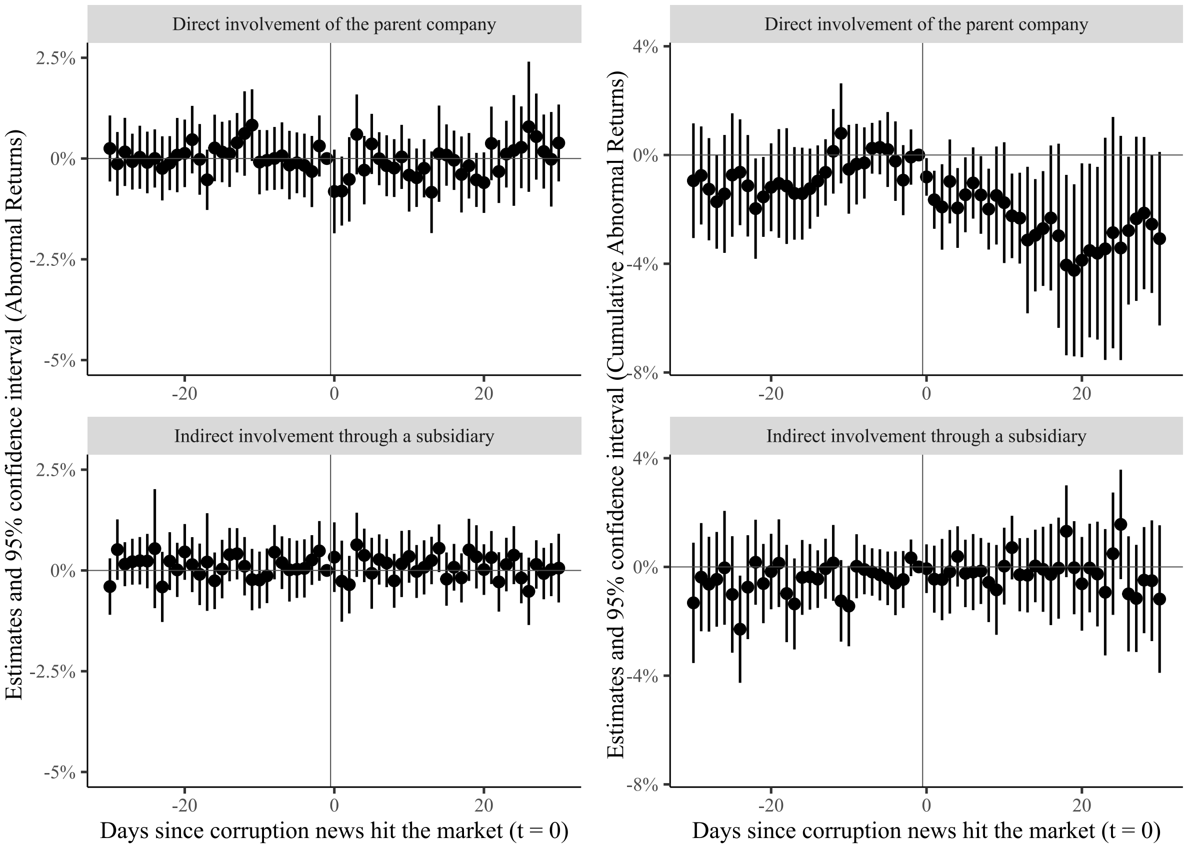


Table H.1: Heterogeneous effects of corruption disclosure on parent companies' stock returns, conditional on involved entity nature. No synthetic counterfactual

