State Sovereignty and Multinational Crime. How Private Networks Extend State Prohibition of Corporate Bribery Beyond Borders

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Acknowledgements

On my first day as a doctoral student, back in October 2019, my supervisor told me that earning a PhD is like running a marathon. She could not imagine how accurate this judgment would become just a few months later, when a pandemic hit the world. Studying in a PhD program has been quite *the* marathon, for me and for many more graduate students in the past three years. We have run our way through lockdowns, online conferencing, and travel restrictions.

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Abstract

Political economy has documented multinational companies' strategy to evade laws against criminal conduct by fragmenting structures across borders. In many issue areas, states attempt to regulate these criminal behaviors by negotiating common rules under international organizations (IOs). How do IO-negotiated rules enable state regulation of multinational companies? In this dissertation, three independent articles address this question focusing on anti-corporate corruption. They argue that states can leverage firms' cross-border ownership chains for regulatory purposes.

The first article studies the impact of home-imposed anti-bribery laws on foreign investment. Similar policies are often argued to put regulated companies at a disadvantage abroad. Instead, the paper finds that home-imposed regulation can improve the competitive position of companies even in a range of corrupt economies. Thus, multinational corporate ownership can diffuse regulatory standards. The second article studies what makes public authorities apply their extraterritorial regulations against certain foreign companies. It finds that regulators leverage territorial connections of foreign companies in order to apply their extraterritorial criminal laws. Thus, corporate ownership enables enforcement of regulations. The third paper evaluates reputational sanctions imposed by investors on financial markets. It finds that firms suffer heavy losses on equity markets when they are directly involved in a corruption scandal, but they can minimize harm by hiding misconduct behind complex layers of corporate ownership. Thus, informal market-based penalties are not complete substitutes for formal state-based regulation.

This thesis offers a detailed view on prospects for state coercive practices *vis-à-vis* multinationals. It shows that states can wield effective regulation against companies' cross-border misconduct

by leveraging their very cross-border structure. Results speak to various literatures in political science, including those on corporate social responsibility, international law and organizations, and corruption. The dissertation also offers an important empirical contribution by collecting a novel dataset on anti-bribery enforcement events.

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Introduction

Motivation: Corporate crime and international organizations

Multinational companies (MNCs) exploit their cross-border structures to further financial crime. MNCs typically fragment illicit activity in complex chains of subsidiaries organized across borders. Companies can purpose such structures to exploit loopholes and arbitrage overlapping legislations. For instance, this allows "tax planning" (Arel-Bundock, 2017) or outsourcing of criminal transactions to unregulated partners (Chapman et al., 2020). Corporate criminals can also conceal their identity behind complex networks of shell companies located across borders with opaque structures and hidden beneficial ownership. Such opaque corporate structures conceal criminal behavior. Companies can thus use foreign subsidiaries to bribe local public officials (Jensen and Malesky, 2018; Malesky et al., 2015), launder ill-gotten gains (Sharman, 2010), and even finance transnational terrorism (Findley et al., 2015).

Scholars in political science and economics have long recognized that the international mobility of MNCs challenges attempts by states to impose sovereign regulatory policies (Rodrik, 2011; Rudra, 2008; Strange, 1996). Preventing corporate crime, in particular, represents a problem of collective action for countries that host MNCs and for those where companies are legally incorporated (commonly referred to as "home countries"). Host states might find it hard to unilaterally impose standards on foreign capital when it represents a necessary source of income. If regulatory policies impose costs on MNCs, foreign companies might threaten to leave the country (Hirschman, 1970). This strategy could potentially induce a race to the bottom in regulatory or democratic

standards among uncoordinated host countries hoping to maintain a stable influx of foreign capital[1]. When it comes to financial crime, moreover, host countries often lack either the capacity or the interest to police large and opaque corporate groups. This is particularly the case when political elites can extract private gains from corporate misbehavior, as in the case of bribery (Gueorguiev and Malesky, 2012). Regulation also represents a collective action problem for home countries. Regardless of the policy choices of other countries, each home state faces an incentive to turn a blind eye to its companies' foreign criminal activity, if it does not bear its negative externalities or can (indirectly) rip its economic benefits (Gilbert and Sharman, 2016; Kapstein, 1989).

In many issue areas, states attempt to solve this coordination problem and regulate corporate criminal transactions by negotiating common binding rules through international organizations (IOs). This is consistent with classic international relations scholarship that views IOs and international regimes[2] as tools to solve collective action problems in an increasingly interdependent world (Keohane and Nye, 1973; Keohane, 1984). Examples of regulatory regimes coordinating corporate standards span across issue-areas. They include: international agreements criminalizing corporate bribery such as the OECD Anti-Bribery Convention (of particular relevance in this dissertation); international forums promoting the adoption of common anti-money laundering standards such as the Financial Action Task Force (FATF); and frameworks to prohibit corporate tax evasion such as the OECD/G20 Inclusive Framework on Base Erosion and Profit Shifting (BEPS)[3]. Effectively, integration of states into a system of regulatory IOs, laws, and regimes alters the traditional form

[1]The relationship between globalization and a regulatory race to the bottom is very contested and this dissertation touches on it only marginally. A non-representative selection of seminal studies that speak to the topic – spanning across environmental regulation, democracy, labor rights, and taxation – includes Drezner (2001), Jensen (2008), Malesky and Mosley (2018), Mosley and Uno (2007), Plümper et al. (2009), Rudra (2008), and Vogel (1997).

[2]International regimes are here broadly defined as "sets of [...] principles, norms, rules, and decision-making procedures around which actors' expectations converge in a given area of international relations" (Krasner, 1982, 186).

[3]These examples differ, among many respect, depending on the type of obligations they impose on states. For instance, whereas the OECD/G20 BEPS Framework is an instrument of soft law – thus, it does not bind participating states – the OECD Anti-Bribery Convention legally binds signatories to adopt and enforce domestic anti-bribery regulations. However, even instruments of soft law can have far reaching consequences. For instance, a December 2022 implementation of the OECD/G20 BEPS Framework at the European Union level will bind member states to adopt domestic minimum corporate tax regulations, see: `https://www.consilium.europa.eu/en/press/press-releases/2022/12/12/international-taxation-council-reaches-agreement-on-a-minimum-level-of-taxation-for-largest-corporations/`. The classic framework on the distinction between hard and soft international law is provided by Abbott and Snidal (2000).

of state sovereignty (Ruggie, 1993; Wendt, 1992), allowing states to exercise their coercive power over cross-border crimes of multinational companies.

Against this background, a recent literature in international political economy has assessed the effectiveness of these IOs (Baradaran et al., 2012). In particular, the emphasis has been on whether companies, *de facto* responsible for compliance with corporate regulatory regimes, leverage their fragmented structures to evade regimes' principles, norms, and laws. Evidence is mixed. In the case of corporate corruption, Jensen and Malesky (2018) provide experimental evidence that companies subject to the OECD Anti-Bribery Convention reduced their propensity to bribe. Similarly, Morse's comprehensive work (2022) shows that banks respond to blacklists produced by FATF by disinvesting from countries sponsoring terrorism; thus complying with the anti-money laundering standards. However, Chapman et al. (2020) show that companies under anti-bribery laws might very well be outsourcing corrupt behavior to unregulated partners. Experimental evidence has also documented the extreme ease with which a criminal enterprise could register anonymous shell companies at law firms (Findley et al., 2015; Sharman, 2010) or open bank accounts (Findley et al., 2021). Are prospects of corporate criminal regulation, negotiated under IO umbrellas, condemned to be "in retreat" (Strange, 1996), *vis-à-vis* internationally fragmented MNCs?

This dissertation is primarily situated within this literature. Broadly speaking, I address the question: How do IO-negotiated rules enable state regulation of multinational companies? The thesis is organized in three independent papers that explore this question from different angles. Contrary to a certain pessimism in the previous literature on IOs' regulatory possibilities, I argue that internationally-negotiated rules provide states with the possibility to expand their regulatory reach even beyond the traditional limit of their sovereignty – *i.e.* territory. This is true, at least, for states that are situated at the core of international regulatory regimes, consistently with an argument made by Drezner (2008).

I make my case moving from the acknowledgment that multinational companies resemble more fragmented transnational chains of more- or less- loosely connected rings, rather than unitary actors moving freely across territories. MNCs are organized in corporate groups where lower-

level companies – called "subsidiaries" – are owned by higher-level ones – "parents" (Kerner, 2014). Each of these companies is bound to a specific territory due to the sunk costs involved in foreign investment (Dunning, 2015; Frieden, 1991). I claim that IO-negotiated policies give states the chance to leverage these cross-border connections, to expand regulatory prerogatives beyond territory (Farrell and Newman, 2019; Hirst et al., 2015). That is, cross-border fragmentation of MNCs turns from a regulatory liability into a coercive asset.

Whereas previous literature has mainly focused on rule-making prerogatives, I make my case by considering the understudied attribute of policing against multinational companies' crime (Andreas, 2004; Thomson, 1995). In particular, I study laws devoted to fight corporate corruption. The two most important legal instruments in the criminalization of corporate corruption are, respectively, a state policy and an international treaty: the 1977 US Foreign Corrupt Practices Act (FCPA) and the 1997 OECD Anti-Bribery Convention. Here, I provide some brief information on these legal texts and their history. Further details on both pieces can be found in the first and second papers of this dissertation.

Until the 1990s, corporate bribery used to be legal – even tax-deductible, see Gutterman (2015) – for MNCs headquartered in the world's most advanced economies. The notable exception was the US, that had adopted an anti-bribery legislation as early as in 1977, when Congress unanimously passed the FCPA in response to corporate corruption scandals emerged from Watergate investigations. The Act established civil and criminal responsibility for US companies engaging in foreign corruption. Notwithstanding such powerful provisions, however, the FCPA remained virtually silent for over twenty years, as US administrations feared its enforcement would tilt the playing field of international competition against American companies[4]. Similar anti-bribery provisions were not, in fact, adopted by most industrialized economies, whose companies would be free to bribe in their conduct of international business without facing legal repercussions at home.

The history of anti-bribery criminalization changed in 1997, when the US managed to have

[4]For excellent reviews on the history of the anti-corruption regime, see Abbott and Snidal (2002) and Spahn (2013).

OECD partners adopt FCPA-like policies[5] with an Anti-Bribery Convention[6]. The ratification of this agreement marked a profound change in the approach of US authorities to the enforcement of the FCPA (Brewster, 2017), as it provided them with the possibility to vigorously enforce the law against US companies and foreign firms as well – thanks to a very broad interpretation of extraterritorial jurisdiction, something I study in paper 2. At the time of writing, a total of 44 countries (all OECD members and 6 non-members: Argentina, Brazil, Bulgaria, Peru, South Africa, and Russia) have ratified the Convention and adopted policies that criminalize the payment of bribes by domestically-incorporated companies in international business transactions. The OECD Convention is a legally-binding treaty and is commonly regarded as a milestone in the global criminalization of bribery (Abbott and Snidal, 2002). Ratifier home states now scrutinize and prosecute bribery perpetrated beyond national borders by their companies, foreign employees, or subsidiaries

Critics have pointed out the limits of this regime and, more generally, of attempts to regulate transnational corporate crime by means of state-based laws. Three points mainly sustain these criticisms. First, these regulations would undermine multinational companies' foreign business (Cuervo-Cazurra, 2008). They would increase costs to regulated companies, putting them at a disadvantage over their unregulated competitors. Thus, regulation would be politically unsustainable (Kapstein, 1989). Second, enforcement of these policies would fail against fragmentation of MNCs' financial crime. Multinational companies can fragment legal and productive structures across borders to evade state-based regulations. This "regulatory arbitrage" has been observed in money laundering (Findley et al., 2015; Sharman, 2010), tax evasion (Arel-Bundock, 2017), and corruption (Chapman et al., 2020). Third, and as a result of these limitations, informal penalties imposed by markets against corporate criminal behavior should be preferred over formal state-based regulations to induce deterrence (Sampath et al., 2018). Based on these three points, states and IOs would appear

[5]Signatories of the OECD Anti-Bribery Convention have all adopted policies that are regarded by legal scholars as analogue to the US FCPA in terms of definitions, criminal provisions, and jurisdictional scope (Brewster, 2017; Leibold, 2014).

[6]Officially called: "OECD Convention on Combating Bribery of Foreign Public Officials in International Business Transactions", see: `https://www.oecd.org/corruption/oecdantibriberyconvention.htm`. Throughout the dissertation, I refer to it as OECD Anti-Bribery Convention, OECD Convention, or simply the Convention.

in a regulatory "retreat" from attempts to prevent transnational corporate crime. The picture would be similar to that in Susan Strange's seminal book (1996): regulatory prospects would be frustrated when confronted with transnational corporate groups. In other words, cross-border fragmentation of companies would undermine one of the key functions of the state: the use of coercion to obtain abidance by law.

Each of the three papers in this dissertation challenges one of the three criticisms raised against state-based regulation of multinational crime. The papers make three distinct contributions that detail how countries can, in fact, use IO-negotiated policies to leverage multinationals' structures for regulatory purposes. Together, they challenge the claim that territorial fragmentation of multinational companies' supply and ownership chains necessarily undermines state regulatory action, ultimately preventing prospects for regulation.

Structure of the dissertation: Synopsis of the three papers

The dissertation is made of three independent studies that address the question: How do IO-negotiated rules enable state regulation of multinational companies? The papers support my claim that IO-negotiated rules allow states to use cross-border fragmentation of MNCs into a regulatory leverage, at least for countries located at the core of international regulatory regimes. The first paper asks: What is the effect of IO-negotiated corporate policies on firms' foreign investment choices? I show that IO-negotiated rules diffuse domestic regulatory policies abroad, by leveraging foreign subsidiaries of domestic companies, without necessarily undermining these firms' business. Thus, state-based regulations are politically viable. The second paper asks: What gives a country the power to enforce its IO-negotiated regulations against a foreign subject? I argue that states leverage domestic subsidiaries of foreign companies to make them comply with local laws and cooperate with local authorities. Thus, state-based regulations are enforceable beyond border by exploiting cross-border corporate ownership connections. Finally, the last paper asks: How do financial markets respond to the enforcement of IO-negotiated rules? I contend that market-imposed informal penalties following enforcement of IO-negotiated regulations misalign with state-based laws under

important points of view and that companies can strategically avoid them. Thus, market-based regulations of companies are not a complete substitute to formal state-based policing.

Paper 1: The effect of home-imposed regulations on foreign business

The first paper focuses on IO-negotiated regulations imposed by home states on foreign subsidiaries of a domestically incorporated company. This regulatory strategy extends abroad the jurisdiction of home policies and conscripts domestic companies to diffuse regulated business models downstream of their corporate chains. Figure 1 sketches the scope of this strategy: a home country member of a regulatory regime extends its policies downstream of a corporate chain, by enlisting a domestic parent company to rule on activities of a foreign subsidiary, potentially in breach of domestic standards. Similar strategies are documented beyond anti-corruption (Jensen and Malesky, 2018), for instance in labor (Malesky and Mosley, 2018) and environmental standards (Prakash and Potoski, 2007; Vogel, 1997).

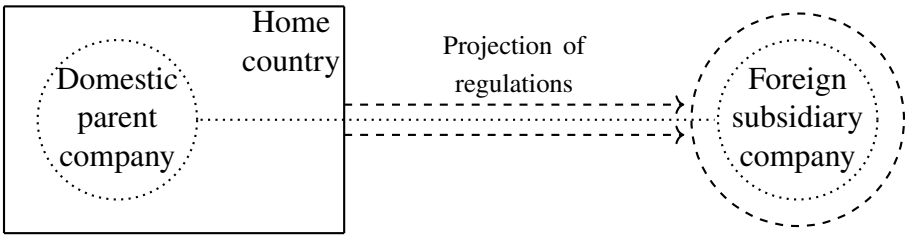


Figure 1: Conceptual scope of Paper 1. Extension of home country's regulation to target foreign subsidiaries' operations

Pessimistic accounts claim that the diffusion of similar standards abroad compromises international competitiveness of companies because it adds unnecessary cost to their operations (Iraldo et al., 2011). When competing with firms that are not imposed similar standards by their home countries, the claim goes, home-imposed regulations make companies operate at a disadvantage (Cuervo-Cazurra, 2008). The claim would then justify the expectation of a regulatory race-to-the-bottom, because states would implement weaker regulatory standards in order to protect firms' competitiveness (Kapstein, 1989). In other words, market pressures would force states to back

down from the regulatory ambition to rule on foreign behaviors. However, evidence also exists that companies' business can profit from regulations (Genovese, 2021; Kennard, 2020; Perlman and Sykes, 2017) and expectations of a regulatory race-to-the-bottom are often disproved (Prakash and Potoski, 2006).

The first paper thus studies the effect of home-imposed regulatory standards on foreign investment of regulated companies. I argue that the regulatory strategy of enlisting domestic companies to diffuse cleaner business standards in host countries down the line of corporate ownership chains does not necessarily undermine business. Rather, regulatory policies change the setup of companies' foreign competitive position, leading them to redirect investment choices towards destinations where they can fully exploit cleaner standards mandated by home regulators.

I focus on anti-corruption laws adopted by 44 countries under the 1997 OECD Anti-Bribery Convention. I argue that foreign investment choices of companies incorporated in these countries are affected by these domestic policies in a non-linear manner. Competitiveness is unaffected in non-corrupt host countries – as in these destinations companies are not at risk of being demanded bribes. Instead, investment is enhanced in mid-corrupt host economies because here stricter anti-corruption standards operate as a tying-hand mechanism that liberates companies from bribe requests (Perlman and Sykes, 2017). Regulated companies fully exploit this mechanism to get an advantage over their unregulated competitors. I argue that a negative effect of regulation on investment holds only in *extremely* corrupt host economies, where the tying-hand mechanism is not sufficient to enhance firms' position in bargaining with local public officials (Ades and Di Tella, 1999).

The argument is supported by analyses of various data sources. First, a firm-level analysis of investment decisions by 3871 major multinational companies between 2006 and 2011 shows that the probability of investing into a moderately corrupt economy increases of up to 31% for regulated companies (when compared with similar unregulated firms). Host destinations in this category of countries include economies with mid-corruption levels like Singapore, Brazil, China, Indonesia, Italy, Mexico, and the United Arab Emirates. I find that regulated companies are worse off only in countries with extreme levels of corruption (such as Egypt, India, Kazakhstan, Nigeria, or Russia):

here the probability of investing decreases by up to 58%. Second, I model country-dyadic foreign direct investment (FDI) flows between 1994 and 2006 in a generalized synthetic control design (Xu, 2017) and find my firm-level results are robust even when aggregated at a country-level. To sum up, the paper shows that regulated businesses are not necessarily disadvantaged against unregulated competitors in an integrated international economic arena. Thus, IO-negotiated rules allow states to project policing prerogatives abroad, by leveraging the very cross-border structure of companies.

Paper 2: Enforcement of extraterritorial policies along corporate chains

In the second paper, I delve into one key way corporate ownership chains can be leveraged to further regulatory standards: through extraterritorial powers. I argue that this example shows how the potential firm strategy to evade regulations by creating complex corporate groups (see Chapman et al., 2020; Findley et al., 2015) can backfire. A group of countries – labelled "global sheriffs" in the paper – have in fact provided broad interpretations to their jurisdictional claims, extending their legal arms extraterritorially (Kaczmarek and Newman, 2011; Putnam, 2009). The most important "global sheriff" is arguably the US, which prosecutes foreign crime in realms including transnational terrorism, drug trafficking, and corporate crime (Andreas and Nadelmann, 2008; Garrett, 2011). However, there is evidence that "global sheriffs" like the US investigate only a fraction of the misconduct they have potential jurisdiction on. Determinants of such selection remain understudied.

In the paper, I study what explains selection of cases to be prosecuted extraterritorially. I answer that "global sheriffs" leverage corporate ownership chains and establish jurisdiction over a foreign company's (alleged) misconduct by leveraging domestic subsidiaries, thus exercising regulatory pressures upstream of a corporate group. That is, prosecutors exploit *territorial* connections of a foreign company to their country in order to enforce domestic laws with *extra*territorial reach. The scope of the paper is sketched in Figure 2. A "global sheriff" extends its regulatory reach over a foreign parent company's alleged misconduct, by leveraging connections to a domestic subsidiary.

I focus on prosecutors in the US and on the extraterritorial application of the US anti-bribery

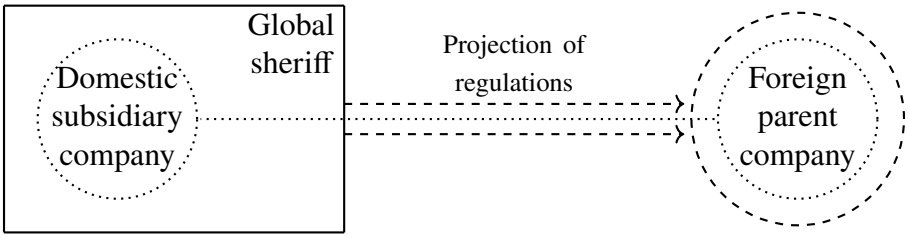


Figure 2: Conceptual scope of Paper 2. A "global sheriff" country applies its regulatory policy against a foreign parent leveraging domestic corporate connections

policy, the 1977 Foreign Corrupt Practices Act (FCPA). When a foreign parent company invests in the US, and establishes a subsidiary on American soil, it exposes itself to the reach of US prosecutors. Borrowing an expression from strategic management studies, that company increases its legal "liability of foreignness" (Johns and Wellhausen, 2021; Zaheer, 1995) *vis-à-vis* comparable foreign firms that have no similar territorial presence in the country. I argue that prosecutors exploit such exposure to their economy in order to obtain *de jure* authority on a case and especially apply their provisions *de facto*. In particular, I claim that foreign companies with a territorial connection to the US are more likely to cooperate with local prosecutors, when they are suspect of misbehavior, and feed them with classified information and documents necessary to investigate a case.

I provide evidence by collecting novel data on investigations for corporate corruption initiated under the umbrella of the international anti-bribery regime. I web-scrape text documents from a collection of worldwide anti-bribery actions[7] and obtain a dataset where individual firms are alleged to have paid bribes in the conduct of international business. For this paper, I keep observations relative to 402 non-US companies alleged of bribery by authorities of any country between 1997 and 2021. I measure whether each of these suspected companies has ever been investigated under the broad-reaching terms of the American FCPA. I merge this information with firm-level data including investment locations. My analysis shows that the likelihood of being investigated by US agencies almost doubles (0.30 increase over a baseline probability of 0.33) for suspected companies with at least one investment in the US. Most importantly, I show that this effect is largely driven by voluntary cooperation of companies with prosecutors, as implied by my argument: I find that the

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

probability that a suspected company offers cooperation to US authorities increases by about 0.20 when that company has at least one investment on US soil. I illustrate this mechanism with two examples drawn from the telecommunication industry.

This result offers a twofold implication for my argument that states can leverage multinational fragmentation for regulatory purposes. First, it challenges the conventional view that territoriality is a liability that constrains regulators' action against supposedly liquid illicit flows. Rather, I show that fragmentation of capital across borders creates territorial *connections* that can be leveraged as a regulatory bargaining chip. This result aligns with the "weaponized interdependence" framework (Farrell and Newman, 2019) to the extent that it shows how "networked liabilities" of companies (Crasnic et al., 2017) can be used for coercive purposes. Private companies' fragmented operations create chokepoints that states can strategically exploit to extend their policy reach abroad (Farrell and Newman, 2022). However, a second implication offers a nuanced conclusion. The paper also shows that even powerful regulators, that are located at the core of regulatory regimes (Drezner, 2008) and that on paper are capable of imposing standards on companies from all over the world, need some territorial connection with a company in order to effectively exercise coercive prerogatives. Thus, I document a limitation for regulatory prospects: only countries with sufficient connections to the chokepoints of cross-border ownership chains might have the ability to exercise this regulatory strategy.

Paper 3: Market-based informal penalties across corporate groups

The third paper moves from the conclusion that it might take being at the core of the international economic architecture in order to exercise coercion on fragmented company chains by legal means. I move from here and study one way of regulating multinational companies that potentially complements formal legal tools: sanctions imposed by financial markets. An estimated 80% of every dollar lost in value by a company, following news of its involvement in financial fraud, is imposed by financial markets (Karpoff et al., 2008; Sampath et al., 2018). That is, formal penalties only account for the remaining 20%. Similar kind of penalties are documented in cases of companies violating human rights (Kreitmeir et al., 2020) or involved in negative environmental, social, and

governance reports (Capelle-Blancard and Petit, 2019; Krüger, 2015). It is thus often argued that markets can substitute for weak state capacity to regulate complex corporate chains (Fukuyama, 2016; Morse, 2022). To the extent that financial markets impose effective sanctions against companies' misconduct, investors could thus behave as a sort of "global civil society" (Ruggie, 2018), naming-and-shaming corporate criminals that states struggle at sanctioning.

However, it is unclear whether investors perform such regulatory function across a fragmented corporate chain. Namely, studies have not assessed whether markets penalize companies for misconduct by their subsidiaries. This is a concerning gap because the secrecy that comes with such fragmentation of ownership can be purposed to further financial crime (Sharman, 2010) and to evade regulations (Chapman et al., 2020; Findley et al., 2014). Filling this gap thus allows to study the scope and limits of financial markets' regulatory function against corporate crime.

In the final paper, I attempt to fill this gap. I ask: How do financial markets respond to the enforcement of IO-negotiated rules? Figure 3 sketches the conceptual scope of this last paper. I study penalties imposed by markets in two distinct cases, defined over which entity is responsible of alleged misconduct inside a corporate group (indicated in grey). A first case concerns direct involvement of a parent company in the misconduct. In this case, the parent company is directly alleged of responsibility in a corporate misconduct. A second case, instead, concerns involvement of a parent company only through a subsidiary.

I argue that fragmented ownership shields companies from penalties imposed by investors on equity markets. The mechanism by which markets impose penalties is reputation-based (Alexander, 1999). When information on potential criminal misconduct hits markets, investors respond by expanding their supply and contracting the demand of involved companies' shares, due to concerns that negative publicity will undermine future profitability of the company. This drives down prices of companies involved in a scandal. I argue that this effect is moderated by the distance between the involved entity and the parent. The negative effect is non-mediated if the entity involved in a scandal is the parent company itself. It becomes smaller in magnitude as the involved entity is more distant in the corporate group, because control over (alleged) misconduct is also diluted (Alexander

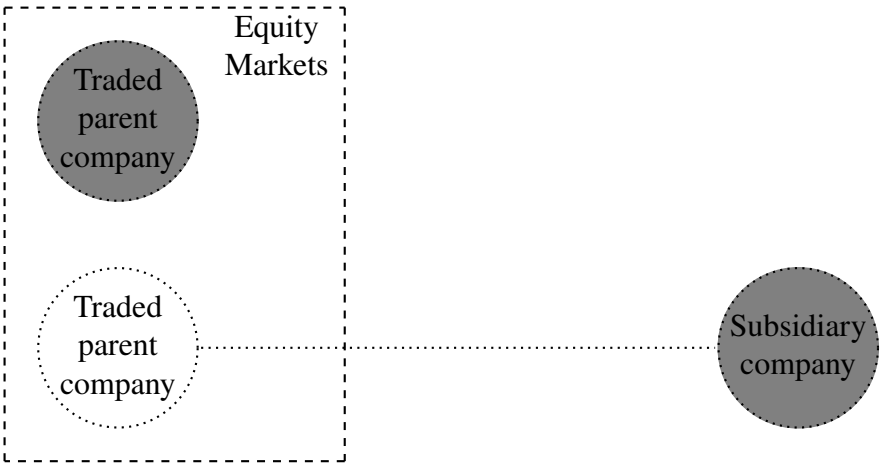


Figure 3: Conceptual scope of paper 3. Market-imposed regulatory penalties along the entire corporate ownership chain

and Cohen, 1999).

Empirics in the third paper test my argument by relying on an event-study design. I draw on data from paper 2 and select 264 events where 214 companies were suspected of violating the US FCPA. I retrieve the date information on alleged malfeasance first hit the market and I code the ownership relationship between the involved entity and the parent. I classify events in three categories: 1) those where the parent company was directly alleged of malfeasance (53% of the cases), those where involvement was indirect and occurred through a wholly-owned subsidiary (24%), and those where indirect involvement occurred through a majority-owned conduit (23%). My event-study design retrieves causal estimates of the release of information on firms' daily stock returns, by imputing synthetic counterfactuals on the day information hit the market. I estimate significant losses for parent companies when involvement is direct. In this case, markets amplify the regulatory performance of prosecutors. When directly involved in a corruption scandal, the median company in my dataset lost about $0.30 per share on the first two days after an unexpected release of information, equalling a loss in market capitalization of about $132 million. The effect cumulates to a loss of about $516 million still detectable about 20 days after the scandal. However, no significant effect is detected in either category of indirect involvement.

A potential inconsistency occurs when considering the role of reputational costs in this paper

and the arguments made in the previous two. The first two papers claim that companies include reputational costs in their rational choices. In paper 1, regulated companies change investment destinations if they expect reputational damage for involvement in bribery in host countries. In paper 2, corruption scandals turn into reputational costs for involved companies, leading them to cooperate with local authorities of countries they are more exposed to. Instead, paper 3 questions whether reputational effects are in place: it finds stock market responses only following scandals involving parent companies directly. It does not find significant effects (neither statistically nor substantively) for scandals where the parent is involved indirectly. What is the role of reputational costs in firms' rational assessments? The contradiction is solved by distinguishing which entity is involved in a corporate bribery scandal. Arguments in papers 1 and 2 refer to corporate scandals (or expectation thereof) involving an entire corporate group, crucially including the parent. Their (expected) costs enter the rational choice made by parent companies and determine investment choices (paper 1) and cooperation with authorities (paper 2). Paper 3 does not question that reputational costs exist for similar cases of direct involvement in a scandal: it does identify significant losses for parents involved in corruption in a similar way. Instead, scandals do not generate significant market reactions for the parent company only when involvement is exclusively indirect and successfully concealed inside a corporate group.

The third paper thus concludes the dissertation with a grim look on prospects to leverage corporate ownership structures for regulatory purposes. Whereas the first two papers showed that states can leverage IO-negotiated rules to navigate corporate ownership chains downstream or upstream and impose *formal* regulations, the third shows that these very structures can be purposed to shield a company from negative *informal* responses on financial markets. The paper contributes a final piece to the discussion on a "regulatory retreat" of IOs and states from markets. If IO-negotiated policies struggled at regulating complex multinational corporate groups, as implied by this account, market responses to financial crime could be reasonably looked at as a sort of regulatory device (Fukuyama, 2016). This is consistent with a large literature in international relations that considers reputation a powerful device to ensure compliance with international norms (Baradaran et al., 2012;

Chayes and Chayes, 1993; Simmons, 1998, 2010). Contrary to this expectation, the paper shows that corporate ownership structures are a device that performs a threefold cynical anti-regulatory function. They do not only allow to conceal criminal transactions (Findley et al., 2015) or to evade regulations (Chapman et al., 2020), as shown by previous studies. They also protect companies from reputational sanctions imposed on financial markets, were misconduct to be made public. In other words, the paper documents a case of "retreat of markets" from expected regulatory functions.

Implications, contributions, and future research avenues

The three papers use anti-corruption as a case-study to offer a nuanced view on prospects for states to regulate globalization. They show states are not constrained by territorial limits in their exercise of regulatory powers over private actors with fragmented territorial operations. Rather, regulatory frameworks or laws negotiated under IOs provide states with scope for expanding their regulatory reach beyond territory, by exploiting the very connections that fragmentation of MNCs creates. If anything, the real regulatory failure documented in the papers is on the side of markets, which are often presumed – or feared, see Johns et al. (2019) – to make up for the limits of state-based governance (Fukuyama, 2016; Ruggie, 2018).

External validity of the study's conclusions depends on its scope conditions. The dissertation focuses on bribery, which is one of the many possible forms of corruption (Heywood and Rose, 2014). I also do not focus on other types of corporate crime or non-criminal misbehavior. Generalizing my concluding remarks to other types of misbehavior hinges on the features of the international regimes designed for their prevention. The anti-bribery regime, in fact, has a peculiar history and features that other regimes might lack. First, it has been in existence for almost thirty years. Other regimes, as the anti-corporate tax evasion, present a very similar history but are way younger[8]. Moreover, the anti-bribery regime is constituted by a multitude of agreements at various multilateral levels (including the OECD, UN, and EU) that share common anti-bribery principles, rules, and norms (Krasner, 1982) and that are legally binding for partner states (Abbott and Snidal, 2002).

[8]Consider, for instance, the much more recent OECD/G20 Inclusive Framework on Base-Erosion and Profit-Shifting (BEPS), aimed at fighting corporate tax avoidance.

These agreements cover the world's main headquarters of MNCs, therefore the main exporters of bribery (Picci, 2018). Finally, the anti-bribery regime is characterized by the central role played by US agencies, (Kaczmarek and Newman, 2011; Tomashevskiy, 2021) which have almost a 50-year long experience with anti-bribery laws, special anti-corruption units (Brewster, 2014), and impose disclosure of government investigations to add reputational costs on top of formal fines (Garrett, 2011; Sampath et al., 2018). Other regimes lack a "global sheriff" performing similar functions at the core of their design. Similar mechanisms of disclosure have been proposed only recently by the US Securities and Exchange Commission (SEC) for reducing companies' emissions[9]. It is still too early to study their effects.

Net of scope conditions, conclusions touch on important debates in political science and international relations and open up to future avenues of study. They call into question conventional pessimistic views on the effective limits of regulation. Interactions between MNCs and states blur the "grand dichotomy" (Bobbio, 1989) between a public, sovereign sphere that monopolizes coercion and enforces rules (Owen and Strong, 2004) in opposition to a private one that must abide by them. MNCs often intervene in regulation through public-private governance partnerships (Ruggie, 2002), lobbies (Richter et al., 2009), or bribery (Harstad and Svensson, 2011). Scholars have often claimed that these interactions show how MNCs' power dwarves state prerogatives and sovereignty (Bordo et al., 1999; Strange, 1996; Vernon, 1981), potentially even substituting them in shaping the rules of the international economic architecture (Johns et al., 2019). Others, instead, have stressed that the prominence of internationally mobile capital makes state policies ever more relevant (Frieden, 1991; Hirst et al., 2015; Jensen, 2005). This dissertation contributes to this debate by emphasizing that IOs and regulatory regimes alter the traditional form of state sovereignty (Ruggie, 1993; Wendt, 1992), pushing its boundaries beyond territory to effectively rule over foreign misconduct of MNCs.

Results also contribute to literatures on global governance, policy diffusion, and state compliance with international law. The literature on global governance has emphasized the challenges posed by economic interdependence to state action. States' integration in the international economic system

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

makes problems and policy solutions interdependent (Keohane and Nye, 1973). For example, globalization makes it hard for states to implement autonomous macroeconomic policies (Rodrik, 2011). States' regulatory policies also become dependent on one another (Gilardi, 2010), as the race-to-the-bottom argument suggests (Prakash and Kollman, 2003; Rudra, 2008). State capacity to impose corporate social responsibility standards on fragmented supply chains might also be undermined, as in the example of labor rights protection (Mosley, 2017). When companies fragment their legal structures across territories, moreover, conflicting jurisdictional claims among sovereign states can emerge (Efrat and Newman, 2016; Madsen et al., 2021). Past literature in global governance has moved from these premises to study conditions for territorially-bound countries to implement autonomous policies on internationally mobile capital (Mosley, 2003). I contribute to this literature by showing that the very cross-border ownership chains that build up complex interdependence can be leveraged by states to impose their coercive practices.

My claim that IOs enable states to leverage cross-border ownership chains to project their regulation abroad resonates with an argument made by a growing number of historians of international relations about the dawn of the moder international system. These scholars have pointed out that early modern states redefined their sovereign prerogatives beyond medieval practices, and expanded them beyond their territories, by leveraging early transnational economic networks of private actors (Bartlett, 1994; Cooney, 1980; Srivastava, 2022). As these studies show, nation-states gained the power to project their political ambitions abroad by leveraging profit-making groups of merchants (Thomson, 1996) or late XVI century chartered corporations (Sharman and Phillips, 2020). I argue that a similar relationship still characterizes contemporary state sovereignty in a global economic arena.

The thesis also sheds light on a yet-unexplored mechanism of policy diffusion. So far, the literature on policy diffusion has mainly posited mechanisms that are based on the flow of new information between countries (Simmons and Elkins, 2004). Instead, I show that policy diffusion can be exercised through cross-border ownership chains. To the extent that corporate groups can be thought of as cross-border networks, whose nodes are located and bound to a specific territory,

I contribute to a growing literature on how states exploit transnational private networks to enforce coercive powers (Farrell and Newman, 2019). I do so by studying a process where states leverage companies' cross-border activities to diffuse, enforce, and amplify the effect of regulatory policies.

Beyond financial crime, some conclusions from the dissertation have general implications for global crime, which represents a significant governance problem for states (Andreas, 2004). Criminals' fragmented activities across border escape state coercive powers. This has been documented in the cases of transnational terrorism (Sandler and Enders, 2004), organized crime (Williams, 1994), and drug trafficking (Shelley, 2014). Scholars have studied how states manage to police global flows of money, goods, services, and people in order to prevent these phenomena (Andreas and Nadelmann, 2008; Jo and Simmons, 2016; Simmons et al., 2018). The general conclusion is that crimes that spill over across borders require a transnational solution by states. To this aim, states attempt to coordinate their action by harmonizing legal responses to international crime using international regimes (Keohane, 1984; Ruggie, 2018). I document the scope of state action against similar transnational problems. In particular, I show that the very fragmented structure of global crime can be purposed as a regulatory leverage.

Questions that touch on core issues of international law arise. The dissertation shows that states can effectively regulate transnational corporate crime. However, evidence exists of countries in defiance of their duties to hold corporate criminals accountable for foreign misconduct (Findley et al., 2015; Jensen and Malesky, 2018). When such duties are defined under international agreements, states effectively defect from obligations to comply with international law (Brewster, 2014). If multinational ownership does not necessarily hinder state regulatory capacity, why do countries often fail to hold companies accountable for foreign crime? This question speaks to the long-lasting debate on causes of state compliance with international law (Chayes and Chayes, 1993; Simmons, 1998; Von Stein, 2005). It offers the possibility to renew the focus on causes of state *defection* from international agreements.

Multiple questions also emerge on the effects of using IO-negotiated rules to project state regulation. My dissertation shows states extending their regulatory reach beyond borders, affecting

behaviors of private actors in foreign jurisdictions. Crucially, such extension of states' power beyond border occurs underneath the umbrella of a common regulatory regime where states agreed on harmonized rules. This calls into question the responses of other states to similar projections of power, specifically in contexts where a similar regime does not exist. Questions that could be studied include: Do standards imposed on regulated companies abroad spread to foreign *un*regulated competitors? Or else does this difference in standard open up to opportunities for further arbitrage (for a similar argument, see Chapman et al., 2020)? How do public opinions abroad respond to prosecution of local companies by foreign "global sheriffs", *e.g.* US agencies (Madsen et al., 2021)? Do such foreign prosecutors substitute or complement the activity of domestic agencies? Can authorities learn from these foreign prosecutors (Gilardi, 2010)? Future studies could move from the contributions of this dissertation to address these novel questions opened up by the three papers.

Paper 1

Do Corporate Regulations Deter or Stimulate Investment? The Effect of the OECD Anti-Bribery Convention on FDI

Abstract

International organizations (IOs) help countries coordinate their regulatory response against firms' financial crime. By leveraging multilateral rules, states threaten prosecution at home and conscript companies to diffuse sustainable business models abroad. These policies are often criticized for pushing firms' investment away from host economies with lax regulatory standards, where financial crime is more likely to happen. Yet, regulations should also cut crime-induced informal costs and favor investment. This paper reconciles these opposite expectations and shows they are special cases of a single argument. I study the impact of multilateral anti-bribery rules on firms' cross-border investment. I claim that their effect depends on the level of corruption of the host economy. It is null in non-corrupt countries. It is positive where corruption is moderate: here, laws provide a leverage to refuse paying bribes and cut related costs. The effect is negative where corruption is endemic: anti-bribery laws expose firms to additional regulatory costs. I offer evidence in favor of the argument by leveraging various data sources. Data on investment by 3871 firms between 2006 and 2011 show that regulated corporations have a +31% probability of investing in moderately corrupt economies than unregulated firms, which plummets to −58% in extremely corrupt countries. A synthetic counterfactual study of country-dyadic investments corroborates this finding. IO-mandated policies change the economic setup of international competition, not necessarily to the detriment of subject firms' foreign business.

1.1 Introduction

Corporate crime consists of very complex cross-border transactions. For instance, a multinational company (MNC) can bribe in a market outside its home country's jurisdiction to circumvent local competition or extract rents (Malesky et al., 2015). These bribe payments, then, pass through streaks of bank accounts across several countries (Cooley and Sharman, 2017). Alternatively, corporate criminals can conceal illicit funds in jurisdictions with poor money-laundering standards (Findley et al., 2015; Sharman, 2011).

Countries attempt to regulate such flows and solve coordination problems by adopting common rules under the umbrella of international organizations (IOs) (Keohane, 1984). Members of the Organization for Economic Cooperation and Development (OECD), for instance, agreed on the adoption of common anti-bribery policies in 1997 to solve a prisoners' dilemma-like stalemate (see Abbott and Snidal, 2002; Tarullo, 2004). Similarly, in 1989 the G7 launched the Financial Action Task Force, to coordinate anti-money laundering efforts. Finally, in 2013 the OECD and G20 started a joint framework aimed at combating corporate tax evasion in the form of base erosion and profit shifting (BEPS). Through these IOs, states "stretch the arm of their laws" beyond borders, to prohibit foreign misconduct by companies incorporated in their jurisdictions (Kaczmarek and Newman, 2011). These IOs conscript firms under their regulatory umbrella to diffuse sustainable corporate standards, under threat of prosecution.

Research showed that corporate regulations embedded in IOs can effectively curb criminal behavior (Jensen and Malesky, 2018; Morse, 2019). But how do they affect firms' legitimate activity, such as their foreign investment? Political economy research advances two opposite expectations. First, policies would raise additional costs for regulated companies, thus *deterring* investment. For instance, anti-bribery laws would increase risk of investing into corrupt countries for regulated companies, because exposure to bureaucrats' bribe requests is higher there (Gueorguiev and Malesky, 2012) and thus the risk of facing prosecution under IO-negotiated rules (Cuervo-Cazurra, 2008). Yet, an opposite hypothesis expects that regulatory policies *empower* firms'

foreign investment. They would force companies to devise business models that cut costs induced by uncertainty of criminal practices in countries that otherwise lack regulatory standards. For instance, anti-bribery provisions can tie companies' hands, allowing them to refuse bribe-requests and cut down costs of corruption (Davis, 2002). IOs would thus offer regulated companies an advantage when investing into countries with lax standards of economic activity.

How to reconcile these opposite expectations? In this paper I propose a single argument to unify them. I show that the two claims, which I label respectively *deterrence* and *empowerment*, are observable special cases of a single general dynamic. I study regulations that prosecute foreign bribe payments by MNCs adopted under a common IO umbrella.

In this context, I address the question: What is the effect of IO-negotiated anti-bribery policies on firms' foreign investment choices? My answer unifies the *deterrence* and *empowerment* hypotheses, by claiming that IO-negotiated anti-bribery policies subject regulated companies to both mechanisms simultaneously, when deciding on a foreign investment in a corrupt economy. I claim that their net effect on investment depends on the level of corruption of the host economy. Anti-bribery policies provide regulated firms with a legal leverage which is strong enough to refuse bribe requests only where public officials' bargaining power is relatively weak. Since public officials' power to demand bribes increases in the level of corruption of a country (Ades and Di Tella, 1999), I expect anti-bribery policies will favor investments in moderately corrupt host economies. Policies will deter investments only in *extremely* corrupt economies, where companies cannot refuse bribe requests. In other words, I claim that regulatory policies adopted under an IO umbrella do not necessarily disadvantage firms in international competition. Rather, they alter investment conditions faced by companies and lead them to restructure foreign investment choices accordingly.

Empirically, I study laws under the 1997 OECD Anti-Bribery Convention[1], that criminalized foreign bribe payments by companies from 44 ratifier countries. Two exercises support my argument. First, I leverage data from Beazer and Blake (2018) and model individual decisions by 3871 firms

[1]For the sake of brevity, in the text I refer to the "1997 OECD Convention on Combating Bribery of Foreign Public Officials in International Business Transactions" as "OECD Anti-Bribery Convention", "OECD Convention", "the Convention", or similar (always capitalized).

to invest in a foreign location between 2006 and 2011. I show that firms under OECD anti-bribery policies make investment decisions conditionally on the level of corruption of the host economy, non-linearly. I find that firms from ratifiers are no more likely than their unregulated competitors to invest in non-corrupt economies. They are up to 31% more likely to invest in moderately corrupt host economies. Instead, they are 58% less likely to invest in *extremely* corrupt destinations. This exercise provides insights in support of my argument at the level of investment decision-makers. Second, I employ time-varying country-dyadic data in a generalized synthetic control design to achieve a more robust identification of the proposed effect. Results confirm findings from the firm-level analysis. Empirics show that multilateral anti-bribery policies affect firms' investment into corrupt economies, but not necessarily in a negative way. In fact, they favor investment of regulated companies into a range of moderately corrupt countries.

This paper leverages the anti-corruption case to reconcile two opposed expectations on the effects of international law for business. To the best of my knowledge, it represents the first attempt at such reconciliation. The anti-corruption case, I argue, shows that IOs can diffuse corporate regulations without necessarily undermining companies' licit activity. These are good news for ensuring sustainable business models through multilateral negotiations. However, conclusions are bad news for host countries with *extremely* weak regulatory standards, which would perhaps need IOs to diffuse regulated business practices the most. Here, the strategy backfires. These countries are left exposed to investments from unregulated firms, who can arguably commit corporate felonies, remain unpunished, and reinforce existing levels of criminal activity. This pessimistic conclusion adds to recent findings on the perverse effects of anti-corruption IOs induced by different standards among firms (Brazys and Kotsadam, 2020; Chapman et al., 2020) or by corrupt member states (Hafner-Burton and Schneider, 2019).

The study concludes that IO-negotiated corporate regulations do not univocally disadvantage or favor companies in international business. Rather, they alter the conditions of firms' international competition, interact with institutional characteristics of foreign host markets, and lead companies to restructure investment accordingly. This takeaway potentially travels beyond the anti-bribery

regime. The effect of corporate regulations for business has in fact been studied across several domains (*e.g.* Bruno and Claessens, 2010; Genovese, 2020; Kennard, 2020; Wintoki, 2007). The regulatory strategy I document is a form of corporate policy diffusion (Simmons and Elkins, 2004), where home countries negotiate common rules for business under an IO umbrella and conscript domestically-incorporated companies to diffuse them abroad. Policy areas where states operate similarly include the prevention of money laundering, corporate tax evasion, human and labor rights violation, or environmental damage. The paper thus speaks to a literature in international political economy that studies the determinants of such strategy (Putnam, 2009), its effects on other states (Kaczmarek and Newman, 2011), and on companies (Findley et al., 2015; Jensen and Malesky, 2018; Kalyanpur and Newman, 2019; Morse, 2019).