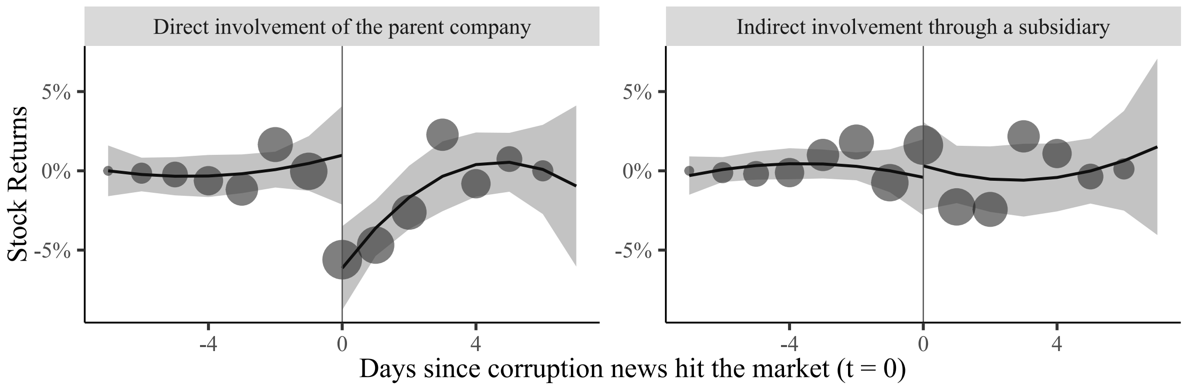
	Returns				*Cumulative Returns*		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Corruption	-0.709*	-0.922**	-0.931**	-0.876*			
	(0.339)	(0.343)	(0.344)	(0.341)			
Post-corruption					-2.078*	-2.126*	-2.159*
					(0.940)	(0.923)	(0.869)
Corruption × Indirect	0.947+	1.195*	1.195*	1.126*			
	(0.488)	(0.503)	(0.503)	(0.496)			
Post-corruption × Indirect					2.023+	2.036+	2.344*
					(1.112)	(1.101)	(1.035)
Indirect	0.085	0.104	0.083		-0.556	-0.588	
	(0.087)	(0.092)	(0.086)		(1.089)	(1.200)	
Abnormal Returns (t–1)		-0.016	-0.027	-0.091**			
		(0.037)	(0.032)	(0.029)			
(Intercept)	-0.091	-0.103			1.300		
	(0.069)	(0.071)			(0.808)		
Num.Obs.	3819	3591	3591	3591	5607	5607	5607
R2	0.003	0.005	0.017	0.086	0.004	0.103	0.808
R2 Adj.	0.002	0.004	0.011	0.014	0.003	0.099	0.798
Year FE			Yes			Yes	
Event FE				Yes			Yes
Event window	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +10]	[-10, +20]	[-10, +20]	[-10, +20]

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

I Time to corruption as an instrument

In this section I show that I can find similar results when adopting an entirely different identification strategy. Instead of relying on a synthetic counterfactual of stock prices, here I use time-to-the-day of FCPA *Investigation* as an instrument for firms' *Returns*. Assuming that the timing of news of an enforcement is exogenous (an assumption I defended in the main text), we can estimate the effect of corruption by comparing *Returns* to companies right before and right after news broke out, by taking a sufficiently small window around the day news hit the public. Effectively, this is analogous to estimating a regression discontinuity design where the running variable is represented by the "days from the enforcement news" with a cutoff at day 0. The procedure is exemplified in Figure I.1, which plots average daily *Returns* to each company based on the distance from the news of enforcement in a window of 7 days before – 7 days after the event. Similarly to earlier visualizations, the first panel shows cases of direct involvement in corruption, the second cases of indirect involvement. I introduce a quadratic polynomial and triangular kernel weights on each side of the discontinuity represented by day 0 (the day that corruption news hit the public). The local average treatment effect (LATE) of enforcement news on firms' *Returns* (by each type of involvement) can be estimated by taking the distance between the intercepts of the two linear models with the vertical line at day 0.