Finally, the study speaks to an important question in international political economy: whether, and to what extent, international institutions affect behaviors of private transnational companies (Gray, 2009). A vast scholarship has addressed this question studying the effect of IOs regulating licit transactions on foreign investment. Typical examples include institutions creating fora for arbitration in investment disputes or those protecting investors' rights (Allee and Peinhardt, 2011; Biglaiser and DeRouen, 2010; Neumayer and Spess, 2005; Skovgaard Poulsen, 2014; Tobin and Rose-Ackerman, 2011). I address the same question from a different angle. I study what effect international *criminal* laws, and IOs aimed at keeping economic exchanges above board, have on private cross-border investment.

1.2 FDI, bribery, and the anti-bribery regime

With a foreign direct investment (FDI), a "parent" company, located in a home country, establishes ownership[2] of a "subsidiary" firm in a host country. The transaction rests on a cost-benefit evaluation. Firms go multinational if advantages outweigh costs (Dunning, 1980) and political risk (Jensen, 2008).

[2]In this article I explicitly do not consider other strategies to invest in a foreign market than ownership, such as licensing or joint ventures with local partners (Das, 1999).

Companies can bribe local public officials in the conduct of international business (Pinto and Zhu, 2016). "Active bribery"[3] – that is, the offer of a bribe payment – is documented in the registration of an MNC and procurement in corrupt economies (Gueorguiev and Malesky, 2012). It does not univocally affect the cost-benefit evaluation made by firms. On the one hand, corruption offers advantages to foreign companies, "greasing the wheels of commerce": it allows firms to enter local markets, overcome competitors, and extract rents (Knutsen et al., 2017; Malesky et al., 2015; Søreide, 2006; Zhu, 2017). On the other, it increases costs faced by a firm, because it works as a tax demanded by public officials (Treisman, 2007; Wei, 2000) and it creates uncertain and inefficient contracts (Lambsdorff, 2002; Rose-Ackerman, 1975).

Home states have adopted corporate policies to intervene in this calculus and deter foreign bribe-payments made by their firms (Cuervo-Cazurra, 2008). By threatening judicial repercussions at home, these policies conscript domestically-incorporated firms to diffuse anti-bribery standards abroad. In order to be viable, anti-bribery policies have historically been harmonized under the umbrella of international organizations[4]. United States history offers a meaningful example. The US was the first country to prohibit foreign bribery by American companies in 1977, when Congress passed the Foreign Corrupt Practices Act (FCPA). Notwithstanding the powerful provisions in this Act, which established civil and criminal responsibility for foreign corruption, enforcement of the FCPA lagged for decades due to the absence of similar regulations among non-US competitors (Tarullo, 2004). Until the 1990s, the rest of the industrialized countries lacked policies to criminalize foreign bribe-payments. States like Germany or France were even offering tax abatements for bribe costs incurred in international business by their companies, if properly declared (Gutterman, 2015). In such a context, enforcing the FCPA would have tilted the playing field of international competition against US-based companies. It was only when similar policies were adopted at the OECD in 1997,

[3]I abide by a traditional definition of bribery as a specific instance of corruption (Heywood, 1997). It is an informal contract between a private bribe-payer (a firm) and a public official bribe-taker, who exploits a position of power and exchanges a favorable decision for an illicit payment. In particular, I consider *foreign* bribery, where the bribe-payer and payee are of different nationalities and bribes cross borders. These informal contracts typically involve the discretionary award of a public order or licence and they are usually associated with investments (Della Porta and Vannucci, 1999).

[4]Reconstructing extensively the history of the anti-bribery regime is an insightful exercise that, yet, exceeds the scope of this article. For excellent reviews and critical accounts, see Abbott and Snidal (2002); Bukovansky (2006); Leibold (2014).

with an Anti-Bribery Convention[5], that US authorities could start to enforce their own anti-bribery law against multinational companies (Brewster, 2017).

In the anti-corruption regime, the OECD Convention is regarded as a prominent treaty and a milestone. It is a legally-binding agreement, that resulted from a decade-long effort by US administrations to push for multilateral rules against foreign bribery (Abbott and Snidal, 2002; Tarullo, 2004). Under the Convention, signatory countries have all adopted laws that are regarded by legal scholars as analogue to the US FCPA (Brewster, 2017; Leibold, 2014). Similarly to the FCPA, the Convention defines bribery as: "to offer, promise or give any undue pecuniary or other advantage [...] to a foreign public official [...] in order that the official act or refrain from acting in relation to the performance of official duties, in order to obtain or retain business or other improper advantage in the conduct of international business" (Article 1)[6]. Similarly to the FCPA, moreover, the Convention demands signatory parties to adopt measures necessary for making such actions a criminal offence. It also establishes mutual legal assistance among parties and a system to review enforcement. The Convention was initially signed by 34 countries (all 29 OECD members plus 5 non-OECD states). Since its ratification, 10 more countries have joined the Convention.

Ratifier home states now scrutinize and prosecute bribery perpetrated beyond national borders by their companies, foreign employees, or subsidiaries[7]. Crucially, enforcement of this anti-bribery regime does not rest solely on individual countries' efforts. Rather, other member countries can (and do) substitute for weak enforcement by partners[8]. The most notable country applying this strategy is arguably the US: with the ratification of the 1997 OECD Convention, the United States expanded significantly the extraterritorial provisions of its own anti-bribery policy, the FCPA (Leibold, 2014).

[5]The Convention is considered among the strongest anti-corruption regulations. MNCs under this regulatory umbrella account for more than 80% of global outbound foreign direct investment stocks and include 95 of the 100 largest non-financial enterprises (OECD, 2018). As of August 2022, 44 ratifier countries include all current OECD members and 6 non-member states: Argentina, Brazil, Bulgaria, Peru, South Africa, and Russia.

[6]See the text of the Convention: `https://www.oecd.org/daf/anti-bribery/ConvCombatBribery_ENG.pdf`.

[7]For example, in June 2019 the US corporation Walmart Inc. disbursed $282 million to US federal authorities in admission of corrupt payments made by its Brazilian subsidiary. See statements from the DOJ: `https://www.justice.gov/opa/pr/walmart-inc-and-brazil-based-subsidiary-agree-pay-137-million-resolve-foreign-corrupt` and the SEC: `https://www.sec.gov/news/press-release/2019-102` (both last accessed on July 28th, 2021).

[8]This is made possible by the broad terms of the Convention's jurisdiction, Article 4.

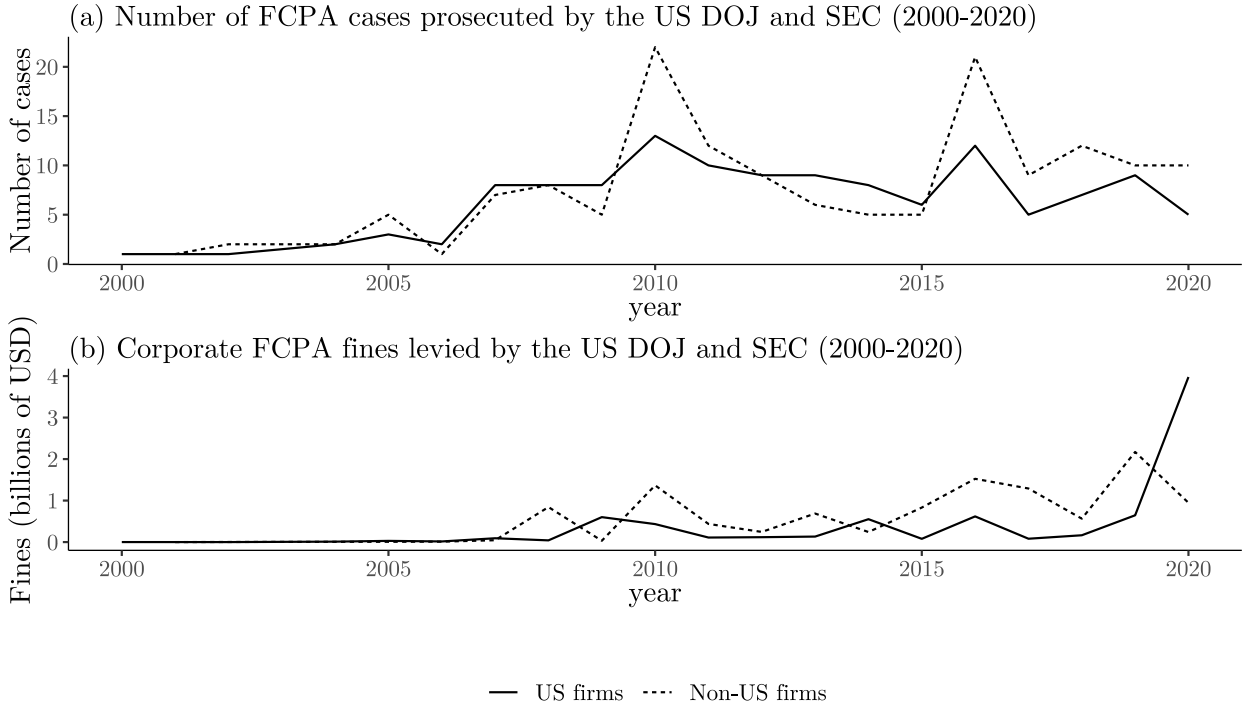


Figure 1.1: FCPA cases enforced by the DOJ and SEC, number of cases and amount of fines. Data from the Violation Tracker, Good Jobs First: `https://violationtracker.goodjobsfirst.org`

As a result, since the early 2000s US regulatory agencies – the Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) – could threaten firms headquartered in OECD Convention's ratifiers of prosecution *in the US* for violating the FCPA (Spahn, 2013), a strategy that some legal scholars describe as "international-competition neutral" (Brewster, 2017, 1615). Cooperation among anti-corruption IO members is well-documented (Gest and Grigorescu, 2010). Since the early 2000s, such mutual assistance allowed US prosecutors vast support from authorities of other signatories to prosecute foreign companies in the US (Kaczmarek and Newman, 2011). Figure 1.1 describes this approach by plotting the yearly number of FCPA cases enforced by the DOJ and SEC involving US and non-US companies and the total fines levied. Starting from 2005, US federal agencies enforced the FCPA vigorously against non-US based companies.

1.3 The conditional effect of anti-bribery regulations on FDI

Recent evidence shows that the OECD Convention was effective in curbing regulated companies' foreign corrupt behaviors (Jensen and Malesky, 2018). How do its policies affect firms' *legitimate* economic activity, instead? Answers from political economy are divided in two camps, which currently lack unification. In this section, I first resume mechanisms and expectations generated by each camp, before arguing that their implications are observable in host economies with different levels of corruption.

I label the first argument *deterrence*. Its proponents stress that anti-bribery standards impose costs for regulated firms that deter investment into corrupt economies. In these countries, the likelihood that a bureaucrat will demand a bribe in the conduct of business is higher. If they pay bribes, regulated companies are at risk of legal costs. Home-country judicial authorities (or those who enforce anti-bribery policies extraterritorially, *e.g.* the US) levy blockbuster fines from guilty companies. But monetary disbursements are not limited to penalties. Authorities, especially in the US, often mandate costly re-structuring of corporate organization to ensure compliance with anti-bribery standards in the future[9] (Garrett, 2011). Finally, financial markets impose reputational

[9]Authorities often turn executive offices of guilty companies inside out. Suspect firms usually set up sys-

costs for corporate corruption. When discovered, foreign bribery turns into scandals with wide international resonance. Markets react to these stories: following a bribery scandal, about 80% of every lost dollar in a firm's share value is due to market-based sanctions (Sampath et al., 2018).

A second account, which I label *empowerment*, draws the opposite conclusion. According to this argument, anti-corruption policies would, in fact, favor investment of regulated companies into corrupt economies. Anti-bribery regulations offer advantages to companies when investing abroad. Namely, they tie companies' hands, offering them a legal leverage to refuse bribe requests from public officials (Davis, 2002). Perlman and Sykes (2017) gathered extensive qualitative evidence from US Congressional hearings with Chief Executive Officers and interviews with corporate and legal practitioners. They conclude that "[regulated] firms exposed to demands for illicit payments may be in a better position to resist those demands without losing valuable business opportunities" (156). In other words, firms subject to anti-bribery standards find they can leverage these rules to enhance their bargaining power *vis-à-vis* that of local public officials (Hakkala et al., 2008; Kaufmann and Wei, 1999). They can thus refuse bribe requests, cut down informal costs and uncertainty induced by corruption, and invest more efficiently (Lambsdorff, 2002; Rose-Ackerman, 1975).

I argue that, in fact, both pulls simultaneously affect the conditions that regulated companies face when deciding whether to invest into a foreign corrupt economy. Before investing into such countries, any company assesses the likelihood that its subsidiaries will have to pay bribes to local public officials in the conduct of business, whether at the startup phase of an investment or *ex-post* (Barassi and Zhou, 2012). Assessing this likelihood means evaluating the relative bargaining power between the firm's subsidiaries and local bureaucrats. Here, I intend bargaining power in a broad sense, as the power of public officials to demand bribes from foreign companies and win their resistances. As argued by the *empowerment* claim, regulated firms might evaluate that

tems of internal investigations, employ third-parties for monitoring internal activities (at least for a probatory period), and periodically rotate international offices to avoid managers established personal connections with local authorities. For a textbook example, see the measures implemented by Siemens AG after an infamous worldwide bribery scandal: `https://www.complianceweek.com/how-siemens-worked-to-fix-a-culture-of-institutionalized-corruption/14915.article` (last accessed on July the 28th, 2021).

their subsidiaries in a corrupt country could resist bribe requests, enabling them to operate more efficiently. However, this can only be successful where public officials' bargaining power is relatively weak. In countries where corruption is endemic, instead, public officials' bribe requests cannot be easily turned down (Ades and Di Tella, 1999; Svensson, 2003). In these contexts, regulation does not provide an effective legal leverage. Rather, it exposes companies to the risk of facing further legal costs at home. Here, regulated firms anticipate they would operate at higher costs, as expected by *deterrence*.

These evaluations, then, translate into actual investment decisions. This argument therefore maps onto three possible comparisons of investment choices made by regulated and unregulated firms, corresponding to three levels of host-country corruption. In a first scenario, firms face the decision of investing into a non-corrupt host economy[10]. Here, regulated firms do not face different investment conditions than (comparable) unregulated competitors, because bureaucrats would be very unlikely to demand bribe payments. Regulated and unregulated firms would make similar investment decisions. This argument justifies the following hypothesis: *Regulated firms are as likely to invest in non-corrupt economies as unregulated companies*.

In a second scenario, firms evaluate investing into a moderately corrupt host economy. I define as "moderately corrupt" those economies where public officials might demand bribe payments to foreign companies, but their operating space for successfully doing so is limited[11]. Here, local public officials could demand bribes to investors, either in the process of starting up a subsidiary or *ex-post*. However, regulated firms could leverage provisions they are subject to, under anti-corruption IOs, in order to turn such requests down and operate more efficiently. They will be more likely to invest in these destinations than similar unregulated competitors, who cannot exploit the

[10] An example could be Denmark, regularly placed at the top of charts for perception of corruption from the business community. See: https://www.ganintegrity.com/portal/country-profiles/denmark/ (last accessed on January the 12th, 2022).

[11] A country like Taiwan, for instance, can be regarded as a moderately corrupt business environment. The country has a relatively low score in corruption perception estimates. See: https://www.transparency.org/en/countries/taiwan. However, instances of corruption in public procurement exist. Transport companies like Alstom SA and Airbus were involved in bribery when securing large public contracts in Taiwan, see: https://www.ft.com/content/f7a01a60-442b-11ea-abea-0c7a29cd66fe and https://www.reuters.com/article/alstom-corruption-sentencing-idUSL1N13820V20151113 (last accessed on January the 12th, 2022).

same legal leverage to cut costs. That is, anti-bribery policies should favor investment of regulated firms into moderately corrupt economies. I formulate the following hypothesis: *Regulated firms are more likely to invest in moderately corrupt economies than unregulated companies*.

Finally, in a third scenario firms evaluate an investment into an extremely corrupt host country[12]. In contexts of endemic corruption, firms would not be able to turn down bribe requests, which are an expected business custom and sometimes even a necessary condition to entry (Zhu, 2017). This is true for regulated and unregulated companies. However, those under the umbrella of anti-corruption IOs are exposed to additional legal costs at home if they are caught paying bribes. They will be less likely to invest here than their unregulated competitors. Anti-bribery policies therefore deter investment of regulated companies into extremely corrupt destinations. A final hypothesis, specific to this scenario, is thus the following: *Regulated firms are less likely to invest in extremely corrupt economies than unregulated companies*.

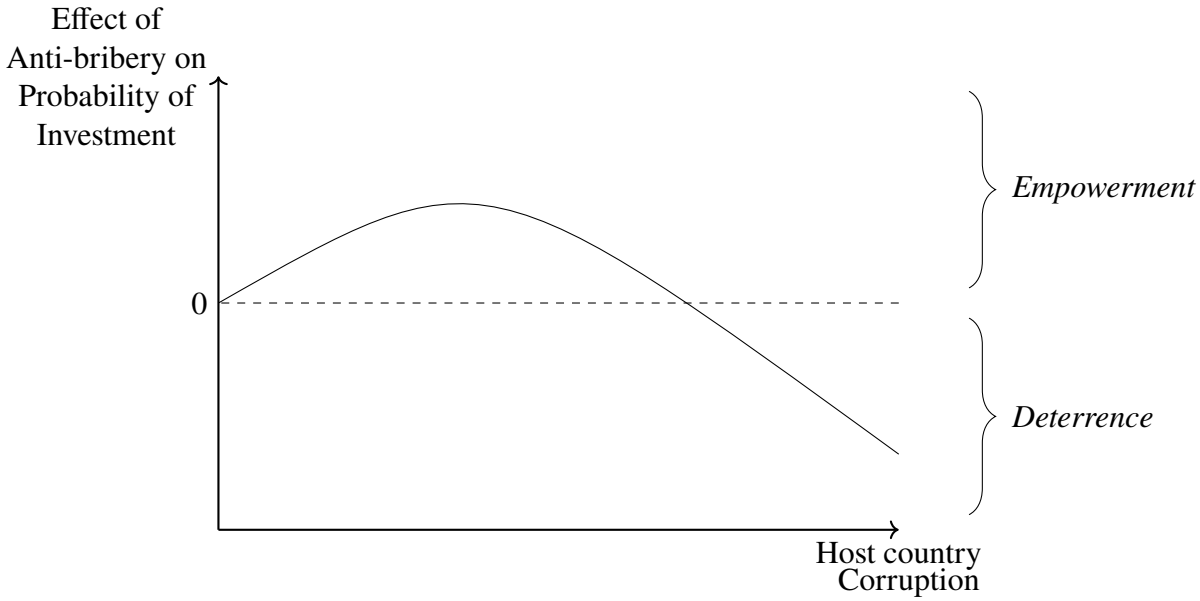


Figure 1.2: Expected effect of anti-bribery laws on investments, conditional on host country corruption

Figure 1.2 generalizes my expectation beyond these discrete scenarios. It sketches the effect of

[12]For example, the business community typically regards Nigeria as a country where corruption is endemic and corrupt fees are expected for the conduct of business. See: `https://www.pwc.com/ng/en/press-room/impact-of-corruption-on-nigeria-s-economy.html` (last accessed on January the 11th, 2022).

anti-bribery policies on the probability of a foreign investment (y-axis), at given levels of corruption of the host country (x-axis). It shows that *deterrence* and *empowerment* can be seen as special cases of the same argument. For low levels of corruption of the host country, the effect should be null. As the host economy becomes more corrupt, regulation advantages firms and increases their probability to invest. *Empowerment* prevails here. As the host country becomes more corrupt, this effect reaches a maximum, decreases, and reverses. In extremely corrupt host countries, anti-bribery policies disadvantage firms due to higher risk of prosecution. Here, *deterrence* dominates and the effect is negative. I thus expect the effect of anti-bribery policies on investment to depend non-linearly on the level of corruption of the host economy.

1.4 Empirical analysis

I claim that anti-bribery policies affect foreign investment decisions by companies conditional on the level of corruption of host economies, in the non-monotonic manner sketched by Figure 1.2. I test my argument focusing on anti-bribery policies under the OECD Convention. I propose two empirical exercises to test my argument. The first one uses firm-level data on foreign investment decisions. It offers insights into the micro-level decision-making process of investors. Results are based on a selection on observables design and lend broad support to my theoretical claim. The second empirical exercise uses country-level data on dyadic FDI flows. Its goal is to provide a more solid identification on top of the micro-level evidence from the first exercise. It employs a generalized synthetic counterfactual design to corroborate expectations from the theory and results from the firm-level study.

1.4.1 Firm-level data

I estimate a firm-level model of investment decisions conditional on corruption. A firm f from country i is observed to invest in country j ($I_{fij} = 1$) only if its latent propensity to invest, I^*_{fij}, is greater than 0. Equation 1.1 expresses the latent variable I^*_{fij}. It is a function of whether country i is

a ratifier of the OECD Convention ($S_i = 1$), and of a continuous measure for the level of corruption of the host country (C_j). Corruption also appears as a squared term (C_j^2). Both C_j and C_j^2 are multiplied by S_i. This represents the statement that the effect of the OECD Convention on the propensity to invest abroad is non-linear in the level of corruption of the host country, as in Figure 1.2. Matrix $\mathbf{X_{fij}}$ includes covariates while u_{fij} is the idiosyncratic error term.

$$I^*_{fij} = \beta_1 \, S_i \times C_j^2 + \beta_2 \, S_i \times C_j + \beta_3 \, S_i + \beta_4 \, C_j^2 + \beta_5 \, C_j + \mathbf{X'_{fij}}\gamma + u_{fij} \tag{1.1}$$

From equation 1.1, the effect of the OECD Convention on the propensity of a firm to invest equals the partial derivative of I^*_{fij} with respect to S_i:

$$\frac{\partial I^*_{fij}}{\partial S_i} = \beta_1 \, C_j^2 + \beta_2 \, C_j + \beta_3 \tag{1.2}$$

Equation 1.2 represents the non-linear effect of the OECD Convention on the propensity of a firm to invest abroad, conditional on the level of corruption of the host country. It is a parabola with an expected inverted-U shape, as in Figure 1.2. Therefore, β_1 is expected to be negative, β_2 positive, and β_3 null.

I estimate equation 1.1 with firm-level data drawn from the Orbis Corporate Ownership Database[13], retrieved from Beazer and Blake (2018). This dataset reports information on foreign subsidiary incorporations made by 3871 individual parent firms between 2006 and 2011. It reports the country of origin of the parent firm (home country) and that of the subsidiary (host country) for each incorporation. Represented home economies are 63, host countries are 85. Data also include firm-level, country-level, and dyadic covariates.

The dataset reports the "ultimate parent" of each foreign subsidiary. It excludes financial investments and small firms[14]. These selections ensure the sample represents a population composed

[13]Firm-level data are provided by Bureau van Dijk (BvD), a Moody's company that obtains information from compulsory reports that public authorities mandate. Both listed and non-listed firms must disclose information. BvD retrieves and cross-checks it from various country-specific sources.

[14]The "ultimate parent" is defined as the firm owning more than 25% in stakes of the foreign subsidiary. Financial companies, insurance firms, hedge funds, and investment banks are excluded. Small firms have less than one million

of large MNCs and investments represent long-term foreign productive enterprises, rather than speculative ventures[15]. Orbis data have about a two-year lag between the disclosure of firms' information and its reporting in the data. They are also not suited to derive firm-level time-series (Kalemli-Ozcan et al., 2015a). Both issues are avoided here employing a cross-section of observations reporting *any* investment of these companies between 2006 and 2011. Figure B.1 in Appendix describes industry classification of these companies.

This dataset is an optimal source to test my expectation at the investor-level for two reasons. First, it reports multilevel information on the home and host country of each company. This allows me to study my conditional argument where the effect of a home country policy is mediated by host country characteristics and the outcome occurs at a firm-level. Second, it comprises only large companies involved in long-term foreign investments for productive purposes. Smaller companies or enterprises with short-term investment goals would likely not face the same type of decision problem my theory advances. They should be excluded from the study.

Skeptical readers might be concerned that, between 2006 and 2011, the OECD Convention had no dent against foreign bribery, because many signatories ramped up enforcement of their laws in later years. However, my argument focuses on the way adoption of anti-bribery laws affects investment conditions, regardless of their level of enforcement. Moreover, even though OECD Convention ratifiers laxly enforced their policies in the early 2000s, US authorities exploited ratification of the Convention to vigorously apply the FCPA against non-US companies headquartered in signatories (Brewster, 2017; Leibold, 2014), as described by Figure 1.1 and anecdotal evidence[16]. Finally, lax enforcement should draw towards the null equally the *deterrence* effect (because companies would not be discouraged from bribing) and the *empowerment* effect (because companies would not have a credible legal leverage to refuse bribe requests). If enforcement strengthened the two mechanisms, any significant finding would likely be larger in magnitude in periods with stronger enforcement. In

euros in operating revenues a year, total assets less than two million euros, and less than 15 employees.

[15]The conventional threshold distinguishing FDI from portfolio investment is 10% in fact. A threshold of 25% is imposed here in order to detect the *ultimate* owner of a firm.

[16]See the harsh terms of the 2008 plead between the DOJ and the German-based company Siemens AG: `https://www.justice.gov/archive/opa/pr/2008/December/08-crm-1105.html` (last accessed on August 24th, 2022).

appendix, I address these concerns empirically too by extending data from Beazer and Blake (2018) to consider investments made by these very firms until 2018. Results hold[17].

I follow Beazer and Blake (2018) and code a binary outcome variable, called *Subsidiary*, representing whether a firm f from country i has incorporated a subsidiary in country j between 2006 and 2011. The binary dependent variable has a dyadic form. For each parent company f from country i it is assigned a 1 if the firm is reported to have set up a subsidiary in the host country j in the time period of interest. It is assigned a 0, instead, if no subsidiary was established in the (potential) host country j[18]. Potential host countries are all economies where a subsidiary has been established by at least one firm in the dataset. This is supposed to represent all attractive host countries. In this multilevel dataset, therefore, each observation represents a parent firm f from home country i evaluating whether to establish a subsidiary in a potential host country j.

My main explanatory variable is the binary indicator *OECD Ratifier*. It represents whether the home country i of a parent firm f has ratified the OECD Anti-Bribery Convention by 2005[19]. As for all my covariates I consider 2005, for it is the year before the start of my cross-section. Table B.1 in appendix reports all 35 home countries that were signatories of the OECD Convention by 2005 and that are represented in the sample, alongside the 28 home countries that were not signatories by 2005 and that are represented in the sample.

Next, I need a measure of the moderator: level of corruption in host country j. Measuring corruption is notoriously difficult. The most common indexes are survey-based and include the World Bank Control of Corruption Estimate (CCE) or Transparency International's Corruption Perception Index. These indicators are typically built surveying the general population about perceptions or first-hand experiences of corruption. They are criticized for being weak indicators of the real level of corruption in a country (Olken, 2009). Social desirability biases answers about

[17] See Table B.5.

[18] I depart from Beazer and Blake (2018) and impose the condition $i \neq j$, which I deem appropriate in the case of foreign investment. Results do not change significantly when relaxing this condition.

[19] I consider only countries for which the Convention had entered into force by 2005, to make sure that anti-bribery legislations under the OECD Convention were in place at the time my cross-section starts. Information on ratification status was retrieved from the OECD website: `http://www.oecd.org/daf/anti-bribery/WGBRatificationStatus.pdf` (last accessed on July the 28th, 2021).

first-hand experiences (Treisman, 2007). Annual survey-based measures, moreover, are subject to confirmation bias if respondents' answers are informed by previous releases. Finally, these indexes often implicitly adopt a definition of corruption not aligned with respondents' or researchers' (Heywood, 1997). Similar issues are a notorious source of inconsistency in empirical studies on corruption (Gueorguiev and Malesky, 2012).

Alternative solutions leverage observable information. Measures like the number of bribery cases, however, are no reliable indicators of corruption since effective crime takes place out of sight. Moreover, they might reflect the enforcement of the Convention by home countries, rather than levels of corruption of the host. The Public Administration Corruption Index (PACI), from Escresa and Picci (2017), offers a valid alternative. The index relies on observable cases of cross-border corruption, but leverages variation in their geographic distribution to derive a measure of relative corruption among host countries. It is based on the following intuition. Suppose we observed that a large share of bribery cases exported from home country x involve host country y, but country y makes up for a relatively modest share of x's economic outflows. This would be evidence that y is relatively more corrupt than other partners of x, because it tends to attract relatively more bribes. The PACI generalizes this intuition, measuring corruption of each host country as the deviation between the *observed* geographic distribution of bribes paid in it and the distribution that could be *expected* if all countries were equally corrupted.

I adopt this measure of corruption and discuss its assumptions in Appendix. In a robustness test, I substitute it with traditional perception-based measures and verify that my results hold[20]. Escresa and Picci (2017) compute a PACI measure employing information between 1997 and 2012. For each host country j in my dataset, I re-compute the index using only information relative to bribes paid between 1997 and 2005 included, since my cross-section starts in 2006. To do so, I draw on the database provided by the authors about observed cases of cross-border bribery. I follow the authors' suggestion and take the natural logarithm of the PACI measure +1 to reduce the skewness of its distribution, and exclude countries for which information is not sufficient to compute a reliable

[20]See Table B.4 in Appendix.

index. The resulting measure *Host PACI* is my main indicator of corruption of the host economy. It ranges from a minimum of 0 (corresponding to non-corrupt economies) to a maximum of 8.90.

I follow Beazer and Blake (2018) and explain my binary outcome variable in a multilevel logit model[21]. This is a forced choice to specify the cross-level interaction in equation 1.1 (Bell and Jones, 2015). This model choice is also suited to the dataset structure, where a firm investing abroad is cross-nested in a directed dyad, and in its home and host countries. Multilevel unobserved heterogeneity in this complex nesting can easily confound the explanation of the outcome variable, therefore it must be properly modelled. For instance, research found that companies gain from investing in the same destinations as their co-nationals (Johns and Wellhausen, 2016). To account for similar clustering, all specifications include random intercepts at the dyad, home, and host countries-level. A further specification adds industry-level intercepts to account for sector-specific heterogeneity. Since no clear hierarchy can be discerned in the data structure, I employ a cross-classified random effect model. A multilevel regression also correctly models the thousands of repeated observations generated by the dyadic structure of the dataset. If their correlation were not properly accounted for, this large number of repeated observations would artificially reduce standard errors and produce unreliable tests of hypotheses.

Finally, I include a series of covariates to control for potential confounders and increase precision of my estimates. I consider the 2005 value for all, so that each covariate represents values of potential confounders before the beginning of my cross-sectional dataset. First, I control for economic and institutional features of the host country: its (logged) Gross Domestic Product (GDP), per capita GDP, total trade, and net FDI inflows (both as percentages of GDP). I also include its Political Constraint (POLCON) III index, a binary indicator for democracy from Cheibub et al. (2010), and a measure for judicial independence from Linzer and Staton (2015). Next, I control for home country features that could affect the likelihood it adopted anti-bribery policies: wealth (measured as logged GDP and GDP growth rate) and level of judicial independence. Then, I control for country-dyadic covariates: a measure of the distance in kilometres between capitals of the home and host country

[21] I maximize the log-likelihood function of this model with a Gauss-Hermite Quadrature method.

and binary indicators measuring whether a bilateral investment treaty (BIT) was signed by the dyad, whether the two countries have a past colonial relationship, and whether they have a common first or official language. Finally, I control for firm-level features: the number of host countries each firm operates in, its age, and its total assets (all logged). Summary statistics are reported in Appendix[22].

Results

Table 1.1 presents my results relative to the variables of interest only[23]. All models condition the effect of *OECD Ratifier* on the first and second-degree terms of the host country's corruption measure *Host PACI*. In order to prevent suppression effects (Lenz and Sahn, 2021), Table 1.1 first includes only the variables of interest (1). Then, it adds controls at the level of host and home countries (2). Then it adds firm-level covariates (3), then dyadic controls (4). Finally, it adds industry-level intercepts (5).

Results are consistent with expectations. The coefficient associated with the interaction between *OECD Ratifier* and the squared *Host PACI* is negative in size and estimated with precision. It is distinguishable from zero at the 0.05 conventional level of significance in all specifications but model 3. Here the estimation is less precise, and the coefficient falls short of the conventional threshold (p-value: 0.06). Estimates of the coefficient of the interaction with the linear *Host PACI* term are also positive and statistically significant at the 0.05 conventional level, but for Model 3 (p-value: 0.07).

The coefficient associated with the un-interacted *OECD Ratifier*, instead, is never distinguishable from zero. This means that, when the host country has the lowest possible corruption level (*Host PACI* = 0), it is not possible to discern an effect of anti-bribery standards on investment decisions. This is consistent with my expectation that anti-bribery policies should not enter firms' decision-making process when investing in non-corrupt economies.

Interpretation of results is particularly complex in multiplicative models (Brambor et al., 2006).

[22]See Table B.2. In the estimation procedure I recenter the distribution of all covariates around their means to help convergence. Descriptive statistics are reported before recentering distributions of these variables.

[23]Full disclosure of all estimates is provided in Table B.3.

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Ratifier × Host PACI^2	−0.033**	−0.038**	−0.023^{+}	−0.031*	−0.034*
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
OECD Ratifier × Host PACI	0.197*	0.225*	0.163^{+}	0.206*	0.220*
	(0.090)	(0.092)	(0.090)	(0.096)	(0.096)
OECD Ratifier	−0.016	−0.034	−0.213	−0.267	−0.282
	(0.165)	(0.192)	(0.246)	(0.205)	(0.205)
Host PACI^2	−0.041	0.013	0.003	0.011	0.013
	(0.033)	(0.029)	(0.026)	(0.027)	(0.028)
Host PACI	−0.097	−0.007	0.023	−0.008	−0.036
	(0.286)	(0.242)	(0.221)	(0.230)	(0.231)
Random intercepts	Yes	Yes	Yes	Yes	Yes
Industry intercepts					Yes
Country-level controls		Yes	Yes	Yes	Yes
Dyad-level controls			Yes	Yes	Yes
Firm-level controls				Yes	Yes
N. of host countries	85	84	84	84	84
N. of home countries	62	61	61	58	57
Observations	320,913	315,657	315,657	289,732	285,295
Log Likelihood	−31,266.030	−31,117.490	−30,957.630	−25,107.560	−24,775.210
Akaike Inf. Crit.	62,550.060	62,272.990	61,961.250	50,267.110	49,604.410
Note:					+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 1.1: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models

I compute the marginal effect of anti-bribery policies at given levels of corruption to evaluate if the argument represented by Figure 1.2 is supported. I compute the change in predicted probability when *OECD Ratifier* varies from 0 to 1 holding everything else at its mean. I compute this effect for given levels of *Host PACI*. This is equivalent to measuring the change in the predicted probability of an investment when comparing an average regulated company to an average unregulated company, conditional on observables, at given values of *Host PACI*. I compute 95% confidence intervals of this estimated difference simulating 1000 draws from its sampling distribution (King et al., 2000).

Figure 1.3 shows the results obtained when considering the estimates of model 1 in Table 1.1. It

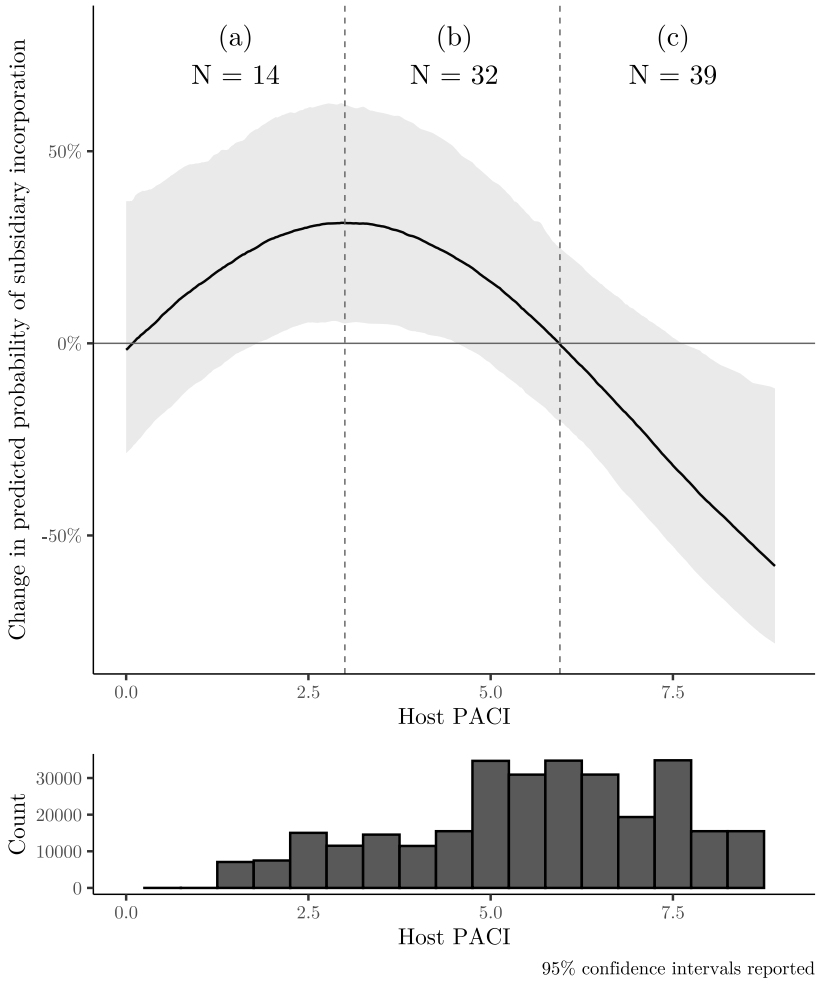


Figure 1.3: The non-linear effect of *OECD Ratifier* on *Subsidiary*, conditional on *Host PACI*

also reports data support for the mediator variable, to ensure results do not depend on extrapolation or interpolation (Hainmueller et al., 2019). Results obtained using the estimates of the other models are consistent with these ones, although confidence intervals become larger, especially for very non-corrupt host economies where data support is limited. When *OECD Ratifier* changes from 0 to 1, the predicted probability that a firm will incorporate a subsidiary changes conditionally on the level of corruption of the host economy, in a non-monotonic way.

The effect can be roughly divided in panels (a), (b), and (c). Figure 1.3 reports the number of host countries included in each panel, under the corresponding label. In panel (a) the change in predicted probability is close to zero for least corrupt host economies (*e.g.*: Canada, Denmark, Sweden). Then, it increases as the host country becomes more corrupt, indicating that firms from countries with anti-bribery policies have a higher probability of investing here. At its maximum, firms from ratifiers have a 31% higher probability of investing than their competitors. Host countries corresponding to the maximum of the *empowerment* effect are Singapore and Taiwan. In panel (b), as the host country becomes more corrupt, the effect of regulation remains positive but declines in size. This indicates that OECD anti-bribery policies still benefit regulated firms investing in economies like Brazil, China, Indonesia, Italy, Mexico, and the United Arab Emirates, but to a lesser extent. For extreme levels of corruption, as in panel (c), firms from ratifier countries are worse off. They have a lower probability of investing here than their unregulated counterparts, a quantity that reaches a lowest point of -58% for host countries at the right-end of the corruption scale like Egypt, India, Kazakhstan, Nigeria, or Russia.

I propose extensive tests to show robustness of these results in Appendix. I first show that an interaction of *OECD Ratifier* with a first-degree polynomial of *Host PACI* produces insignificant estimates. This provides confidence that the effect of the Convention on investment is not linear in corruption. I then substitute the PACI with traditional, perception-based indexes of corruption and show that results hold. I also use the original PACI measure in Escresa and Picci (2017) to enlarge the set of host countries included in the analysis. Then, I exclude outlier countries or firms from signatories that ratified the Convention in the 2006-2011 cross-section. Finally, I replicate results

for an extended dataset considering additional investments made by these firms until 2018. Results hold to all such tests. In Appendix, I also offer a placebo test that confirms my expectations. I show that the effect of the OECD Convention is detected only for firms active in industries where bribes are typically paid. I find no effect in sectors where bribery is not customary.

1.4.2 Country-dyadic data

The previous section provides micro-level evidence that investment behaviors of firms who are subject to OECD anti-bribery policies depend non-linearly on the level of corruption of the host country. Yet, the analysis suffers from two issues. First, it focuses on cross-sectional information between 2006 and 2011. It cannot study change in investment behavior after ratification of the Convention. Second, selection under OECD policies is not random. Firms under OECD policies have very specific characteristics that distinguish them from those who are not subject to such policies. If random effects and controls did not account for such differences, the conditional independence assumption would be violated and the previous analysis would wrongly attribute to anti-bribery policies the effect of these idiosincrasies. For instance, economies adopting the OECD Convention generally belong to the Global North and corrupt host economies tend to be concentrated in the Global South. Do results in Table 1.1 represent the deterrent effect of institutions in these host countries for companies headquartered in the Global North (*e.g.* Beazer and Blake, 2018), rather than a genuine effect of the Convention in corrupt host economies?

Time-varying data would provide a solution to both problems. They would permit to study changes in investment behavior after the adoption of the Convention. They would also allow to hold constant characteristics that are time-invariant, at least in short time-windows, like institutional features. This would improve internal validity of estimates. Unfortunately, Orbis data are not well suited to construct time-series (Kalemli-Ozcan et al., 2015a). I therefore proceed differently. I leverage country-level dyadic FDI data from the United Nations Conference on Trade and Development (UNCTAD). My hypothesis is firm-level and predicts probability of an investment rather than its size. Aggregate FDI flows can obscure individual firms' investment decisions (Kerner, 2014).

Yet, I contend a dyadic analysis represents the best feasible solution to tackle the two problems highlighted above. My theory proposes an effect of a home-country policy is conditional on host-country characteristics. Country-level data on investment flows in directed dyads should therefore be able to capture this effect.

I retrieve UNCTAD dyadic country-level data on foreign investment, country-, and dyad-level covariates from Beazer and Blake (2018). My dependent variable is the logarithm of dyad-level FDI flows. Information ranges from 1994 to 2006, included. It thus covers the period preceding and following the ratification of the OECD Convention. It also spans until the very beginning of my firm-level cross-section, thus offering a snapshot of how investment conditions changed before its onset. Represented home economies are 101 and host countries are 108. The number of directed dyads included is 3591. I report descriptive statistics in Table C.1.

I test my conditional argument by adopting a binning approach, which allows to study non-linear conditional effects. I divide dyads in five subsamples depending on the level of corruption of the host country in the dyad. I measure corruption using the same 2005 *Host PACI* index computed for the firm-level analysis[24]. The five subsamples are defined based on quintiles of the *Host PACI* distribution. The choice to divide the distribution in five parts is purely empirical, as it guarantees enough observations in each bin. Alternative feasible choices (using tertiles and quartiles) provide consistent evidence of a non-linear effect of the Convention on investment, conditional on the corruption level of host economies[25]. A total of 1679 directed dyads report information for the dependent variable. I estimate the effect of the OECD Convention within each of the five bins to study the impact of anti-bribery policies conditionally on the level of corruption of the host economy.

[24]I choose the 2005 value for consistency with the firm-level analysis. The choice appears appropriate given that corruption is a particularly sticky institutional characteristic with little time variation (Treisman, 2007). The relevant variation in levels of corruption most likely takes place between countries rather than for individual countries over time, especially in a short time window as the one of my analysis.

[25]Figures C.1 and C.2.

Two approaches could be followed for estimating the effect of the OECD Convention. The first would be a difference-in-differences approach. This model divides units into a treatment group, represented by dyads whose home country ratified the OECD Convention, and a control group, represented by those whose home country is not a ratifier. It then estimates the average treatment effect of the treated (ATT) dyads as the difference between changes in FDI flows for the treatment group after treatment and changes in FDI flows for the control group. The (untestable) identifying assumption is that trends in FDI flows of the treatment and the control group would have proceeded parallel, after the treatment, had the treatment not taken place.

Regardless of the plausibility of the identifying assumption, however, a reason of concern with this design has to do with the timing of ratification. Countries ratified the OECD Convention at staggered times, thus a two-way fixed-effect (2FE) estimation strategy would have to be adopted. However, a recent literature points out the estimator can produce wrong comparisons between groups at different times of their treatment in this kind of staggered-treatment situation, if heterogeneity in effect is in place (Goodman-Bacon, 2018). Estimators that address this issue and do not assume homogeneous effects usually require balanced panel datasets, whereas mine is unbalanced.

Given that treatment timing is staggered, and my dataset is unbalanced, I follow a different approach. I draw on information from the control group to synthesize counterfactuals for dyads in the treatment group. A synthetic counterfactual is a weighted average of a single treated unit imputed using available information from untreated units in the sample (Abadie et al., 2015). The algorithms building such synthetic unit aim at maximizing similarity in pre-treatment trends between the treated unit and the counterfactual. This allows to make more credible inferences on estimated ATTs. Xu (2017) proposed a generalization of the approach that extends to a panel with several treated units and a sufficient number of untreated ones. The methodology takes into account potential heterogeneity in treatment effects and treatment timing. It also works with unbalanced panel data. It is thus an appealing choice for my case study. The method imputes one synthetic counterfactual for each treated unit, and derives an average effect from this data. It also computes measures of uncertainty around the estimated ATTs. I apply this methodology to impute an untreated

counterfactual for each treated dyad in each of the five bins[26].

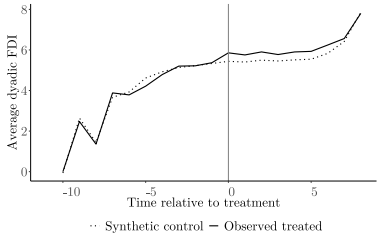
This methodology results in a much smaller sample, because only dyads with enough pre-treatment observations are kept, to ensure estimates do not depend on extrapolations. Excluding some dyads might introduce a bias if missingness correlates with outcome and treatment grouping. In a robustness test reported in Appendix, I replicate the analysis with traditional 2FE models that employ all available dyads, and show my results do not hinge on my synthetic estimation strategy and its data selection.

Results

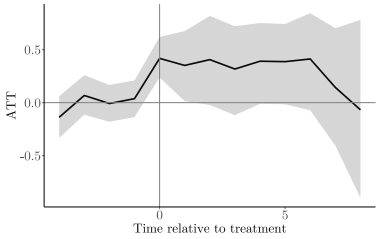
Figure 1.4 reports results obtained in each corruption bin. Average pre-treatment trends of the synthetic counterfactuals closely approximate observed average trends of treated dyads in all bins. This lends confidence that synthetic control units were properly imputed. Post-treatment differences in average flows between observed and synthetic controls confirm expectations from the theory. On average, dyads with extremely clean host economies (first bin) saw a small or insignificant increase in their FDI flows in the post-treatment period. A positive effect, instead, is detected for dyads with moderately corrupt host economies (second and third bins). Post-treatment differences between observed and synthetic FDI dyadic flows are not significant for units in the fourth bin. Finally, FDI flows from ratifiers to the Convention were negatively affected for dyads with extremely corrupt host countries (fifth bin).

Figure 1.5 reports the average effect over the entire time period for each of the five bins. It also reports the distribution of the *Host PACI* variable and the number of dyads in each bin. Estimates across the fives bins reproduce the inverted-U pattern seen in the firm-level analysis. For dyads in the first bin (those with host countries like Australia, Canada, Denmark, and Sweden) entry into force of the Convention seems to have had a small effect on investment. As the host economy in a dyad gets

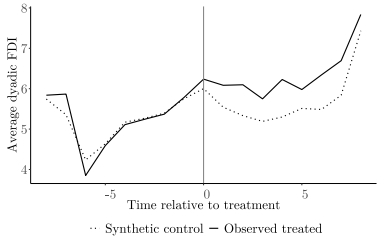
[26]In the estimation procedure I impose a 2FE model specification. I employ time-varying covariates at the level of the host country, home country, and dyad that are also adopted in the firm-level analysis. This is done to improve the synthetic counterfactual imputation. I drop all treated dyads without at least five pre-treatment observations. This is a recommended practice to obtain reliable synthetic control units (Xu, 2017). An Expectation Maximization algorithm has been applied to obtain more precise synthetic counterfactuals. A cross-validation procedure has also been applied to estimate the best number of factor loadings. Standard errors are estimated with 1000 bootstrap iterations.



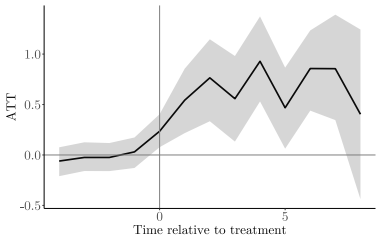
(a) Bin 1: Average dyadic flows



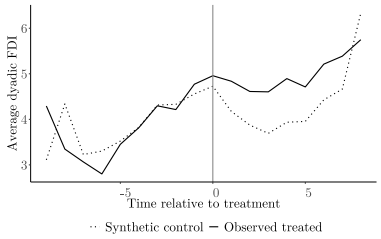
(b) Bin 1: Estimated ATT



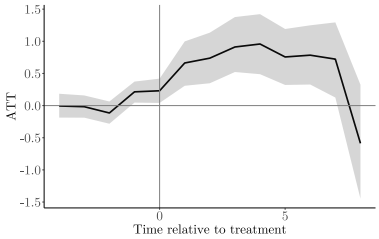
(c) Bin 2: Average dyadic flows



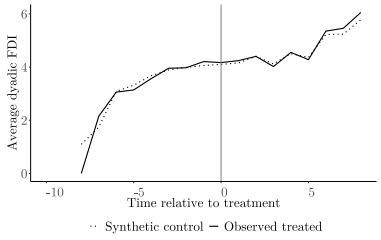
(d) Bin 2: Estimated ATT



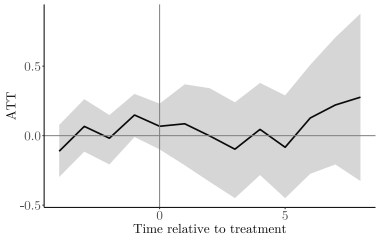
(e) Bin 3: Average dyadic flows



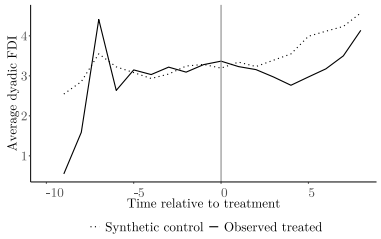
(f) Bin 3: Estimated ATT



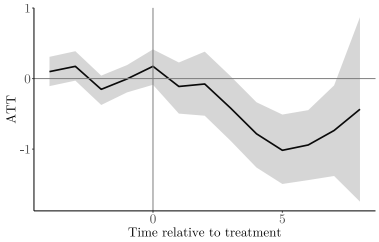
(g) Bin 4: Average dyadic flows



(h) Bin 4: Estimated ATT



(i) Bin 5: Average dyadic flows



(j) Bin 5: Estimated ATT

Figure 1.4: Country-level data. Generalized synthetic control method. Average trends and estimated ATT in the five bins

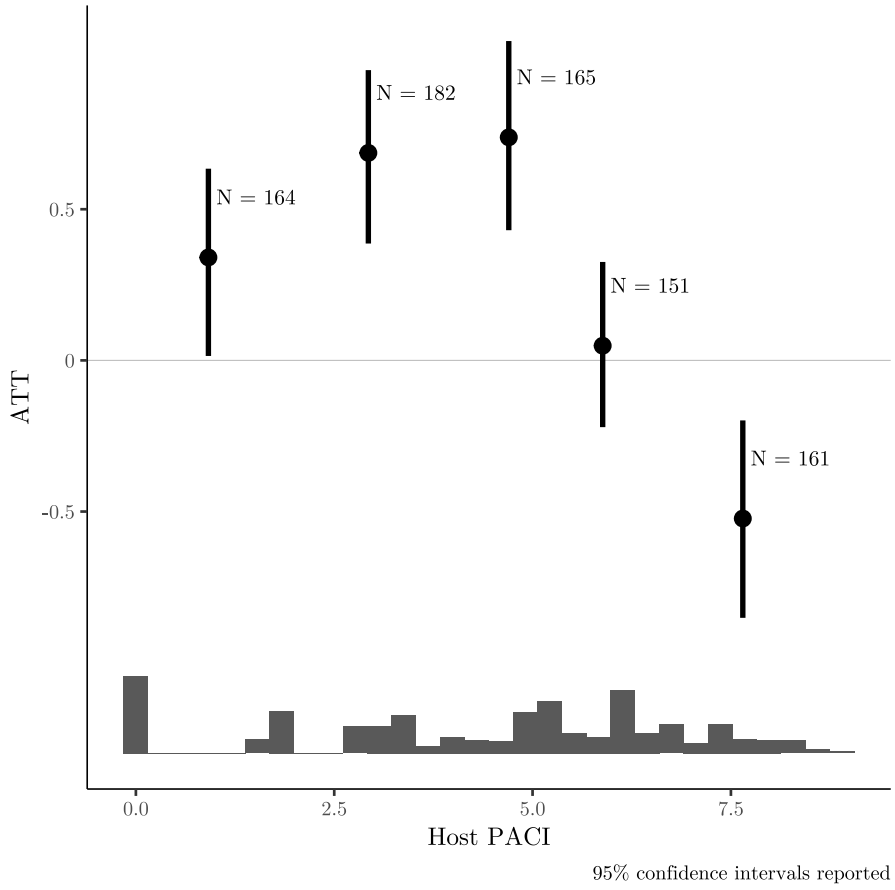


Figure 1.5: Country-level data. Effect estimates from synthetic counterfactual designs

moderately corrupt, the estimated effect is positive and statistically significant. This is true for dyads in the second and third bins, whose host economies include Brazil, Italy, Mexico, Singapore, and Taiwan. When converted from the logarithmic scale, estimates inform us that ratification increased FDI flows in these dyads by about 1.99 million constant US dollars in FDI flows (second bin), and 2.09 million (third bin) on average. Then, the effect declines in the fourth bin, where it is not statistically significant. It becomes negative and significant for dyads with extremely corrupt host economies including Kazakhstan, Nigeria, and Uzbekistan. In this bin the estimated ATT was a reduction of about 1.69 million dollars in FDI flows. Put together, these results provide more evidence in support of the argument proposed.

In Appendix, I show results are similar when binning *Host PACI* using tertiles or quartiles (Figures C.1 and C.2). I also test robustness of these estimates by adopting a 2FE design (Figure

C.3). The design uses all dyads, therefore ensuring results from Figure 1.5 do not depend on dropping dyads without enough pre-treatment observations. In Appendix, I also tackle the rollout of treatment timing issue by introducing a "buffer" for the interval of years 1999–2001, when countries were ratifying the OECD Convention at staggered times. I exclude observations in these years to force the analysis back to a traditional two groups and two time-periods difference-in-differences. Results are consistent with those presented here.

1.5 Concluding remarks

States often use international organizations to negotiate corporate regulations that keep transnational economic activity of their companies above board. What is the effect of these policies on legitimate economic activity, like foreign investment? It is often argued that imposing standards of foreign economic behavior disadvantages companies in countries where economic crime is common, *vis-à-vis* unregulated competitors. IO-negotiated regulatory efforts would thus jeopardize investments. Yet, regulated business models should also be more efficient and favor companies in countries where informal costs are expected in the conduct of business.

This article studies the effect of multilateral corporate regulations on companies' foreign investments, in the case of anti-bribery policies. I first detail two competing arguments about the effect of anti-bribery policies on FDI. The first one, *deterrence*, argues that firms under anti-bribery policies operate at a disadvantage in corrupt economies due to expected legal costs at home. It claims they will invest less in these economies, as a result. The second one, *empowerment*, expects the opposite, arguing that firms under anti-bribery policies can leverage these legal standards to refuse bribe requests and cut expenses. I contribute to the debate by attempting to rejoin the two claims. I argue that both mechanisms are simultaneously at play. Their net effect depends on the level of corruption of the host economy, because the bargaining power of public officials increases in it, and so does their operating space for demanding bribes. *Deterrence* and *empowerment* are therefore special cases of a single, general logic. Whereas *empowerment* holds in moderately corrupt countries,

deterrence prevails in extremely corrupt ones.

The main contribution offered is a test of my argument in two empirical exercises that focus on policies adopted under the 1997 OECD Anti-Bribery Convention. First, I employ firm-level data on investment decisions by 3781 firms between 2006 and 2011. Multilevel logit models show that firms from ratifiers of the OECD Convention are up to 31% more likely to invest in moderately corrupt economies, but 58% less likely to invest in extremely corrupt countries. Second, I corroborate these findings using country-dyadic data on investment flows in a generalized synthetic control design. The value of these two exercises is in their conjunction. Internal validity of firm-level estimates rests on a demanding selection on observables design. However, findings appear stronger thanks to evidence from the country-level analysis, that proposes a more credible identification strategy. On the other hand, country-level estimates can be criticized of an ecological fallacy for using aggregated data to test a firm-level theory. Their estimates, yet, appear more credible in light of the micro-level evidence from the first empirical exercise.

Limitations of the study open up to various future lines of inquiry. I argue that home countries' adoption of anti-bribery laws makes firms better or worse-off in international business depending on the leverage available to refuse bribe requests. This mechanism is not tested by the present analysis, although previous studies lend plausibility to it (Ades and Di Tella, 1999; Perlman and Sykes, 2017; Svensson, 2003). It might be possible that anti-bribery policies affect the behavior of public officials too. The overall observed effect on investment might thus be the compounded result of these different mechanisms, which the study cannot disentangle[27]. Future qualitative work could further investigate this mechanism against possible alternatives. Finally, the study explicitly does not consider strategies to invest in a foreign market other than establishing ownership. Licensing and joint ventures, yet, are potential ways for firms to invest in a foreign economy. They can expose firms from ratifiers of the Convention to a lower risk of interaction with corrupt public officials, and might therefore be a preferred strategy (see Chapman et al., 2020; Zhu and Shi, 2019). A

[27]Sector-specific evidence presented in Appendix (Table B.6) suggests that the effect in place involves only industries where bribes are a custom, and not the rest. This is consistent with the mechanism advanced, which should not hold in industries where bribes are no usual custom, but does not allow a final word on the matter.

future study could therefore investigate the effect of the Convention on these alternative investment strategies.

Net of its limitations, the study offers contributions that generalize beyond anti-bribery policies. It provides insights into the effects of efforts to regulate the global political economy. It thus speaks to a literature on international regimes that assessed the effect of international regulatory institutions on firms (Findley et al., 2015; Jensen and Malesky, 2018; Morse, 2019). I study the side effects of IOs regulating nefarious flows and keeping business above board. I show that a multilateral approach to the diffusion of sustainable business models does not penalize firms' legitimate activity in all countries with lax regulatory standards. Rather, IO-mandated corporate models can facilitate companies in a range of countries where financial crime would otherwise be common.

This is good news for the possibility to conjugate corporate regulatory efforts with economic activity. Recent regulatory initiatives, like the OECD/G20 BEPS Inclusive Framework, could learn an encouraging lesson from the study. Implications also travel to regulatory areas that potentially include human and labor rights violations, money laundering, and environmental crime.

A caveat concerns host countries with *extremely* weak regulatory standards. Here the strategy backfires. Regulated firms are on average more likely to abandon these economies, which are exposed to unregulated companies. To the extent that such unregulated firms can violate these standards, this could lead to a further decline of business standards in these economies. This pessimistic conclusion aligns with existing studies on the perverse regulatory effects of corporate policies induced by different standards among competitors (Brazys and Kotsadam, 2020; Chapman et al., 2020).

Appendix 1.A The *Host PACI* measure

In this section I present the Public Administration Corruption Index (PACI), proposed by Escresa and Picci (2017) and adopted in this study. The PACI relies on the following intuition: suppose all countries were equally corrupt. Then the number of observed cases of cross-border bribery occurring in a country should be proportional to its economic inflows: bribery would simply be more likely to occur where more funds were inflowing. Imagine in fact we observed that a large share of bribes paid by firms from country x abroad are paid in country y, but country y is not a major commercial partner of x. This is evidence that public officials in country y are more corrupt than those in the other partners of x, because they attract more bribes than what could be expected by simply looking at economic flows. The PACI generalizes and formalizes this intuition. For each country y, it is computed as the ratio between the number of observed cross-border bribes paid by firms from the set of all countries X ($X \not\ni y$) to y's public officials, and the number of cases that could be expected based on trade flows between all xy pairs. It thus measures by how much *observed* cases of cross-border corruption involving public officials of a country depart from cases that could be *expected* assuming all countries were equally corrupt and corruption of y were only proportional to trade inflows.

What matters for the PACI to be valid is thus the spatial distribution of cases of cross-border corruption. The index relies on the assumption that the probability of observing a corrupt transaction involving firms from country x and public officials in country y does not depend on the identity of country y (Escresa and Picci, 2017). One could reasonably expect very corrupt countries to be less likely to enforce cases of corruption. This would violate the assumption and threaten the validity of the PACI. For this reason the index does not consider cases of corruption that were enforced only in country y, and includes exclusively cases that were prosecuted by at least one foreign country[28]. A second important assumption that needs to hold is that the number of cross-border transactions is proportional to bilateral trade flows (as opposed to other economic flows like FDI). Escresa and Picci

[28]Evidence for most cases of cross-border bribery, anyway, does not originate in the country where the bribe is paid but in that where the firm is headquartered (Escresa and Picci, 2017).

(2017) argue that many transactions are not reflected in FDI flows or stocks, and that investments eventually enable trade flows between countries. Thus, they argue, trade flows are a good proxy of economic flows between pairs of countries.

Appendix 1.B Firm-level analysis

1.B.1 Descriptive statistics

Figure B.1 breaks down companies in the dataset according to their North American Industry Classification Standard (NAICS) 2-digit code.

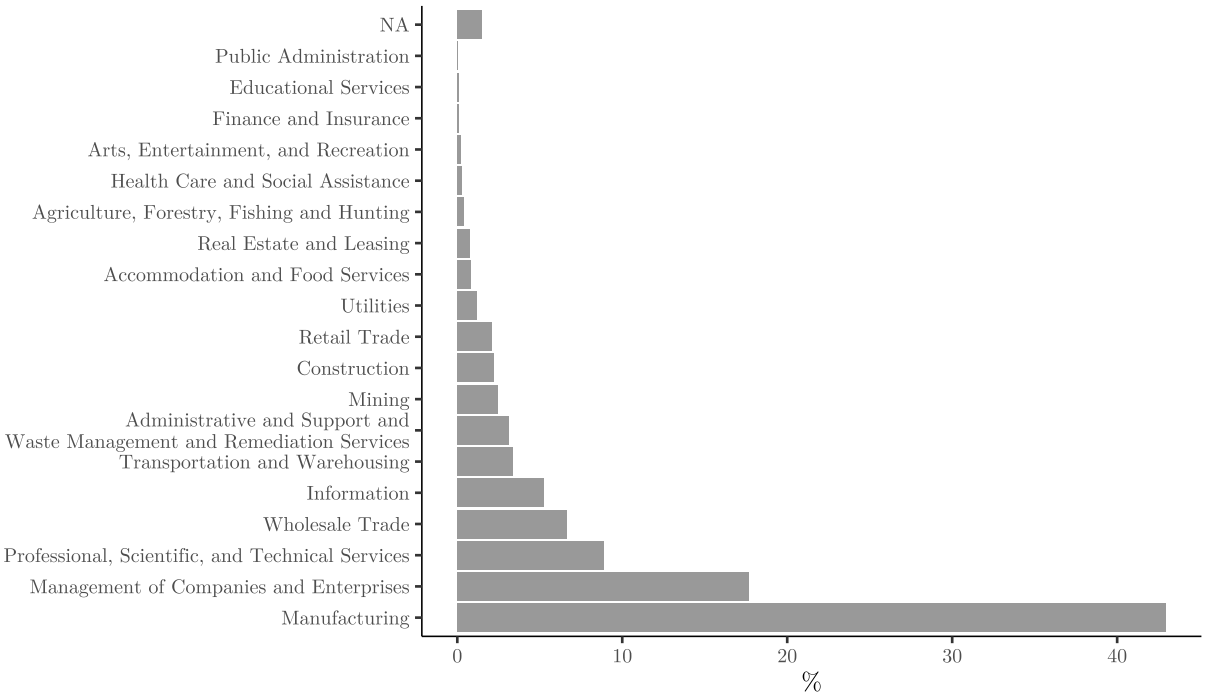


Figure B.1: Firm-level database description: Percentage of firms in the database by NAICS-2 code

Table B.2 presents descriptive statistics for all variables included in the firm-level models. I retrieve from Beazer and Blake (2018) data for the variables Subsidiary, Home GDP (log), Home GDP Growth (%), Home Judiciary Indep., Host GDP (log), Host GDP per Capita, Host FDI (GDP %), Host Trade (GDP %), Host Judiciary Indep., Host Democracy, Host POLCON III, Dyad

	Signatories	Non-ratifiers
1	Austria	United Arab Emirates
2	Australia	Bosnia and Herzegovina
3	Belgium	China, P.R.: Mainland
4	Bulgaria	Colombia
5	Brazil	Costa Rica
6	Canada	Curacao
7	Switzerland	Egypt
8	Chile	Guinea-Bissau
9	Czech Republic	Hong Kong
10	Germany	Croatia
11	Denmark	Israel
12	Estonia	India
13	Spain	Kuwait
14	Finland	Kazakhstan
15	France	Lithuania
16	United Kingdom	Malaysia
17	Greece	Peru
18	Hungary	Philippines
19	Ireland	Qatar
20	Iceland	Romania
21	Italy	Russian Federation
22	Japan	Saudi Arabia
23	Korea, Republic of	Singapore
24	Luxembourg	Thailand
25	Mexico	Taiwan Province of China
26	Netherlands	Uruguay
27	Norway	Venezuela, Republica Bolivariana de
28	New Zealand	South Africa
29	Poland	
30	Portugal	
31	Sweden	
32	Slovenia	
33	Slovak Republic	
34	Turkey	
35	United States	

Table B.1: Firm-level data. Home countries

Distance, Dyad Common Language, Dyad Colonial Relation, Dyad BIT, Firm Age (log), Firm Assets (log), Firm Host Countries (log). Data on anti-bribery actions necessary to build the Host PACI variable are retrieved from the dataset of Escresa and Picci (2017)[29]. Data on Host CCE and Host V-Dem Bribery have been retrieved respectively from the Quality of Governance dataset (Teorell et al., 2020) and from the Varieties of Democracy (V-Dem) core database, version 10 (Coppedge et al., 2020).

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Subsidiary	406,454	0.026	0.158	0	0	0	1
OECD Ratifier	406,454	0.944	0.231	0	1	1	1
Host PACI	329,397	5.171	2.317	0.000	4.032	6.821	8.901
Host PACI (2012)	332,972	5.030	2.261	0.000	3.872	6.548	8.755
Host CCE	402,585	2.677	1.076	1.082	1.833	3.570	4.825
Host V-Dem	402,585	0.203	1.550	−2.838	−0.952	1.614	3.363
Home GDP (log)	403,731	25.594	1.540	18.750	24.109	26.271	27.859
Home GDP Growth (%)	403,731	1.987	1.408	−6.272	1.193	2.163	10.647
Home Judiciary Indep.	406,244	0.895	0.133	0.167	0.886	0.965	0.988
Host GDP (log)	383,261	23.196	1.717	19.414	21.822	24.229	27.859
Host GDP per capita	383,261	1.430	1.445	0.028	0.328	2.334	6.829
Host FDI (GDP %)	383,261	6.533	17.617	−4.258	1.752	5.698	172.716
Host Trade (GDP %)	383,261	0.876	0.533	0.265	0.567	1.038	4.299
Host Judiciary Indep.	398,714	0.558	0.281	0.018	0.331	0.842	0.988
Host Democracy	390,986	0.703	0.457	0	0	1	1
Host POLCON III	383,244	0.311	0.198	0.000	0.127	0.468	0.692
Dyad Distance (km)	386,206	0.656	0.422	0.006	0.261	0.948	1.995
Dyad Common Language	386,206	0.113	0.316	0	0	0	1
Dyad Colonial Relation	386,206	0.051	0.219	0	0	0	1
Dyad BIT	406,454	0.376	0.484	0	0	1	1
Firm Age (log)	400,154	3.312	0.948	0.000	2.639	4.060	5.897
Firm Assets (log)	379,363	13.875	2.115	4.025	12.380	15.328	20.181
Firm Host Countries (log)	406,454	0.678	0.721	0.000	0.000	1.099	3.714

Table B.2: Firm-level data. Summary statistics

[29]I have manually extended this data source following the same procedure adopted by the authors. With my extension the database consists of 1640 cases of anti-bribery prosecution involving 636 different parent firms from 59 nationalities active in 147 countries. Total time coverage goes from 1977 to 2018.

1.B.2 Full disclosure of results

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Ratifier ×	−0.033**	−0.038**	−0.023+	−0.031*	−0.034*
Host PACI^2	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
OECD Ratifier ×	0.197*	0.225*	0.163+	0.206*	0.220*
Host PACI	(0.090)	(0.092)	(0.090)	(0.096)	(0.096)
OECD Ratifier	−0.016	−0.034	−0.213	−0.267	−0.282
	(0.165)	(0.192)	(0.246)	(0.205)	(0.205)
Host PACI^2	−0.041	0.013	0.003	0.011	0.013
	(0.033)	(0.029)	(0.026)	(0.027)	(0.028)
Host PACI	−0.097	−0.007	0.023	−0.008	−0.036
	(0.286)	(0.242)	(0.221)	(0.230)	(0.231)
Host GDP (log)		0.592***	0.652***	0.674***	0.680***
		(0.128)	(0.115)	(0.120)	(0.120)
Host GDP		0.002	−0.042	−0.023	−0.056
per capita		(0.180)	(0.162)	(0.169)	(0.172)
Host FDI		0.010	0.010	0.009	0.010
(GDP %)		(0.009)	(0.008)	(0.009)	(0.009)
Host Trade		−0.225	−0.186	−0.172	−0.155
(GDP %)		(0.335)	(0.303)	(0.315)	(0.316)
Host Judiciary		3.699**	3.537***	3.653***	3.695***
Indep.		(1.150)	(1.035)	(1.079)	(1.085)
Host POLCON III		0.530	0.099	0.156	0.200
		(0.962)	(0.865)	(0.902)	(0.905)
Host Democracy		−0.129	−0.001	−0.016	−0.022
		(0.461)	(0.416)	(0.434)	(0.435)
Home GDP (log)		0.063*	0.138**	0.055+	0.057+
		(0.027)	(0.045)	(0.030)	(0.030)
Home GDP		−0.013	−0.028	−0.005	−0.006
Growth (%)		(0.019)	(0.026)	(0.021)	(0.021)
Home Judiciary		−0.182	−0.256	−0.393	−0.391
Indep.		(0.241)	(0.380)	(0.261)	(0.260)
Dyad BIT			0.087	0.079	0.082
			(0.068)	(0.073)	(0.073)
Dyad Common			0.693***	0.751***	0.742***
Language			(0.092)	(0.100)	(0.101)
Dyad Colonial			0.725***	0.737***	0.732***
Relation			(0.116)	(0.126)	(0.127)
Dyad Distance			−1.229***	−1.102***	−1.105***
			(0.094)	(0.095)	(0.095)
Firm Assets (log)				0.005	0.005
				(0.008)	(0.008)
Firm Age (log)				0.017	0.013
				(0.014)	(0.015)
Firm Host				1.286***	1.287***
Countries (log)				(0.020)	(0.020)
Constant	−3.364***	−5.602***	−5.513***	−6.079***	−6.026***
	(0.605)	(0.642)	(0.606)	(0.610)	(0.612)
Random intercepts	Yes	Yes	Yes	Yes	Yes
Industry intercepts					Yes
N. of host countries	85	84	84	84	84
N. of home countries	62	61	61	58	57
Observations	320,913	315,657	315,657	289,732	285,295
Log Likelihood	−31,266.030	−31,117.490	−30,957.630	−25,107.560	−24,775.210
Akaike Inf. Crit.	62,550.060	62,272.990	61,961.250	50,267.110	49,604.410
Note:					+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table B.3: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models (full disclosure)

1.B.3 Robustness tests

Results for the first tests are reported in Table B.4. In model 1 I replicate the full specification of model 5 in Table 1.1 excluding the squared measure of *Host PACI* and its interaction with *OECD Ratifier* to show that the effect of the OECD Convention on *Subsidiary* is not conditional on a linear measure of corruption. No term involved in the interaction is found to be statistically significant. I then replicate model 5 of Table 1.1 using more traditional, perception-based indexes of corruption. First, I use the "Executive bribery and corrupt exchanges" measure from V-Dem (Coppedge et al., 2020). The measure is a Bayesian-based index that relies on both objective and survey information, and is generally considered an improvement of traditional perception-based indexes. Next, I employ the World Bank's CCE, rescaled so as to range from 0 to 5. In both cases, lower values indicate higher levels of corruption. Results obtained remain substantively the same.

Next, I consider the possibility that the main measure of corruption I adopt restricts the sample excessively and introduces a source of selection. Computing the 2005 version of *Host PACI* reduces the number of host countries in the analysis because it relies on fewer observations of the dataset from Escresa and Picci (2017). To test whether results hold with an extended sample of host countries, I replicate model 5 of Table B.3 using the version of the index computed and published by Escresa and Picci (2017), which employs information until 2012 and includes more host countries[30]. Results obtained when using this version of the index are substantively the same as the ones discussed before.

As a further test I consider the hypothesis that results might be driven by some outlier countries. China figures as a very likely candidate: the country has not ratified the Convention and it is generally considered a rather corrupt bureaucracy. Yet, it is involved in the world economy as both a major importer and exporter of investments. I therefore replicate the analysis excluding observations relative to firms from this country or investing in it. Results do not change significantly with this exclusion. Next, in two countries the Convention has entered into force within the time window of

[30]I deem the choice appropriate, since corruption is a very sticky phenomenon with little time variation. Correlation between the two versions of the index indeed equals 0.98.

the cross-section (2006-2011): Israel and South Africa. Thus, their firms might have been subject to anti-bribery policies even though *OECD Ratifier* assigns them a value of 0. I therefore replicate the analysis excluding them. Results, again, do not change significantly.

Finally, I replicate results from Table 1.1 extending the dataset to include all investment made by these same companies until 2018. I follow the same procedure in Beazer and Blake (2018) to extend investment data from Orbis. Results are reported in Table B.5. Point estimates of variables of interest are substantively unchanged and statistically significant.

	Dependent variable:					
	Subsidiary					
	First degree	V-Dem	CCE	PACI (2012)	No China	No Israel No South Africa
	(1)	(2)	(3)	(4)	(5)	(6)
OECD Ratifier × Host PACI^2					−0.029*	−0.024$^+$
					(0.012)	(0.014)
OECD Ratifier × Host PACI	−0.007				0.168$^+$	0.168$^+$
	(0.034)				(0.089)	(0.096)
OECD Ratifier × Host V-Dem Bribery^2		−0.075*				
		(0.032)				
OECD Ratifier × Host V-Dem Bribery		0.133$^+$				
		(0.071)				
OECD Ratifier × Host CCE^2			−0.213**			
			(0.082)			
OECD Ratifier × Host CCE			1.360*			
			(0.543)			
OECD Ratifier × Host PACI^2 (2012)				−0.048***		
				(0.014)		
OECD Ratifier × Host PACI (2012)				0.277**		
				(0.097)		
OECD Ratifier	−0.067	0.021	−2.023*	−0.260	−0.188	−0.197
	(0.191)	(0.181)	(0.847)	(0.193)	(0.196)	(0.225)
Host PACI^2					0.016	0.012
					(0.026)	(0.027)
Host PACI	0.039				−0.069	−0.101
	(0.106)				(0.221)	(0.230)
Host V-Dem Bribery^2		0.133*				
		(0.062)				
Host V-Dem Bribery		−0.089				
		(0.159)				
Host CCE^2			0.154			
			(0.148)			
Host CCE			−0.660			
			(1.012)			
Host PACI^2 (2012)				0.020		
				(0.028)		
Host PACI (2012)				−0.057		
				(0.234)		
Host GDP (log)	0.723***	0.751***	0.737***	0.627***	0.678***	0.687***
	(0.107)	(0.087)	(0.088)	(0.115)	(0.121)	(0.113)
Host GDP per capita	−0.065	−0.173	−0.185	0.218	−0.044	−0.145
	(0.173)	(0.165)	(0.187)	(0.225)	(0.167)	(0.169)
Host FDI (GDP %)	0.010	0.013$^+$	0.014$^+$	0.006	0.011	0.012
	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Host Trade (GDP %)	−0.105	−0.148	−0.168	−0.366	−0.210	−0.217
	(0.311)	(0.281)	(0.292)	(0.315)	(0.304)	(0.298)
Host Judiciary Indep.	3.640***	3.205**	2.453$^+$	2.930**	3.685***	4.367***
	(1.084)	(1.066)	(1.374)	(1.084)	(1.036)	(1.115)
Host POLCON III	0.248	0.455	0.422	0.028	0.201	0.366
	(0.904)	(0.820)	(0.829)	(0.892)	(0.865)	(0.850)
Host Democracy	0.015	0.005	0.068	0.038	−0.050	−0.382
	(0.431)	(0.397)	(0.413)	(0.412)	(0.418)	(0.460)
Home GDP (log)	0.058$^+$	0.048	0.048	0.052$^+$	0.080*	0.074*
	(0.031)	(0.035)	(0.035)	(0.030)	(0.032)	(0.029)
Home GDP Growth (%)	−0.005	−0.002	−0.002	−0.002	−0.001	−0.003
	(0.021)	(0.022)	(0.022)	(0.020)	(0.023)	(0.021)
Home Judiciary Indep.	−0.398	−0.372	−0.382	−0.328	−0.253	−0.333
	(0.263)	(0.293)	(0.292)	(0.257)	(0.271)	(0.289)
Dyad BIT	0.065	0.078	0.064	0.092	0.189***	0.166***
	(0.073)	(0.070)	(0.070)	(0.072)	(0.049)	(0.049)
Dyad Common Language	0.748***	0.791***	0.787***	0.707***	0.651***	0.657***
	(0.101)	(0.098)	(0.098)	(0.100)	(0.045)	(0.045)
Dyad Colonial Relation	0.734***	0.759***	0.759***	0.761***	0.304***	0.292***
	(0.127)	(0.120)	(0.120)	(0.123)	(0.052)	(0.052)
Dyad Distance	−1.126***	−1.241***	−1.237***	−1.061***	−1.129***	−1.069***
	(0.096)	(0.090)	(0.090)	(0.093)	(0.059)	(0.058)
Firm Assets (log)	0.005	0.008	0.008	0.006	0.005	0.005
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Firm Age (log)	0.013	0.010	0.010	0.009	0.012	0.011
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Firm Host Countries (log)	1.287***	1.274***	1.274***	1.277***	1.271***	1.270***
	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Constant	−6.105***	−6.193***	−5.361**	−5.984***	−5.854***	−5.655***
	(0.589)	(0.258)	(1.642)	(0.628)	(0.585)	(0.619)
Random intercepts	Yes	Yes	Yes	Yes	Yes	Yes
N. of host countries	83	99	99	85	82	81
N. of home countries	56	56	56	56	55	54
Observations	285,295	340,554	340,554	291,945	280,767	275,705
Akaike Inf. Crit.	49,607.020	55,424.820	55,423.580	53,329.030	49,350.190	49,272.190
Note:						+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table B.4: Firm-level data. Robustness tests of multilevel logit models

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Ratifier × Host PACI^2	−0.037***	−0.045***	−0.024*	−0.030**	−0.033**
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
OECD Ratifier × Host PACI	0.261***	0.307***	0.193*	0.236**	0.247**
	(0.076)	(0.078)	(0.077)	(0.083)	(0.083)
OECD Ratifier	0.126	−0.094	−0.172	−0.250	−0.265
	(0.190)	(0.243)	(0.254)	(0.223)	(0.227)
Host PACI^2	−0.038	0.029	0.011	0.018	0.021
	(0.034)	(0.029)	(0.027)	(0.028)	(0.028)
Host PACI	−0.166	−0.087	0.007	−0.024	−0.046
	(0.302)	(0.248)	(0.232)	(0.241)	(0.242)
Host GDP (log)		0.719***	0.784***	0.829***	0.837***
		(0.132)	(0.123)	(0.128)	(0.128)
Host GDP per capita		−0.038	−0.088	−0.084	−0.103
		(0.185)	(0.171)	(0.178)	(0.179)
Host FDI (GDP %)		0.005	0.004	0.004	0.004
		(0.010)	(0.009)	(0.009)	(0.009)
Host Trade (GDP %)		−0.0003	0.069	0.081	0.090
		(0.344)	(0.320)	(0.332)	(0.333)
Host Judiciary Indep.		4.697***	4.596***	4.777***	4.795***
		(1.197)	(1.112)	(1.155)	(1.158)
Host POLCON III		0.295	−0.130	−0.123	−0.101
		(1.007)	(0.935)	(0.972)	(0.973)
Host Democracy		−0.307	−0.174	−0.197	−0.200
		(0.478)	(0.444)	(0.461)	(0.462)
Home GDP (log)		0.099*	0.144**	−0.010	−0.031
		(0.048)	(0.051)	(0.041)	(0.042)
Home GDP Growth (%)		0.011	0.003	0.041+	0.029
		(0.028)	(0.029)	(0.025)	(0.025)
Home Judiciary Indep.		0.534	0.394	0.738*	0.668+
		(0.405)	(0.428)	(0.348)	(0.354)
Dyad BIT			−0.001	−0.018	−0.009
			(0.062)	(0.067)	(0.068)
Dyad Common Language			0.738***	0.794***	0.786***
			(0.092)	(0.101)	(0.102)
Dyad Colonial Relation			0.744***	0.803***	0.817***
			(0.112)	(0.123)	(0.124)
Dyad Distance			−1.189***	−1.304***	−1.322***
			(0.088)	(0.094)	(0.095)
Firm Assets (log)				0.186***	0.208***
				(0.006)	(0.007)
Firm Age (log)				0.134***	0.071***
				(0.011)	(0.012)
Firm Host Countries (log)				0.908***	0.902***
				(0.015)	(0.015)
Constant	−2.984***	−27.554***	−29.235***	−30.910***	−30.767***
	(0.653)	(3.677)	(3.472)	(3.517)	(3.530)
Random intercepts	Yes	Yes	Yes	Yes	Yes
Industry intercepts					Yes
N. of host countries	85	84	84	84	84
N. of home countries	62	61	61	58	57
Observations	320,913	315,657	315,657	289,732	285,295
Log Likelihood	−46,765.540	−46,500.700	−46,311.960	−37,958.790	−37,224.890
Akaike Inf. Crit.	93,549.090	93,039.410	92,669.920	75,969.590	74,503.790
Note:					+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table B.5: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models. Extended data

1.B.4 Placebo test: Sector-level analysis

I further investigate my argument moving to a sector-specific analysis, which also works as a placebo test. If my argument is correct, the mechanism should be observable only in industries where bribes are typically paid. In sectors where bribery is no typical custom, instead, anti-bribery policies should not enter firms' decision-making. I exploit information in the database from Escresa and Picci (2017) to test this implication. From this data I first obtain a list of industries with at least one reported case of cross-border bribery prosecution before 2005. I argue that these industries represent sectors where bribes are more often paid[31]. I then replicate the analysis proposed in Table 1.1 within two distinct sub-samples of industries: one including those where bribes were paid at least once (which I call "test"), and one including the rest of the sectors in the sample ("placebo").

Table B.6 reports results obtained. For each subsample I replicate the model including no controls (only random effects) and all controls. Estimates of the coefficients associated with the interaction terms are consistent with the ones presented in Table 1.1 for the "test" subsample. Standard errors shrink in this subsample, resulting in estimates that are significant at smaller conventional levels. This indicates that coefficients are estimated with even more precision. They are never distinguishable from zero, instead, in the "placebo" subsample. This provides further confidence on my argument. The conditional effect of anti-bribery policies is observed only in industries where corruption is likely customary.

[31]I consider only cases enforced at least by one other country than the one where bribes were paid, to mitigate concerns about reliability of information.

	Dependent variable:			
	Subsidiary			
	Test	Test	Placebo	Placebo
	(1)	(2)	(3)	(4)
OECD Ratifier × Host PACI^2	−0.040**	−0.043**	−0.006	0.005
	(0.014)	(0.015)	(0.027)	(0.029)
OECD Ratifier × Host PACI	0.248*	0.299**	0.003	−0.123
	(0.099)	(0.106)	(0.192)	(0.206)
OECD Ratifier	−0.023	−0.397^{+}	0.106	0.250
	(0.181)	(0.222)	(0.331)	(0.387)
Host PACI^2	−0.034	0.021	−0.072	−0.014
	(0.032)	(0.027)	(0.046)	(0.041)
Host PACI	−0.134	−0.095	0.089	0.192
	(0.281)	(0.226)	(0.378)	(0.319)
Host GDP (log)		0.667***		0.718***
		(0.115)		(0.152)
Host GDP per capita		−0.049		0.008
		(0.164)		(0.218)
Host FDI (GDP %)		0.009		0.010
		(0.008)		(0.010)
Host Trade (GDP %)		−0.160		−0.126
		(0.303)		(0.430)
Host Judiciary Indep.		3.655***		3.005*
		(1.036)		(1.370)
Host POLCON III		0.147		0.447
		(0.865)		(1.128)
Host Democracy		−0.040		0.527
		(0.416)		(0.559)
Home GDP (log)		0.063*		0.034
		(0.030)		(0.032)
Home GDP Growth (%)		−0.005		0.009
		(0.021)		(0.039)
Home Judiciary Indep.		−0.379		0.023
		(0.271)		(0.325)
Dyad BIT		0.046		0.327*
		(0.077)		(0.133)
Dyad Common Language		0.686***		0.762***
		(0.105)		(0.143)
Dyad Colonial Relation		0.667***		0.700***
		(0.132)		(0.177)
Dyad Distance		−1.138***		−0.697***
		(0.100)		(0.137)
Firm Assets (log)		0.007		0.0005
		(0.009)		(0.021)
Firm Age (log)		0.010		0.040
		(0.016)		(0.037)
Firm Host Countries (log)		1.288***		1.243***
		(0.022)		(0.051)
Constant	−3.339***	−5.885***	−3.355***	−6.463***
	(0.602)	(0.592)	(0.751)	(0.782)
Random intercepts	Yes	Yes	Yes	Yes
N. of host countries	85	84	85	84
N. of home countries	58	53	41	39
Observations	262,075	236,609	54,097	48,686
Log Likelihood	−25,757.560	−20,778.850	−5,159.393	−4,114.255
Akaike Inf. Crit.	51,535.120	41,611.710	10,338.780	8,282.511
Note:		+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001		

Table B.6: Firm-level data. Market-specific effects of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models

Appendix 1.C Dyadic country-level analysis

1.C.1 Descriptive statistics

Table C.1 presents descriptive statistics for all variables included in the dyadic analysis. I retrieve from Beazer and Blake (2018) data for all variables but the OECD Convention (binary) and Host PACI (same indicator as for the firm-level analysis). All covariates are measured as in the firm-level analysis.

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Dyad FDI (log)	8,852	3.154	2.593	0.000	0.619	5.141	11.466
Dyad FDI (binary)	44,125	0.251	0.434	0	0	1	1
OECD Convention	44,125	0.259	0.438	0	0	1	1
Host PACI	35,910	4.381	2.418	0.000	2.740	6.147	8.901
Host FDI (GDP %)	41,812	3.567	8.203	−32.347	0.832	4.057	172.716
Host GDP per capita	42,332	17.717	14.799	0.249	4.497	28.515	74.164
Host Trade (GDP %)	42,659	80.504	52.989	0.309	50.629	95.277	437.387
Host POLCON III	41,840	0.348	0.204	0.000	0.173	0.507	0.720
Host Democracy	43,373	0.714	0.452	0	0	1	1
Host GDP (log)	42,363	25.867	1.909	18.809	24.503	27.189	30.188
Host Judiciary Indep.	44,055	0.632	0.297	0.016	0.382	0.949	0.989
Home GDP per capita	43,813	16.164	12.340	0.399	5.933	26.459	74.164
Home GDP growth (%)	43,745	3.239	4.296	−30.694	1.621	5.030	90.468
Home GDP (log)	43,813	25.980	1.823	20.205	24.704	27.148	30.188
Home Judiciary Indep.	44,125	0.645	0.276	0.074	0.405	0.944	0.989
Dyad BIT	44,125	0.254	0.435	0	0	1	1

Table C.1: Dyadic country-level data. Summary statistics

1.C.2 Robustness tests with binning approach

Alternative binning choices

I first propose alternative ways to bin the moderator *Host PACI*. I bin the variable in tertiles and quartiles of the *Host PACI* distribution. Estimated ATTs in each bins for these cases are reported in Figures C.1 and C.2 respectively. Overall, they provide robust evidence that the Convention had a positive effect on regulated investment into moderately corrupt economies but a negative effect into extremely corrupt countries.

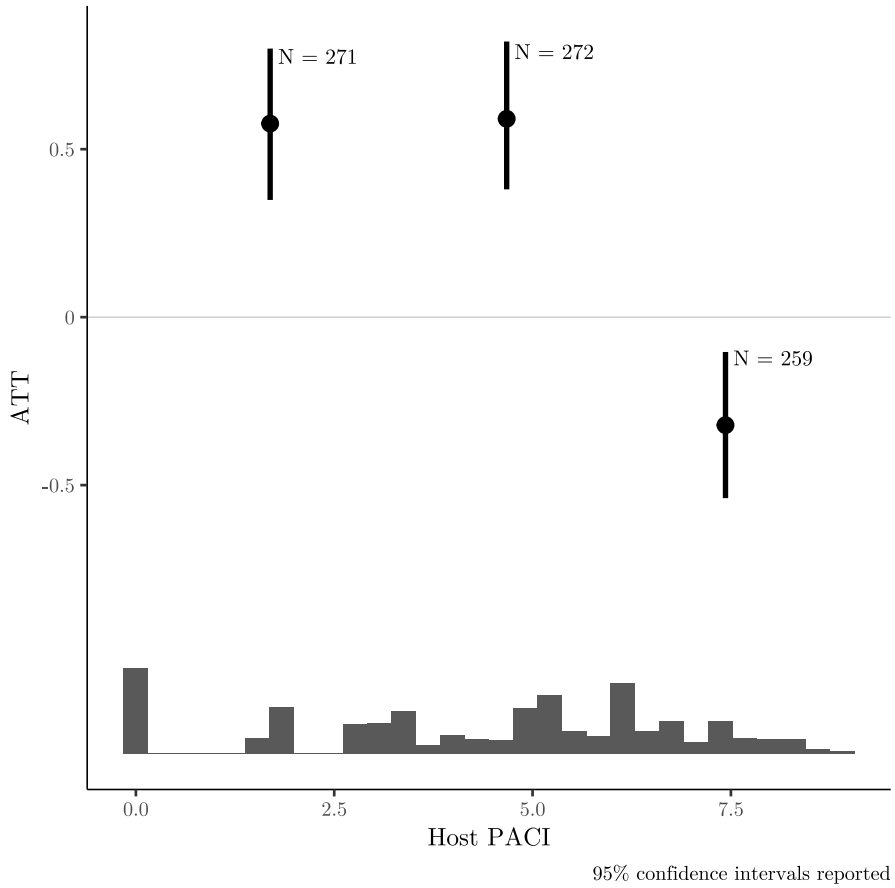


Figure C.1: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on tertiles of *Host PACI*

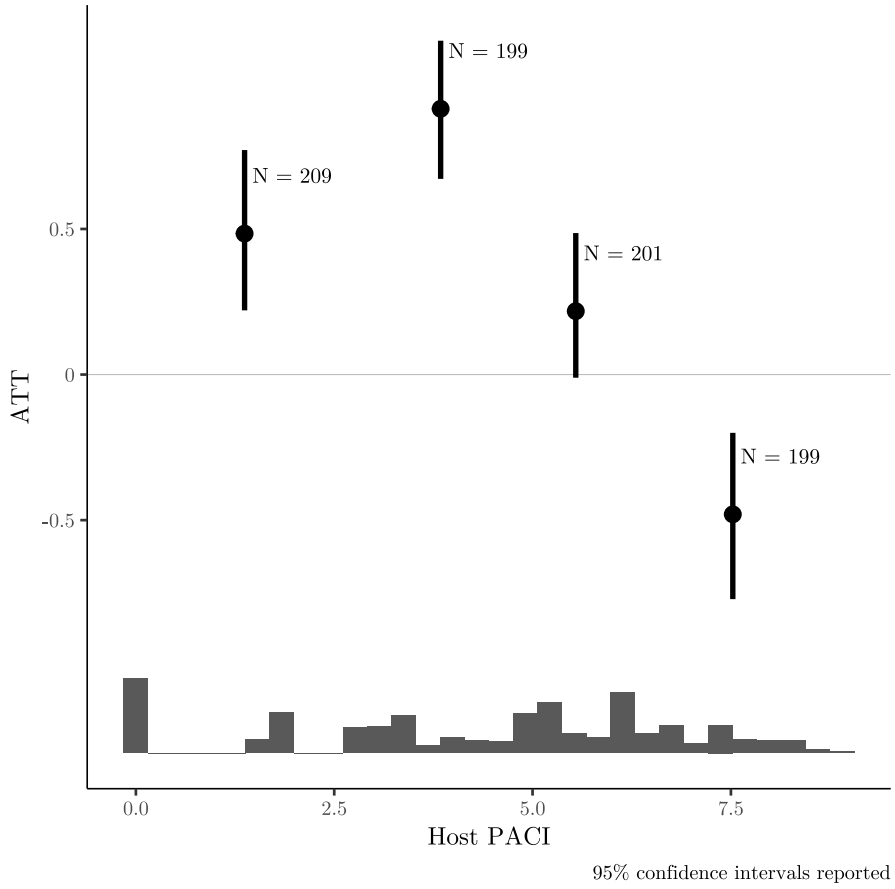


Figure C.2: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on quartiles of *Host PACI*

2FE design

As an alternative to the synthetic counterfactual design, I adopt a two-way fixed-effect (2FE) strategy. I include a binary treatment variable *OECD Convention* that takes value 1 after the Convention entered into force for dyads whose home country is a ratifier. It includes fixed effects at the dyad and at the year-level. The estimate associated with *OECD Convention* can be interpreted as the ATT from a difference-in-differences design under the assumption that trends in investment between dyads with and without a ratifier home country would have been the same in the absence of the Convention ("parallel trends assumption"). Time-varying control variables at the level of the host country, home country, and dyad are the same as the ones adopted in the firm-level analysis.