Figure I.1: Regression discontinuity design when using time to the news of enforcement as a running variable. Example of application when adopting a bandwidth of 7 days before and after the event and quadratic polynomial. Observations are weighted with a triangular kernel



I estimate LATEs for cases of direct and indirect involvement in a series of regression discontinuity designs. First, I focus on cases of direct involvement (Table I.1). In models 1–3, I fit a regression discontinuity design with linear, quadratic, and cubic local polynomials, respectively, and select 10 days of bandwidth around the cutoff (day 0). In models 4–6, I use procedures by Calonico, Cattaneo and Titiunik (2014) for optimal bandwidth selection and estimation of a robust and bias-corrected RDD. Across the models, I find that corruption news lead to negative *Returns* ranging from about 0.5% to 1.3% in case of direct implication. When replicating the procedure to cases of indirect involvement (Table I.2), however, I estimate no significant LATE.

Table I.1: Regression discontinuity design in time for direct involvement in corruption

	Simple RDD			Robust RDD		
	(1)	(2)	(3)	(4)	(5)	(6)
LATE	-0.499+	-0.975*	-1.292*	-0.784*	-1.078*	-1.146+
	(0.301)	(0.471)	(0.622)	(0.398)	(0.521)	(0.616)
Num.Obs.	2090	2090	2090	1768	2246	2654
Bandwidth length (days)	10	10	10	8.94	11.23	13.81
Bandwidth Selection	Manual	Manual	Manual	Optimal	Optimal	Optimal
Kernel	None	None	None	Triangular	Triangular	Triangular
Polynomial (LATE estimation)	1st	2nd	3rd	1st	2nd	3rd
Polynomial (bias correction)	None	None	None	2nd	3rd	4th
Firm-clustered SEs	Yes	Yes	Yes	Yes	Yes	Yes

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Models 1–3 report an RDD in time estimated with first, second, and third order local polynomials on both sides of the discontinuity (time = 0). Models 4–6 report a robust RDD estimated using procedure in Calonico, Cattaneo and Titiunik (2014) and first, second, and third order local polynomials.

Table I.2: Regression discontinuity design in time for indirect involvement in corruption

	Simple RDD			Robust RDD		
	(1)	(2)	(3)	(4)	(5)	(6)
LATE	-0.128	0.021	-0.054	-0.106	-0.004	0.060
	(0.272)	(0.410)	(0.588)	(0.330)	(0.464)	(0.504)
Num.Obs.	1729	1729	1729	2000	2000	2752
Bandwidth length (days)	10	10	10	12.82	12.54	16.39
Bandwidth Selection	Manual	Manual	Manual	Optimal	Optimal	Optimal
Kernel	None	None	None	Triangular	Triangular	Triangular
Polynomial (LATE estimation)	1st	2nd	3rd	1st	2nd	3rd
Polynomial (bias correction)	None	None	None	2nd	3rd	4th
Firm-clustered SEs	Yes	Yes	Yes	Yes	Yes	Yes

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Models 1–3 report an RDD in time estimated with first, second, and third order local polynomials on both sides of the discontinuity (time = 0). Models 4–6 report a robust RDD estimated using procedure in Calonico, Cattaneo and Titiunik (2014) and first, second, and third order local polynomials.

I offer one final test to further probe this alternative design. In Figure I.2, I replicate models 2 from Tables I.1 and I.2 when varying bandwidth size (meaning, the number of days before and after the event) from 2 to 20. The top two panels present estimates obtained when studying *Returns* of companies' stocks in cases of direct (left) and indirect (right) involvement in news of FCPA enforcement. I fit a second-order polynomial on either side of the discontinuity. Across bandwidths greater than 5 and smaller than 15, estimates are negative and statistically distinguishable from zero at a 0.05 level of significance for cases of direct involvement. Some estimates borderline statistical significance but overall the evidence indicates a reduction of about 1% in stock returns for cases of direct involvement. Instead, I find no significant effect for cases of indirect involvement. At the bottom of the figure, I replicate the procedure but I study *Abnormal Returns* to these companies. In this test, I intend this as a dependent variable capturing firms' stock returns "cleaned" from broader market trends. When I do so, estimates for cases of direct involvement are negative and precisely estimated, with estimates of size consistent with the previous ones. Instead, cases of indirect involvement are smaller and never statistically significant.

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