A well-known problem emerges with 2FE when treatment timing varies between units. In that case the estimator produces wrong comparisons between groups at different times of their treatment (Imai and Kim, 2020). This is unfortunately the case with the OECD Convention. The problem is known to affect 2FE estimates particularly when the proportion of never-treated units is small, because of the weighting scheme implemented by 2FE (Goodman-Bacon, 2018). In my case, about half of the directed dyads were never treated[32] thus the problem appears less concerning. Nevertheless, I tackle the problem as follows. First, I estimate my 2FE models with staggered treatment and justify it based on the large share of never-treated dyads. Second, I address the potential issue of staggered treatment with a simple solution. I exploit the fact that for most economies the Convention entered into force either in 1999 or in 2001. I then exclude observations in the "buffer" years 1999–2001 and compare pre-1999 dyad-level investment flows to post-2001 observations[33]. Assuming the effect of the Convention on investment was not extinguished in the immediate short term, the method allows me to detect differences between the two periods and reduces the problem to a canonical 2-groups and 2-periods setup.

I estimate 2FE models using ordinary least squares (OLS). First, I estimate ATTs considering

[32] 1733 dyads out of 3591 include a home country that did not ratify the Convention.

[33] In this case I also exclude from the analysis all dyads including either Ireland or Estonia as home country since the Convention entered into force there in 2003 and 2005 respectively, that is within the time-frame of my UNCTAD dataset but outside the "buffer" three-years period.

all observations, including those in the "buffer" years 1999–2001 when home countries ratified the Convention at staggered times. Figure C.3 reports estimates obtained in the five subsamples, their confidence intervals, number of observations in each bin, and the distribution of the moderator *Host PACI* variable. I first introduce only the *OECD Convention* variable and fixed effects (panel a). Next, I introduce all controls at the host country-, home country, and dyad-level (panel b). I interact covariates with year fixed effect to control for differential observable trends across dyads[34]. Panels c and d of Figure C.3 reproduce the same specifications of panels a and b, with the exclusion of observations in the "buffer" years 1999–2001. Standard errors are always clustered at the dyad level.

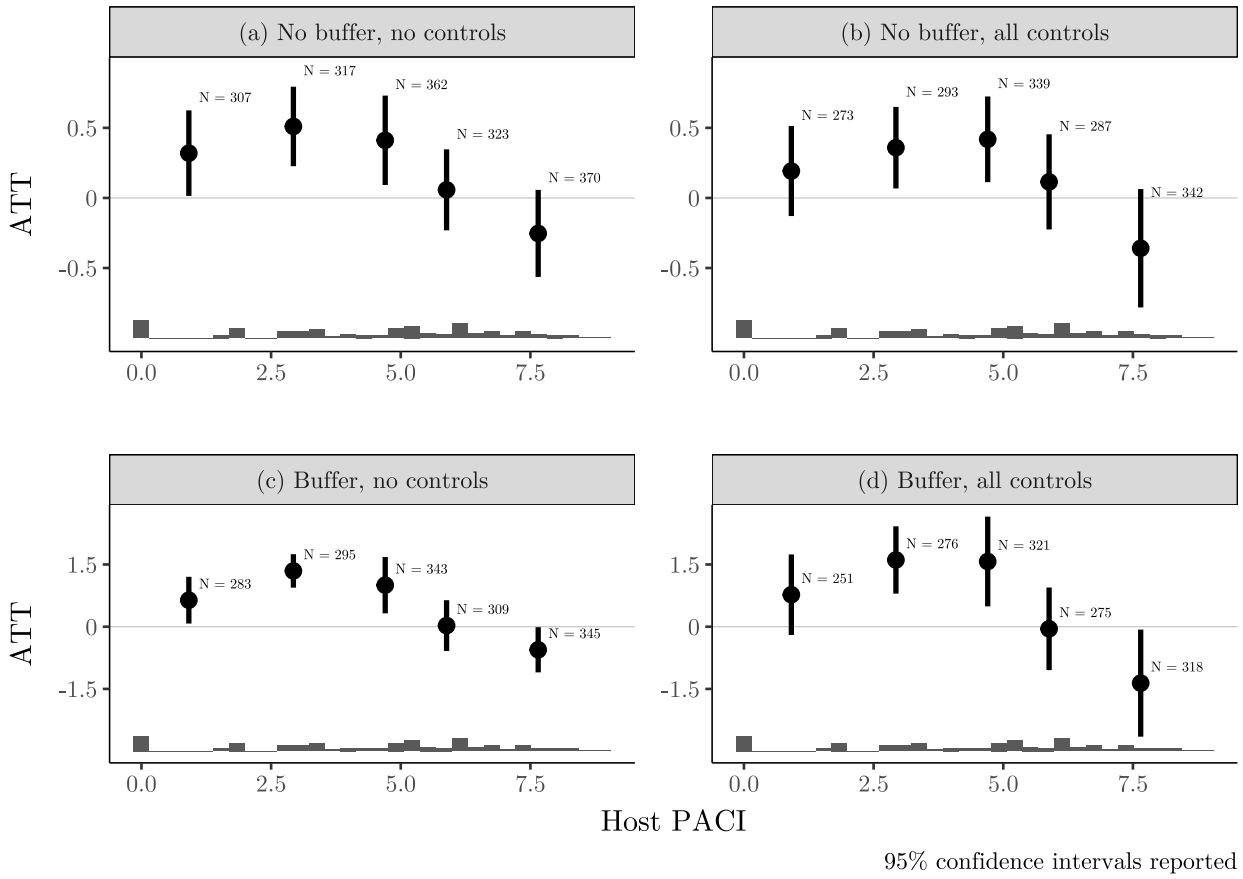


Figure C.3: Country-level data: 2FE binning estimator.

Estimates across the fives bins reproduce the inverted-U pattern seen in the synthetic counter-

[34]Results are essentially unchanged in the restricted model specification where interaction coefficients are imposed to equal 0 and controls are simply added to the model.

factual design in all panels. Entry into force of the Convention seems to have had no effect on investment for dyads in the first bin. The estimated effect is positive and statistically significant in all specifications of the second and third bin, with moderately corrupt host economies. Then, the effect declines and becomes negative for dyads with extremely corrupt host economies.

Paper 2

Global Firms and Global Sheriffs? Why Territory Matters for Extraterritorial Enforcement of International Regimes

Abstract

Under international regimes, states have adopted laws to prohibit multinational companies from committing financial crime. Although global firms can evade regulations creating complex ownership structures, some countries enforce laws *extraterritorially*. They prosecute firms regardless of their nationality, behaving as "global sheriffs" of a regime. However, these countries only prosecute a fraction of the foreign firms under their jurisdiction. This variation remains largely unexplained. I study this phenomenon focusing on US prosecution of foreign companies. I argue that US authorities are more likely to prosecute foreign companies that have US investment. Formally, this is no requirement for the application of American extraterritorial regulations. Yet, US prosecutors exploit reputational risk induced by the investment, which increases exposure to the US public opinion. They use it to obtain cooperation by the firm and retrieve necessary information to build a case. I test my argument building a novel firm-level data on enforcement of policies under the anti-bribery regime. I merge this data with information on non-US companies' investment in the US. I find that the probability that US authorities investigate a suspect foreign company increases from 0.33 to about 0.61 when it has investment in the US. The study shows that even powerful extraterritorial regulators need a territorial leverage to rule on foreign multinationals.

2.1 Introduction

Globalization creates complex governance tasks for sovereign states (Keohane and Nye, 1973). States solve coordination problems in an interdependent world by establishing international regimes and overcome their territorial limits (Keohane, 1984). However, a group of transnational subjects can evade regulations under international regimes: global criminals (Cooley and Sharman, 2017). Criminals can evade regulations under international regimes by fragmenting illicit activities across borders. This "regulatory arbitrage" makes it challenging to prosecute perpetrators of cyber crimes, human rights violations, drug trafficking, and transnational terrorism (Andreas, 2004; Cooley and Sharman, 2017; Shelley, 2014; Williams, 1994). The problem is exacerbated when it comes to regulating crime by multinational companies (MNCs). Firms can *legally* fragment their nationalities and activities across borders in order to evade tax regimes, labor rights standards, or policies against money laundering and corruption (Arel-Bundock, 2017; Chapman et al., 2020; Findley et al., 2015; Mosley, 2017).

Are sovereign states condemned to be helpless in face of increasingly complex global crimes? On the contrary, some countries have adopted policies to prosecute foreign subjects as if they were domestic, acting as "global sheriffs" of international regimes (Andreas and Nadelmann, 2008). This strategy potentially counters regulatory arbitrage, because criminals cannot leverage their nationality to evade state coercion. The current champion of this approach to law enforcement is the US. United States federal authorities prosecute crime perpetrated by foreigners including transnational terrorism, violations of unilateral sanctions, and crime by MNCs (Andreas, 2005; Biglaiser and Lektzian, 2011; Putnam, 2009). In the realm of corporate regulation, American prosecutors levy billions of dollars in fines each year for corporate crimes committed by non-US companies. Such broad *extraterritorial* approach to regulatory enforcement makes the US arguably the most relevant regulator of corporations. Yet, "global sheriffs" do not appear to be enforcing their laws against all cases they could prosecute (Ruggie, 2018).

Against this background, in this paper I set to answer the question: What gives a country the

power to enforce its IO-negotiated regulations against a foreign subject and behave as a "global sheriff"? This question hinges on the nature of power under international regimes. Political science and international relations scholarships have dedicated scant attention to the sources of *de facto* extraterritorial behavior (Baradaran et al., 2012), despite its relevance for contemporary regulatory regimes (Putnam, 2009). The lack of clear data on cases that authorities *could have* prosecuted exacerbates the problem. As a result, the question is left substantively unanswered[1].

I set to fill this gap and study how public authorities select which foreign subjects to investigate under extraterritorial policies. I focus on corporate regulations. I argue that prosecutors gain the *extra*territorial capacity to rule over a foreign firm depending on its territorial connection to their economy. Formally, MNCs need not be physically present in a country to be subject to its extraterritorial provisions. Extraterritorial jurisdiction on a corporate wrongdoing can be usually established on the basis of much weaker connections[2]. Yet, foreign investment creates jobs, business, and activities that expose a company to the local public opinion. In comparison to foreign firms with no investment, one that invests is a well-visible entity. This induces a larger reputational damage for the parent company in the host country, when scandals for corporate crime emerged (Karpoff et al., 2008). Companies attempt to minimize such damage by signalling compliance through cooperation with prosecutors (Garrett, 2014). Cooperation gives prosecutors information and documents to investigate a corporate case. Thus, cases where the implicated foreign company is territorially connected to prosecutors are easier to build.

I find evidence for this argument studying US prosecution of non-US companies for events of

[1]In the realm of corporate regulations, claims are often made that extraterritorial regulators target foreign firms so as to facilitate domestic companies in international competition. A former manager for the French energy company Alstom, for instance, denounced the relentless approach of US judicial authorities against non-US companies in a recent book (Pierucci, 2019). He alleged US regulators prosecute foreign companies so as to advantage US companies. His book has recently become a best-seller in China: `https://www.washingtonpost.com/world/2019/06/07/an-unlikely-winner-china-us-trade-war-french-businessmans-book-about-his-battle-with-doj/`.

[2]Extraterritoriality is usually supported by legal principles such as the "effects doctrine" – stating that misconduct by foreign subjects occurring abroad may be regulated by a state because of its effects on interests within the domain of the state – or the "presence doctrine" – stating that misconduct by foreign subjects occurring abroad may be regulated by a state if the misbehavior is partly in connection to its domain. None of the two requires *territorial* presence, strictly speaking. For instance, the United States exercises its authority on corporate crimes where the implicated company has used internet servers located on US soil, has paid the transaction in US dollars, holds securities in US banks, or has used US means of communication like US mail and transports (Leibold, 2014).

bribery. I show that even such a powerful extraterritorial regulator requires territorial connections to a foreign company in order to stretch the arm of its law abroad. This is a puzzling finding from two points of view. First, from the perspective of the hegemonic country in the international regime that I study. The US lead the construction of the global anti-bribery regime in the 1990s, spending significant efforts to this goal (Abbott and Snidal, 2002). Its establishment formally enabled US authorities to stretch the arm of the US anti-corruption laws abroad (Brewster, 2014; Kaczmarek and Newman, 2011). Yet, my findings indicate that law enforcers still depend to a great extent on territorial connections to *de facto* enforce this regime. This is surprising because US prosecutors enjoy substantial freedom in their choice of cases to investigate (Tomashevskiy, 2021). Territoriality is not a jurisdictional requirement for the application of extraterritorial American law (Leibold, 2014). Cooperation between corporate defendants and authorities is also the standard in the US system[3]. Hence, US regulators would have the prerogatives and power to prosecute foreign companies without necessarily relying on territoriality to investigate a case. Second, results are puzzling from the perspective of non-American firms. By investing in the US, foreign companies rebrand themselves as American, a strategy that is supposed to reduce their "liability of foreignness" (Zaheer, 1995). However, I find that this strategy backfires, exposing companies to the legal reach of local prosecutors.

The choice to study the anti-bribery regime allows to overcome the obvious problem that we can only observe cases that US authorities investigated. We are not able to observe the universe of *enforceable* cases that federal agencies chose not to consider. This prevents to explain selection of cases and forces recent studies on the matter to focus only on explaining size of penalties imposed by US agencies on companies that were prosecuted (Choi and Davis, 2014; Tomashevskiy, 2021).

Two unique features of the US-sponsored international anti-bribery regime offer a way around the problem. First, the regime has diffused US-like anti-bribery policies to member states, that are now mandated to prohibit their MNCs from paying bribes to foreign public officials in international transactions (Bukovansky, 2006). Thus, a larger set of cases is observable involving non-US com-

[3]Companies usually settle charges with authorities avoiding perils of a trial. Legal instruments used include non-prosecution agreements (NPAs) and deferred prosecution agreements (DPAs), see Garrett (2014).

panies in violation of US-like anti-corruption policies around the globe, not necessarily prosecuted by the US. Second, the regime strengthened US capacity to enforce its own extraterritorial policy (Brewster, 2014). Because of that, virtually *all* observable anti-bribery cases under this regime umbrella were in principle prosecutable by US authorities, who need only a marginal connection of the defendant non-US company to their economy to claim jurisdiction (Leibold, 2014).

I leverage these characteristics and construct a novel dataset on observable events where non-US companies violated the anti-bribery regime, by (allegedly) paying bribes to foreign public officials. I explore this dataset and explain why the US investigated about 38% of non-US companies involved in corporate bribery scandals[4]. I test my argument merging this data with firm-level information on foreign investment. A selection on observables design shows that the probability a suspected firm will be investigated by American authorities increases by about 0.28 for foreign companies with investments in the US, when holding constant important confounders. A sensitivity analysis shows the assumption of no omitted variable bias is credible in this context. Finally, I provide additional case-study and large-N evidence on the reputational mechanism supporting my empirical expectations.

The study offers valuable contributions to political science. It addresses a debate on state sovereignty and globalization. I offer a nuanced account between positions declaring the "retreat of states" from markets (Strange, 1996) and those claiming states can vigorously engage with them (Andreas and Nadelmann, 2008; Kaczmarek and Newman, 2011). I provide evidence that state coercive capacity beyond borders is feasible, although determined to a great extent by territory. The finding that public authorities can leverage MNCs' cross-border ownership chains for regulatory purposes is good news for prospects of regulating multinational crime. However, I find that even a powerful regulator like the United States needs a territorial connection in order for extraterritoriality to be viable.

Finally, the study provides novel political-economic insights. In line with the emerging "weaponized interdependence" literature (Farrell and Newman, 2019), I show that states lever-

[4]Original computation based on the dataset used in the article. See Table A.1.

age their position as nodes of transnational economic exchanges to gain a *de facto* prerogative to prosecute foreign companies. I contribute to this literature by offering a theory that does not assume national interest as a determinant of state action. Rather, my explanation moves from the reputational costs that companies face when they are involved in a criminal scandal. Moreover, I argue that the exercise of state coercive authority is induced by different degrees in the "liability of foreignness" (Zaheer, 1995) among companies. This is akin to recent studies investigating differences in government treatment among companies operating in a foreign market (Johns and Wellhausen, 2021; Wellhausen, 2015). I contribute to this recent literature by investigating the behavior of one specific branch of the government: public prosecutors.

2.2 Extraterritorial regulation of multinational companies

Regulating multinational companies is a challenging task for states. To this end, countries coordinate their behavior by making vast use of international regimes (Keohane, 1984). International regimes coordinate state responses to global issues including violations of labor and human rights (Abouharb and Cingranelli, 2006; Jo and Simmons, 2016), corruption (Kaczmarek and Newman, 2011), financial crime (Findley et al., 2015), tax evasion (Crasnic and Hakelberg, 2021), and environmental damage (Kennard, 2020). One of the mechanisms supporting coordinated responses to corporate crime is the threat of judiciary repercussions at home for firms that commit crime abroad, which is the focus of the first paper in this dissertation.

Effectiveness of such regimes ultimately depends on levels of compliance with them (Simmons, 2010). In the context of corporate regulatory regimes, the literature distinguishes between *de jure* compliance – *i.e.*, state enforcement[5] of laws negotiated under regimes' umbrellas – and *de facto* compliance – regulated companies' law abidance (Baradaran et al., 2012; Jensen and Malesky, 2018). The literature posits two problems are in the way of effective enforcement. First, a *regulator's dilemma* impedes *de jure* compliance (Kapstein, 1989). Self-interested countries are subject to a

[5]In the context of this study, I define enforcement purely as the application of laws adopted under a given international regime by member states. This definition is not necessarily coincident with alternative characterizations of enforcement found in the literature on international institutions and compliance.

competitive pressure to under-enforce their laws for crime perpetrated by nationals abroad, if they do not bear its negative externalities[6] (Eilstrup-Sangiovanni and Sharman, 2019). Second, *regulatory arbitrage* undermines *de facto* compliance. Transnational firms can fragment ownership structures and supply chains across borders to evade criminal laws (Arel-Bundock, 2017; Chapman et al., 2020; Fisman et al., 2008).

A vibrant debate has then emerged to study if, net of these two problems, state enforcement of laws under international regimes is effective. Mixed evidence exists on the efficacy of enforcement. Some studies find that it successfully reduces transnational corruption (Jensen and Malesky, 2018), others provide concerning evidence on the lack of effectiveness against financial crimes (Findley et al., 2015). Many conclude formal state-based law cannot hold private actors accountable for misconduct along cross-border supply chains (Ruggie, 2018). If so, they continue, the only feasible tool to hold corporations accountable for transnational misbehavior could be in the hands of a global civil society, which could punish market actors when information on their misconduct arises (Kreitmeir et al., 2020).

By focusing on these two problems, this important literature in political science has overlooked a prerogative of states that potentially overcomes them both: extraterritoriality. States do not only enforce their regulations on *domestic* firms for criminal behavior abroad. In fact, some countries apply their policies on *foreign* subjects as well, effectively behaving as "global sheriffs" of a regime (Slaughter, 2004). The country that currently champions such extraterritorial approach to enforcement is the United States[7] (Putnam, 2009). Effectively, its authorities enforce US corporate policies against firms from all over the world and threaten repercussions *in the US* for violating American criminal policies (Garrett, 2011). US authorities even use this as a tool to sanction foreign countries (Tomashevskiy, 2021).

Extraterritoriality is potentially a powerful tool to regulate global markets. First, extraterritoriality overcomes the regulator's dilemma induced by self-interest. If it is national interest that causes

[6]For instance, investigations on alleged bribery of Saudi Arabian officials by the British corporation BAE Systems were repeatedly halted by UK governments, due to national interest concerns (Gilbert and Sharman, 2016).

[7]Examples of US extraterritorial corporate regulations include (but are not limited to) corrupt exchanges, violations of unilateral trade sanctions, taxation of foreign-owned assets, data usage, intellectual property.

political intrusions and halts regime enforcement, it appears a third-party "global sheriff" has no reasonable incentive to shield *foreign* companies for violating criminal laws abroad[8]. Moreover, to the extent that the legal net cast by the regulator is sufficiently large, it can effectively prevent evasion of transnational supply chains from regulatory regimes, as nationality is no obstacle to extraterritorial enforcement.

Yet, extraterritoriality is far from being the norm in the enforcement of corporate regulatory laws[9]. First, only a few countries have adopted instruments to prosecute foreign companies. Differences from this point of view are generally explained because of legal obstacles or technological limitations (Eilstrup-Sangiovanni and Sharman, 2019). Second, countries enforcing regulations extraterritorially show striking variation in their capacity to do so. The US only prosecutes a handful of foreign companies every year under each of its corporate regulations, a number which is significantly below that of potential cases it has jurisdiction on (Garrett, 2011). Figure 2.1 shows that foreign companies are significantly under-represented in the set of cases prosecuted by the US Department of Justice (DOJ), although fines extracted from these companies tend to be higher than those from domestic firms. When it comes to allegations for certain crimes by MNCs, including human rights violations, these numbers drop significantly (Kreitmeir et al., 2020; Ruggie, 2018).

What gives a country the capacity to behave as a "global sheriff" and enforce a regulatory regime against a certain foreign company? We still know too little to answer this question, despite its relevance for the literature on international regimes. Political science and its sub-disciplines have dedicated surprisingly scant attention to extraterritoriality[10]. International law has studied the issue more extensively. Legal scholars generally conclude that the gap between potential cases and those that are actually enforced is due to the costs and complexity of embarking on cross-

[8]If anything critics of the extraterritorial application of US corporate law contend quite the *opposite*: that American authorities unfairly apply their regulations with disproportionate energy against foreign companies, to alter the playing field of international economic competition and ultimately favor US companies (Leibold, 2014). As an example of such claims, see: "À qui profite la lutte anticorruption? Le piège General Electric", *Le Monde Diplomatique*, September 2019: `https://www.monde-diplomatique.fr/2019/09/A/60335`.

[9]I suspect that the reticence of political science to address the topic can be partially justified precisely because extraterritoriality is rather the exception in corporate governance than the rule (Ruggie, 2018). I believe this is yet one more reason making a study about the sources of extraterritorial capacity a relevant contribution.

[10]This literature has rather focused on its effects, see Efrat and Newman (2016) or Kaczmarek and Newman (2011).

Figure 2.1: Corporate fines levied (a) and number of prosecuted cases (b) from the US DOJ by nationality of indicted firm, 2000 – 2020.

Note: Data retrieved from the Corporate Prosecution Registry (Garrett and Ashley, 2019).

border prosecutions (Brewster, 2014). From a political science perspective this is only a partial explanation, however, as it does not justify why legal resources are devoted to the selection of a *specific* extraterritorial case over others.

Two potential explanations are provided by the political science on the matter. Putnam (2009) argues that the US intervenes extraterritorially when foreign violations undermine the integrity of American domestic norms. Although plausible, this claim is not able to explain what determines selection of a company over another that violated the same US domestic norm. Some corporate practitioners contest these decisions are in fact political and are made to advantage US companies in key industries (Pierucci, 2019).

A second potential explanation comes from proponents of the so-called "weaponized interde-

pendence" argument. This theory claims countries can leverage transnational economic connections to gain advantage over rival nations and coerce them (Farrell and Newman, 2019). Next section presents an argument that largely builds on insights from this theoretical framework. I draw from here the notion that economic interdependence creates the conditions for countries to exercise their coercive (regulatory) prerogatives. Yet, I claim this is not done in pursuit of national interest. In fact, I contend it is purely the result of career and professional incentives to bureaucrats in charge of regime enforcement. My argument focuses on the United States case and considers the two key actors of extraterritorial regime enforcement: global firms and national prosecutors.

2.3 Global firms and global sheriffs

Imagine two very similar non-US companies, Red Apple Cigarettes and Morley Cigarettes, were suspected of violating some US extraterritorial corporate policy in a certain foreign country. Whereas Red Apple has a local US subsidiary, called American Red Apple, Morley is only publicly traded on the New York Stock Exchange. Besides that, it has no significant presence in the United States. In this fictitious case, the US has jurisdiction to rule over both misconducts[11]. Will US authorities be more likely to enforce their policies against either of the two? I argue that territorial presence empowers state authorities to behave extraterritorially, making Red Apple a more likely target of US courts. Although extraterritorial jurisdiction formally does not require the physical presence of a firm in a country, I argue that a country leverages the investment of a foreign firm on its territory to gain the *de facto* capacity to behave as a "global sheriff" and investigate it.

In this section, I illustrate my argument by means of this fictitious running example. However, in the mechanism section (2.7), I describe two real-life cases by drawing on the specific scope of this paper: US extraterritorial investigation for non-US corporate bribery. The two cases of alleged bribery presented there closely resemble the fake examples made up here. They involved two telecommunication companies. The first one was Millicom International Cellular SA, a

[11]Red Apple would be subject to US corporate policies due to its presence in the domain of the state, and Morley due to its requirement to file 20-F forms to the Securities and Exchange Commission.

Luxembourg-incorporated company with investment in the US, alleged of bribery in Guatemala and investigated by US authorities under the extraterritorial terms of the US anti-corruption policy. The second one was MTN Group limited, a South Africa-based company with no physical investment in the US, which was alleged of bribery in Iran but never investigated by US agencies, even though their involvement was often demanded by competitors and could have been justified under the "presence doctrine" (footnote 2).

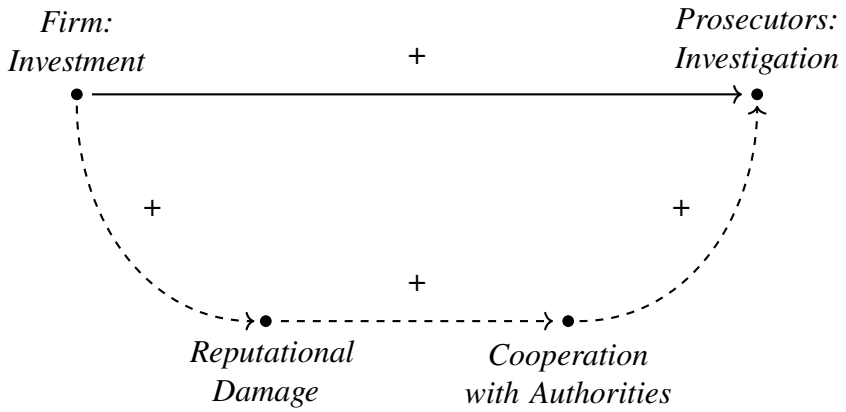


Figure 2.2: Effect of US investment on likelihood of US investigation for suspected companies

Note: The chart reports the expected observable effect as a solid line and the underlying mechanism as a dashed line. Expected effect signs are also reported.

In a nutshell, my argument explains these differences by assuming that judicial authorities have incentives for bringing cases against foreign companies, but they are constrained by limited resources. Therefore, they must focus on cases offering a higher likelihood of success. This, in turn, depends on availability of information. I move from these premises and advance the theory sketched in Figure 2.2, which summarizes my observable expectation (top arrow) and its underlying mechanism (bottom chain). Suspected foreign firms with an investment in the prosecutors' economy represent an easier case to investigate than those with no such physical presence and will therefore be more likely selected by authorities. Their exposure to the prosecutors' economy makes them suffer larger reputational damages for violating local corporate policies than their counterparts without similar investment. In other words, their "liability of foreignness" (Zaheer, 1995) is larger. Increased reputational damage makes cooperation with local authorities a more appealing strategy to these

companies. By cooperating, they signal to the public opinion they are complying with authorities, in the hope to minimize reputational damage. Crucially, by cooperating a company also provides information and evidence to prosecutors, making such cases on average easier to investigate for regulators. When choosing among suspected foreign companies, prosecutors will therefore tend to investigate those territorially bound to their country, expecting that they will be more likely to cooperate. In this section, I first elaborate on the assumptions that motivate prosecutors' behavior in my argument. Next, I expand on my mechanism detailing foreign firms' behavior when scandals emerge.

2.3.1 Global sheriffs: Incentives to prosecute and resource constraints

My argument moves from the assumption that judicial authorities have incentives for enforcing laws against foreign corporate defendants, pertaining to their careers and to their office work[12]. This is a reasonable premise from the point of view of individual-level incentives to pursue these cases. First, when establishing a case it is relatively easier for prosecutors to investigate a company than the individuals within its organization who are responsible for committing the crime[13] (Garrett, 2014). The damage suffered from a corporate scandals is a serious concern for implicated companies, which typically accept guilt and try settling allegations without going to court, so as to minimize the time spent under the spotlight of newspapers and the public opinion. In the US system, out-of-court legal instruments allowing this outcome include deferred prosecution (DPA) or non-prosecution agreements (NPA). These costly solutions expedite the resolution of corporate legal matters. They are aimed at avoiding a "death sentence" for a company (Alexander and Arlen, 2018; Garrett, 2018): they prevent perils of judiciary prosecution for the firm, but involve large costs. This solution is

[12]A full study of the determinants of authorities' incentives to prosecute foreign corporate defendants is outside the scope of this article. Rather, here I present anecdotal evidence and literature from legal studies to show the plausibility of this claim. However, it is possible that other, non-career related, and untested explanations motivate public prosecutors' incentive to enforce criminal laws against foreign corporate defendants. Nonetheless, in my theory, bureaucrats' career incentives only serve to back the assumption that public prosecutors will, on expectation, look for easier cases to enforce and win. The rest of the argument – *i.e.,* the fact that foreign companies with an investment will be more likely to represent "easier cases" for prosecutors – does not necessarily follow from the plausibility of this assumption.

[13]Former FBI director James Comey referred to the DOJ as the "Chickenshit Club" precisely for an alleged tendency to prosecute easier corporate cases rather than more complex individual ones. See: `https://www.ft.com/content/102ffa00-5bf4-11e7-9bc8-8055f264aa8b`

precluded to individuals, who also have incentives to go through lengthy trials and appeals in order to maximize their chances of resulting innocent (Davis, 2019).

Corporate cases therefore provide a good career opportunity to prosecutors, as they are easier to build. Cases against *foreign* firms prove particularly appealing for monetary and prestige reasons. Successful officers often build thriving careers from winning cases against foreign companies[14] (Choi and Pritchard, 2018; DeHaan et al., 2015). The resonance of these cases also benefits prosecutors in prestige, building them a reputation as tough opposers of economic crime. Corporate scandals involving foreign companies regularly make it to the first pages of newspapers and attract the attention of news outlets around the world. They often involve corporations with vast activities across borders, large sums of dirty money mobilized, and vast rents illicitly extracted. On top of this, the blatantly absurd way these resources are sometimes spent makes for easy eye-catching stories[15].

Prosecutors also have office-level incentives to enforce their laws against foreign corporations, pertaining to their budget. Offices in charge of enforcing corporate criminal laws are public bodies. Budget decisions are thus usually political. Fines and monetary settlements levied are partly employed to compensate those who suffered from the economic crime, but a large share typically ends up in the National Treasury, particularly when victims are hard to identify (Turk, 2012). Zeal of prosecutors' offices thus increases revenues for a government and signals those in charge of budget decisions that the office is of strategic importance.

Empirical evidence confirms that cases against foreign companies tend to be much more remunerative. Fines levied by the US DOJ from foreign corporations tend to be significantly higher

[14]To provide a few anecdotes, prior to his 2016 appointment as chief of the DOJ Fraud Section, Daniel Kahn had been the lead prosecutor of cases among the top anti-bribery enforcement actions in the US against foreign entities (See: `https://fcpablog.com/2016/06/02/doj-names-permanent-chief-of-fcpa-unit/`). His subordinates at the Fraud Section share similar successful stories. After a successful experience as a line prosecutor at the DOJ, Albert Stieglitz was seconded overseas, to the UK Serious Fraud Office (See his biography: `https://www.pli.edu/faculty/albert--stieglitz-32298`).

[15]Corrupt corporate money was allegedly used by Malaysian public officials to fund part of the production of Martin Scorsese's movie "The Wolf of Wall Street", with bitter irony – see: `https://www.theguardian.com/business/2019/dec/19/goldman-sachs-close-to-2bn-settlement-over-1mdb-scandal-malaysia` –, and in Equatorial Guinea to buy an iconic crystal-studded glove once owned by Michael Jackson – see : `https://abcnews.go.com/Blotter/doj-seeks-jackson-glove-dictators-son/story?id=14812081`.

than those from domestic corporations[16]. In the case of foreign bribery, with the sole exception of Goldman Sachs, the top 10 monetary corporate settlements for bribery with US agencies have all been paid by non-US entities[17]. This pattern stands out even when controlling for the egregiousness of the bribe paid (Choi and Davis, 2014).

Therefore, in the example presented above, US prosecutors have incentives for making a case against both Red Apple Cigarettes and Morley once they obtain information on their violations of corporate law. Yet, prosecutors must focus only on cases where the likelihood of success is higher, because foreign prosecution is a costly endeavour and resources to initiate cases are scarce (Brewster, 2014). US authorities bring only a handful of cases against foreign companies each year, as panel b in Figure 2.1 shows. When broken down to the various types of corporate offences, this figure is reduced to just a few cases[18].

2.3.2 Global firms: Reputational damage and incentives to cooperate

Which of the two imaginary companies in our running example will more likely be prosecuted by US authorities? Based on my assumptions, the answer will depend on what case will be easier to build. The most valuable resource prosecutors need to build a case is information on the alleged misconduct, and access to financial records or corporate documents. Public authorities from the headquarter of the foreign company usually offer support thanks to multilateral legal assistance networks (Brewster, 2014; Kaczmarek and Newman, 2011). Yet, it is cooperation with indicted companies that proves crucial to build a case (Baer, 2018; Davis, 2019). Cooperation of defendants and information disclosure are usually provided in order to enter an NPA or DPA program. Among the steps of these programs, the firm usually sets up internal investigations on the alleged misconduct. Authorities retrieve information and classified documents from such type of cooperation as a condition to grant non-prosecution (or defer it).

[16]Data from Garrett and Ashley (2019) (plotted in Figure 2.1) reveal that the average fine imposed by the DOJ on a US corporation is $5,192,672 whereas that imposed on a non-US company is $38,368,610. The difference is statistically significant with p-value 7.6×10^{-9}.

[17]See the top 10 monetary settlements from the FCPA blog: `https://fcpablog.com/2020/10/26/wall-street-bank-earns-top-spot-on-fcpa-blog-top-ten-list/`.

[18]In the years when cases of foreign bribery peaked, for instance, the DOJ prosecuted less than 15 yearly cases in all.

Companies cooperate with US authorities in order to reduce the reputational damage that a corporate scandal generates. Evidence shows that firms are imposed harsh costs on financial markets when their corporate social responsibility records are undermined (Capelle-Blancard and Petit, 2019; Krüger, 2015). This is particularly true for criminal prosecution. Around 80% of each dollar lost in the share value of a company, following judiciary prosecution, comes from market-imposed penalties (Sampath et al., 2018). This leaves only 20% of losses to fines and disbursements imposed by authorities[19]. Markets impose penalties because unveiled criminal behaviors provide information that a company operates inefficiently, or that it will likely incur in legal costs in the future. Both issues concern investors, who negatively update their expectations on the profitability of implicated companies and restructure their portfolios accordingly. By offering cooperation, companies can minimize these reputational penalties. They can reduce the time they spend under the spotlight of the public opinion, avoiding the long process of in-court trials. Moreover, they signal compliance with corporate laws. They frame the misconduct as the behavior of a "bad apple" inside the company, rather than a structural issue (Garrett, 2014).

Yet, cooperation through NPAs or DPAs is costly. Usually the firm is required to admit guilt, pay large settlements, and undertake a complete re-organization of its corporate structure and culture to comply with regulatory standards in the future (Garrett, 2011). NPA and DPA terms also often mandate the company changes executive offices composition and organizes systems of internal investigations including third-party observers. Non-US companies can also incur in transaction costs and frictions caused by their inexperience with the US legal system (Leibold, 2014). A company will therefore evaluate the opportunity cost of refusing cooperation (and its expenses) based on how much it values potential benefits of signalling a compliant behavior. Opportunity cost of refusing cooperation with US authorities will be higher for those foreign companies that risk larger reputational damage for corporate scandals.

I argue that companies with an investment on US soil will tend to suffer increased reputational costs for these types of scandals. Similar companies have branches, offices, jobs, and businesses

[19]This means that it is not relevant whether a firm was *actually* guilty of the alleged crime or not: firms suffer harsh reputational penalties even as a result of mere criminal allegations.

(Kerner, 2014) that potentially expose a relatively larger share of the firms' activity to the local public opinion. Having more at stake, these companies will perceive higher potential reputational costs in case of a corporate scandal. They will therefore face higher opportunity costs of refusing cooperation. Hence, when information on potential corporate law violation emerges, they will be more likely to cooperate with authorities than comparable companies with no similar exposure to the US economy. This provides prosecutors the information they need to bring a case and makes it more likely that they will be able to enforce regulation against these companies.

In the running example introduced above, Red Apple will be more likely to cooperate with prosecutors than Morley. This makes the Red Apple case one where the availability of information will likely be higher than in the Morley one. All else equal, I expect US authorities will be more likely to dedicate scarce legal resources to cases against foreign companies with investment in the country, than others.

2.4 The US and the global anti-bribery regime

A study aimed at explaining the selection of foreign companies by prosecutors quickly runs into a very obvious problem: only *enforced* cases are observable. Cases agencies had information on and decided not to investigate are, instead, impossible to observe. My solution to this problem exploits two unique features of the case I study: the US-sponsored international anti-bribery regime that was built starting in the 1990s. First, construction of the regime diffused US-like anti-bribery policies to its member states. Second, the regime enables US authorities to enforce their own anti-bribery policy extraterritorially.

The United States was the first country to adopt a regulation preventing American companies from paying bribes to foreign public officials[20], with the 1977 Foreign Corrupt Practices Act

[20]Corruption places relevant costs onto bribe-paying companies, as discussed more extensively in the first paper of this dissertation. Obvious costs include the payment of corruption fees. The original dataset supporting this study shows that companies investigated for bribery pay fees in a range from about $150 to more than $1 billion, with an average of about $225 million. However, paying the bribe fee is often worth its price as a means of overcoming competition (Malesky et al., 2015). The average corrupt contract in my dataset had an expected profit of $1 billion, reaching a maximum of about $22 billion. The most significant cost faced by companies for foreign bribery, however, comes from the sum of formal penalties or fees and reputational damages on financial markets. Data from (Garrett and Ashley,

(FCPA)[21]. Ever since the law was adopted, US administrations have faced concerns by American companies who were worried the law would disadvantage them *vis-à-vis* foreign competitors that did not risk judiciary repercussions for paying bribes overseas[22]. They responded to such concerns in two ways. First, they lobbied for the adoption of a common anti-bribery regulation among partner countries, an effort which had to wait 20 years before resulting in a number of anti-bribery international agreements signed in the 1990s (Abbott and Snidal, 2002). Second, they expanded the jurisdiction of the FCPA to cover non-US companies with rounds of amendments in 1988 and in 1998.

Two consequences of its peculiar history make the regime an optimal case to study why US authorities choose to prosecute certain non-US companies. First, countries participating in this US-sponsored international anti-bribery regime have adopted policies that closely resemble the US FCPA, and that prohibit the same type of corrupt payments (Brewster, 2014). This is particularly true for the 43 countries taking part, alongside the US, to the OECD Anti-Bribery Convention. The Convention is an instrument of hard international law mandating its participants to enforce anti-bribery standards against foreign corrupt payments by their MNCs (Abbott and Snidal, 2002). The existence of this agreement makes it possible to observe, from 1997 on, a much larger set of violations of anti-bribery rules by companies around the world, than what would be observed by simply looking at US-based enforcement.

Second, the construction of the regime dramatically expanded US authority over non-US companies. After its 1998 amendments the FCPA formally applies to foreign corporations suspected of foreign bribe payments with just a marginal connection to the US. This includes non-US companies listed on a US stock exchange or otherwise obliged to submit periodic reports to the Securities

2019) show that US authorities have imposed fines on prosecuted companies for up to $4 billion for involvement into corrupt contracts. The third paper of this dissertation investigates more extensively the financial cost of companies' involvement into corruption and their limits.

[21]The FCPA emerged from the blows of the Watergate scandal. Among the many illicit payments uncovered by the Watergate Special Prosecutor offices, US corporations were found to have paid bribes in various foreign countries to members of governments, of cabinets, of parties, and of the public administration believed to be close to American interests and values (Abbott and Snidal, 2002).

[22]A few countries, notably France and West Germany, had even made cross-border bribery payments tax-deductible for their companies (Gutterman, 2015).

and Exchange Commission (SEC), but also non-issuing companies furthering any act of bribery to a foreign official *while in the US* (Leibold, 2014). The latter category does not cover exclusively misbehavior *physically* occurring on US soil. In fact, the use of US mail, phone calls or internet communication using US providers, interstate or international travel with connection to the US, and even bank wire transfers or transactions operated in US dollars can be sufficient for the SEC and the DOJ to invoke jurisdiction on a company from anywhere in the world, as long as such connections were instrumental to the corrupt exchange. This interpretation of the "presence doctrine" (see footnote 2) is so broad that often its justification appears stretched to legal experts (Garrett, 2011). Cross-border networks of mutual legal assistance among authorities belonging to this regime facilitates US federal agencies to enforce their laws against foreign subjects (Kaczmarek and Newman, 2011). As a result, nowadays virtually *all* corporations under the regulatory umbrella of the anti-bribery regime are considered liable under FCPA provisions, regardless of their presence on US soil (Leibold, 2014).

By focusing on this specific regime I am thus able to observe a vast set of similar violations of anti-bribery standards by non-US companies. Some were investigated by US authorities, others were not. Given that the US has the authority to behave as the "global sheriff" of this regime and prosecute virtually all these cases, I am able to distinguish between cases involving non-US companies that were prosecuted by US authorities and those that were only prosecuted by non-US courts.

I argue these cases comprise the ones information was available for. This observable set clearly does not include *all* possible violations of anti-bribery standards. Namely it does not include cases that (*i*) no involved authority had information on and that (*ii*) all informed authorities willingly chose not to investigate. The first source of selection does not really represent an issue for this study, as US authorities cannot choose to focus on violations nobody knew of. The second, instead, might introduce an issue of selection bias. If unobserved cases *willingly* overlooked by all informed judicial authorities were also systematically different in their US investment behavior, I would be unable to observe certain bribery cases that differ systematically from the observable ones in terms of the

involved companies' presence in the US. I assume this is not happening: hence deliberate selection of non-US cases out of the set of observable ones is independent of the US ownership structure of the involved companies[23]. Even in the restrictive case this assumption were violated, yet, focusing on observable cases allows to explain what drives US prosecution of non-US companies in *publicly known* violation of the anti-bribery regime. Next section details how I construct an original dataset recording anti-bribery violations by non-US companies.

2.5 Data collection

My argument explains decisions of US authorities to investigate a non-US company as a function of its investment in the US. I test this expectation in the case of the anti-bribery regime. Two crucial pieces of information are therefore required. First, I need to retrieve information on cases of corporate bribery by non-US companies which were investigated and were not investigated by US authorities. Second, I need to reconstruct investment of the involved firms in the US (or lack thereof). The next two subsections detail how I constructed an original dataset containing both pieces of information.

2.5.1 Anti-corporate bribery enforcement cases

I retrieve information on anti-bribery enforcement actions by US and non-US authorities combining different data sources. The main one is the TRACE Compendium. TRACE International is a business association providing risk management services, particularly focused on anti-bribery legal, reputational, and economic risk. Its Compendium[24] is an open database made of 841 text documents summarising events of cross-border corporate corruption in violation of the international anti-bribery regime, and related law enforcement actions. TRACE constructs it drawing from legal

[23] A violation of this assumption would occur, for instance, if overseas bribe payments by non-US companies with a significantly large presence in the US were known by authorities of some countries and were willingly and coordinately overlooked. I believe this scenario to be rather unrealistic, particularly since countries where bribes were paid can often extract a valuable reimbursement from judicial actions.

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

documents, newspaper articles, and leakages or official releases of corporate files.

Two features make this database the perfect source for my data collection. First, the case selection. TRACE only considers events of cross-border corruption where the bribe-payer is an (individual acting on behalf of an) enterprise headquartered in a certain country and the bribe-payee is a public official of a different nationality. It does not consider events of corruption that are purely domestic, *i.e.* those where bribe-payer and payee are of the same nationality. These events technically do not fall under the scope of the international anti-bribery regime. Second, TRACE reports events of anti-bribery law enforcement initiated by US *and* non-US authorities alike. This allows me to tell cases that were enforced by US prosecutors from those that US agencies did not enforce.

I employ a mix of web-scraping techniques and hand-coding to collect information from these 841 text documents[25]. A single TRACE document can be particularly complex. It can refer to a single bribe-payer company or multiple ones. Under the same corrupt scheme, bribe-payers can funnel illicit money into the pockets of public officials from a single country or multiple ones. Finally, enforcement actions by a national authority can cover any or all of these criminal transactions. I disentangle this complex information and code information on individual payments made by specific subjects to foreign officials of a certain nationality, and enforced by a given national authority.

Once I have obtained information on events of anti-bribery enforcement, I collect data on the corporate identity of the bribe-payers. First, I identify the company paying the bribe from the scraped TRACE documents. Where individuals are reported to have paid a bribe, I try to gather information on the company they were bribing on behalf of, if the information is available. Where this information is not available, I flag these records as payments made by individuals, and I later

[25]I check completeness of this dataset drawing from data generously provided by Escresa and Picci (2017). I also check my data using sources specific to US FCPA enforcement: Garrett and Ashley (2019)'s data on DOJ prosecution, Stanford Law School's Foreign Corrupt Practices Act Clearinghouse dataset (see: `http://fcpa.stanford.edu/enforcement-actions.html?page=1.0`), and data from the Violation Tracker by Good Job First (see: `https://www.goodjobsfirst.org/violation-tracker`). I test accuracy of coded information by randomly selecting 30% of the observations and checking the way automated techniques coded information.

discard them[26]. I draw on several providers of company data to retrieve correct information on the headquarter country and identity of each corporate bribe-payer. My main source for this is Bureau van Dijk's Orbis Corporate Ownership database. Where Orbis does not report information on the included companies in my dataset, I draw from alternative sources[27]. I retrieve this information employing mandatory filings by civil authorities in charge of overseeing security markets (like the SEC or UK Companies House reports), datasets about leaked offshore corporate documents[28], NGO information (like those provided by the UN Global Compact program, or development agencies), and private information providers on company data (Bloomberg, Dun & Bradstreet, and Crunchbase).

The final dataset I obtain is composed of 3,031 distinct events. The unit of such dataset is a bribe payment by a single subject (a company or an individual) to public officials of a single nationality, prosecuted by authorities of a single country. Reported bribes were paid by a total of 767 companies headquartered in 75 different countries, and by 34 individuals of 16 different nationalities. They involve a total of 1,245 different events of cross-border bribe payments to foreign officials from 160 different countries, from 1972 to 2021. A total of 1,583 distinct enforcement events[29] were initiated by national agencies of 111 countries in total.

This rich dataset contains some information that is not necessary for my study. I therefore proceed at selecting only the relevant information. First I remove all observations referring to events of corruption taking place before 1997. I consider this year the real birth of the anti-bribery regime, with the ratification of the OECD Anti-Bribery Convention. Legal research has shown that the agreement represented a turning point for the possibility to enforce the US FCPA (Brewster, 2014). Next, I remove all cases of cross-border bribery where information on the company individuals were bribing on behalf of was not available.

[26]In 45 TRACE records it was not possible to discern which companies individual bribe-payers were paying on behalf of. In the majority of these cases criminals set up fictitious firms or shell companies to conceal bribe-payments and/or pocket kickbacks. Usually they conceived complex schemes for securing private advantages.

[27]Only 45 out of 767 companies were not appearing in Orbis and required to obtain information with alternative means.

[28]Like `https://offshoreleaks.icij.org` or `https://wikileaks.org`.

[29]This figure departs significantly from official TRACE statistics because I count enforcement actions differently. TRACE considers multiple events of enforcement by agencies from different countries as a single event, while in my dataset each individual enforcement by an agency represents a single event of prosecution, even if multiple countries are prosecuting the same corruption case.

My interest is in explaining extraterritorial prosecution of non-US companies by American federal authorities. I therefore discard all cases of cross-border bribery where the bribe-payer company is a US-incorporated firm. Cases in this category do not fall under the extraterritorial application of FCPA terms, therefore they are not of interest for the study. Finally, I discard cases where the bribe-payee is a US public official. Cases under this category do not fall under the jurisdiction of the FCPA, and are prosecuted in light of legislations against domestic bribery.

These selections leave me with a dataset of 1,921 observations involving 425 non-US companies. These firms were involved in a total of 677 distinct corrupt exchanges involving non-US public officials, prosecuted in 934 different events of enforcement around the world. Finally, I collapse these observations by firm and obtain my binary dependent variable *Investigation* measuring whether each firm of the 425 non-US companies involved in at least one bribery event was ever investigated by US authorities (DOJ or SEC) or not[30].

2.5.2 US investment

I collect information on the presence of these companies in the US economy. I aim at constructing a binary firm-level explanatory variable *US Subsidiary* that takes value 1 if and only if a given company has a subsidiary in the US.

I rely on Orbis to obtain data on investment of the companies of interest through US subsidiaries. Out of the 425 companies in the dataset, 402 have information available in Orbis. I collect data on subsidiaries of each of these 402 firms around the world. Orbis reports information gathered from mandatory filings made by companies to agencies regulating securities.

Orbis reports that the 402 companies of interest own a total of 58,734 unique subsidiaries in 198 different countries. Not all these ownership relations are of interest, though. Some of these subsidiaries can be shell companies, financial services, and short-term ownership operations that

[30]A minority of companies (4 out of 428) were investigated by other US agencies in connection to events of bribery, including the Federal Bureau of Investigation, the Department of State, and the Department of Commerce. Since these agencies are not in charge of enforcing the FCPA, I code these cases as *not* being investigated by the US, as I am interested in explaining enforcement of FCPA extraterritorial terms. However, a replication of all results presented in the next section relaxing this condition showed my estimates do not hinge on it. This robustness test is not reported in the paper to keep the Appendix compact enough, but is available upon request.

have little to do with the type of long-term foreign investment implied by my argument (Kerner, 2014). I proceed at making appropriate selections following practices recommended by previous studies that use Orbis data (Beazer and Blake, 2018) and established guidelines on dealing with this data source (Kalemli-Ozcan et al., 2015b).

I get information on whether each of the 402 companies owns at least one US subsidiary. I define a subsidiary according to two criteria. First, it should not be what Orbis classifies as a "small" company. Hence, I consider only subsidiaries that haver higher operating revenue than one million US dollars, more than two million US dollars in total assets, and more than 15 employees. Second, a parent should enjoy at least 50% of voting power in a company for it to be considered its subsidiary (Kalemli-Ozcan et al., 2015b). These two criteria ensure I am only considering non-US long-term and substantive investment in the US, the kind of enterprise that is likely to induce reputational liabilities advanced by my theory.

For each company in the dataset I construct my explanatory variable of interest *US Subsidiary* measuring whether the company has at least one subsidiary in the US. Out of the 402 companies, 49 have a subsidiary in the country[31] while 353 do not.

2.5.3 Firm-level controls

My argument expects non-US companies suspected of bribery will be disproportionately likely to be investigated by federal authorities if they have an investment in the US. The effect of interest can be confounded by a number of potential factors that might make it more likely for a company to invest in the US, while simultaneously making it more likely that US agencies will investigate it. The analysis should control for these factors.

First, foreign companies can be more likely to invest in the US if their production and ownership chains make them more projected abroad. At the same time, firms in industries that fragment production across borders can also be more likely to be involved in events of bribery, simply

[31]Imposing the condition that only non-small companies count as subsidiaries shrinks substantively this "treatment group". Relaxing it increases the treatment group to a total of 174 companies. Results presented in the next section are overall the same (although slightly smaller in size) when this condition is relaxed. They are available upon request.

because of the larger possibilities of interaction with foreign public officials. I therefore extract from Orbis information on the number of total subsidiaries (defined following the same criteria outlined above) for each company – *Global Subsidiaries* – and for the number of branches directly controlled by each company – *Global Branches*. The number of employees can also play a similar confounding effect: companies with more employees around the world can have better resources to invest in the US and be more likely to be exposed to bribery. I therefore measure the logarithm of the total number of employees for each company + 1: *Global Employees* ($log + 1$).

The wealth of a company can also confound the relationship of interest. Richer firms might have the means and interest to embark in a US investment, an initiative which entails considerable sunk costs. At the same time, US authorities might find wealthier firms as more appealing targets, if they expect they could extract larger fines from them. I therefore measure the average *Parent Revenues* and *Parent Assets* as reported by Orbis, measuring respectively operating revenues and total assets' worth in millions of US dollars. I also measure the average number of employees (*Parent Employees*) to get a sense of the firm's size.

Finally, foreign companies that tend to be more often under scrutiny of their domestic agencies might be less likely to invest in the US as a means of reducing judicial pressure on their activity. At the same time, US agencies might refrain from prosecuting a company if it is already investigated by other authorities. To control for this aspect I measure, for each company, the mean number of prosecuting countries in the events of bribery that involve it (*Number of Prosecutors*).

Companies from specific nationalities can also be facilitated when investing in the US because of unobservable characteristics. Perhaps special relations between their home countries and the US facilitates the bureaucratic process of opening a subsidiary on American soil. If US authorities investigate more thoroughly companies from these home countries as a foreign policy tool (see Tomashevskiy, 2021), then the origin of a company can confound the relationship of interest. Industry-specific features can also play a similar role. To account for both problems, I record the home country of each company and its industrial sector based on the two-digits North American Industry Classification System (NAICS). I introduce fixed effects at these levels to remove such

unobserved heterogeneity.

2.5.4 Data description

I present summary statistics of my cross-sectional dataset in Appendix (Table A.1). Table 2.1 provides information on the average value of each variable for companies with or without a US subsidiary. Non-US companies with no US subsidiaries have a baseline 0.33 probability of being investigated by US authorities when they are involved in events of bribery. This confirms the vigorous extraterritorial activity of federal authorities documented by previous studies. This probability increases by 0.40 in the group of companies with at least one US subsidiaries. Firms in this group have a 0.73 probability of being investigated by US authorities, when they are involved in events of bribery. The difference is statistically distinguishable from zero at a 0.05 conventional level of significance.

	No Subsid. (N=353)		Subsid. (N=49)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Investigation	0.33	0.47	0.73	0.45	0.40	0.07
Global Subsidiaries	0.01	0.02	0.04	0.04	0.03	0.01
Global Branches	0.02	0.12	0.07	0.32	0.05	0.05
Global Employees (log+1)	0.85	1.37	3.43	1.12	2.58	0.18
Number of Prosecutors (mean)	1.48	0.91	1.37	0.55	-0.11	0.09
Parent Revenues (mean)	0.12	0.37	0.39	0.46	0.27	0.07
Parent Assets (mean)	0.37	1.49	2.79	6.17	2.42	0.89
Parent Employees (mean)	0.00	0.00	0.00	0.00	0.00	0.00

Table 2.1: Bribe-payer non-US companies. Observable covariates balance table

This difference provides some initial descriptive evidence on the relationship advanced by my argument. Yet, Table 2.1 also shows that almost all the other covariates are strongly unbalanced in the sample. On average, firms with at least one investment in the US tend to have more extended activities in the world, and they tend to be larger. To the extent that these features correlate with the likelihood of US investigation too, the difference-in-means presented above provides a confounded picture. Next section adopts a selection on observables design to hold constant these potential sources of endogeneity.

2.6 Results

In this section I estimate a series of linear probability models (LPM) of the *Investigation* binary dependent variable. I explain the dependent variable using my main explanatory variable *US Subsidiary* and I hold constant relevant observable sources of endogeneity presented in the previous section. I estimate LPMs using ordinary least squares (OLS) for ease of interpretation of their coefficients. I cluster all standard errors at the home-country level to account for possible correlation at this level in the error term and obtain larger measures of uncertainty that factor in co-dependence of observations.

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.403***	0.349***	0.344***	0.279**	0.303**
	(0.076)	(0.091)	(0.101)	(0.105)	(0.104)
Global Subsidiaries		-1.061	-0.772	-0.018	-0.041
		(0.871)	(0.960)	(1.165)	(1.057)
Global Branches		0.358**	0.215	0.187*	0.148
		(0.124)	(0.160)	(0.084)	(0.090)
Global Employees (log+1)		0.027	0.019	0.016	0.040
		(0.024)	(0.029)	(0.030)	(0.033)
Number of Prosecutors (mean)			0.177**	0.138+	0.133+
			(0.058)	(0.075)	(0.068)
Parent Revenues (mean)			0.151*	0.196*	0.140
			(0.071)	(0.094)	(0.145)
Parent Assets (mean)			0.013+	0.018**	0.015+
			(0.007)	(0.006)	(0.009)
Parent Employees (mean)			-61.596***	-74.496*	-85.837
			(17.664)	(34.456)	(69.198)
(Intercept)	0.331***	0.311***	0.061	-0.162	-0.409*
	(0.041)	(0.048)	(0.104)	(0.099)	(0.166)
Num.Obs.	402	402	303	303	301
Country FE				Yes	Yes
Industry FE					Yes
R2	0.074	0.091	0.203	0.433	0.486
R2 Adj.	0.072	0.082	0.181	0.296	0.308
F	31.886	9.973	9.365	3.149	2.736

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

Table 2.2: Linear probability models of Investigation. Binary main explanatory variable

Note: Coefficients estimated using OLS. Standard errors are clustered at the home country level.

Table 2.2 reports LPM estimates. I include my control variables step-wise and carefully given

the limited number of observations in my dataset and data missingness for some covariates. This also avoids problems of suppression effects in a selection on observables design (Lenz and Sahn, 2021). The first model introduces only my binary explanatory variable *US Subsidiary*. Its estimated effect is obviously the exact same difference-in-means reported in Table 2.1: non-US companies with at least one subsidiary in the US are about 0.40 more likely to be investigated by federal authorities when they are involved in events of bribery, over a baseline likelihood of 0.33. In the second model I introduce controls relative to the global spread of a company. I control for the worldwide number of subsidiaries, of branches, and for the (logged) number of employees. The estimated effect of *US Subsidiary* remains substantively the same: an increase of about 0.35 in the likelihood of being investigated by federal authorities.

In model 3, I introduce controls relative to the size of the company (average revenues, assets, and number of employees) and I control for the average number of prosecuting countries in cases involving it. Introducing these controls shrinks the sample size considerably due to missing data for variables on company sizes. Nevertheless the effect of *US Subsidiary* remains substantively the same: an increase by about 0.34 in the probability of being investigated. Finally, in models 4 and 5 I control for country-specific and industry-specific idiosyncratic heterogeneity by including a categorical home country variable (*Country FE*) and a categorical industry variable (*Industry FE*). Estimates still show that companies with at least one *US Subsidiary* are about 0.28 to 0.30 more likely to be investigated by federal authorities.

All estimated effects of *US Subsidiary* are distinguishable from zero at a 0.05 conventional level of significance. The effect size in the most conservative of the estimated models (model 4, with the smallest point estimate and the largest measure of uncertainty) is such that the probability of being investigated by federal authorities increases by a factor between 0.07 and 0.49 for firms with at least one *US Subsidiary* (when looking at the boundaries of a 95% confidence interval). This amounts to a percentage increase in the probability of being investigated by at least 21% and at most 148% (when considering the baseline 0.33 probability of investigation for companies with no US investment).

I test robustness of these results extensively. All tests are reported and discussed in Appendix. The overall picture they provide is consistent with the one that emerges from Table 2.2. First, I assess the possibility that results are driven by any one outlier in the sample. I adopt a jackknife approach and re-estimate my most complete model (no. 5 from Table 2.2) leaving each one company out from the model once. Next, I assess the potential concern that Chinese companies are driving the results for reasons unrelated to my causal story. I exclude all Chinese companies from the sample and re-estimate my models. In a following test I address the concern that FCPA jurisdiction might not be equally easy to claim on all companies in my sample. I leverage the fact that companies trading securities on US-based exchanges unambigously fall under FCPA jurisdiction. I first control for this additional variable, then condition on it restricting my sample to the 145 US-traded foreign companies. Next, I restrict the causal quantity of interest to an average treatment effect on the treated (ATT) companies. I estimate it in two ways: with an OLS regression imputation estimator (Lin, 2013) and with a Mahalanobis matching procedure. Next, I show that estimates are not model-dependent: I employ a logit model, a random effect linear model, and a random effect logit model. I also propose alternative operationalizations of my dependent and main explanatory variables. Finally, I disaggregate my company-level data into a multilevel dataset where each row represents a company involved in a specific corrupt incident in a given host country. This allows me to control for the potential confounding effect of investigation of an incident by the home country, and for heterogeneity at the host-country level.

2.6.1 Discussion

Estimates provided in the selection on observables analysis inform us that, on average, non-US companies that are exposed to the US economy through an investment in the country are at least 0.28 more likely to be investigated by US authorities than comparable companies with no investment in the US. Internal validity of these results rests on the demanding conditional independence assumption, which claims that models control for all potential factors causing companies with US investment to be systematically different than those without similar exposure, and that also affect

the dependent variable. This assumption is essentially untestable and hard to defend. It is, yet, possible to hypothesize the direction of potential sources of bias to understand the extent to which the sign of the estimates is credible.

One potential source of omitted variable bias is the unobservable preference of companies for bribing in international business. Non-US companies that are more inclined to secure contracts by means of bribing are more likely to end up being targeted by US authorities, simply because they have higher chances to appear on their radar. At the same time, they are likely aware of the extraterritorial provisions of the American anti-bribery policy. They might therefore decide to reduce their exposure to the US economy, by avoiding investing in the country, precisely in order to limit the risk of prosecution. If that is the case, then the propensity to bribe is a potential confounder threatening results presented.

A similar likely confounder is the propensity to pay *larger* bribes. Non-US companies that are more used to pay larger bribes in international business might be more likely target of US authorities (Choi and Davis, 2014). At the same time, they might have an incentive to limit their presence in the US economy to avoid the risk of prosecution.

In both cases, yet, the direction of the resulting selection bias would be negative, because propensity to bribe (or to bribe more) would be positively causing the dependent variable but negatively affecting exposure to the US economy. Therefore estimates presented in Table 2.2 would in fact be *under*estimating the real effect size of *US Subsidiary* on *Investigation*. Their point estimates would be smaller than the real effect size. This is true in general for all sources of *negative* selection bias. They would threaten the substantive validity of estimated effects because their exclusion causes coefficients to be smaller than the real effect sizes. Yet, even if point estimates would be unreliable, the effect of exposure to the US economy on the probability of being investigated by US authorities would nevertheless be positive.

Potential sources of selection bias should be positive in order to effectively threaten the *sign* of the estimates provided. Their effects on the dependent variable *and* treatment variable *US Subsidiary* should have the same sign (either both positive or both negative). My selection on observables

design assumes similar sources of endogeneity are not taking place. Is this reasonable? How large should a hypothetical unobserved confounder be in order to threaten the presented estimates? Recent tools for sensitivity analysis introduced by Cinelli and Hazlett (2020) allow to ground this assumption into more concrete terms. These tools allow the analyst to use an *observed* severe confounder as a reference point to study how larger an *unobserved* confounder should be in order to flip sign of estimated effect or to make it statistically insignificant. Impact of confounders is expressed in terms of partial R-squared, *i.e.* in terms of the proportion of residual variation of the treatment or outcome variable that the confounder explains.

I select the size of the parent firm (expressed in terms of asset value) as the most severe observed confounder in my dataset. Larger parent companies have the means to invest in the US market and they also tend to be more likely targets of US prosecutors' activity. There are substantial barriers and costs to invest into the US. An investment typically consists in the establishment (or acquisition) of expensive productive sites in the host country (Kerner, 2014). This represent a significant sunk cost (Jensen et al., 2012). Investing in the US also comes with costs of a different nature. Investors should adapt to the US regulation. They are required to file detailed periodical reports to the SEC. Files must abide by a specific form and typically require the assistance of legal consultants. Legal and organizational costs also come from adapting to the US regulatory framework. This is a documented problem specifically in the case of anti-bribery regulations. Whereas US companies have been subject to FCPA terms since 1977, a non-US company incurs in significant organizational costs to re-organize its structure in order to be FCPA-compliant (Garrett, 2014; Leibold, 2014). On average, only larger firms might be able to absorb similar costs. These considerations find descriptive confirmation in my sample. Table 2.1 shows that, on average, foreign companies with an investment in the US are more than 7 times larger than companies with no presence in the country, when looking at the value of their assets.

Larger companies, in turn, might also appear more often on US authorities' radar. They are more likely to have extended operations around the globe, for the same reason as they are more likely to invest in the US. Hence, the likelihood that they end up being involved in bribery scandals is higher

than for smaller firms. They might also be more appealing targets for prosecutors, who can extract larger fines from them. US authorities will therefore be more likely to investigate these companies, all else equal. The size of a company is likely to be the most relevant observed confounder of my effect of interest. If this argument is correct, this confounder introduces a positive selection bias, the kind of bias my analysis should be concerned about. I then proceed to test sensitivity of the estimated effect from model 5 in Table 2.2 using this observed covariate as a benchmark[32].

Table 2.3 provides a summary of its sensitivity to omitted variable bias. In this model the treatment variable alone explains about 4.7% in the variation of the outcome variable. This portion is rather large, considering that it stands even after accounting for the explanatory power of covariates and fixed effects. The sensitivity analysis informs that it would take an unobserved confounder explaining more than 19.9% of the remaining variance of *both* treatment and outcome variables to bring the point estimate to 0. Such a large effect on the residual variation from both variables appears unreasonable, thus providing confidence in the estimated effect. The table also informs that an unobserved confounder explaining 8.6% of the residual variation of treatment and outcome variables would be sufficient to move the t-statistic of the estimated effect within the 95% critical values interval, thus making it no longer statistically significant.

Outcome: *Investigation*

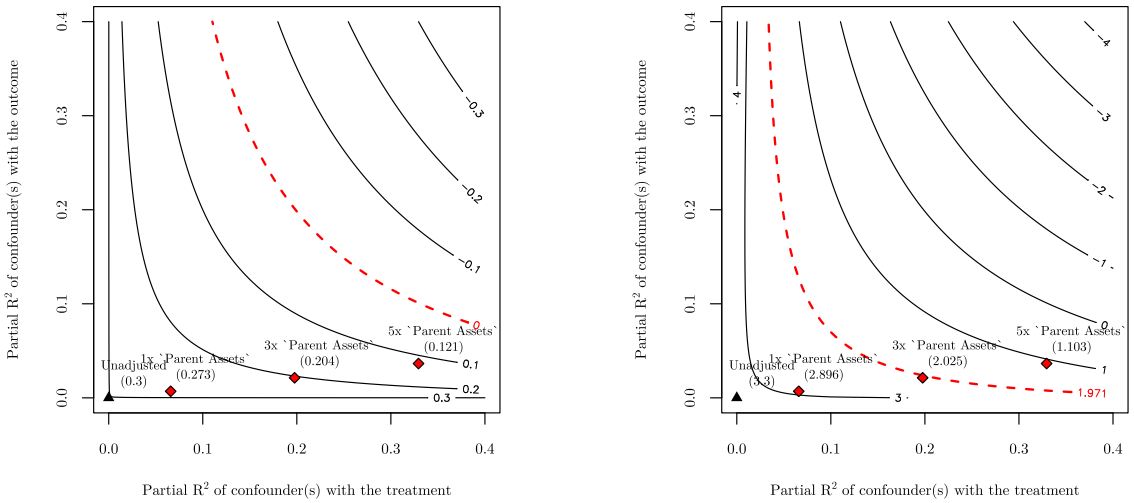
Treatment:	Est.	S.E.	t-value	$R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.05}$
US Subsidiary	0.303	0.091	3.324	4.7%	19.9%	8.6%
df = 223				*Bound (1x Parent Assets)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ = 0.7%, $R^2_{D\sim Z\|\mathbf{X}}$ = 6.6%		

Table 2.3: Sensitivity analysis of estimated effects

How strong should such an unobserved confounder be, in comparison to the chosen *Parent Assets* observed benchmark? Figure 2.3 provides this piece of information. Panel 2.3a shows that it would take an unobserved confounder *more* than five times as large as *Parent Assets* to flip the sign of the point estimate. If there existed an omitted variable even five times as large as the chosen benchmark, the point-estimated effect would still be positive and sizeable (a 0.121 increase in the probability of *Investigation*). Again, it seems unreasonable to have such a large unobserved

[32]I choose model 5 as it is the most complete specification in my main analysis. Sensitivity analyses performed using other observable covariates as benchmarks provided less conservative results and are available upon request.

confounder in place, which provides confidence on the estimated effects. Yet, Panel 2.3b provides a cautionary tale on my results. Any unobserved confounder that explained more than three times the variation of treatment and outcome that *Parent Assets* explains would move the t-statistics to a value below the conventional critical value of a 95% confidence interval in the relevant t-student distribution for the model. The presence of any unobserved confounder of this size or larger would therefore mean the true estimated effect is not statistically significant at a conventional 0.05 level. Although this limitation is also a function of limited sample size, it offers a concrete boundary for the validity of my results.



(a) Point estimates

(b) T-statistics

Figure 2.3: Sensitivity analysis

Estimates from Table 2.2 therefore rest on the assumption that hypothetical unobserved confounders of this size do not exist. Table 2.3 and Figure 2.3 provide concrete boundaries to the impact an omitted variable should have on treatment and outcome variables in order to make the effect estimated in this section completely unreliable.

2.7 Mechanism: reputational damage and cooperation

The previous section shows that companies suspected to be in violation of US anti-corruption policy are more likely to be investigated by US authorities if they have an investment in the US. Results hold conditioning on relevant observable confounders and the identifying assumption appears reasonable in light of the sensitivity analysis. However, the analysis does not shed light on the precise mechanism driving the proposed effect. I argue that the effect is driven by the availability of willingly disclosed information to prosecutors induced by reputational damage (see Figure 2.2). However this mechanism is currently not tested.

In fact, a plausible alternative mechanism would claim that foreign companies with investment in the US have physical assets in the country that are not easy to relocate. It would then be simpler for US authorities to enforce sentences against foreign companies with a foot in the US, not because they are more likely to cooperate with authorities, nor because they suffer larger reputational penalties, but because they will be more easily coerced to pay fines. Alternatively, US authorities might find it easier to claim legitimacy for obtaining subpoena and coerce companies into disclosing information. These different mechanisms might also be simultaneously at place.

In this section I provide additional evidence to support my mechanism. First, I investigate two cases that illustrate my mechanism. Second, I present large-N evidence that generalize my logic and back the claim that reputational incentives lead companies with significant presence in the US to cooperate with American authorities.

2.7.1 Small-N evidence

Here I illustrate my mechanism leveraging two concrete examples. The first involves a foreign company with significant territorial ties to the US which discovered evidence about internal corrupt behaviors and disclosed it to US authorities. American prosecutors opened an investigation into the matter and eventually found the company innocent of the conduct, deciding to close all investigations. The second case involves a foreign company trading its stocks as American Depositary Receipts

(ADRs) in the US but lacking a significant physical presence in the United States, as defined above. The company was investigated by foreign authorities but US agencies never intervened.

The case studies illustrate a striking difference in the type of interaction between companies and authorities. The first one shows a company exposed to the US and linked to its territory, which goes into significant steps to show compliance with local anti-corruption laws and reduce its liability of foreignness and reputational damage. US authorities begin an investigation in the matter and eventually find the company innocent. The second one shows a company that is insulated from the US regulatory reach and reputational damage in the US, namely because of a lack of territorial connection. Notwithstanding the severity of the case, no reputational damage is observed. The company never provided cooperation with US authorities, who never intervened on the matter.

Case 1 Millicom International Cellular SA is a telecommunication company incorporated in Luxembourg, that serves primarily the Latin American market through its Tigo brand. The company currently provides services to approximately 55 million customers in Latin America[33]. It trades equities on the US NASDAQ Stock Exchange (since January 2019) under the symbol TIGO and trades ADRs over-the-counter in the US under symbol MICCF (since January 2012). Crucially for my argument, it is also physically present in the US through its wholly-owned Millicom International Services, whose corporate offices are at Number 255 of Giralda Avenue, Miami.

On October 21, 2015 Millicom issued a press release announcing to have recently discovered potential violations of the FCPA in Guatemala, as part of the operations conducted by its joint venture Comunicaciones Celulares S.A. with billionaire Mario Lopez Estrada. Evidence emerged as part of an internal investigation conducted by the company itself with support of the law firm Covington & Burling LLP. The company announced allegations had been disclosed with authorities in the US and guaranteed full cooperation with prosecutors[34].

Reputational damage looked significant in the aftermath of the press release[35]. In the month of October 2015, following the publication of allegations, the price of the company's stocks traded

[33]See: `https://tinyurl.com/bloomberg-millicom`.

[34]See press release: `https://fcpa.stanford.edu/fcpac/releases/2000/001091.pdf`.

[35]All stock prices data presented in this section have been downloaded from the Refinitiv Workspace.

under the symbol MICCF went from $60.2 per share (October 2, 2015) to $54.6 per share (November 9, 2015) amounting to a loss in value of about 9.30%. For comparison, in about the same period the overall value of the NASDAQ 100 index increased by about 9.12% instead, indicating a general upward trend of the market. The Department of Justice opened an investigation into potential FCPA violations as soon as information was disclosed by the company and cooperation started. However, and despite the significant damage suffered on international markets, three years later the DOJ announced to have concluded its investigations and found no evidence of misbehavior. No further action would be taken, as a result[36].

Case 2 A comparable corruption case involved the telecommunication company MTN Group Limited. The company is incorporated in South Africa and primarily serves the African market. It is considered the largest telecommunication provider in the continent and the first company in Sub Saharan Africa for market capitalisation[37]. The company trades securities in the US over-the-counter as ADRs (ticker symbol: MTNOY). However, it has no physical presence in the US through a foreign direct investment. FCPA jurisdiction has often been imposed by the SEC and DOJ on foreign companies trading ADRs on US exchanges[38]. On the basis of these precedents, the company can be considered liable under FCPA terms.

On February 2, 2012 the Turkish telecommunication company Turkcell alleged that MTN had bribed government officials in Iran in order to beat competition. The Turkish company claimed MTN obtained the cancellation of a licence previously granted by the Iranian government to Turkcell in exchange for bribes and the promise of assistance to Iran on nuclear issues at the International Atomic Energy Agency (IAEA)[39]. In March 2012, Turkcell attempted to involve US authorities directly into the matter by filing a civil law suit in the US District Court for the District of Columbia,

[36]See interim report by the company to announce end of investigation: `https://fcpa.stanford.edu/fcpac/releases/4000/002968.pdf`.

[37]See: `https://tinyurl.com/businessinsider-mtn`.

[38]Illustrative examples where US agencies explicitly mentioned trading ADRs to establish jurisdiction include cases: *S.E.C. v. Montedison, S.p.A*, No. 96-2631 (November 21, 1996); *S.E.C. v. ABB, Ltd.*, No 05-1141 (July 6, 2004); *United States of America v. Siemens AG*, Case 08-cr-00367 (December 12, 2008).

[39]See: `https://www.diligenciagroup.com/blogs/mtn-more-than-a-corruption-and-bribery-accusation`.

demanding about $4 billion in damages[40]. However, US authorities are not reported to have ever initiated any investigation onto potential FCPA violations related to the matter. Nor there is evidence that US prosecutors ever investigated an unrelated bribery case involving the MTN Group in Nigeria[41].

MTN responded to the Iranian allegations by setting up an internal investigation into the matter. No evidence exists that they ever disclosed information resulting from such investigation with US authorities. Reputational costs inflicted on the company in the US also seem remarkably small. In the month of February 2012, the stock price of MTNOY even *increased* its value by 2.25% on the US stock market, moving from $16.9 (February 1, 2012) to $17.2 per share (February 17, 2012), in line with a general increase of the overall market at that time (NASDAQ 100: +6.27%). Investigations by the South African authorities on the matter are reportedly still ongoing so the company is considered innocent until proven guilty. However, on February 15, 2019 the South African police arrested the former South African ambassador in Iran for allegedly facilitating MTN bribes to Iranian government officials, thus lending credibility to allegations[42].

2.7.2 Large-N evidence

In this section I provide additional large-N evidence to support my mechanism. I leverage information in my data and show that foreign companies with US investment are more likely to *willingly* offer cooperation and provide information to US authorities, as in the Millicom case presented above. My mechanism suggests that US authorities find it easier to investigate foreign companies with a US investment because these companies are more likely to offer cooperation and provide information.

For each event of corruption in my dataset, I code how relevant information was provided to authorities. In particular, I code whether each company is offering cooperation voluntarily to US

[40]See text of the law suit: https://www.politicsweb.co.za/news-and-analysis/iran-turkcells-case-against-mtn.

[41]See: https://www.financeuncovered.org/stories/mtns-8bn-illegal-foreign-exchange-scandal.

[42]See: https://www.reuters.com/article/us-mtn-iran-arrests-idUSKCN1Q41PK.

authorities. To this aim, I consider as wilful disclosure of information only cases when a company is providing information through any of voluntary disclosure, cooperation, or internal investigation[43]. I then aggregate this information by company, as I do for my main firm-level cross-sectional dataset, and measure whether each of the 402 non-US companies has *ever* offered cooperation to US authorities.

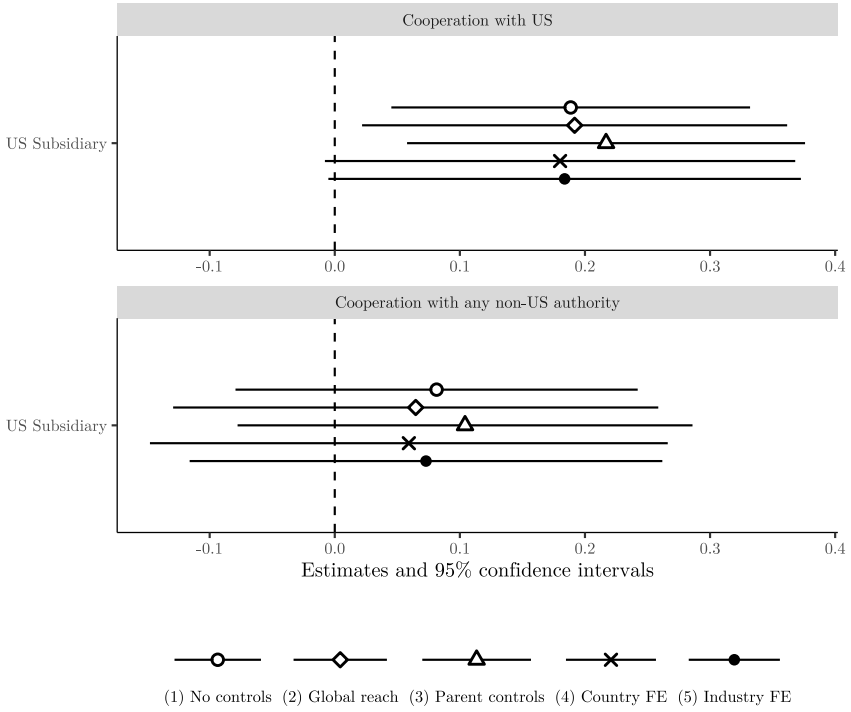


Figure 2.4: Change in probability to offer cooperation to US authorities and to authorities from any other country. Linear probability models

Finally, I explain this new dependent variable in the same linear probability models adopted in Table 2.2. Results for the main variable of interest are reported in the top panel of Figure 2.4 (full disclosure in Appendix). My models confirm expectations from the mechanism. Suspected foreign companies with a US investment increase their likelihood to offer cooperation to US agencies by about 0.18 – 0.22. This result is not statistically significant at a 0.05 conventional level only in models 4 and 5, where estimates fall just short of this threshold (p-values 0.060 and 0.056 respectively).

[43]Non-cooperative channels information is provided to US agencies include their own investigations, foreign courts' assistance, whistleblowers, and media or investigative reports.

Skeptical readers might be concerned that companies that have invested on US soil have unobservable characteristics that make them more likely to offer cooperation to judicial authorities in general. Perhaps only companies of a more compliant nature decide to embark in a foreign investment on US soil, knowing they would be exposed to the reach of this "global sheriff". If so, cooperation would have nothing to do with the goal to improve their reputations in the eyes of the US public, following a scandal. Rather, it would be a byproduct of their unobservable idiosyncratic nature.

This skepticism does not find support in my data. If the argument were correct, companies with a US investment should be more likely to offer cooperation also to regulatory agencies from other countries, when they are involved in corrupt scandals. In fact, I find no evidence of this implication. The lower panel of Figure 2.4 shows results of a placebo test where I re-estimated my LPM models explaining whether the company has ever offered cooperation (defined as above) to the authorities *of any other country* (excluding the US) after having been involved in corruption scandals. Companies with a US investment are no more likely than their counterparts to offer cooperation to non-US authorities. Point estimates are much smaller than in the top panel, ranging from a minimum of 0.06 to a maximum of 0.10. Moreover, estimates are never statistically significant, with p-values that range from 0.26 to 0.57. Full disclosure of results is in Appendix.

2.8 Conclusion

Globalization creates complex governance tasks for states. Global problems spill over across borders and challenge states' territorially-bound regulation. States solve these problems by establishing international regimes (Keohane, 1984). Global crime, however, challenges state authority because criminals can fragment activities across borders and evade laws. A few countries overcome this challenge by enforcing regimes *extraterritorially*: they rule on foreign subjects as if they were domestic. In the context of regulating multinational companies, states use extraterritoriality to oversee global supply chains and prohibit nefarious transactions. States that apply these policies

vigorously effectively behave as "global sheriffs" of international regimes. For instance, the US prosecutes foreign companies in violation of domestic corporate criminal laws and levies fines in the order of billions of dollars. Nonetheless, US authorities only prosecute a fraction of potential cases they have jurisdiction on. Determinants of such selection of foreign companies are still understudied in the international political economy scholarship.

In this paper I filled this gap by studying investigation of foreign companies suspected of illicit transactions. I focused on US investigation. I argued that US federal agencies are more likely to investigate a foreign company if that firm is exposed to the US economy through a foreign investment in the country. A similar type of exposure increases the reputational damage that the company suffers for allegations of crime committed abroad. Authorities have bureaucratic and professional incentives to leverage such reputational liabilities and more easily win a case. Potential losses induced by staying under the spotlight of the local public opinion induce the company to settle charges out-of-court. The company thus cooperates with prosecutors and shares information they need to investigate a case. An investment in the US thus makes these companies an easier target for federal agencies in comparison to similar foreign companies with no physical presence in the country.

I tested my argument focusing on the case of the anti-bribery regime. I collected a novel dataset on accusations of bribery against multinational companies by web-scraping text documents from an archive of bribery events. I linked this data with information on the subsidiary structure of non-US companies alleged to have paid bribes in international business transactions. Linear models showed that the probability that US agencies will investigate a non-US company suspect of bribery increases by at least 0.28 when the firm has a subsidiary in the US.

These findings are puzzling and open up to further questions for the literature on international regimes. The US sponsored the creation of the international anti-bribery regime in the 1990s. It took American administrations 20 years to win resistances among western partners and secure an OECD agreement on the matter. Before this, the White House had failed to secure anti-corruption treaties at the United Nations and International Chamber of Commerce (Abbott and Snidal, 2002).

The US had to leverage its hegemonic position and the emergence of corruption scandals among western partners in the 1990s in order to pressure them into adopting anti-corruption laws similar to the FCPA (Brewster, 2017). Legal scholars have observed that the establishment of this regime gave American prosecutors the possibility to apply their anti-corruption law against foreign companies (Brewster, 2014) and extend their legal arms beyond borders (Kaczmarek and Newman, 2011). Yet, I observe that territoriality still constrains to a great extent the *de facto* capacity of US prosecutors to investigate foreign companies.

Why has US extraterritorial enforcement of this international regime been established on paper, but found significant constraints in practice? Previous studies point to political motives that shape American regulatory action (Tomashevskiy, 2021). I point to a different answer: a distance exists between an administration's political goals and the incentives of agents who are in charge of enforcing a regime. Future studies on agents' aims and determinants of day-to-day regulatory action could provide relevant insights for the literature on compliance with international regimes.

More general considerations can be drawn for the extraterritorial regulatory strategy. I show that this strategy needs some kind of territorial leverage in order to be exercised. Public authorities can exploit chokepoints created by multinational corporate ownership structures to apply their coercive powers against foreign subjects, consistently with a literature on "weaponized interdependence" (Farrell and Newman, 2022). However, even a powerful regulator like the US is less likely to investigate a foreign company in the absence of such territorial bargaining chip. Results offer a nuanced position between views claiming states' territorially-bound action is incapable of addressing transnational problems and accounts claiming to observe a renewed engagement of regulators in global governance.

Findings travel beyond the regulation of multinational companies, to include prevention of cyber crime, drug trafficking, and transnational terrorism. Moreover, they potentially travel to non-US regulatory powers that also attempt to stretch the arm of their laws beyond their borders, like the United Kingdom or Switzerland. Results have especially relevant implications for weaker regulators – *e.g.* Nigeria (see Crasnic et al., 2017) – that find it hard to leverage a likewise centrality

over transnational economic exchanges. Finally, my study also provides a new perspective on "weaponized interdependence" (Farrell and Newman, 2019), which claims that states can leverage their economic power to exercise their regulatory prerogatives. I show that this dynamic is not necessarily a byproduct of national interest, as this literature tends to assume. Rather, it can be the result of purely domestic incentives to bureaucrats in charge of exercising a state's coercive power.

Appendix 2.A Descriptive statistics

Table A.1 reports descriptive statistics of variables included in the analysis. *Investigation* is a binary indicator for whether a company is ever investigated by the US (DOJ or the SEC) in the TRACE Compendium or not. *Investigation (share)* measures the proportion of cases involving a company the US investigated from the TRACE Compendium. *Number of Prosecutors (mean)* measures the average number of countries investigating cases involving each company. All other variables come from the Orbis Corporate Ownership Database. *US Subsidiary* is a binary indicator for whether a company has a majority-owned subsidiary in the US. *US Subsidiary Employees (log+1)* reports the natural logarithm of the count of employees in the US + 1 for each company. *Global Subsidiaries* counts the number of majority-owned subsidiaries in the world for each company (expressed in thousands). *Global Branches* counts the number of branches in the world for each company (expressed in thousands). *Global Employees (log+1)* reports the natural logaithm of the count of employees in the world + 1 for each company. *Parent Revenues (mean)* reports the average revenues for each parent company as available from Orbis in hundreds of billions of dollars. *Parent Assets (mean)* reports the average value of assets to each parent company as available from Orbis in hundreds of billions of dollars. *Parent Employees (mean)* reports the average number of employees to each parent company in hundreds of billions.

	N	Mean	SD	Min	Median	Max
Investigation	402	0.38	0.49	0.00	0.00	1.00
Investigation (share)	402	0.35	0.46	0.00	0.00	1.00
US Subsidiary	402	0.12	0.33	0.00	0.00	1.00
US Subsidiary Employees (log+1)	402	1.06	2.90	0.00	0.00	11.36
Global Subsidiaries	402	0.01	0.03	0.00	0.00	0.21
Global Branches	402	0.02	0.16	0.00	0.00	2.16
Global Employees (log+1)	402	1.17	1.59	0.00	0.20	6.23
Number of Prosecutors (mean)	402	1.47	0.87	1.00	1.00	10.42
Parent Revenues (mean)	367	0.15	0.40	0.00	0.02	3.98
Parent Assets (mean)	349	0.71	2.80	0.00	0.04	26.12
Parent Employees (mean)	344	0.00	0.00	0.00	0.00	0.01

Table A.1: Bribe-payer non-US companies. Summary Statistics

Appendix 2.B Outlier exclusion

First, I re-estimate model 5 in Table 2.2 adopting a jackknife re-sampling approach. I drop one company from the sample and re-estimate the exact same model. I iterate the procedure 301 times so as to exclude all companies from the sample once. Figure B.1 shows estimated coefficients and 95% confidence levels. It also reports the main result from the full model (model 5) in Table 2.2 for comparison.

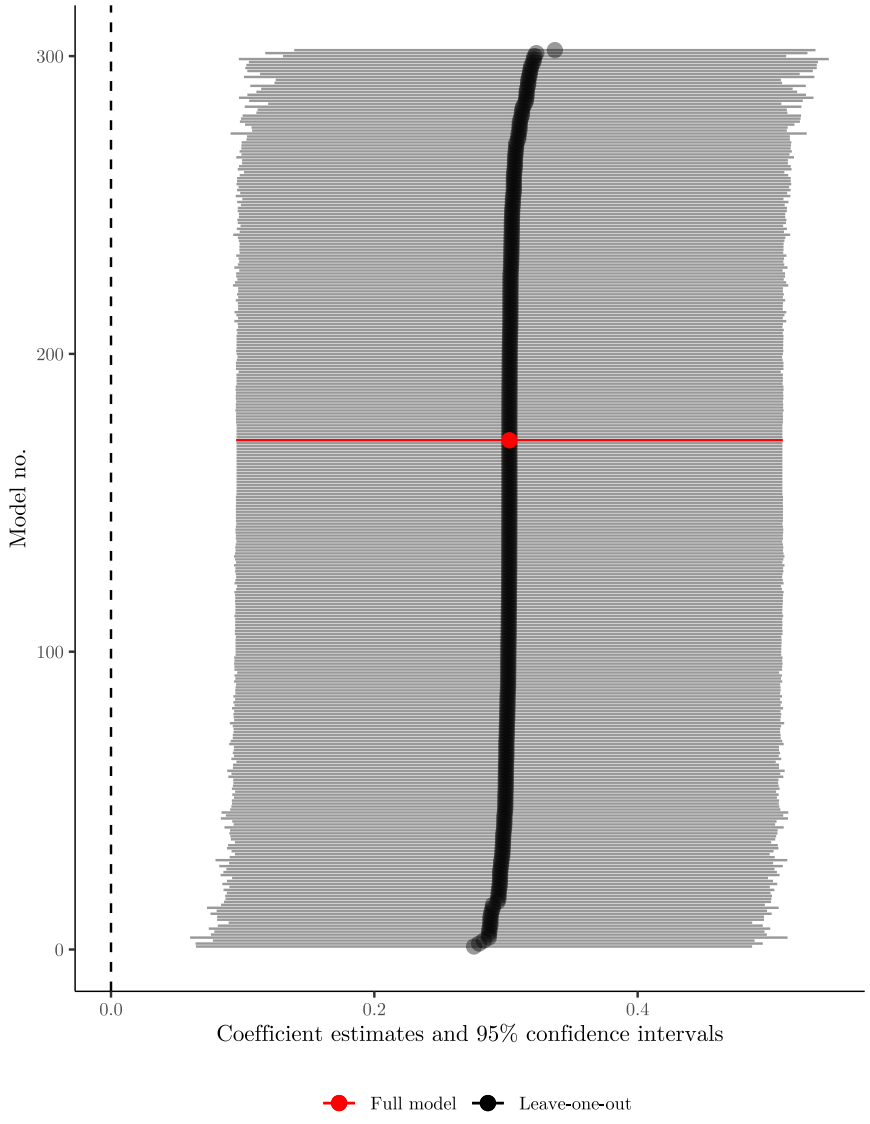


Figure B.1: Leave-one-out estimates of main effect from model 5, Table 2.2

Appendix 2.C Chinese companies

I re-estimate models from Table 2.2 excluding all Chinese companies from the sample. The goal of this exercise is to assess potential concerns that Chinese companies are driving the main results for reasons unrelated to my causal story. Chinese companies might be less likely to invest in the US for reasons that include bureaucratic constraints, unfamiliarity with the American business environment, and geopolitical concerns. At the same time, they might be less likely to be investigated by US agencies as a byproduct of the lack of formal cooperation between US and Chinese prosecutors (China is not part of the OECD Anti-Bribery Convention, hence no formal channel for cooperation is established between anti-bribery authorities of these countries). Table C.1 reports results obtained when excluding Chinese companies from the data. Estimates are consistent with previous results.

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.395***	0.319***	0.313**	0.264*	0.295**
	(0.077)	(0.090)	(0.101)	(0.107)	(0.105)
Global Subsidiaries		-1.517+	-1.621	-0.584	-0.371
		(0.819)	(1.196)	(1.641)	(1.502)
Global Branches		0.346**	0.225	0.182*	0.140
		(0.117)	(0.152)	(0.075)	(0.087)
Global Employees (log+1)		0.041+	0.032	0.026	0.054+
		(0.022)	(0.029)	(0.031)	(0.032)
Number of Prosecutors (mean)			0.172**	0.138+	0.133+
			(0.057)	(0.076)	(0.070)
Parent Revenues (mean)			0.201	0.187	0.101
			(0.141)	(0.182)	(0.231)
Parent Assets (mean)			0.011	0.017**	0.014+
			(0.007)	(0.006)	(0.008)
Parent Employees (mean)			-55.021**	-53.863	-72.243
			(19.857)	(35.657)	(83.968)
(Intercept)	0.339***	0.313***	0.069	-0.183+	-0.442*
	(0.043)	(0.050)	(0.106)	(0.100)	(0.188)
Num.Obs.	382	382	289	289	287
Country FE				Yes	Yes
Industry FE					Yes
R2	0.073	0.094	0.199	0.428	0.486
R2 Adj.	0.071	0.084	0.176	0.284	0.300
F	30.130	9.775	8.672	2.973	2.612

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

Table C.1: Linear probability models of Investigation. Binary main explanatory variable. Excluding Chinese firms

Appendix 2.D FCPA jurisdictional reach

A potential reason of concern with the main results comes from the fact that it might not be equally feasible for US agencies to claim jurisdiction on all companies in my sample. It might not make sense to compare the difference in likelihood of being investigated for foreign companies with and without a US investment, if not all these companies can easily fall under US prosecutors' jurisdiction. My strategy for addressing this problem relies on the fact that foreign companies that are listed on a US-based stock exchange are mandated to file periodic reports to the SEC, therefore they univocally fall under the FCPA jurisdiction. To be sure, even companies that are not listed on an American stock exchange must abide by FCPA terms, given its extraterritorial reach. Yet, US-traded companies represent a minimum set of companies that are univocally subject to FCPA terms. I therefore retrieve information on whether each foreign company in the data trades stocks on a US exchange. I code this variable as binary. Information comes from Orbis. This data source, yet, does not report companies that trade their stocks on US exchange markets as American Depositary Receipts as US-traded. I manually complete this information coding these companies as US-traded.

I address the potential endogeneity concern in two ways. First, I simply control for whether the company is US-listed and re-estimate all models in Table 2.2 including this additional variable. Results are reported in Table D.1. Results are consistent with previous estimates: they are positive and sizeable. They inform that the probability of being investigated is estimated to increase by a factor between 0.19 and 0.26 for companies with a US investment, even when controlling for whether the company trades stocks on an American exchange. Estimates are all distinguishable from zero at a 0.05 conventional level of significance.

This approach, yet, might not be sufficient to address the potential concern above. Skeptical readers might believe that companies that do not fall univocally under FCPA terms should be excluded from the sample altogether. I therefore *condition* my analysis on whether each company is US-traded and restrict my sample to the 145 companies that satisfy this condition. I re-estimate all models from Table 2.2 in this smaller subset of companies. Results are reported in table D.2.

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.194*	0.246**	0.256**	0.221*	0.248*
	(0.082)	(0.081)	(0.095)	(0.103)	(0.099)
US-Traded	0.329***	0.360***	0.326***	0.301***	0.290**
	(0.056)	(0.060)	(0.078)	(0.081)	(0.090)
Global Subsidiaries		-1.241+	-1.084	-0.518	-0.515
		(0.711)	(0.872)	(1.356)	(1.179)
Global Branches		0.279***	0.142	0.149+	0.127
		(0.077)	(0.110)	(0.080)	(0.093)
Global Employees (log+1)		-0.018	-0.026	-0.022	0.001
		(0.020)	(0.029)	(0.031)	(0.038)
Number of Prosecutors (mean)			0.176**	0.143*	0.139*
			(0.054)	(0.067)	(0.065)
Parent Revenues (mean)			0.150+	0.187+	0.178
			(0.077)	(0.109)	(0.177)
Parent Assets (mean)			0.013+	0.016**	0.010
			(0.007)	(0.006)	(0.008)
Parent Employees (mean)			-44.646*	-70.030*	-89.710
			(21.438)	(32.514)	(76.313)
(Intercept)	0.238***	0.250***	0.010	-0.063	-0.221
	(0.046)	(0.049)	(0.091)	(0.101)	(0.213)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
R2	0.160	0.176	0.271	0.476	0.518
R2 Adj.	0.156	0.165	0.248	0.346	0.349
F	37.979	16.893	12.077	3.657	3.063

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

Table D.1: Linear probability models of Investigation. Binary main explanatory variable. Control for US-listing

Overall, the picture is consistent with the one presented in the main text. The estimated effect is always positive and sizeable, informing that the probability of investigation increases by a factor between 0.19 and 0.25 when the company has an investment on US soil, even in this smaller subsample of US-traded companies. Estimates, yet, are statistically distinguishable from zero at a 0.05 conventional level of significance only in models 1, 2, and 3. Overall, I deem these results consistent with previous findings and I attribute the lack of statistical significance in models 4 and 5 to the underpowered sample that sustains this analysis.

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.186*	0.211*	0.248*	0.216+	0.188
	(0.085)	(0.090)	(0.096)	(0.118)	(0.123)
Global Subsidiaries		-1.746+	-0.825	-0.174	-0.121
		(1.002)	(1.818)	(2.085)	(2.476)
Global Branches		0.251***	0.120	0.037	0.073
		(0.062)	(0.122)	(0.096)	(0.141)
Global Employees (log+1)		0.004	-0.010	-0.006	0.034
		(0.033)	(0.044)	(0.046)	(0.056)
Number of Prosecutors (mean)			0.261***	0.303***	0.237**
			(0.052)	(0.060)	(0.085)
Parent Revenues (mean)			0.147	0.112	0.116
			(0.136)	(0.198)	(0.233)
Parent Assets (mean)			0.014+	0.021**	0.012
			(0.008)	(0.008)	(0.022)
Parent Employees (mean)			-80.528*	-89.476*	-160.936+
			(32.561)	(41.465)	(91.263)
(Intercept)	0.570***	0.586***	0.193	-0.349**	-0.182
	(0.054)	(0.083)	(0.139)	(0.120)	(0.172)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	145	145	125	125	124
R2	0.032	0.063	0.203	0.448	0.528
R2 Adj.	0.025	0.036	0.148	0.213	0.194
F	4.656	2.345	3.700	1.909	1.582

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

Table D.2: Linear probability models of Investigation. Binary main explanatory variable. Only US-traded companies

Appendix 2.E ATT estimation

One further potential reason of concern with the analysis comes from the different size of the treatment and control groups in my data. The dataset includes only 49 companies with at least one US subsidiary, whereas there are 353 firms with no US subsidiary (Table 2.1). OLS provide a variance-weighted estimate of the average treatment effect by using observable covariates to impute an untreated counterfactual for each treated unit *and* a treated counterfactual for each untreated unit (Aronow and Samii, 2016). In my case this latter endeavor might ask a lot to the data, as OLS try to use information on 49 treated companies to impute counterfactuals for 353 untreated units.

To tackle this concern, in my first robustness test I estimate a much narrower causal quantity: an average treatment effect on treated (ATT) units. This simply means using observable information on 353 untreated companies to impute counterfactuals for 49 treated firms. I estimate the ATT in two ways. First, I re-estimate models from Table 2.2 applying a regression imputation estimator as proposed by Lin (2013). The estimator is obtained by simply interacting the binary treatment variable with re-centred versions of the (numeric) covariates. The model removes the variance-weighting scheme OLS performs. Then, computing the marginal effect from this model with respect to treated units is a straightforward way to retrieve an estimate of the ATT.

Results obtained when applying this procedure are reported in Table E.1. Estimates of the ATT are consistent with the argument advanced. Models estimate that the probability of being investigated by the US increases of about 0.23 – 0.40 for the 49 treated companies with respect to imputed units obtained from comparable untreated firms under observable features. Estimates are all distinguishable from zero at a 0.05 conventional level of significance.

Second, I re-estimate the same quantity adopting a matching procedure. I matched the 49 treated companies to 49 untreated companies. Matches were found using Mahalanobis distance according to the same covariates included in the full specification of model 2.2. I also performed bias adjustment for these very covariates as they are all numeric. I matched each treated observation to one untreated observation only, since increasing the number of matches even by one unit resulted

	(1)	(2)	(3)	(4)	(5)
ATT	0.403***	0.336***	0.323***	0.231*	0.254*
	(0.076)	(0.094)	(0.094)	(0.106)	(0.11)
Firm spread		Yes	Yes	Yes	Yes
Furn controls			Yes	Yes	Yes
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
R2	0.074	0.098	0.217	0.453	0.507
R2 Adj.	0.072	0.082	0.176	0.300	0.315
F	31.886	6.094	5.294	2.962	2.641

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

Table E.1: Linear probability models of Investigation. Regression imputation estimator

in an unbalanced match. Figure E.1 reports the estimate of the ATT and the achieved balance in covariates. The estimate is consistent with the one presented earlier. The probability of being investigated by US authorities increases by about .23 for the 49 companies with investment in the US. Estimate is distinguishable from zero at a 0.05 conventional level of significance.

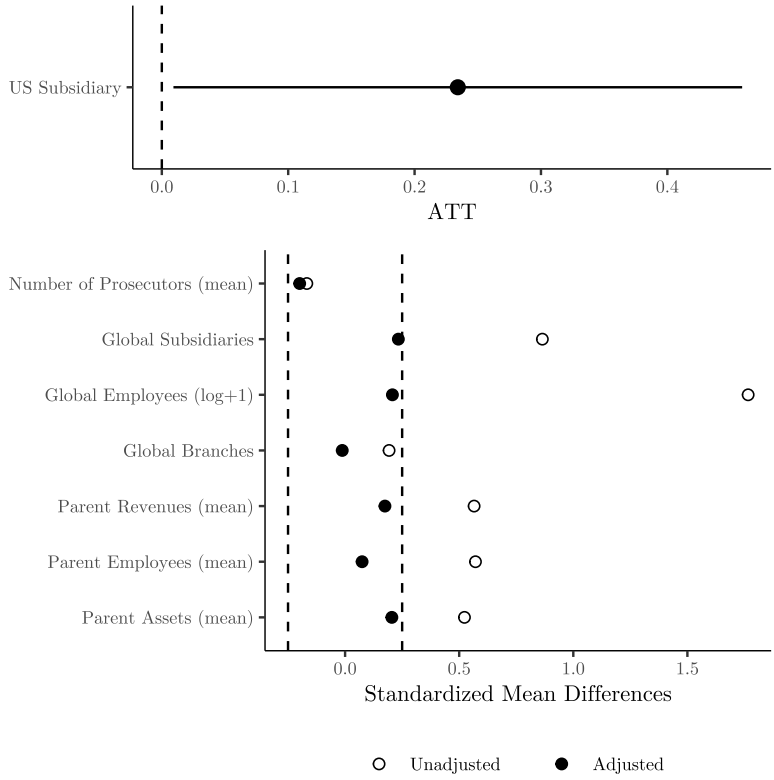


Figure E.1: Mahalanobis matching. ATT estimate and balance in covariates

Appendix 2.F Model dependence

Next, I tackle the concern that OLS do not correctly model the distribution of my binary dependent variable. A LPM provides coefficients that represent differences in conditional means of the dependent variable, which are quantities of substantive interest. Yet, a binary dependent variable is more appropriately modelled using a logit model, where predicted values are constrained to range only between the values of 0 and 1. Table F.1 re-proposes models estimated above using this alternative functional form. Estimates are robust. Predicted probabilities inform us of very similar quantities as the ones documented earlier. In model 1, non-US companies with a $US\ Subsidiary$ have a 0.73 probability of being investigated when they are involved in events of bribery. This is a substantive increase with respect to their counterparts with no presence in the US (predicted probability for this group: 0.33). Effect is very similar in the full specification of model 5 (increase in predicted probability from 0.40 to 0.84 when holding all numeric covariates to their means and fixed effects to the mode).

The inclusion of home country and industry fixed-effects removes heterogeneity at this level, but only allows to study variation within each category of these variables. Random effects provide a more flexible way of modelling multilevel heterogeneity (Bell and Jones, 2015). I re-estimate models 4 and 5 from Table 2.2 using random intercepts at the home country and industry-level in a multilevel linear and multilevel logit model. Results are robust to these model choices (Table F.2).

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	1.720***	1.674***	1.732**	1.854**	2.066***
	(0.379)	(0.468)	(0.574)	(0.626)	(0.626)
Global Subsidiaries		-7.229	-3.525	-3.149	-6.172
		(5.563)	(5.314)	(7.892)	(6.721)
Global Branches		12.441**	10.681**	16.648*	18.168*
		(4.657)	(4.136)	(7.514)	(7.354)
Global Employees (log+1)		0.084	0.038	0.037	0.261
		(0.115)	(0.155)	(0.209)	(0.236)
Number of Prosecutors (mean)			1.276***	1.599***	1.697***
			(0.305)	(0.319)	(0.359)
Parent Revenues (mean)			0.511	0.666	0.315
			(0.660)	(1.100)	(1.183)
Parent Assets (mean)			0.165+	0.302+	0.253
			(0.092)	(0.163)	(0.227)
Parent Employees (mean)			-383.174	-486.033	-607.304+
			(238.787)	(302.628)	(330.583)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
AIC	509.1	497.7	345.5	346.7	356.1
BIC	517.1	517.7	378.9	569.5	645.3
Log.Lik.	-252.573	-243.863	-163.761	-113.341	-100.058

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

Table F.1: Logit models of Investigation. Binary main explanatory variable

	Multilevel linear		Multilevel logit	
	(1)	(2)	(3)	(4)
US Subsidiary	0.302***	0.315***	1.784***	1.866***
	(0.081)	(0.082)	(0.524)	(0.521)
Global Subsidiaries	-0.259	-0.263	-2.819	-2.612
	(1.198)	(1.183)	(8.525)	(8.500)
Global Branches	0.220	0.205	13.988*	14.447*
	(0.154)	(0.154)	(6.933)	(6.962)
Global Employees (log+1)	0.020	0.030	0.059	0.106
	(0.026)	(0.026)	(0.168)	(0.164)
Number of Prosecutors (mean)	0.153***	0.155***	1.457***	1.468***
	(0.030)	(0.030)	(0.283)	(0.277)
Parent Revenues (mean)	0.186*	0.161+	0.722	0.614
	(0.086)	(0.087)	(0.893)	(0.792)
Parent Assets (mean)	0.014	0.013	0.213	0.206
	(0.010)	(0.011)	(0.139)	(0.143)
Parent Employees (mean)	-69.876+	-71.815+	-449.543	-499.539
	(39.098)	(39.133)	(511.414)	(397.978)
(Intercept)	0.346***	0.340***	-0.647*	-0.689*
	(0.042)	(0.048)	(0.304)	(0.310)
Country RE	Yes	Yes	Yes	Yes
Industry RE		Yes		Yes
Num.Obs.	303	301	303	301
AIC	385.8	382.1	329.7	327.8
BIC	426.6	426.6	366.8	368.6

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

Table F.2: Multilevel models of Investigation. Binary main explanatory variable

Appendix 2.G Alternative operationalizations

Next, I propose alternative operationalizations of my key variables. First, I measure US investigation as the share of cases investigated by the US for each company in the dataset (*Investigation Share*). I replicate the same model choices as in Table 2.2 in a linear model estimated using OLS. Results inform us that US authorities on average investigated between 0.21 and 0.30 more of the total cases involving companies with a subsidiary in the country (Table G.1).

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.304***	0.279***	0.276**	0.210*	0.228**
	(0.067)	(0.082)	(0.091)	(0.088)	(0.073)
Global Subsidiaries		-1.300	-0.803	0.050	0.087
		(0.896)	(0.896)	(0.964)	(0.812)
Global Branches		0.386***	0.219	0.226*	0.205*
		(0.106)	(0.167)	(0.103)	(0.090)
Global Employees (log+1)		0.018	0.003	0.001	0.021
		(0.024)	(0.028)	(0.026)	(0.028)
Number of Prosecutors (mean)			0.152*	0.111	0.109
			(0.067)	(0.083)	(0.072)
Parent Revenues (mean)			0.065	0.098	0.044
			(0.061)	(0.068)	(0.123)
Parent Assets (mean)			0.016*	0.021***	0.019*
			(0.008)	(0.006)	(0.007)
Parent Employees (mean)			-34.920*	-42.674+	-54.080
			(15.753)	(25.806)	(67.112)
(Intercept)	0.312***	0.301***	0.099	-0.108	-0.489**
	(0.040)	(0.044)	(0.112)	(0.093)	(0.156)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
R2	0.048	0.068	0.158	0.431	0.495
R2 Adj.	0.045	0.059	0.135	0.293	0.320
F	19.949	7.229	6.897	3.117	2.834

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

Table G.1: Linear models of Investigation Share. Binary main explanatory variable

Finally, I use an alternative explanatory variable measuring the presence of a company in the US economy. I measure the number of employees of each company through US subsidiaries and take the logarithm of the measure +1. I replicate my model specifications of *Investigation* using this alternative explanatory variable and present results obtained from a LPM (Table G.2). Estimates

inform us that a one unit increase in the logarithm of the number of US employees (hence, about a +1% in the number of US employees) increases the probability of investigation by the US by about 3% to 5%. Estimates are distinguishable from zero at a 0.05 conventional level of significance. These latter estimates are robust when using a logit model (Table G.3), random-effect models (Table G.4), and the continuous version of the dependent variable (Table G.5).

	(1)	(2)	(3)	(4)	(5)
US Subsidiary Employees (log+1)	0.045***	0.038***	0.039***	0.031*	0.033**
	(0.008)	(0.010)	(0.012)	(0.012)	(0.012)
Global Subsidiaries		-1.025	-0.803	-0.067	-0.111
		(0.852)	(0.979)	(1.164)	(1.061)
Global Branches		0.350**	0.213	0.183*	0.145
		(0.128)	(0.160)	(0.085)	(0.093)
Global Employees (log+1)		0.027	0.016	0.015	0.038
		(0.024)	(0.030)	(0.031)	(0.034)
Number of Prosecutors (mean)			0.178**	0.138+	0.134+
			(0.058)	(0.076)	(0.069)
Parent Revenues (mean)			0.170*	0.211*	0.157
			(0.071)	(0.091)	(0.141)
Parent Assets (mean)			0.011	0.017**	0.014
			(0.008)	(0.006)	(0.009)
Parent Employees (mean)			-62.509***	-75.048*	-86.584
			(17.944)	(35.343)	(72.126)
(Intercept)	0.333***	0.313***	0.064	-0.159	-0.399*
	(0.041)	(0.048)	(0.105)	(0.099)	(0.167)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
R2	0.071	0.088	0.201	0.430	0.482
R2 Adj.	0.069	0.079	0.179	0.292	0.303
F	30.671	9.551	9.240	3.108	2.694

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

Table G.2: Linear probability models of Investigation. Continuous main explanatory variable

	(1)	(2)	(3)	(4)	(5)
US Subsidiary Employees (log+1)	0.193***	0.187***	0.197**	0.210**	0.226**
	(0.043)	(0.054)	(0.068)	(0.076)	(0.078)
Global Subsidiaries		-6.995	-3.829	-3.504	-7.318
		(5.611)	(5.463)	(7.839)	(7.008)
Global Branches		12.281**	10.453*	15.899*	17.133*
		(4.654)	(4.153)	(7.398)	(7.045)
Global Employees (log+1)		0.081	0.021	0.021	0.245
		(0.119)	(0.160)	(0.211)	(0.241)
Number of Prosecutors (mean)			1.284***	1.600***	1.701***
			(0.308)	(0.322)	(0.361)
Parent Revenues (mean)			0.682	0.859	0.566
			(0.644)	(1.042)	(1.087)
Parent Assets (mean)			0.158+	0.281+	0.230
			(0.093)	(0.170)	(0.220)
Parent Employees (mean)			-392.386	-485.662	-572.278
			(245.955)	(313.431)	(364.229)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
AIC	510.1	499.0	346.2	347.9	357.8
BIC	518.0	518.9	379.6	570.7	646.9
Log.Lik.	-253.027	-244.477	-164.098	-113.932	-100.879

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

Table G.3: Logit models of Investigation. Continuous main explanatory variable

	Multilevel linear		Multilevel logit	
	(1)	(2)	(3)	(4)
US Subsidiary Employees (log+1)	0.034***	0.035***	0.201**	0.209***
	(0.010)	(0.010)	(0.061)	(0.061)
Global Subsidiaries	-0.297	-0.307	-3.065	-2.927
	(1.201)	(1.187)	(8.337)	(8.433)
Global Branches	0.218	0.204	13.392+	13.759*
	(0.155)	(0.154)	(6.871)	(6.945)
Global Employees (log+1)	0.018	0.028	0.042	0.087
	(0.026)	(0.026)	(0.167)	(0.167)
Number of Prosecutors (mean)	0.154***	0.156***	1.457***	1.468***
	(0.030)	(0.030)	(0.282)	(0.276)
Parent Revenues (mean)	0.203*	0.179*	0.873	0.824
	(0.086)	(0.087)	(0.726)	(0.781)
Parent Assets (mean)	0.013	0.012	0.203	0.197
	(0.010)	(0.011)	(0.137)	(0.141)
Parent Employees (mean)	-70.817+	-72.968+	-448.700	-512.540
	(39.198)	(39.239)	(315.907)	(394.951)
(Intercept)	0.347***	0.341***	-0.641*	-0.683*
	(0.042)	(0.048)	(0.300)	(0.306)
Country RE	Yes	Yes	Yes	Yes
Industry RE		Yes		Yes
Num.Obs.	303	301	303	301
AIC	391.4	387.9	330.7	328.9
BIC	432.2	432.3	367.8	369.7

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

Table G.4: Multilevel models of Investigation. Continuous main explanatory variable

	(1)	(2)	(3)	(4)	(5)
US Subsidiary Employees (log+1)	0.034***	0.031**	0.031**	0.022*	0.024**
	(0.007)	(0.009)	(0.011)	(0.010)	(0.009)
Global Subsidiaries		-1.271	-0.827	0.016	0.040
		(0.878)	(0.915)	(0.975)	(0.840)
Global Branches		0.379***	0.218	0.223*	0.202*
		(0.109)	(0.167)	(0.104)	(0.093)
Global Employees (log+1)		0.017	0.001	0.001	0.021
		(0.024)	(0.029)	(0.027)	(0.028)
Number of Prosecutors (mean)			0.152*	0.112	0.109
			(0.067)	(0.083)	(0.072)
Parent Revenues (mean)			0.081	0.110	0.056
			(0.064)	(0.068)	(0.120)
Parent Assets (mean)			0.016+	0.020***	0.019*
			(0.008)	(0.006)	(0.008)
Parent Employees (mean)			-35.737*	-43.738	-55.448
			(16.148)	(26.951)	(69.145)
(Intercept)	0.313***	0.303***	0.101	-0.108	-0.483**
	(0.040)	(0.044)	(0.112)	(0.092)	(0.157)
Country FE				Yes	Yes
Industry FE					Yes
Num.Obs.	402	402	303	303	301
R2	0.046	0.066	0.156	0.428	0.491
R2 Adj.	0.044	0.056	0.133	0.289	0.315
F	19.322	6.973	6.804	3.076	2.789

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

Table G.5: Linear models of Investigation Share. Continuous main explanatory variable

Appendix 2.H Event-disaggregated analysis

As a final robustness check, I assess if results hold when including additional control variables that do not fit the company-level structure of the data used in the main analysis. Whether the home country investigated a criminal offence might be a potential confounder, because US agencies might be less likely to investigate a company when the home country is already taking care of the matter. Moreover, there might be unobservable heterogeneity in the likelihood of investigation that pertains to the level of the country where bribes were paid (host country), rather than to the home-country level. Perhaps US prosecutors' likelihood of investigating a company depends on unobservable idiosyncrasies at this level.

These potential issues require to move the analysis to a dataset where the statistical unit is not the company i, but the case c. I therefore disaggregate my company-level data on 402 firms into a new dataset of 1,218 observations where each row represents a company i involved in a corruption case c in a host country j. I then re-estimate my models from Table 2.2 in a multilevel framework, to account for the complex hierarchy in this new dataset. I include cross-classified random intercepts at the home, industry, host, and firm level to model the complexity of this disaggregated data source. Table H.1 presents the results. I first introduce only the main treatment variable with random intercepts at the home country-level. Next I introduce all controls from the main analysis

Finally, I test robustness of these last estimates in a fixed-effect framework. I substitute random intercepts with fixed effects. Table H.2 presents the results introducing control variables and fixed effects with the same logic as before. However, as each unit-fixed effect removes completely between-unit variation, I cannot include all fixed effects simultaneously. I therefore first introduce home country and industry-fixed effect, as in the main text analysis. Next, I substitute home country with host-country fixed-effect. Results are robust to this choice.

	(1)	(2)	(3)	(4)	(5)	(6)
US Subsidiary	0.143***	0.140**	0.157***	0.138**	0.139**	0.191***
	(0.033)	(0.043)	(0.046)	(0.044)	(0.044)	(0.058)
Global Subsidiaries		-1.799**	-1.878**	-1.645**	-1.606**	-1.061
		(0.593)	(0.590)	(0.567)	(0.566)	(0.811)
Global Branches		0.243*	0.242*	0.237*	0.232*	0.178
		(0.122)	(0.122)	(0.118)	(0.117)	(0.132)
Number of Prosecutors		-0.019*	-0.014	-0.007	-0.006	0.011
		(0.009)	(0.009)	(0.009)	(0.009)	(0.011)
Global Employees (log+1)		0.013	0.023	0.018	0.018	0.014
		(0.014)	(0.014)	(0.014)	(0.014)	(0.019)
Parent Revenues (mean)		0.187***	0.160**	0.146**	0.141**	0.090
		(0.051)	(0.052)	(0.050)	(0.050)	(0.064)
Parent Assets (mean)		0.011+	0.009	0.008	0.007	0.012
		(0.006)	(0.007)	(0.006)	(0.006)	(0.008)
Parent Employees (mean)		-44.312+	-44.272	-40.166	-43.152+	-49.797
		(26.878)	(27.163)	(26.002)	(25.828)	(31.042)
Home Enforced				-0.295***	-0.300***	-0.254***
				(0.032)	(0.032)	(0.032)
(Intercept)	0.271***	0.290***	0.273***	0.349***	0.341***	0.290***
	(0.029)	(0.041)	(0.050)	(0.047)	(0.047)	(0.045)
Home Country RE	Yes	Yes	Yes	Yes	Yes	Yes
Industry RE			Yes	Yes	Yes	Yes
Host Country RE					Yes	Yes
Firm RE						Yes
Num.Obs.	1236	1072	1067	1067	1067	1067
AIC	1591.7	1377.3	1363.4	1290.3	1283.6	1239.9
BIC	1612.2	1432.0	1423.0	1354.9	1353.2	1314.5

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

Table H.1: Investigations event datasets. Random effects models

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.200***	0.134*	0.113*	0.166**	0.190**
	(0.058)	(0.064)	(0.054)	(0.063)	(0.069)
Global Subsidiaries		-2.141*	-2.145**	-0.977	-1.110+
		(0.952)	(0.801)	(0.613)	(0.592)
Global Branches		0.235	0.236	0.152	0.165
		(0.185)	(0.171)	(0.154)	(0.154)
Number of Prosecutors		-0.017*	-0.009	-0.020+	-0.009
		(0.008)	(0.008)	(0.010)	(0.012)
Global Employees (log+1)		0.036	0.033	0.000	0.018
		(0.025)	(0.023)	(0.021)	(0.019)
Parent Revenues (mean)		0.147	0.121	0.104	0.087
		(0.117)	(0.096)	(0.089)	(0.096)
Parent Assets (mean)		0.007	0.006	0.007	-0.003
		(0.010)	(0.010)	(0.008)	(0.010)
Parent Employees (mean)		-36.627	-17.288	-47.067	-46.589
		(40.043)	(35.905)	(30.904)	(30.315)
Home Enforced			-0.290***	-0.364***	-0.352***
			(0.049)	(0.057)	(0.051)
(Intercept)	0.295***	-0.425*	-0.279+	0.154+	-0.167*
	(0.032)	(0.200)	(0.160)	(0.079)	(0.071)
Home Country FE		Yes	Yes		
Industry FE		Yes	Yes		Yes
Host Country FE				Yes	Yes
Num.Obs.	1236	1067	1067	1072	1067
R2	0.034	0.228	0.282	0.284	0.314
R2 Adj.	0.033	0.168	0.226	0.173	0.193
F	43.706	3.790	4.982	2.570	2.593

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

Table H.2: Investigations event datasets. Fixed effects models

Appendix 2.I Cooperation with US authorities

Tables I.1 and I.2 report full disclosure of estimates presented in Figure 2.4.

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.189*	0.192*	0.217**	0.180+	0.184+
	(0.073)	(0.086)	(0.081)	(0.095)	(0.096)
Global Subsidiaries		-0.182	0.254	0.800	0.842
		(0.798)	(0.879)	(1.118)	(1.254)
Global Branches		-0.017	-0.021	0.017	-0.033
		(0.081)	(0.134)	(0.134)	(0.141)
Global Employees (log+1)		0.001	-0.009	-0.025	-0.012
		(0.017)	(0.024)	(0.030)	(0.031)
Number of Prosecutors (mean)			0.102**	0.119*	0.102*
			(0.039)	(0.046)	(0.046)
Parent Revenues (mean)			-0.090*	-0.071	-0.116
			(0.037)	(0.044)	(0.078)
Parent Assets (mean)			-0.002	-0.003	0.000
			(0.010)	(0.012)	(0.010)
Parent Employees (mean)			24.129	21.520	10.597
			(21.425)	(30.920)	(46.897)
(Intercept)	0.077***	0.077***	-0.048	-0.074	-0.313**
	(0.023)	(0.020)	(0.044)	(0.064)	(0.099)
Num.Obs.	401	401	302	302	300
Country FE				Yes	Yes
Industry FE					Yes
R2	0.042	0.043	0.091	0.242	0.305
R2 Adj.	0.040	0.033	0.066	0.057	0.064
F	17.705	4.422	3.645	1.307	1.264

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

Table I.1: Probability to offer cooperation to US authorities. Linear probability models

	(1)	(2)	(3)	(4)	(5)
US Subsidiary	0.081	0.065	0.104	0.059	0.073
	(0.082)	(0.099)	(0.092)	(0.105)	(0.096)
Global Subsidiaries		-0.290	-0.007	1.049	1.065
		(0.897)	(0.888)	(1.250)	(1.363)
Global Branches		-0.080**	-0.126+	-0.034	-0.107
		(0.028)	(0.071)	(0.117)	(0.110)
Global Employees (log+1)		0.012	-0.009	-0.020	-0.020
		(0.021)	(0.020)	(0.025)	(0.024)
Number of Prosecutors (mean)			0.133***	0.133**	0.129**
			(0.037)	(0.046)	(0.048)
Parent Revenues (mean)			-0.148**	-0.050	-0.075
			(0.050)	(0.061)	(0.087)
Parent Assets (mean)			0.002	0.000	0.001
			(0.010)	(0.013)	(0.011)
Parent Employees (mean)			73.267+	12.814	34.500
			(38.819)	(30.092)	(32.566)
(Intercept)	0.102**	0.096**	-0.077	-0.105*	-0.442**
	(0.036)	(0.033)	(0.048)	(0.052)	(0.157)
Num.Obs.	401	401	302	302	300
Country FE				Yes	Yes
Industry FE					Yes
R2	0.007	0.010	0.093	0.292	0.344
R2 Adj.	0.005	0.000	0.068	0.119	0.116
F	2.867	1.017	3.765	1.688	1.511

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

Table I.2: Probability to offer cooperation to non-US authorities (placebo). Linear probability models

Paper 3

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

Abstract

Research shows that, when news of corporate crime emerge, implicated companies' stock prices suffer. Investors thus perform a naming-and-shaming regulatory function against companies' misconduct. However, corporations conceal criminal transactions by fragmenting operations across subsidiaries and shell companies. It is unclear whether fragmented ownership conceals misconduct to investors too. I argue that markets' naming-and-shaming function is moderated by the ownership relation between the parent and the implicated entity. Investors penalize a parent company when it is directly responsible of crime. Instead, penalties decrease if the responsible company is a subsidiary. I test this argument studying corporate bribery. I leverage unexpected revelations of corporate corruption by 214 firms to estimate effects of scandals on stock prices of the parent company. I retrieve causal estimates by imputing synthetic counterfactual daily stock returns. When the parent is directly involved in a scandal, I calculate a median loss of about $132 million in capitalization on the day of a scandal. However, the effect is null when the company is involved via a subsidiary (whether wholly-owned or majority-owned). Findings indicate a regulatory failure. Fragmentation of corporate ownership can be used not only to conceal criminal transactions, but also to protect a firm's market reputation.

3.1 Introduction

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

How can states regulate such complex networks and prevent financial crime? Political science argues that states can find a helping-hand to hold large companies accountable for cross-border misconduct in the naming-and-shaming function performed by civil society actors (Acemoglu and Robinson, 2020; Fukuyama, 2016). Studies have extended this logic and argued that financial markets can complement states' regulatory action by pricing the reputation of a company (Alexander, 1999; Baron et al., 2014; Morse, 2022). News that state authorities are investigating alleged criminal behavior of a company should negatively affect its reputation and consequently its stock prices, thus turning into a sanction (Krüger, 2015). On the one hand markets would behave as a "global civil society" (Ruggie, 2018). On the other, regulators could *de facto* wage sanctions on companies by weaponizing investors' responses. This informal regulatory function would complement institutionalized regulation – potentially even substitute for it, see Kreitmeir et al. (2020) – and deter companies against crime (Alexander and Arlen, 2018; Morse, 2019).

However, the limits of the regulatory power markets leverage through this mechanism are still

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

Against this background, in this paper I ask: How do financial markets respond to the enforcement of IO-negotiated rules? Specifically, do corporate bribery scandals affect parent companies' stocks differently, when comparing events of corruption involving the parent company directly and those involving the parent company through a subsidiary? I theorize that, in fact, fragmentation of ownership insulates a company from damages generated by news of criminal behavior. The literature on penalties for corporate law claims the mechanism inducing penalties on financial markets is reputation-based (Alexander, 1999; Sampath et al., 2018). When a firm is publicly involved in an unexpected criminal event, investors who own its stocks are concerned prospects of future dividends might be damaged by negative publicity. They therefore decide to sell their equities. Increase in the supply of stocks is also met by a shrinkage in demand, as investors direct their purchases towards safer assets. The result is an abnormal drop in stock value.

I claim that this effect depends on where, along the corporate ownership chain, the scandal occurred. It is negative if a parent company is directly involved in a scandal. In this case, the company's reputation is directly at stake and investors restructure their portfolios accordingly. However, the effect intensity decreases if the company is involved in a scandal indirectly, *i.e.* through a subsidiary. In this case, negative publicity does not concern the parent company directly. Markets will therefore struggle to perform their regulatory function when corporate structures obscure ownership. That is, subsidiary companies screen corporate ownership and insulate parent companies from news of a scandal, thereby preventing meaningful financial losses in the wake of

breaking stories reporting criminal misconduct.

For the narrow purposes of this study, I define the reputation of a company as a "product of [its] social construction and validation" (Rao, 1994, 31) by financial market participants, which is performed solely in terms of profitability. This is in line with literatures across political science, business or management studies, and international corporate law that tend to study corporate reputation purely in terms of its consequences on stock markets (Breitinger and Bonardi, 2019). Considering investors as the sole assessors of a company's reputation is a choice that surely minimizes the complexity of firms' reputation. In fact, a much broader set of actors not necessarily concerned with financial returns assess a company's reputation. They include, at a minimum, stakeholders such as non-governmental organizations, customers, business partners, employees, government, and regulators (Büthe, 2010). However, a narrow definition serves the purpose of circumscribing the validity of an otherwise multifaceted concept. Theoretically, it allows to spell out the limit of the argument and its implications, which might not directly travel towards non-financial aspects of reputation. Empirically, it facilitates a measurement of such a complex concept.

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

I apply this design in the case of allegations for violations of US anti-bribery regulations. I construct a novel dataset reporting the day allegations that publicly-traded companies violated US anti-corruption regulations hit the market for the first time. I draw on the original web-scraped dataset on anti-bribery investigations used in the second paper to select companies alleged to violate US anti-corruption law. This yields information on 214 distinct companies involved in 264

corruption scandals. I also code the position of the responsible entity in its corporate group for each event in the dataset. Finally, I obtain daily stock prices data for the parent company in the days preceding and following the release of information.

I find that, when parent companies are directly involved in an anti-corruption investigation, they suffer a statistically significant negative effect on stock prices in the immediate aftermath information is made public. This effect amounts to a loss of about 0.30$ per share for the median company in my data, totalling about $132 million losses per day in terms of market capitalization. The effect size is remarkably similar to that of comparable negative news estimated by previous studies (see Kreitmeir et al., 2020). Even more than two weeks after the event, cumulative returns to companies involved directly in scandals remain about $517 million lower what could be expected had the event not occurred. That is, markets perform a regulatory function by imposing strong and sustained penalties that stick to a company's reputation when it is involved directly in a scandals. However, no statistically significant effect on the price of the parent company's equities is detected at all when a subsidiary is investigated for bribe payments. I further disentangle the null-effect relative to indirect involvement and find that no effect is detected for indirect involvement when considering only wholly-owned subsidiaries or only majority-owned ones.

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

The paper calls into question the extent to which market-based mechanisms can complement and potentially substitute for formal state action in important aspects of the regulation of private transnational actors satisfactorily. It thus speaks to a vast literature on relations between public authorities and privates in the construction of the international economic architecture (see work

as diverse as Johns et al., 2019; Morse, 2022; Ruggie, 2002; Strange, 1996). More broadly, results question whether negative information affects reputation to induce compliance of private actors with international regimes. International relations theory looks at reputation as a powerful device to ensure compliance with international regimes (Simmons, 1998, 2000; Weisiger and Yarhi-Milo, 2015). Since reputation is crucial for explaining private economic decisions too (Garriga, 2016), it is straightforward to expect markets' opinion of companies could also induce respect of international norms (Ruggie, 2018) when companies are directly responsible for compliance or defection (Baradaran et al., 2012; Jensen and Malesky, 2018). I show that this expectation might be disappointed. Investors' behaviors appear to be elastic to negative publicity, but definitely inelastic when involvement into bad news is successfully hidden inside a corporate group. In this case, corporate ownership works as a shield for the parent company's reputation.

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

3.2 Conceptual framework

3.2.1 Formal regulation of financial crime and informal penalties

Countries have laws in place to prohibit multinational companies under their jurisdiction from committing crime. These laws prosecute corporate criminal behavior such as corruption (Jensen and Malesky, 2018), money laundering (Sharman, 2010), financing of transnational terrorism

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

(Shelley, 2014), or trade with sanctioned countries (Andreas, 2005; Putnam, 2009).

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

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

Deterrence against financial misconduct is often achieved by informal means instead. Regulators *de facto* leverage negative shocks on companies' stock prices following the release of information about criminal investigations (Alexander and Arlen, 2018). The market price of a company's financial assets reflects the current assessment by investors on future profitability of the firm and it is updated when information is revealed unexpectedly (Fama, 1970). The reputation of a company is part of such complex price evaluations. Information that a company engaged in misconduct negatively updates investors' opinion of it. Investors re-structure their portfolios by over-selling shares of companies involved in adverse events, for fear that exposure to negative publicity undermines the value of future dividends. Large negative shocks on stock prices are

observed when information on poor environmental, social, and governance performance emerges (Capelle-Blancard and Petit, 2019; Krüger, 2015). For instance, Kreitmeir et al. (2020) estimate that companies in natural resource extraction suffer a loss of about 100 million US dollars following unexpected news of human right violations.

In the case of financial crime, it is argued, the reputational negative effect is compounded by material concerns. Stock-holders restructure their portfolios out of concerns about potentially poor future economic performances. Financial crime introduces rents and uncertainty that weaken prospects of profits (Ades and Di Tella, 1999; Lambsdorff, 2007). In extreme cases, corporate fraud is deliberately exercised at the expense of investors[2]. Moreover, news of criminal investigation can create expectations of fines and monetary settlement with authorities (Garrett, 2011). As a result of these pressures it is estimated that, out of every dollar lost by a company for a case of financial fraud, only 0.20$ come from penalties imposed by regulators. The remaining 0.80$ is due to consequences on involved companies' financial prices (Sampath et al., 2018).

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

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

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

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

(usually referred to as "parent") can own, directly or indirectly, shares of hundreds of subsidiaries. Degrees of ownership can also vary. A parent company can wholly-own a subsidiary, it can be its majoritarian owner (the company that owns the largest percentage of shares), or a minority shareholder. Mergers and acquisitions further complicate these networks. Finally, companies can structure their operations in ways that are more complex than traditional horizontal or vertical integrations, for instance creating joint ventures.

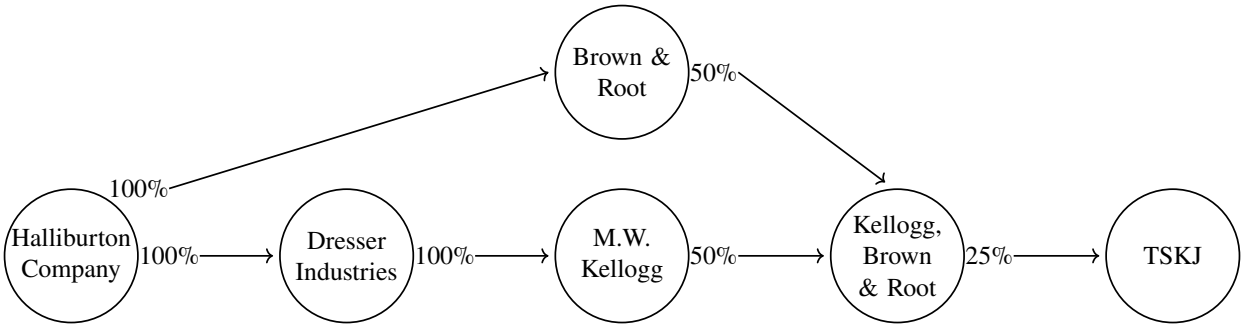


Figure 3.1: Example of corporate ownership structure: Halliburton Company's stakes in the TSKJ joint venture

Circles represent companies, arrows indicate ownership relations, and percentages represent degrees of ownership.

As a result, even for the simplest corporate structures it is often difficult to establish what operations belong to a given corporate group. Figure 3.1 offers a (rather simple) real example by reconstructing the stakes held by the US extractive company Halliburton in a consortium called TSKJ, a joint venture in the oil services industry registered in Madeira, Portugal. The company was formed by the French Technip S.A., Snamprogetti B.V. (of Italian origin but incorporated in the Netherlands), the American Kellogg Brown & Root (KBR), and the Japanese JGC Corporation. Each company owned 25% of TSKJ's shares. Halliburton held indirect control over the consortium ever since 1998, when it acquired Dresser Industries and formed KBR by joining its subsidiary Brown & Root with Dresser's subsidiary M.W. Kellogg. Similar fragmented structures are ideal for furthering illicit transactions. TSKJ became in fact (in-)famous for allegedly funnelling hundreds of million US dollars in bribes to Nigerian public officials between 1995 and 2004 in order to secure contracts for extracting and refining liquified gas on Bonny Island, in the Niger Delta region (Lacey, 2006).

I claim that the opacity of these corporate structures is not only ideal to conceal criminal behaviors to the eyes of public prosecutors. It also makes it difficult for investors to assess which operations belong to a given corporate group. Fragmentation separates the reputation of a parent company from that of its subsidiaries. As a result, a company can use complex corporate structures to shield its public image from that of entities within an ownership chain in case of a corporate scandal. Fragmented corporate ownership can thus be purposed to shield a company from informal penalties on financial markets, when misconduct is revealed to the public.

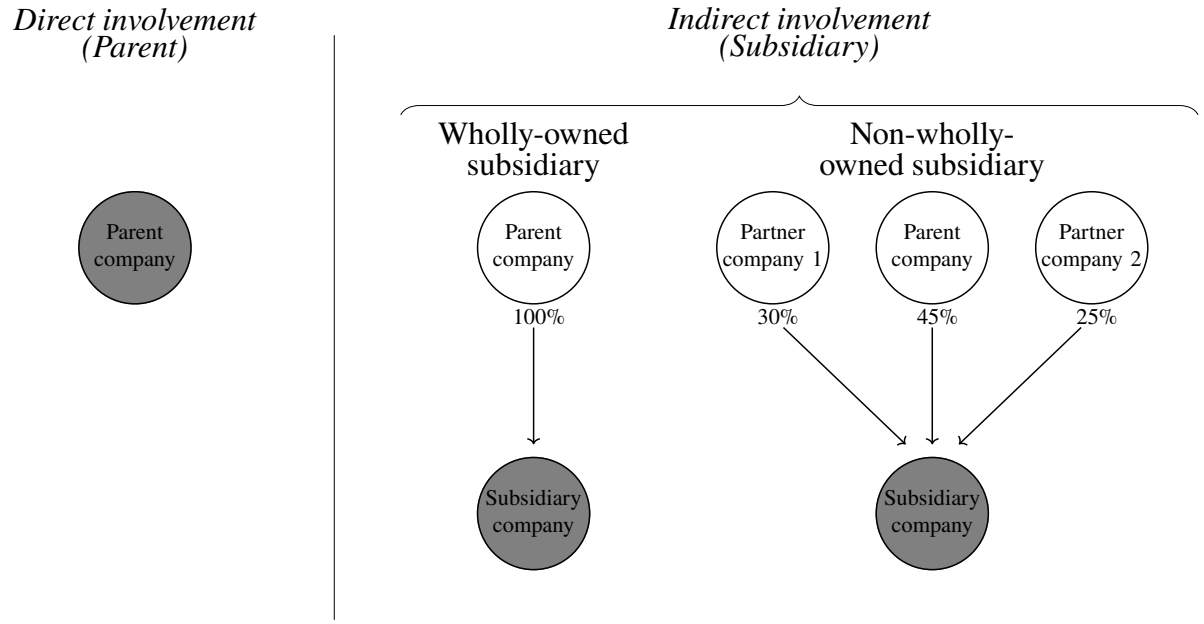


Figure 3.2: Three ways a parent company can be involved in a corporate criminal scandal along its ownership chain: directly, through a wholly-owned subsidiary, or through a non-wholly-owned subsidiary

Note: Circles represent companies, grey circles represent companies investigated for violating corporate criminal regulations, arrows indicate ownership relations, and percentages represent degrees of ownership.

A parent company can be involved in investigations for possible criminal misconduct in three possible ways. Figure 3.2 sketches this conceptual framework. In a first scenario, public authorities directly investigate the parent company for alleged violations of criminal laws. I call this scenario one of *direct involvement* of the parent in a scandal. Alternatively, a company can be involved in a scandal indirectly, *i.e.* through a subsidiary part of its corporate group. In turn, two possible scenarios exist here. The parent company can be involved in a scandal because authorities investigate

potential violations by a wholly-owned subsidiary (second scenario). Alternatively, authorities can investigate a subsidiary that the parent is simply the majoritarian owner of (third scenario). In this conceptual framework, typologies of *indirect involvement* are therefore a function of degrees of integration of the involved entity in the parent company's corporate group.

I argue that the regulatory function performed by financial markets' reputational penalties is not equal across these three scenarios. When ownership of subsidiaries is diluted, so is control by the parent over their operations (Demsetz and Lehn, 1985), including criminal activity (Alexander and Cohen, 1999). The reputation of the parent company will therefore be less compromised, in the eyes of investors, when involvement is indirect. This could appear like an efficient attribution of responsibility from a regulatory perspective: investors would negatively update the standing of companies only when they bear direct responsibility on the alleged misconduct. It is nevertheless concerning given that fragmented ownership is pivotal to further financial crime (Sharman, 2010) and that news of fraud at a minimum imply inefficiency of compliance programs the parent company should implement.

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

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

parent's reputation is not directly at stake. This results in weaker financial penalties.

In particular, I claim that the severity of the damage to reputation decreases with the distance of the involved entity from the parent company. Misconduct by a more integrated subsidiary (as in scenario 2) poses more serious threats to the parent's reputation than one which is more loosely connected to the corporate group, because full ownership implies control over illicit conduct (Alexander and Cohen, 1999). In the third scenario, instead, the parent firm does not even fully own the subsidiary found in breach of financial regulations. The parent company's reputation is less penalized because linkages between the subsidiary and the parent are weaker. The financial consequences are therefore less severe. Investors might update their reputation of the subsidiary and its stock prices might suffer as a result – if the subsidiary is in turn publicly traded. However, prices of the parent company should not be affected. When compared to a case of direct involvement, corporate ownership therefore *insulates* the parent company from a scandal.

Crucially, I argue that the difference in financial effects across these scenarios is due to reputational concerns and not about prospects of formal penalties (which usually include fines or out-of-court monetary settlements). Under most corporate criminal regulations, a parent company is subject to regulation and formal penalties regardless of whether it is involved in a criminal scandal through a subsidiary or directly. Investors should negatively update their expectations on the financial performance of a company, resulting from these formal penalties, equally across the three scenarios. Rather, I claim that *informal* losses are unevenly distributed across the three scenarios, causing direct involvement to yield more negative effects than indirect involvement.

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

I test my argument in the case of violations of the US anti-corruption law. The Foreign Corrupt Practices Act (FCPA) is a 1977 law adopted by the US Congress to prohibit bribe payments by multinational corporations to foreign public officials in the conduct of business overseas. The Act is considered among the strongest corporate criminal regulations (Brewster, 2014). It is applied by

the Department of Justice (DOJ) – in charge of its criminal enforcement – and by the Securities and Exchange Commission (SEC) – tasked with civil enforcement. Although the FCPA is an American regulation, the DOJ and the SEC have effectively become the watchdogs of the *global* anti-bribery regime. These agencies provide a very broad interpretation of the extraterritorial provisions included in the Act since 1997 (Garrett, 2011; Kaczmarek and Newman, 2011). As a result, the FCPA *de facto* applies against misconduct from any US company *and* any non-US company trading on US stock markets[3] or else furthering a bribe payment using US means such as dollars, US mail, American bank accounts, and even email passing through internet servers located on US soil (Leibold, 2014; Tomashevskiy, 2021).

The DOJ or the SEC (or both) open a file on investigations into alleged FCPA violations by a company when information on potential misconduct emerges[4]. To take a real example, in March 2016 the DOJ and SEC opened up investigations into alleged bribery by Shell Nigeria Exploration and Production Co LTD, wholly-owned subsidiary of the Royal Dutch Shell PLC, in connection with the award of rights to drill the Nigerian offshore oil block OPL 245[5]. Very rarely companies alleged of FCPA violations go to court. The long time frame of trials would expose companies to prolonged reputational losses on financial markets. In order to minimize such damage, companies usually settle allegations with prosecutors out of court, through non-prosecution agreements (NPAs) or deferred prosecution agreements (DPAs)[6]. For instance, in April 2020 the Italian oil company ENI SpA entered into a similar agreement with the SEC to settle allegations of bribery in Algeria by SAIPEM, a subsidiary owned for 43% of shares by ENI[7].

Usually, agencies communicate to the public about investigations through press releases[8] only

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

[4]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.

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

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

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

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

after allegations have been settled and companies agreed to pay their fines. Instead, information that similar investigations are ongoing is usually released by companies themselves before the final outcome. Under 1930s US law regulating securities, companies must disclose any information of material relevance for investors. This includes SEC or DOJ investigations into possible FCPA misconduct. Companies are mandated to disclose such information to investors by filing reports to the SEC itself which, since 1993, must be submitted on the Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system[9], a public platform designed precisely to facilitate the flow of information from companies to (potential) investors. In both the Shell and the ENI cases, for instance, the companies informed investors as US authorities opened up an investigation (March 10, 2016 and April 10, 2014 respectively).

Three reasons make the case ideal for comparing the effects of unexpected news about corporate criminal behavior occurring at different levels of a company structure. First, news that US agencies are investigating a company's alleged violation of the FCPA are released in a rather consistent scheme. Information is typically released by companies themselves before press releases by public agencies. Moreover, information is disclosed by filing mandatory forms to the SEC itself, which are available to the general public of (potential) investors. Similar arrangements are in place in some other countries (*e.g.* in the UK through Companies House) but not in all legal systems. By focusing on violations of the US FCPA I can therefore study the effect of unexpected news on financial markets while holding constant heterogeneity that pertains to different legal arrangements.

A second reason makes this case a good test for my argument. Whereas selections of companies into the group of those involved in cases of corporate corruption is likely endogenous to their reputation on the market, the timing information is released can be considered plausibly exogenous. The case can then be used as a plausible natural experiment to study market responses to companies' misconduct. Often, companies are forced to release press statements or to file SEC forms informing investors about upcoming investigative reports on alleged involvement into cases of corruption[10].

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

[10]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/`.

Other times, anti-bribery investigation by US agencies forces companies to delay periodic SEC filings and to submit notes unveiling allegations of corporate corruption[11]. Even when companies disclose about investigation into periodic reports, investors and market analysts cannot necessarily expect involvement of the firm into public investigations.

Finally, anti-bribery represents a least-likely case for the claim that financial markets fail to impose penalties on parents for subsidiaries' misconduct. News about anti-corruption investigations should concern investors regardless of where misconduct takes place in the company's operations, because they signal that the corporate group operates inefficiently. Imagine a competition among firms for public procurement. When the subsidiary of a company bribes an official in order to beat competitors, it adds unnecessary fees to operative cost. Corrupt contracts also involve terms that cannot be legally enforced (Treisman, 2007). Bribe-paying companies must rely on the public official's given word that another firm will not be awarded the contract instead (Lambsdorff, 2007). Moreover, under FCPA terms a parent company is liable for misconduct by its subsidiaries (Lenczowski, 1979). This means that an FCPA violation by a subsidiary can result into material losses (in terms of fines and monetary settlements) for the parent too (Garrett, 2011). If negative effects were not detected in this case, then corporate misconduct by subsidiaries would be unlikely to bear consequences for parents in cases where the material damage of misbehavior is less clear from the corporate group's perspective.

3.4 Data

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

[11] 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`.

observations on stock market indices. In this section I detail the data collection procedure.

To obtain information on cases of corporate corruption, I first identify events of corruption investigated by US agencies against publicly traded companies. I retrieve this information drawing from the dataset on anti-bribery prosecution presented in the second paper of this dissertation. The dataset is obtained by scraping information reported in text documents from the TRACE Compendium[12], an open database made of 841 text documents summarising events of cross-border corporate corruption in violation of the international anti-bribery regime, and related law enforcement actions.

Out of this dataset, I keep only investigations initiated by US agencies (SEC or DOJ) under terms of the Foreign Corrupt Practices Act. This initial selection leads me to 372 companies involved in 478 violations of the US anti-corruption policy in total. The dataset reports the parent entity (*i.e.* the corporate group's global ultimate owner) for each company involved in an event of anti-corruption violation (326 parent companies in total). I retrieve information on whether each of these parent companies publicly trades its stocks on any exchange. I rely on Bureau Van Dijk's Orbis data to retrieve this information. I keep only records relative to companies whose parent entity's stocks are publicly traded. Finally, availability of stock price data further constrains my analysis to consider only events following the year 2002. I thus select 8 events out of the group to be studied[13] . This leads me to a final selection of 214 unique companies involved in 264 events of investigation for violating the US anti-corruption law.

Next, I code which entity was involved in a scandal of corruption, at the time the event was made public, along the corporate ownership chain. First, I measure whether each company found in violation of the US anti-corruption law is the corporate group's global ultimate parent ($Subsidiary = 0$), or a subsidiary[14] ($Subsidiary = 1$). This variable allows me to study whether differences exist among direct or indirect involvement. If a company is involved in a case both directly and through

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

[13]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.

[14]For the sake of simplicity, I consider direct ownership and indirect ownership indistinctively.

a subsidiary, I consider it as a case of direct involvement ($Subsidiary = 0$). Next, I disentangle indirect involvement by coding the *degree* of ownership. I record whether the parent company is directly involved in a scandal ($Ownership = 0$). If not, I measure whether it wholly owns the subsidiary responsible for the event ($Ownership = 1$), or whether it is only the majoritarian owner of shares ($Ownership = 2$)[15].

I mainly employ Orbis data to obtain corporate ownership information. Orbis reports detailed information on corporate ownership structures of companies. It also reports shareholder history, that allows to trace ownership structures at the time allegations of misconduct hit the market. I cross-check this information against a range of alternative sources. First, publicly available reports made by US authorities (SEC and DOJ) at the time of the anti-corruption investigation. Second, extensive web searches to confirm the retrieved information[16]. Where Orbis information conflicts with these alternative sources, I keep information available from reports by US authorities. Where this is not available, I rely on web searches. Out of the 264 events of corruption I consider, 139 (53%) involved the parent company directly, while 125 (47%) involved it through a subsidiary. Out of these 125 events, 64 involved a wholly-owned subsidiary ($Ownership = 1$) and 61 involved a non-wholly-owned one ($Ownership = 2$).

The next step consists in coding, for each violation of the US anti-corruption law, the very day allegations were made public. I employ the Foreign Corrupt Practices Act Clearinghouse (FCPAC) datasets hosted by Stanford University[17]. The FCPAC draws on compulsory company reports from EDGAR, press releases from government agencies, and newspaper articles to establish the earliest date news of a potential violation of the US FCPA by a company were made public. I manually search through the FCPAC database for each instance of FCPA violation selected from above and code the date information was first released.

Table 3.1 provides a snapshot of what my dataset looks like. For each entry, an entity (*Violation*

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

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

[17]See: `https://fcpa.stanford.edu`.

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

Table 3.1: US anti-corruption policy violations: Sample of data

entity) is alleged to have bribed public officials in a foreign market (*Violation country*)[18], thus violating the US FCPA. I report the parent company of the involved entity ($Parent\ Company$), whether the parent was involved in the scandal indirectly ($Subsidiary$), the type of indirect involvement ($Ownership$), the ticker symbol under which the parent company trades its securities ($Ticker$), and the date information on the violation was first made public ($Event$).

After creating this dataset, I can proceed at collecting daily stock prices data. I retrieve all stock price and market indexes information from Refinitiv Workspace. Consistently with typical political-economy applications of the design (Aklin, 2018; Genovese, 2021; Kucik and Pelc, 2016; Wilf, 2016), I measure daily percentage *change* in stock prices with respect to the previous day – which I call $Returns$. This variable is measured as the percentage difference in closing price of a stock at the end of a trading day, with respect to the same value at the end of the previous trading day.

Finally, I retrieve daily data on stock market indexes. This information serves to construct predictive covariates in the research design outlined in the next section. Given that companies in my dataset trade their equities on different exchanges, I retrieve daily percentage changes in values of the most important market indicators. I obtain price history of: S&P 500 Index (SPX), NASDAQ Composite Index (IXIC), NYSE Composite Index (NYA), NASDAQ 100 Index (NDX), Tokyo Stock Exchange REIT Index (TREIT), Shanghai SE Composite Index (SSEC), the Financial Times Stock Exchange 100 Index (FTSE), Euronext 100 Index (N100), Shenzhen SE Composite Index (SZSC), TSX-Toronto Stock Exchange 300 Composite Index (GSPTSE), and the Deutsche

[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 3.1). In other cases, no location is specified at all.

Boerse DAX Index (GDAXI).

3.5 Research design

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

I adopt an event-study research design to test these expectations and estimate the effects of interest. This empirical strategy is widely used in finance studies for estimating market-induced reputational effects of unexpected events on involved companies (Karpoff et al., 2008; Kreitmeir et al., 2020). It has been recently adopted by political economists to assess the effect of international institutions and regulations (Gray, 2009; Wilf, 2016), political communications (Genovese, 2021), elections (Aklin, 2018), and international rulings (Kucik and Pelc, 2016). Here, I employ it to study the heterogeneous effect of state-mandated corporate regulations, as it is moderated by the involved entity's position in the corporate chain.

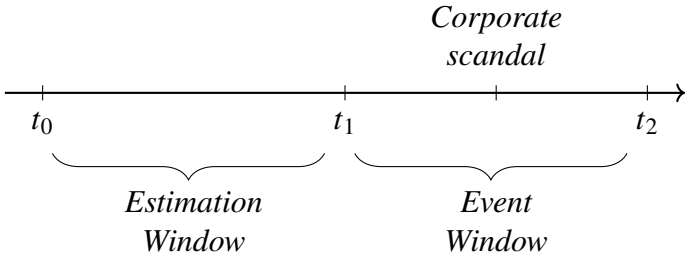


Figure 3.3: Event-study design: Time windows

The design imputes daily synthetic counterfactual *Returns* to each company around an event of interest. It then measures the difference between observed and synthetic counterfactual observations on the day of an event of interest, thus estimating an average treatment effect on the treated (ATT) companies' stock prices. In order to achieve that, I divide daily stock price observations for each

company in two time-windows as presented in Figure 3.3. First, an *estimation window*, predating the unexpected event of interest (from t_0 to t_1). Next, an *event window*, centred around the event whose effect is to be estimated (from t_1 to t_2). For each of the 264 events of corruption, I define an *event window* that starts 30 days before the event and ends 30 days after the event (total length is 61 days per event). The *estimation window* of each company begins 210 days before the beginning of its *event window*[19].

In the *estimation window*, I estimate one market-model relative to each event by explaining the involved company's *Returns* using market-wide indexes. Equation 3.1 summarizes this step. Daily observed *Returns* for each company i involved in an event e, within the *estimation window* ($t_0 \leq t < t_1$), are modelled as a function of the matrix of company-invariant market-wide indexes listed in the previous section ($\mathbf{X_t}$).

My matrix of covariates includes market-wide indexes that are not necessarily relevant to explain returns for a given company. Estimating Equation 3.1 with ordinary least squares (OLS) would result into noisy predictions with large variance, thus returning imprecise counterfactuals and potentially reducing precision of my identification. I adopt a least absolute shrinkage and selection operator (LASSO) procedure for selecting the most predictive indexes in this matrix for each company. The LASSO is a covariate-selection algorithm that effectively associates sets of non-negative weights to each variable in the matrix of covariates $\mathbf{X'_t}$. It then selects the single set of weights $\boldsymbol{w_e}$ that results in the lowest residual sum of squares, hence in the most predictive model (Tibshirani, 1996). Effectively, it sacrifices non-predictive covariates (multiplying them by a 0 weight) to reduce the variance of an OLS estimation. Previous event-analysis designs have shown its superior predictive performance when compared to standard ordinary least square (OLS) market models[20] (Wilf, 2016). Thanks to the LASSO, each market model represents the best feasible predictor of a company i's stock prices before the unexpected event e took place.

$$Returns_{et} = \alpha_e + \mathbf{X'_t}\boldsymbol{w_e}\boldsymbol{\beta_e} + \varepsilon_{et} \quad | \quad t_0 \leq t < t_1 \tag{3.1}$$

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

[20]In a robustness test, I replicate my results using OLS (thus effectively employing all market-wide indexes for all companies) and verify results hold, although estimation becomes less precise. See Section 3.E.

In my LASSO estimation procedure, I adopt a cross-validation procedure for selecting the set of most predictive weights for each individual event e involving a company. I employ 5 folds for each event and select the single vector of weights $\boldsymbol{w_e}$ that minimizes the mean cross-validated error across the sets of weights considered. I then employ this set of weights to determine how covariates enter Equation 3.1 for that specific event. Figure A.1 is a heatmap reporting coefficients $\boldsymbol{w_e}\hat{\boldsymbol{\beta}}_{\boldsymbol{e}}$ with whom indexes enter each of the 264 market models estimated using the LASSO. It also reports the percentage of models that include each of the 11 indexes. The procedure effectively omits non-predictive indicators (represented by white cells). Minor indexes are sacrificed and tend to appear less frequently (*e.g.* Euronext 100 Index enters only 36.4% of the models). More predictive and relevant indexes tend to be more frequently included in the models (*e.g.* the NYSE Composite is included in 59.1% of the models).

The LASSO results in market models with very satisfactory in-sample predictive performances. Figure A.2 reports the distribution of the Normalized Root Mean Squared Error (RMSE) and the R-squared for the 264 market models in the estimation window[21]. All models result in RMSEs with values smaller than 0.20, with the bulk of models yielding a value of just 0.10. Models perform well also from the R-squared perspective: the majority explain at least half of the variation of *Returns* in the *estimation window*.

Once vectors of coefficients α_e and $\boldsymbol{\beta_e}$ have been estimated following this procedure, using *estimation window* data, I use $\hat{\alpha}_e$ and $\hat{\boldsymbol{\beta}}_{\boldsymbol{e}}$ to predict daily stock prices to each company in the *event window* (from t_1 to t_2). Equation 3.2 represents this second step. This phase is effectively imputing daily synthetic counterfactual stock prices to each company in the *event window*, based on models estimated in Equation 3.1. $\widehat{Returns}$ thus represents the best expectable returns to a company in the *event window*, based on information available before unexpected news of corporate crime were

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

released. I take it as a measure of counterfactual *Returns* to a company, had event e not occurred.

$$\widehat{Returns}_{et} = \hat{\alpha}_e + \boldsymbol{w_e}\mathbf{X}_t'\hat{\boldsymbol{\beta}}_e \;\mid\; t_1 \leq t \leq t_2 \tag{3.2}$$

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

$$Abnormal\ Returns_{et} = Returns_{et} - \widehat{Returns}_{et} \;\mid\; t_1 \leq t \leq t_2 \tag{3.3}$$

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

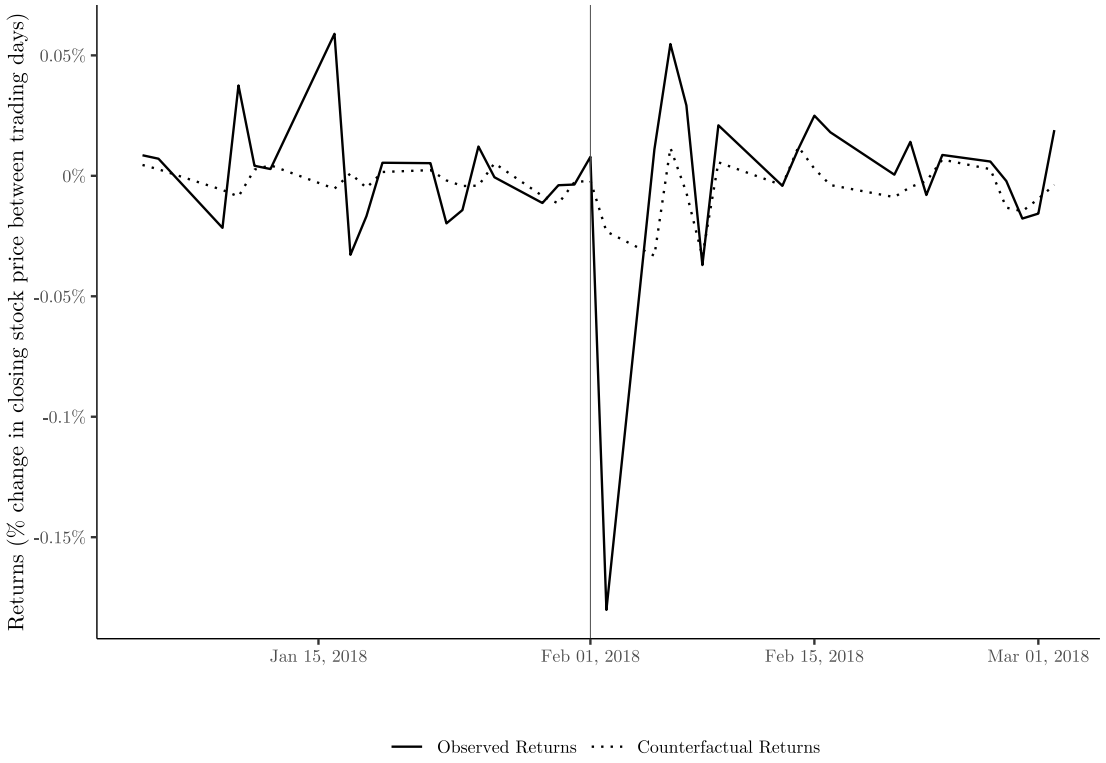


Figure 3.4: Example of the synthetic counterfactual imputation procedure: allegation of FCPA violation by OSI Systems, Inc. in February 2018

Figure 3.4 exemplifies the procedure by drawing on a real case. On February 1, 2018 the

California-based defense technology manufacturer OSI Systems, Inc. (trading on NASDAQ as OSIS) announced its investors that the SEC and the DOJ had opened investigations into potential violations of the FCPA in an unspecified foreign country[22]. As this case exemplifies, in the pre-treatment period the LASSO procedure retrieves estimates of synthetic counterfactuals that closely approximate observed *Returns*. On the day following the announcement of allegations, my synthetic counterfactual imputation estimates an abnormal loss in *Returns* of about 0.16%, indicating that investors responded to this event and the company realized *Returns* that were substantively below what market indicators would have expected. The company was trading at about $66.1 per share before the event. Thus, the value of OSIS lost about $10.6 per share. This equals a loss in market capitalization of more than $201 million, given that the company had about 19 million outstanding shares. The company was then dismissed of all allegations: in a press release on June 5, 2019 it announced that the DOJ and the SEC had closed any investigation on the matter and would not be taking any further actions[23].

In order to average similar effects across different events, I estimate the model reported in Equation 3.4 using data from the *event window*. The model explains the *Abnormal Returns* measure computed in Equation 3.3. It includes a binary treatment variable $Event$ taking value 1 on the day the corporate criminal scandal e was made public, and 0 otherwise. Parameter δ represents the estimand: an average treatment effect of the treated (ATT) companies' stock prices on the day information on potential corruption was released, retrieved by discounting observed returns from changes in stock prices that were expectable had the event never taken place. It measures by how much observed $Returns$ diverge from counterfactual $\widehat{Returns}$, on average, on the day the unexpected event took place.

$$Abnormal\ Returns_{et} = \gamma + \delta Event_{et} + u_{et} \ \mid \ t_1 \leq t \leq t_2 \tag{3.4}$$

To test my argument on the moderating effect of indirect involvement, I apply this design and

[22]See record on the FCPAC: `https://fcpa.stanford.edu/investigation.html?id=380`.
[23]See press statement: `https://fcpa.stanford.edu/fcpac/releases/4000/003168.pdf`.

condition the effect of $Event$ on the position of the involved entity in the ownership chain. First, I compare direct and indirect involvement in a corruption scandal. I estimate Equation 3.4 after interacting the $Event$ treatment variable with my $Subsidiary$ variable, as in Equation 3.5. In a later test, I substitute the $Subsidiary$ moderator with my indicator measuring the degree of ownership ($Ownership$). I expect parameter δ to be negative: when a parent company is directly involved in a corporate corruption scandal, its stock returns should be negatively affected. I expect coefficient ϕ to be positive instead: involvement through a $Subsidiary$ should moderate the negative impact of a scandal, and decrease its magnitude.

$$Abnormal\ Returns_{et} = \gamma + \delta Event_{et} + \phi\ Event_{et} \times Subsidiary_{e} + \theta\ Subsidiary_{e} + u_{et} \quad (3.5)$$

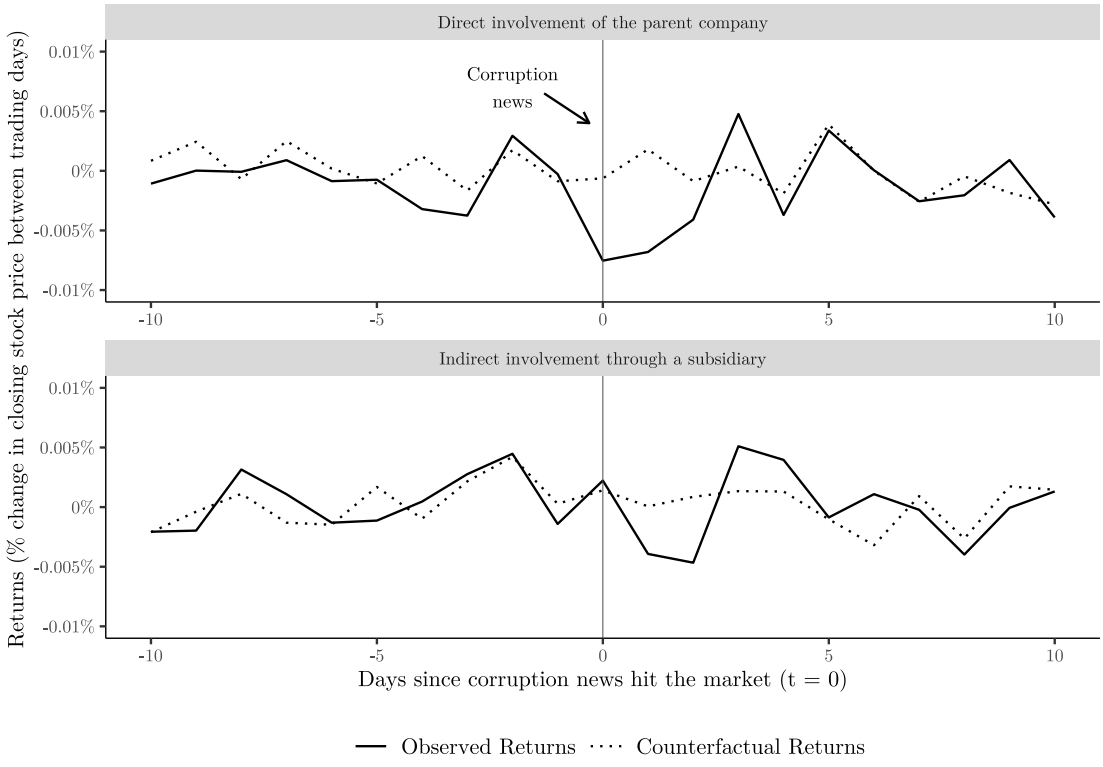


Figure 3.5: Average $Returns$ and $\widehat{Returns}$ in the 10 days before and after the release of corruption news, disaggregated by type of involvement

Note: Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary

Figure 3.5 shows the daily average observed and counterfactual $Returns$ in the 10 days before

and after the $Event$, distinguishing between cases where the parent company was directly involved in a scandal ($Subsidiary = 0$, top panel) from those where involvement happened through a subsidiary ($Subsidiary = 1$, bottom panel). In both panels, pre-treatment average observed $Returns$ are well approximated by average counterfactuals. This indicates the lack of a pre-treatment difference among the two groups. The top panel shows that observed $Returns$ are on average lower than counterfactuals at the closing of the very day corruption news are released (and consistently so in the following two days) when parent companies are involved directly in a scandal. After that, $Returns$ do not seem to depart from their counterfactuals. The picture offered by the bottom panel provides an initial confirmation of expectations from my argument: observed $Returns$ to the parent company are lower than their counterfactuals in the two days following release of corruption news when a subsidiary is involved in a scandal, but this gap appears smaller in size than what is observed in the top panel. After that, trends between observed and counterfactual data get closer again.

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

Figure B.4 shows average *Abnormal Returns* (that is, the difference between the solid and the dotted lines from Figure 3.5) and 95% confidence intervals in the entire 30 days before and after the $Event$. It confirms the interpretation drawn from Figure 3.5 and provides further evidence in favor of my expectation. Next section presents econometric results to assess the size and significance of these descriptives.

3.6 Results

I estimate Equation 3.5 using OLS. I cluster all standard errors at the parent company-level, to account for likely correlation between observations relative to the same firm. Table 3.2 reports my main results. In model 1, I introduce only my variables of interest. In following models, I introduce controls to remove potential sources of endogeneity. First, I introduce a one-day lag of the dependent variable to account for any anticipation effects on the market. Next, I account for year-specific heterogeneity. Research has vastly documented variation in the number of FCPA cases over time (Garrett, 2011) and the dependence of prosecutors' activity on political motives that might also be time-varying (Tomashevskiy, 2021). My data confirm the observation that the intensity of investigation changes with time (Figure B.3). If specific years see a stronger reaction by markets for unobservable reasons unrelated to my causal story, it is possible that my estimates are endogenous. I therefore introduce a year-fixed effect in model 3 (considering the year of the $Event$ day), so as to only focus on explaining within-year variation and removing these potential sources of endogeneity. Finally, specific events might have extreme resonance for reasons that are unrelated to my causal story. In model 4 I substitute year-fixed effect with event-fixed effect to absorb all between-event heterogeneity in stockholders' response.

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

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

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

Table 3.2: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature

In order to appreciate the size of the moderating effect, I calculate marginal effects of the $Event$ on *Abnormal Returns* in the two scenarios of direct and indirect involvement. I present results in Figure 3.6 drawing from model 1 in Table 3.2. Results relative to the other models are similar and available upon request. I observe a statistically significant and negative effect of about -0.01% when the company is involved directly in the scandal. However, the effect is positive in size and statistically insignificant when the company is involved in a scandal only through a subsidiary. This confirms my argument that subsidiary ownership insulates the parent company from a corporate criminal scandal.

How severe are the estimated negative effects of the scandal on the parent company's stocks? The median[24] parent company involved directly in an event of corruption ($Subsidiary = 0$) traded at about \$30.26 per share the day before the $Event$. A -0.01% abnormal loss on the day corruption was made public means that such company lost about \$0.30 in price of its shares due to the unexpected corruption information. To estimate how this loss translates in terms of market capitalization, I

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

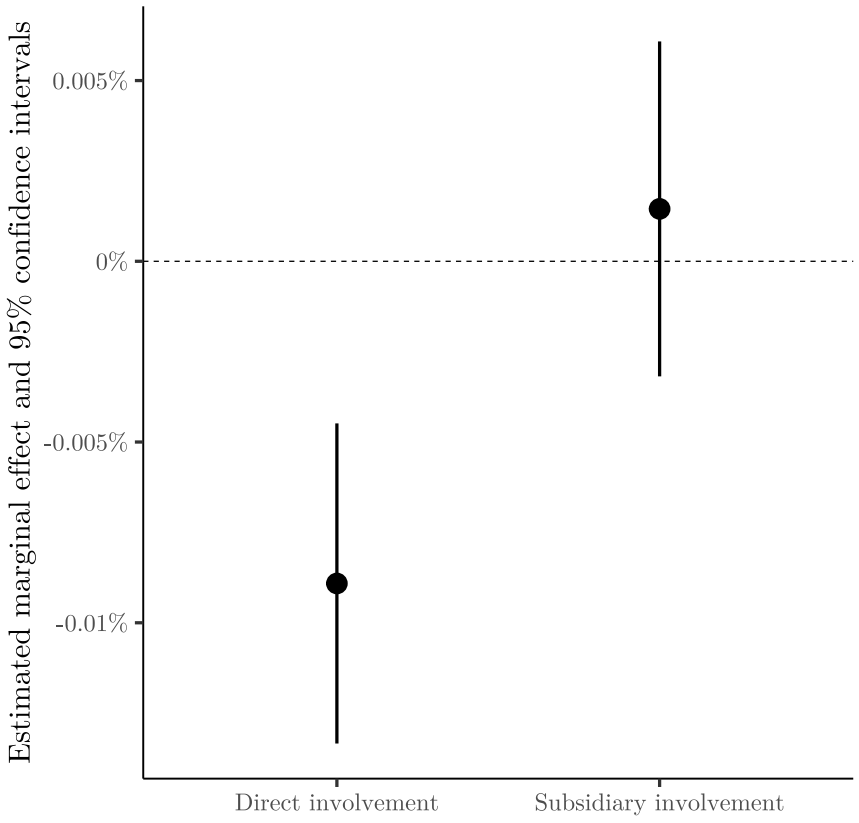


Figure 3.6: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly or through a subsidiary

Note: Results from model 1 in Table 3.2

retrieve from Orbis information on the number of outstanding shares traded by each parent company at the end of the month before each event considered. The median company in my data traded 440,519,000 shares, for a market capitalization of about $13 billion the day before information was released. A loss of $0.30 per share on the day corruption news hit the market amounts to about $132 million in losses. This average effect is remarkably similar to that estimated in case of other negative corporate social responsibility events (Kreitmeir et al., 2020).

How do penalties evolve over time? In order to consider this, I study in more details the narrow time window that goes from ten days before the event until ten days after the event[25]. I perform a fully-fledged event-analysis by including a categorical variable relative to each trading day around

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

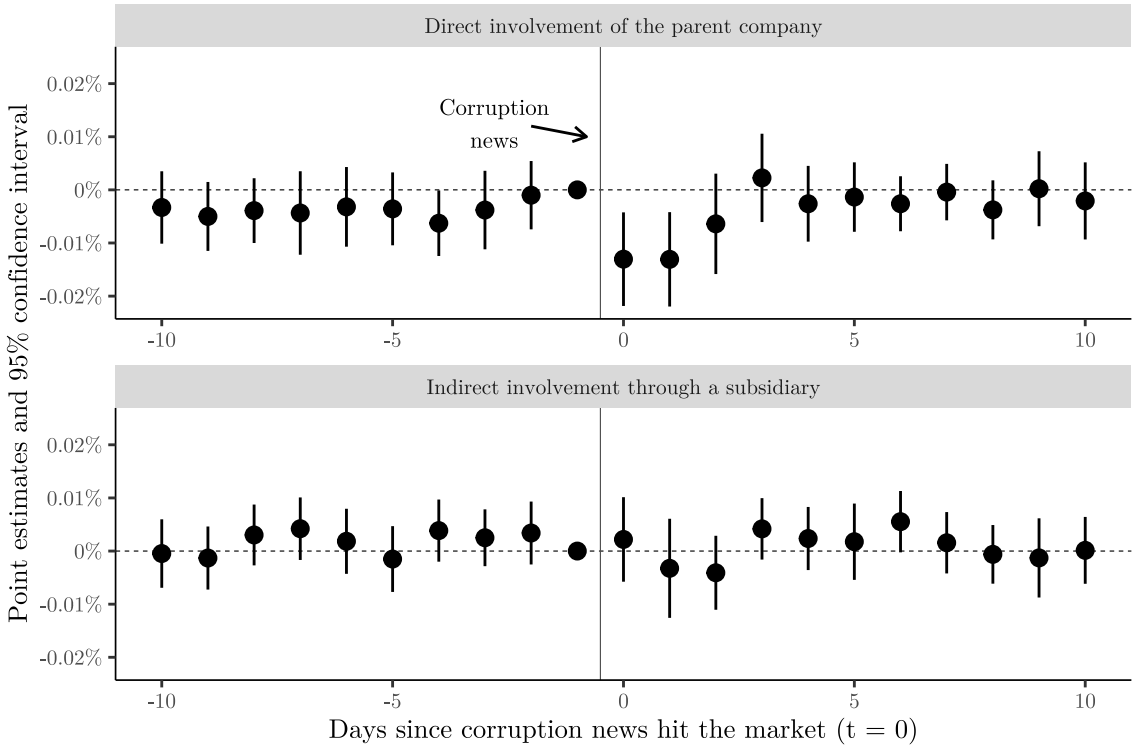


Figure 3.7: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal

the publication of corruption news. I employ the day before the event is made public (day -1) as a baseline category for this variable. I subset my sample according to whether the parent company is directly ($Subsidiary = 0$) or indirectly involved in a scandal ($Subsidiary = 1$). I include one-day lag and event fixed-effect for consistency with the most complete model of Table 3.2. Results are reported in Figure 3.7. Similar results are obtained when omitting all these additional terms (Figure C.1). All standard errors are clustered at the parent company-level.

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

That markets quickly recover from losses to companies' *Returns* is consistent with the efficient market hypothesis (Fama, 1970). But do penalties suffered in the first two days after the $Event$ cumulate to any sustained loss? How would cumulative returns have looked like, had corruption news not hit the market? To address this question, I follow previous event-studies (Aklin, 2018; Wilf, 2016) and calculate *Cumulative Observed* and *Cumulative Expected Returns* by summing, respectively, daily *Returns* and $\widehat{Returns}$ relative to a specific event. Figure 3.8 plots the average trends of these variables distinguishing between cases of direct and indirect involvement. It shows that, over the entire period that follows the release of corruption news, counterfactual *Cumulative*

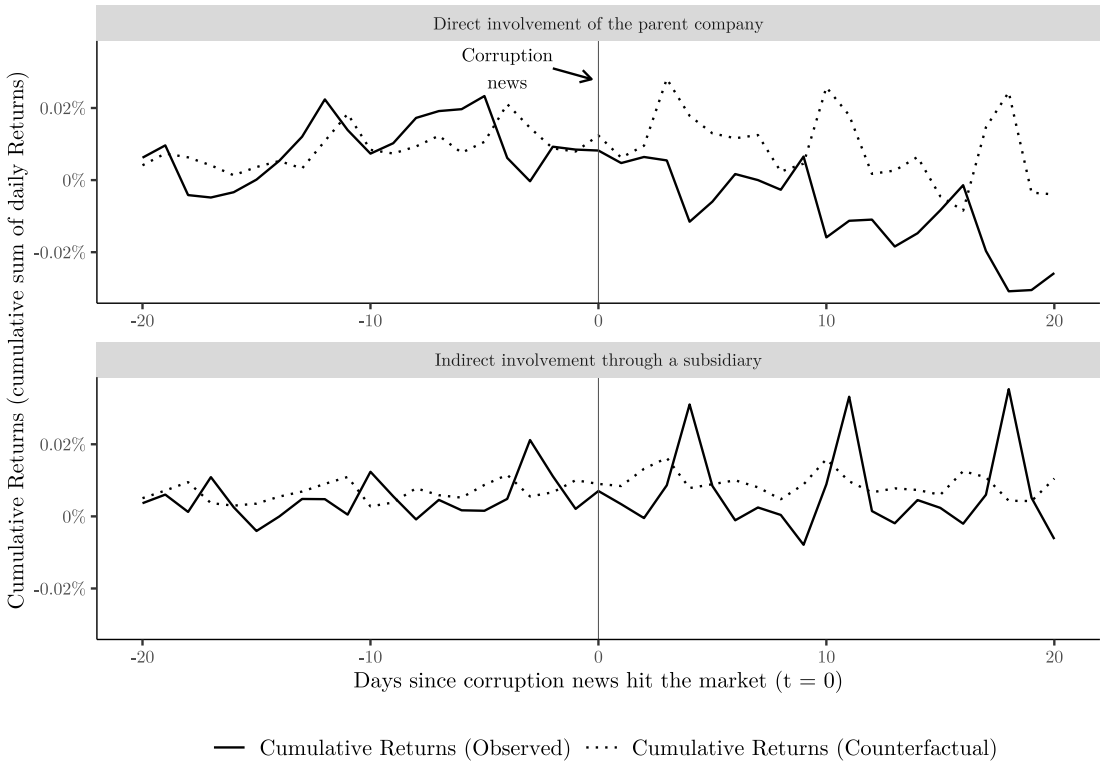


Figure 3.8: Average *Cumulative Observed Returns* and *Cumulative Expected Returns* in the 20 days around the publication of corruption news, disaggregated by type of involvement

Note: Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary

Expected Returns to a company's stocks (counterfactual) are higher than *Observed* ones (factual), when involvement in a scandal is direct. When considering events of direct involvement in a scandal, observable *Cumulative Returns* are significantly below their counterfactuals even 20 days after the event. In Appendix, I propose an event-analysis analogous to that in Figure 3.7 to estimate this cumulative effect. I find that companies involved directly in a corporate corruption scandal experience cumulative losses in the order of 0.04%, equalling about $517 million in median capitalization, detectable with precision at least up to 18 days following the release of corruption news (Figure C.4). No clear difference between observed and counterfactuals is detected, instead, for indirect involvement.

I report extensive robustness tests on my findings in appendix. First, I show that results are not driven by any single outlier – a scandal with significantly negative impact – (see Figures C.5 and

C.6). Next, I address the potential concern that results are driven by arbitrary choices followed in the procedure. I restrict *event windows* to the intervals: $[day\ -10\,, day\ 10]$ and $[day\ -10,\ day\ 0]$, to make sure I consider only data in the immediate proximity of the $Event$ (Tables C.1 and C.2). Then, I exclude from the analysis any event associated with an imprecise estimation from Equation 3.1 (Table C.3, Figures C.7 and C.8). Finally, I replicate my entire analysis substituting LASSO synthetic counterfactuals with OLS ones and verify results do not hinge on the chosen procedure to impute counterfactuals (Section 3.E).

3.6.1 Typologies of indirect involvement

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

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

indicator presented above, to study how the moderating effect of corporate ownership varies across wholly-owned and non-wholly owned subsidiaries.

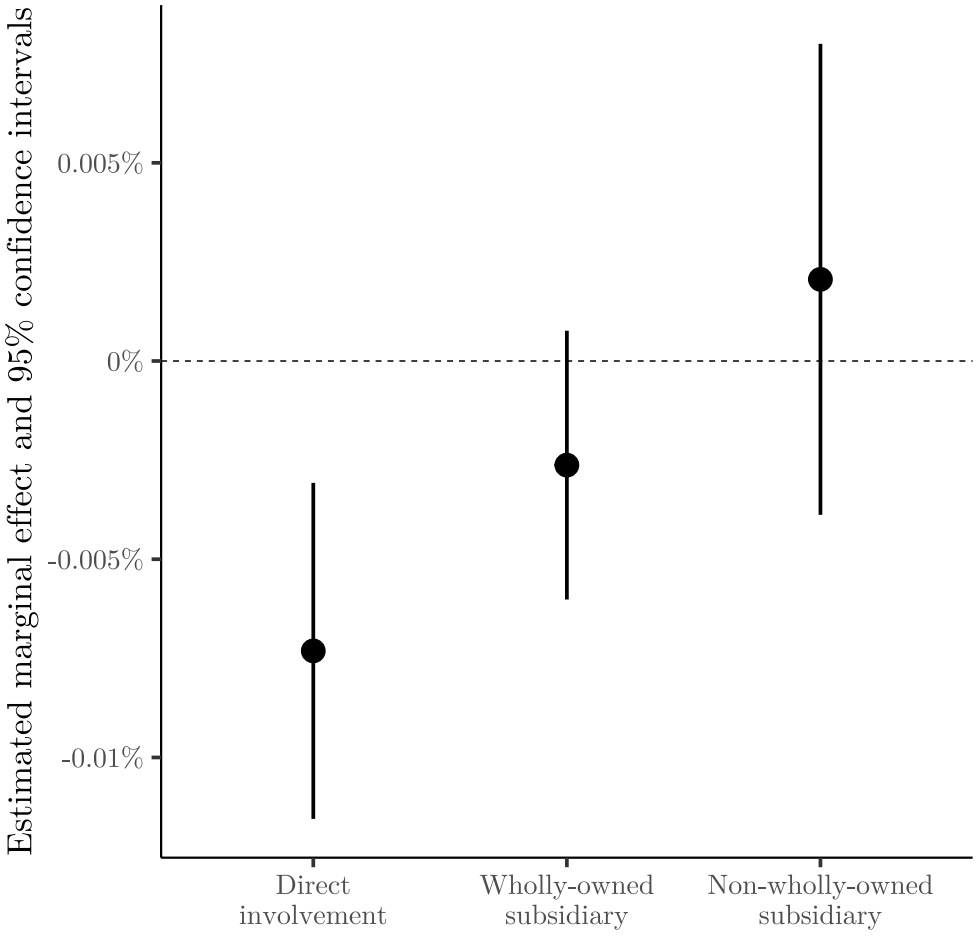


Figure 3.9: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on the degree of ownership by the company of the subsidiary

Note: Results from model 1 in Table D.1

I re-estimate models in Table 3.2 and report the marginal effects from the most complete model in Figure 3.9. Full results are in Table D.1. I observe a negative effect for involvement through a wholly-owned subsidiary, smaller in size than when the parent company is involved directly in a scandal and falling short of the conventional 0.05 level. In a further test, I run a more flexible specification that uses a categorical measure of $Ownership$. This is equivalent to the binning estimator proposed by Hainmueller et al. (2019), because it does not force the moderating effect

to be linear. Resulting marginal effects from Figure D.1 show no significant effect on the parent company's stocks even for involvement in a corruption scandal through a wholly-owned subsidiary (full results in Table D.2). No significant effect at all is detected for involvement through a majority-owned subsidiary.

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

I leverage these differences and calculate a score representing the similarity between the name of the parent and that of the subsidiary in case of indirect involvement in a scandal. I employ a metric for string similarity based on the Levenshtein distance[26], which ranges from 0 (indicating extreme diversity between two strings) and 1 (indicating perfect equality). Next, I re-estimate my event-fixed effect model from Table 3.2, subsetting my sample to cases of indirect involvement only. In this specification, I employ this newly computed similarity score as a moderating variable. To this aim, I employ the binning estimator proposed by Hainmueller et al. (2019), which does not force the moderating effect to be linear. Figure 3.10 reports results and presents three examples of pairs of names ending up in each of the three levels of the moderating variable. I observe no significant

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

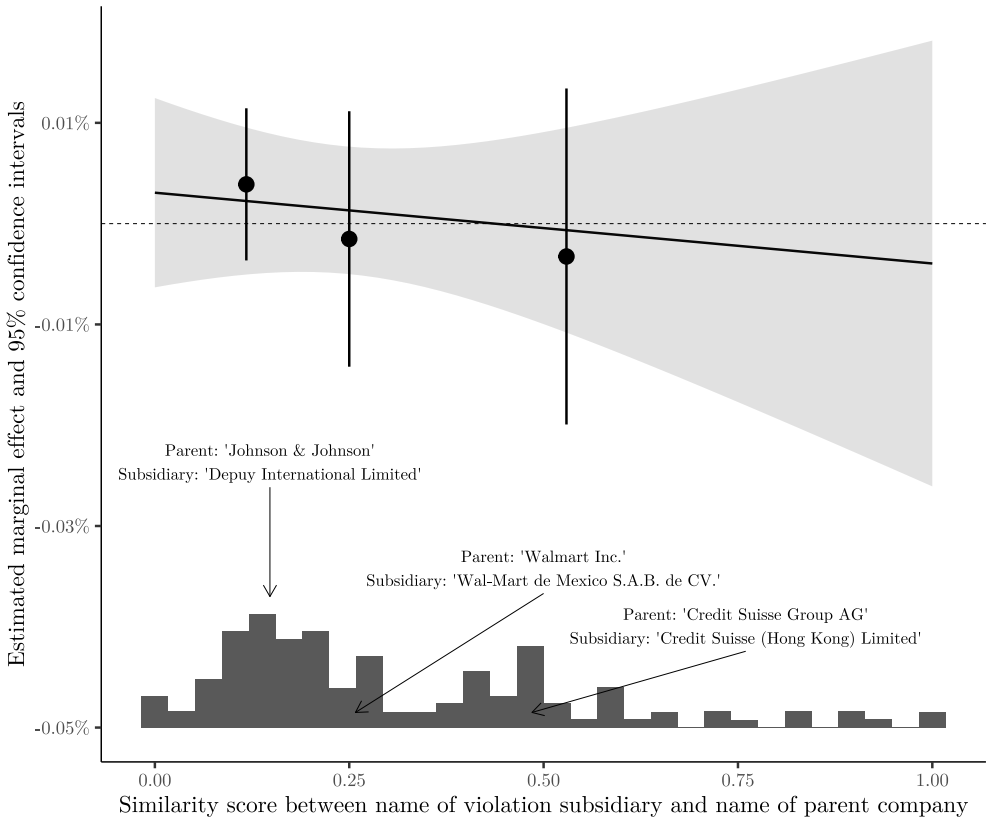


Figure 3.10: Marginal effects of indirect involvement into corporate corruption scandals on the parent company's *Abnormal Returns*, conditional on the degree of similarity between the name of the subsidiary and that of the parent company.

effect for any type of indirect involvement, even when the name of the subsidiary responsible for alleged corruption is as similar to that of the parent as "Credit Suisse (Hong Kong) Limited" is to "Credit Suisse Group AG". This lends confidence against the hypothesis that the null-effect is driven by genuine ignorance on the side of investors about true corporate structures. It suggests investors are in fact cynically avoiding to penalize parents for misconduct by their subsidiaries.

3.7 Conclusion

Multinational companies can exploit their fragmented ownership chains in order to conceal financial crime (Cooley and Sharman, 2017) and evade regulations states cast to prohibit misconduct (Arel-Bundock, 2017; Chapman et al., 2020). This poses a real threat to an effective limitation of nefarious transactions and questions whether formal regulatory provisions bear any deterrence against corporate crime (Baradaran et al., 2012; Findley et al., 2015). It is often argued that formal state-based legal tools can find an unexpected regulatory helping-hand from markets (Morse, 2022). Investors would behave as a "global civil society" (Fukuyama, 2016; Ruggie, 2018) by penalizing companies' stock prices when information on corporate misconduct emerges (Alexander, 1999; Kreitmeir et al., 2020). Authorities would then be able to *de facto* wage sanctions by leveraging the effect that investigations for misconduct bear on companies' asset prices (Alexander and Arlen, 2018; Farrell and Newman, 2019). However, it is not clear whether markets penalize companies for misconduct happening down their ownership chains. The gap is relevant because fragmented ownership can be purposed precisely to further financial crime (Sharman, 2010).

In this paper, I argued that companies can fragment their ownership as a shield against informal penalties imposed by financial markets when information on misconduct emerges. My conceptual framework distinguishes cases where a parent company is directly involved in a scandal and those where involvement happens indirectly – that is, via an owned subsidiary. I claim that markets impose penalties on a company when unexpected allegations of its *direct involvement* in a crime hits the markets, due to concerns that negative publicity undermines the firm's profitability. However, the

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

My empirical tests leveraged an original dataset on 264 investigations for alleged violation of the US anti-corruption criminal law (FCPA) in 214 distinct corporate groups. I retrieved data on the day information of misbehavior first hit the market and daily stock prices of the parent company sitting at the top of each corporate group. I also coded the relationship between the entity (allegedly) responsible for a violation and the parent company. I used this dataset in an event-analysis design that imputes synthetic counterfactuals around the day unexpected information first hit the markets. My results show that parent companies suffer a significant abnormal loss of about 0.01% to their stock returns on the very day following the release of information. This effect amounts to a loss of about $132 million for the median company. However, I show evidence of no effect on the parent company's stock prices when involvement occurs through a subsidiary.

Results indicate a clear failure of the supposed regulatory function performed by markets that is of interest to the international governance literature. Although I provide evidence that markets do penalize companies for direct involvement in misconduct, consistently with important previous studies (Karpoff et al., 2008; Capelle-Blancard and Petit, 2019; Kreitmeir et al., 2020; Morse, 2019), investors do not seem to bite against parent companies for crime conducted by entities down the line of their corporate groups. This is concerning because it shows that companies can strategically fragment ownership to meet a cynical threefold goal: to further financial crime (Findley et al., 2015), to evade regulations (Chapman et al., 2020), *and* to minimize losses on equity markets. This has important implications for debates in governance beyond financial crime, for instance in environmental regulation.

More fundamentally, findings question the extent to which markets are a viable complement (or substitute) for formal state-based regulations, a conclusion that contributes to a long-lasting debate in political science on state-market relations (Ruggie, 2018; Strange, 1996) and on ensuring compliance of private actors with international norms (Baradaran et al., 2012; Jensen and Malesky,

2018). Future research on the matter could learn from these conclusions to study whether and how different forms of corporate integration (*e.g.* vertical vs horizontal integration, joint ventures, and licensing) insulate or expose parents to private regulatory responses by investors. Additionally, scholars of political economy could study whether wordings of negative news by companies in their communications of misbehavior affect markets differently.

A final point for discussion concerns the relation between findings in this paper and arguments advanced in the previous two articles of this dissertation. This paper discusses the extent to which reputational effects for a corporate criminal scandal are in place, given that stock market responses are found only for scandals involving a parent company directly. No significant loss is found for criminal cases involving a parent company only indirectly. This could lead to an apparent contradiction with arguments in papers 1 and 2, which rely on the claim that reputational effects turn into severe costs for companies. In paper 1, this implies that companies alter their investment decisions when expecting that they might incur in reputational costs for engaging in bribery. In paper 2, I argue that actual scandals involving a company turn into reputational costs that induce the firm to cooperate with authorities in countries where the company is most exposed to. The key to solve this apparent contradiction, I claim, is to think carefully about which entity is involved in a corporate criminal case. Arguments in papers 1 and 2 refer to (the expectation of) a scandal that involves the entire company, including the parent. Findings from paper 3 do not question that reputational costs exist in such a type of direct involvement. In fact, I find significant losses for these class of cases. Scandals do not result into significant losses only when involvement is indirect and the scandal does not "propagate" up to the parent company. Moreover, the present article focuses only on a narrow type of reputation: that constructed on financial markets by investors. Any (lack of) effect documented here is only specific to this group of public stakeholders. This does not undermine the possibility that broader reputational costs (such as those implied by arguments in papers 1 and 2) are in place.

Appendix 3.A Estimation procedure

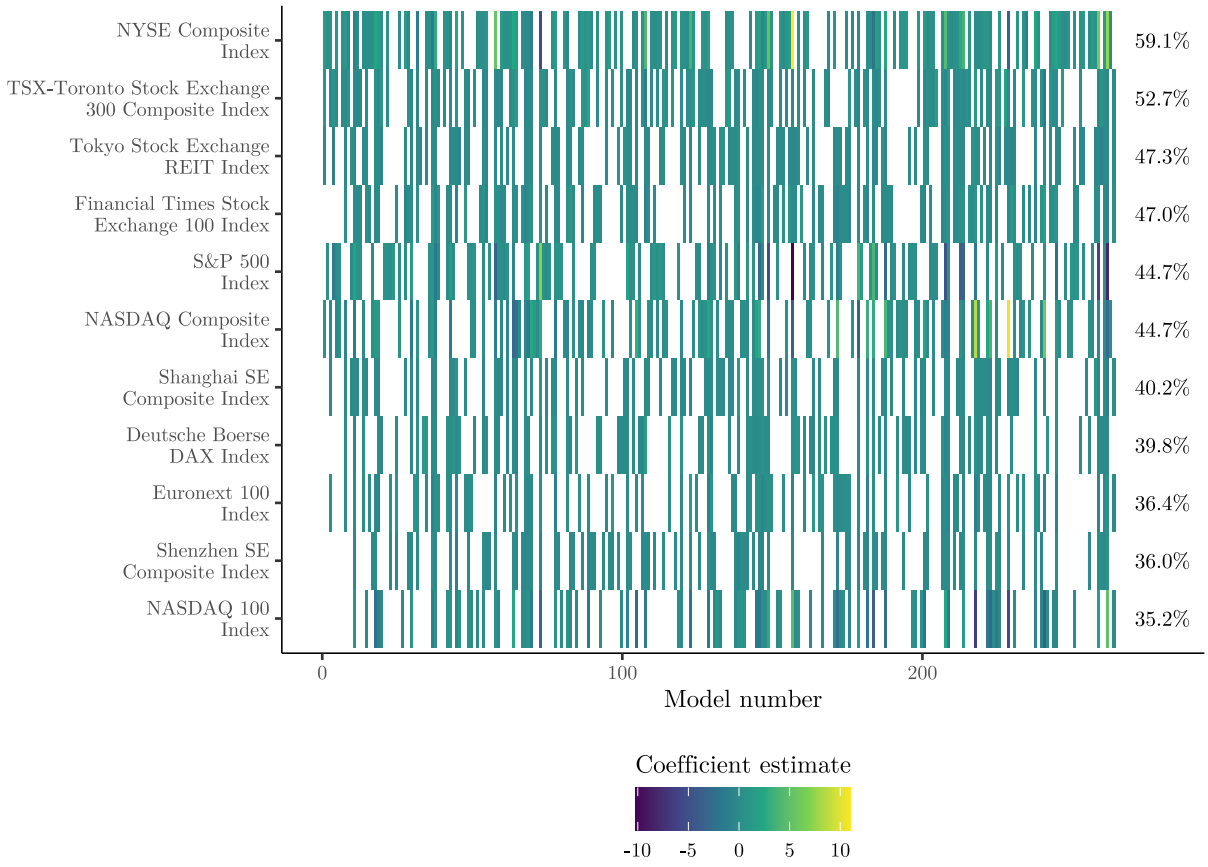


Figure A.1: Heatmap reporting the value of estimated coefficients relative to financial indicators (y-axis) as they enter each of the 264 LASSO market models from the *estimation window* (x-axis)

Note: The plot shows in white indexes that are excluded from a market model and colors cells according to the size of the estimated coefficient (multiplied by the LASSO weight). A percentage is also reported indicating the share of models each index is included in.

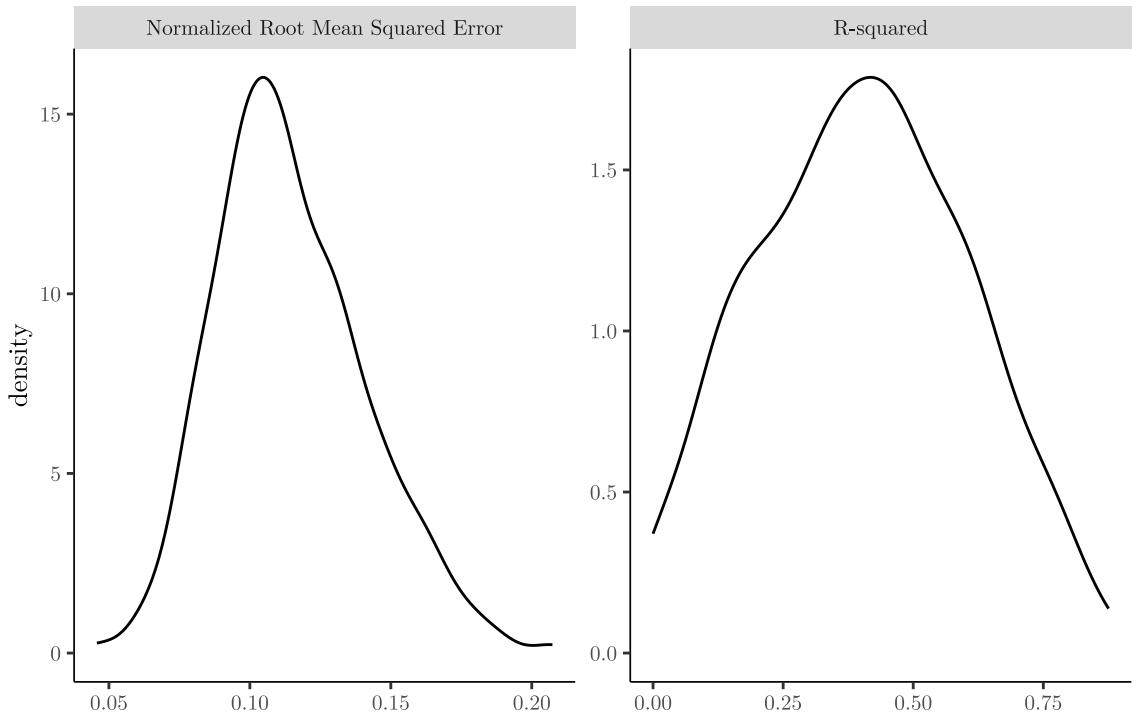


Figure A.2: Distribution of the normalized Root Mean Squared Error (RMSE) and of the R-squared yielded by the 264 market models estimated using the LASSO procedure.

Appendix 3.B Descriptives

3.B.1 Balance in observable covariates across types of involvement

My research design identifies how the effect of the $Event$ on each company's stock $Returns$ changes according to $Subsidiary$ (and $Ownership$). The design imputes synthetic counterfactuals to retrieve what stock $Returns$ would have looked like in the absence of the $Event$. But how comparable are scenarios distinguished by my indicators for corporate ownership? Do firms involved in scandals directly (left-hand side of Figure 3.2) differ fundamentally from those involved in scandals indirectly (right-hand side)? If so, any difference in the identified effect might be due to this fundamental heterogeneity rather than to the mode of involvement in a scandal (direct *vs* indirect).

I retrieve information on characteristics of each parent company involved in an event e to evaluate this. All information is retrieved from the Orbis Corporate Ownership database. For each company involved in an event e I collect time-varying information. First, I measure the number of outstanding shares traded by each company at the end of the month before each event. Second, I measure market capitalization (computed as number of outstanding shares times closing price) on the day before each event for each company. Next, I retrieve information on the companies' revenues, asset value, and number of employees at the end of the solar year before each event. Finally, I measure the number of *Violation countries* for each event (meaning, the number of foreign countries where each company was alleged to have violated the FCPA). I then compute simple difference in means for these variables based on events where involvement was direct ($Subsidiary = 0$) and those where it was indirect ($Subsidiary = 1$).

Table B.1 reports summary statistics for these covariates across these two groups. It shows reassuring evidence that the two groups are balanced with respect at least to these important pre-treatment observable characteristics. All differences in their average values across the two groups are statistically insignificant with large p-values. The signs of the differences, moreover, are mixed

and not implying any consistent imbalance. For instance, companies involved directly tend to have larger market capitalization ($50.31 *vs* $43.99 billion) and are larger by assets ($119.48 *vs* $93.76 billion) but they tend to be smaller by revenues ($27.15 *vs* $30.74 billion) and number of employees (56.45 *vs* 84 thousands). In Figures B.1 and B.2, I show that the two groups are also balanced with respect to time-invariant characteristics including the headquarter country and the industry of activity – according to the 3-digits North American Industry Classification System (NAICS-3).

	Direct involvement (N=139)		Indirect involvement (N=125)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	p
Parent Outstanding Shares (billions)	1.52	2.92	1.49	2.45	-0.04	0.92
Parent Market Capitalization (billion USD)	50.31	84.01	43.99	59.70	-6.31	0.51
Parent Revenue (billion USD)	27.15	48.15	30.74	57.44	3.59	0.59
Parent Assets (billion USD)	119.48	389.14	93.76	268.01	-25.72	0.54
Parent No. Employees (thousands)	56.45	77.31	84.00	217.97	27.55	0.21
Number of violation countries	2.04	2.14	1.82	2.08	-0.22	0.42

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

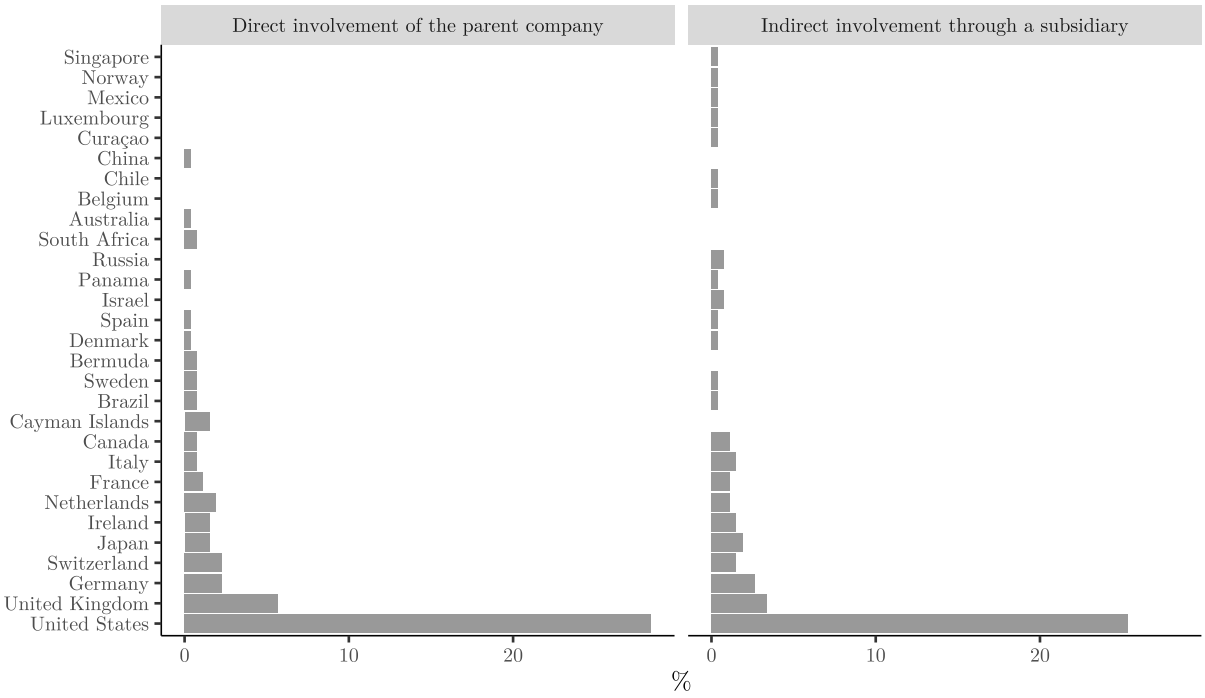


Figure B.1: Proportion of events involving companies by headquarter country, across cases of direct ($Subsidiary = 0$) and indirect involvement ($Subsidiary = 1$).

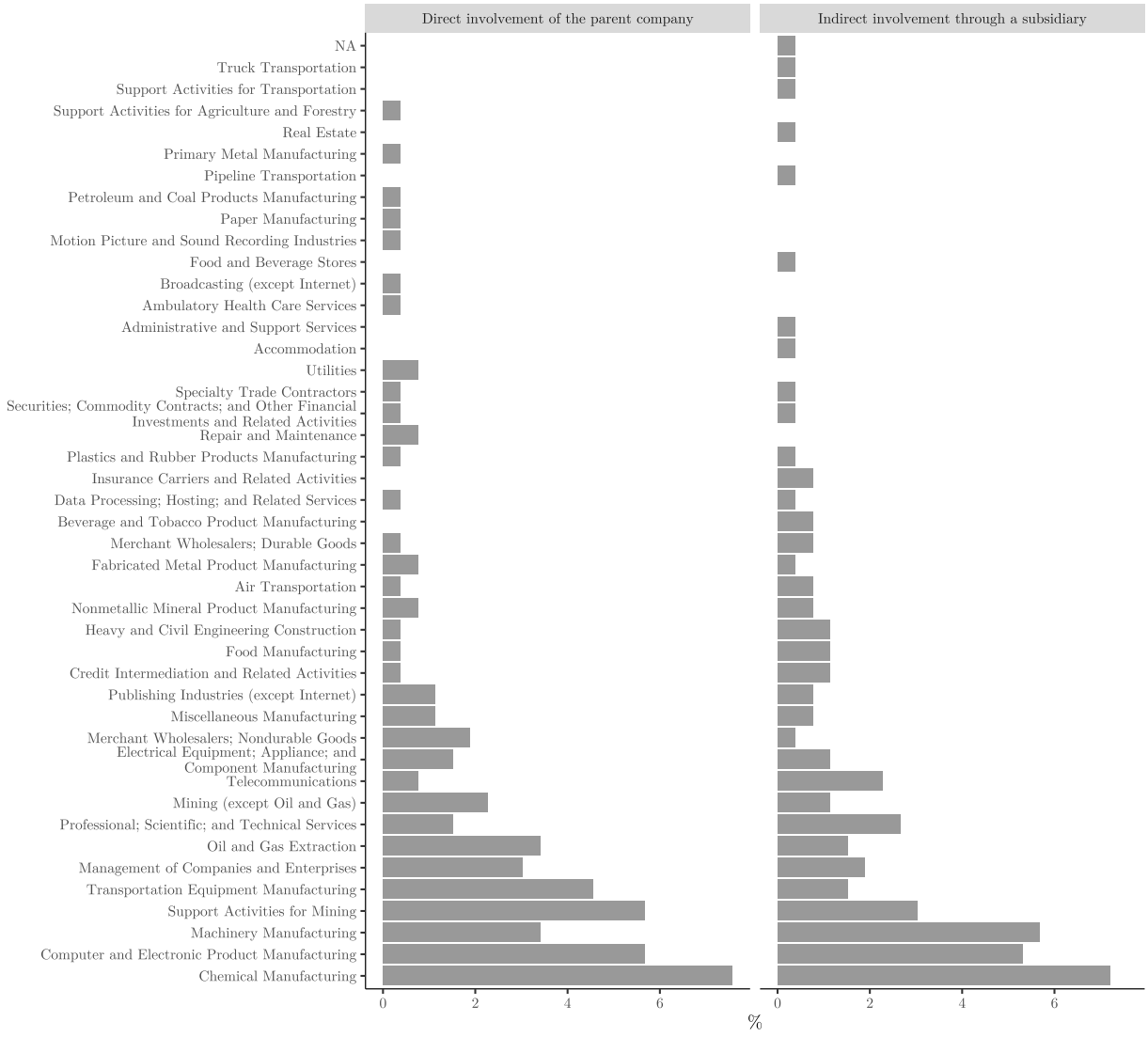


Figure B.2: Proportion of events involving companies by NAICS-3 code, across cases of direct ($Subsidiary = 0$) and indirect involvement ($Subsidiary = 1$).

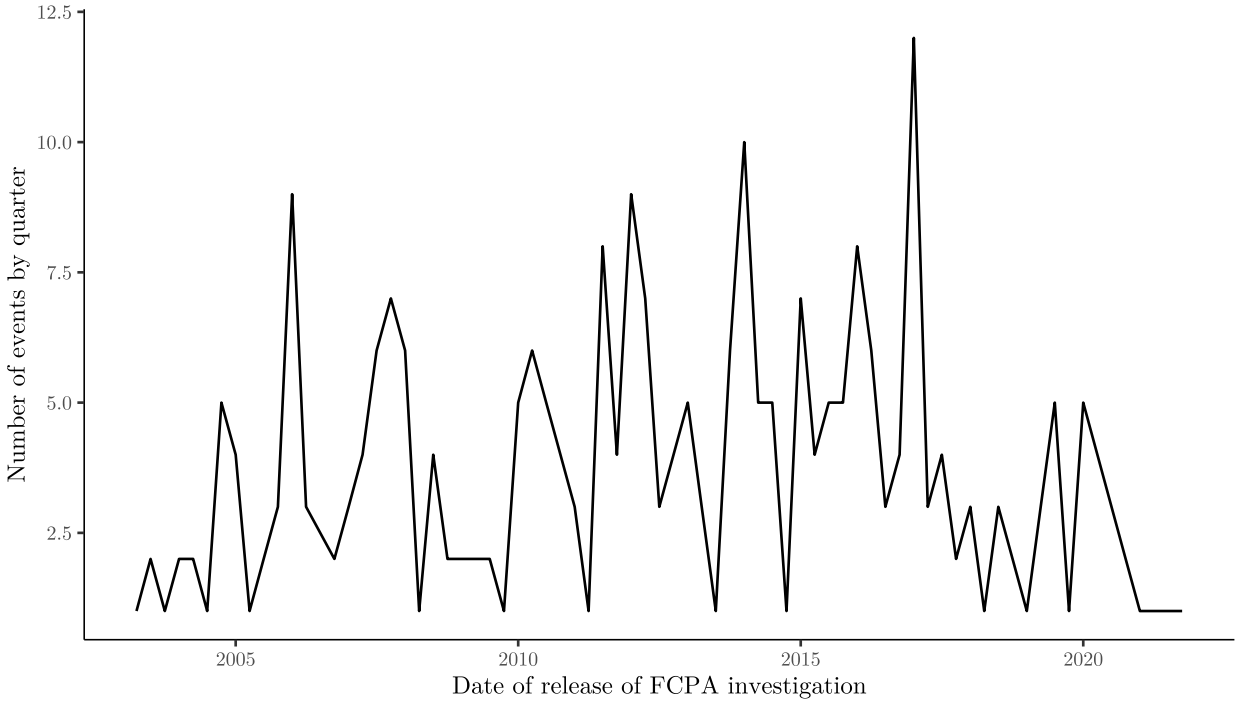


Figure B.3: Distribution of the 264 events of FCPA violation in the dataset, by date of release of news

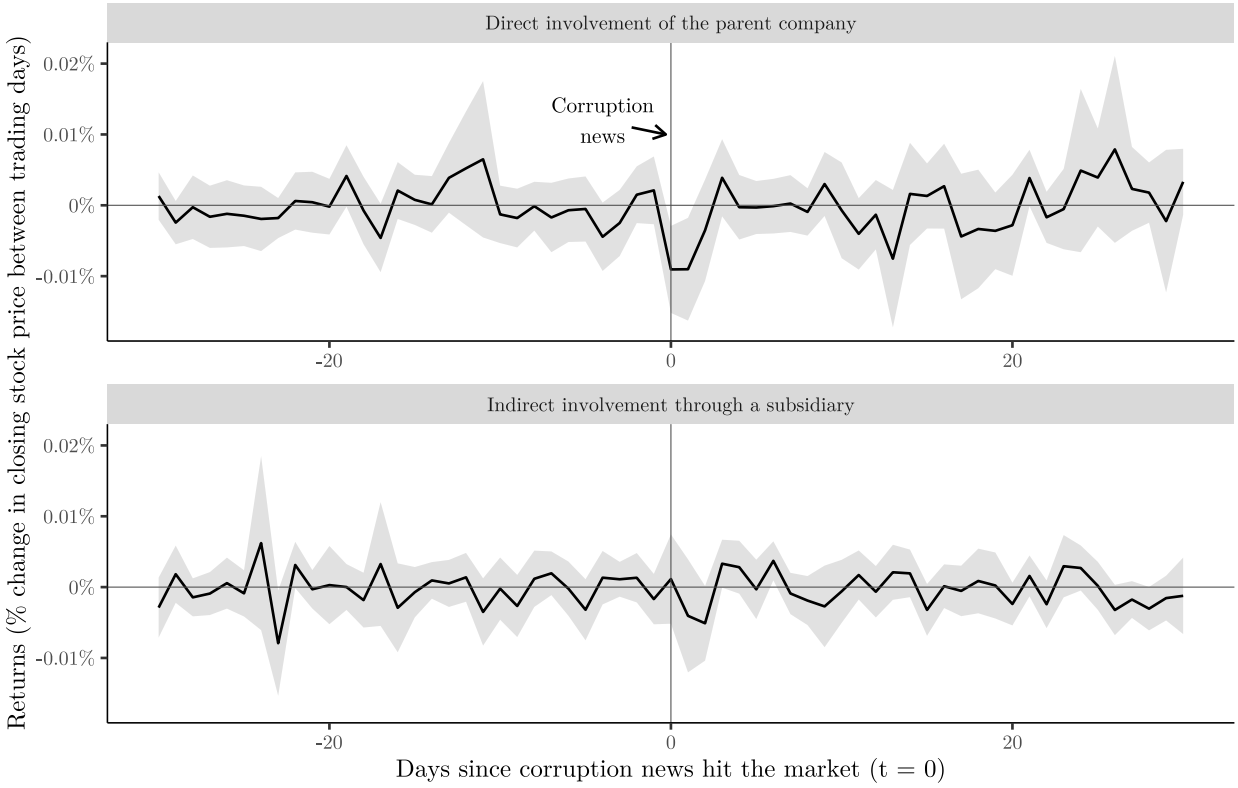


Figure B.4: Average *Abnormal Returns* in the 30 days before and after the release of corruption news, disaggregated by type of involvement

Note: Top panel presents direct involvement of a parent company, bottom panel reports involvement through a subsidiary. 95% confidence intervals around the sample mean are reported

Appendix 3.C LASSO-estimated synthetic counterfactuals

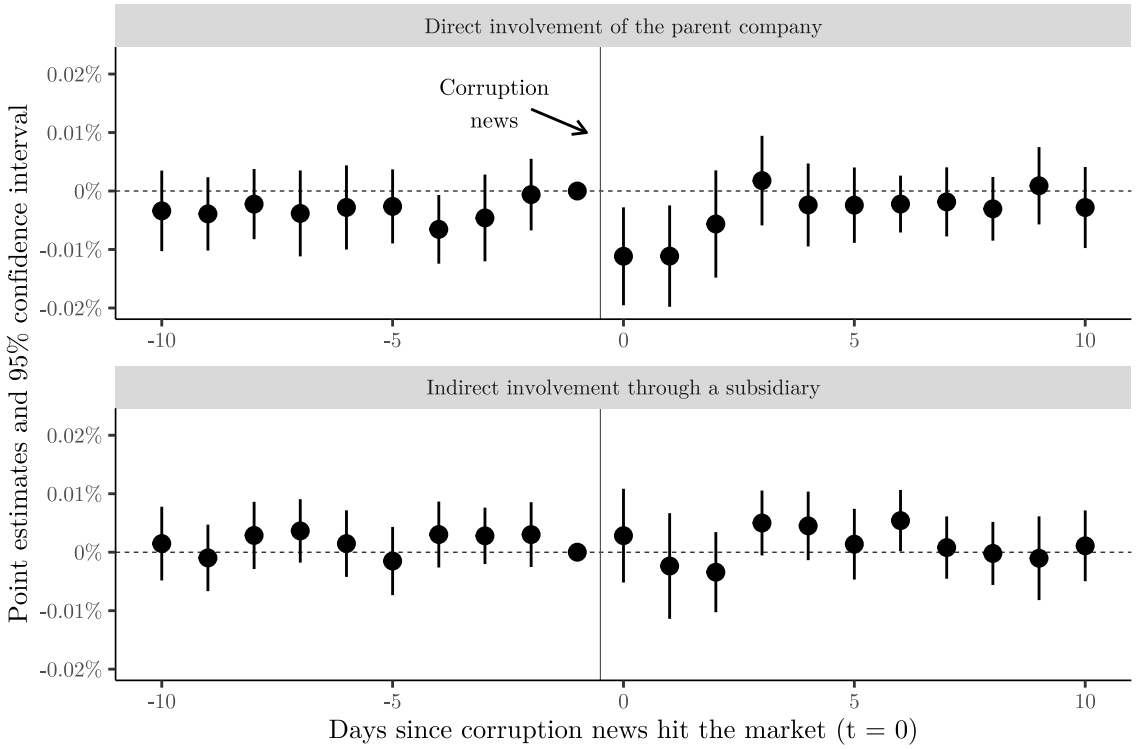


Figure C.1: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Sparse model

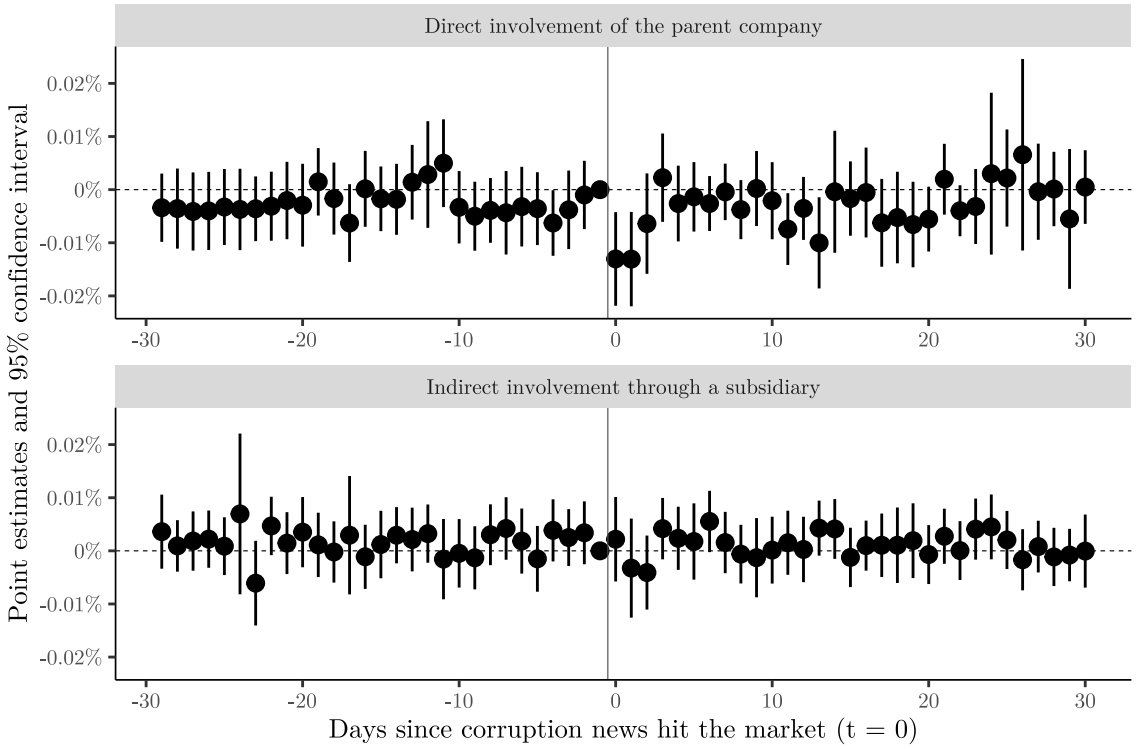


Figure C.2: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full model. Full *event window*

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

3.C.1 Cumulative Abnormal Returns

To quantify the cumulative effect of the $Event$, I model *Cumulative Abnormal Returns*, defined as the daily difference between *Observed* and *Expected Cumulative Returns* in the same fully-fledged event analysis proposed in Figure 3.7. Models include a categorical indicator for each trading day around the $Event$ and an event-fixed effect. Standard errors are clustered at the company-level. Estimated daily coefficients are reported in Figure C.4. In either typology of involvement, there seems to be no clear trend in the dependent variable before the event. In the case of direct involvement, post-event *Cumulative Abnormal Returns* are significantly lower than those of day -1 until at least day 18 after the event. On this day, observed cumulative returns are 0.04% lower what was expectable based on market models. When considering pre-event trading of the median company, this amounts to a loss in market capitalization of almost $517 million, when compared to the pre-event value, detectable more than two weeks after the event. Instead, no significant effect is ever detected for instances of indirect involvement.

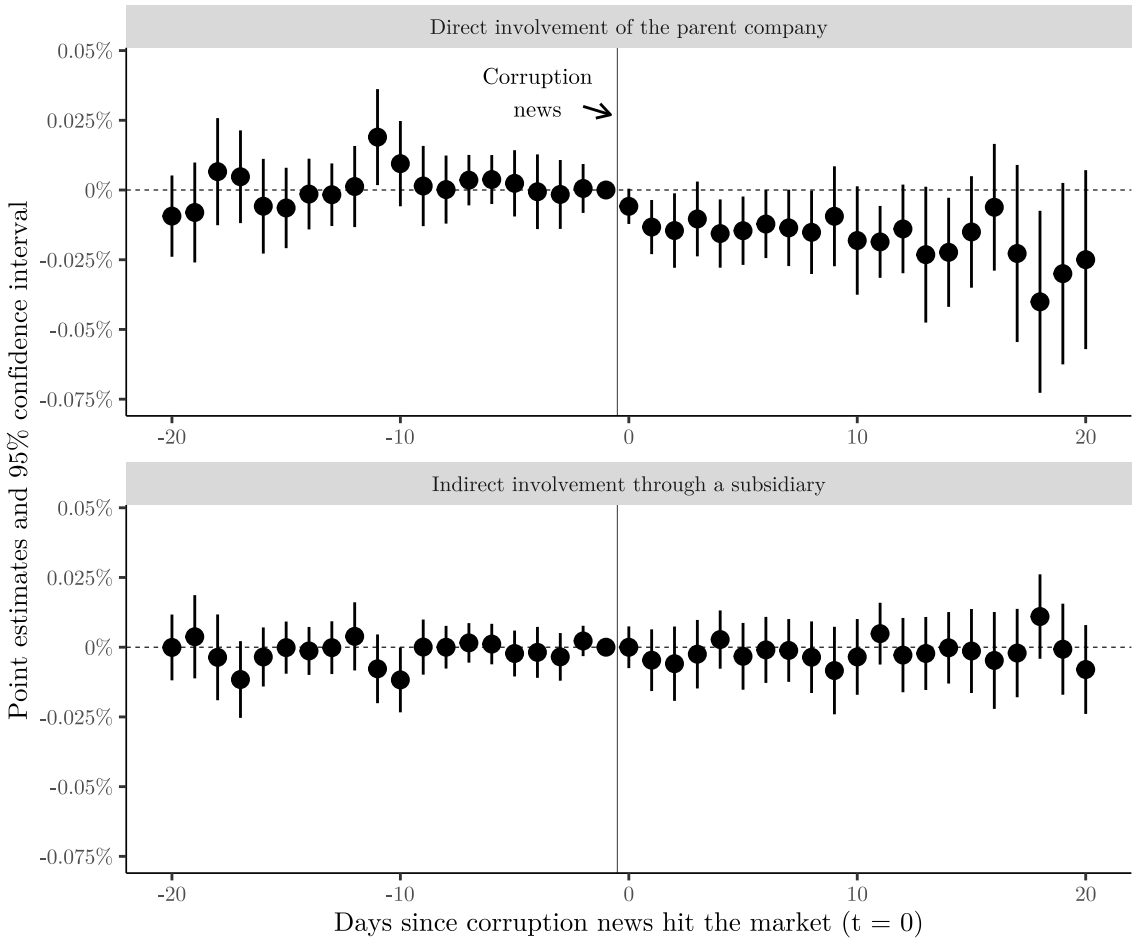


Figure C.4: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal

3.C.2 Abnormal Returns: Robustness tests

I perform extensive robustness tests on my findings. First, I rule out that results are driven by any single outlier (a scandal with significantly negative impact, or a particularly "bad" firm) in my data. I replicate my event analysis from Figure 3.7 multiple times, each time leaving one different event out of the model. I report point estimates and confidence intervals in Figure C.5 (alongside full-sample estimates for comparison). Second, I re-estimate the full model from Table 3.2 following the same leave-one-out approach. Figure C.6 reports estimated coefficients for the un-interacted $Event$ term and the interaction term $Event \times Subsidiary$, alongside their 95% confidence intervals.

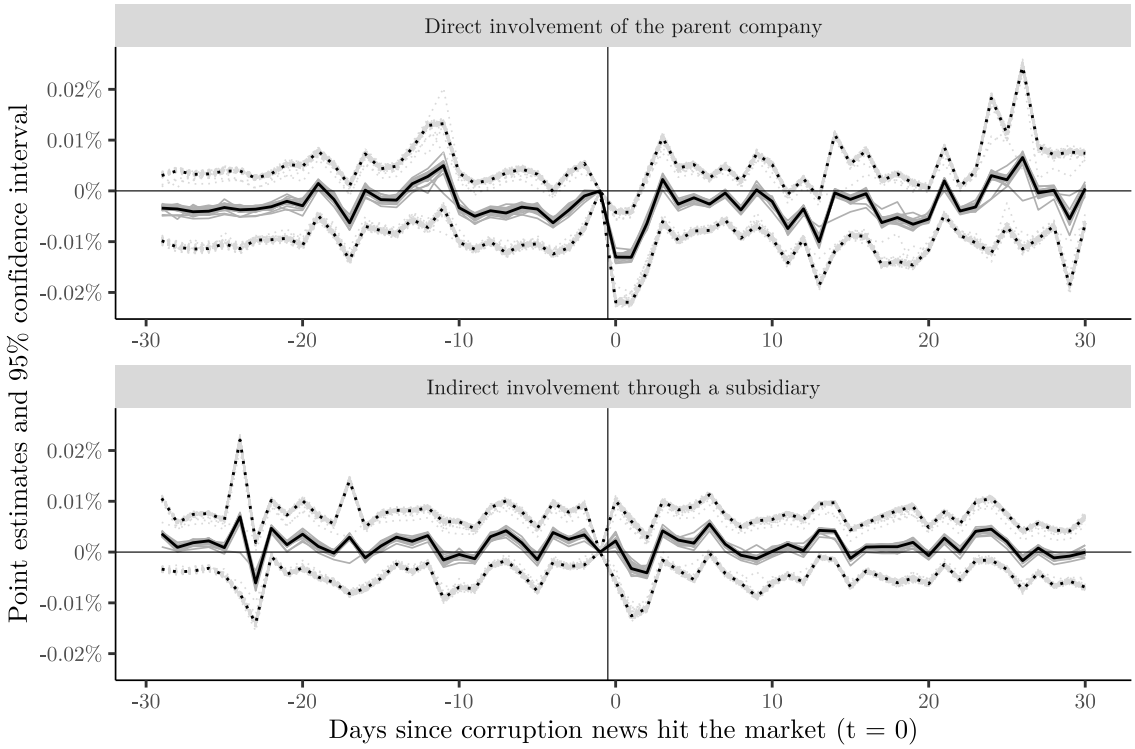


Figure C.5: Leave-one-out event-analysis in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full model. Full *event window*

Note: Plot reports point estimates and 95% confidence intervals obtained when excluding one event at the time from the dataset. Solid lines represent point estimates. Dotted lines represent lower and upper bounds of the confidence intervals. Grey lines represent estimates obtained when leaving one event out whereas black lines report full sample estimates for comparison.

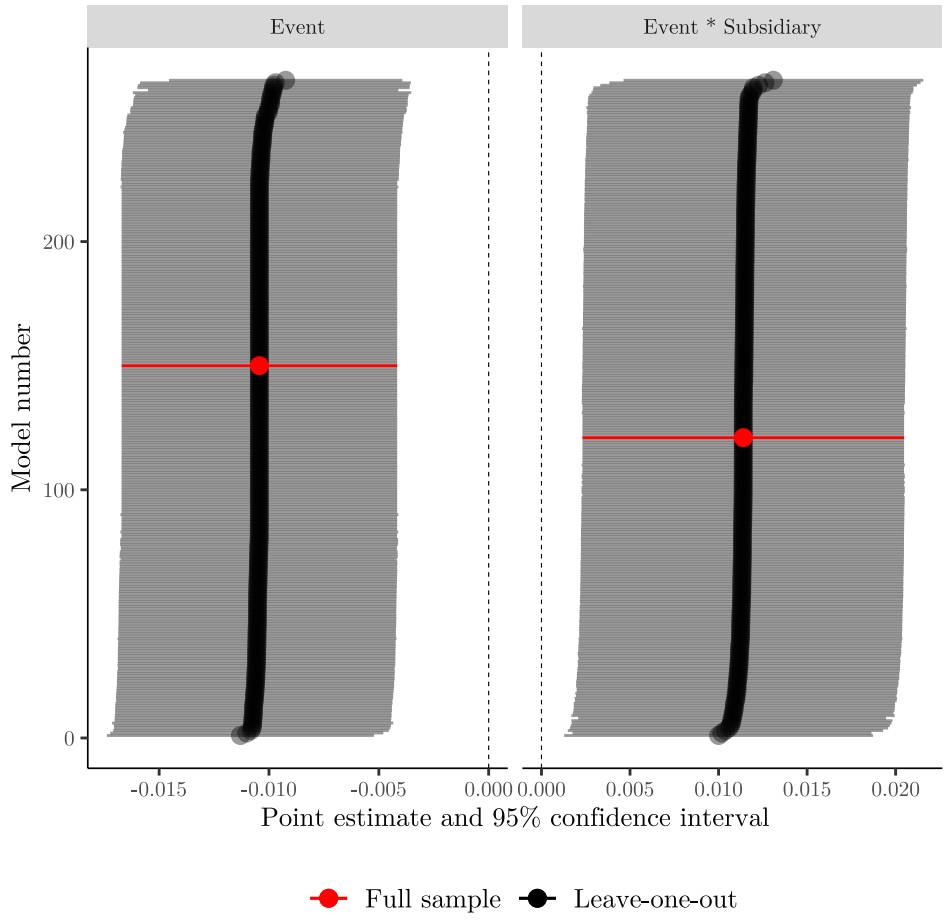


Figure C.6: Replication of model 4 from Table 3.2, leaving one event out of the dataset at a time

Note: Point estimates and 95% confidence intervals reported refer to the un-interacted *Event* term and to the interaction term $Event \times Subsidiary$. Red coefficients represent full-sample estimates from the main text.

Next, I address the potential concern that results are driven by arbitrary choices followed in the procedure. I replicate the entire analysis restricting my *event window* to the 10-days before and 10-days after the *Event*. This verifies results do not hinge on my arbitrary choice for the length of the time window, a conclusion that can also be drawn from the event analysis presented earlier (*e.g.* Figure 3.7). Results in Table C.1 from the same sparse and full models estimated above are consistent with my expectations. In a further test, I restrict *event window* data to the interval $[day\ -10,\ day\ 0]$, to make sure my binary treatment variable only compares *Abnormal Returns* on the day of the $Event$ to the immediate preceding days. Results are consistent with earlier findings (Table C.2).

	(1)	(2)	(3)	(4)	(5)	(6)
Event	-0.008**	-0.009***	-0.006*	-0.007***	-0.008**	-0.009***
	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)
Event × Subsidiary	0.010*	0.011***				
	(0.004)	(0.003)				
Event × Ownership			0.004	0.005*		
			(0.003)	(0.002)		
Event × Wholly-owned Subsidiary					0.013*	0.014***
					(0.006)	(0.004)
Event × Non-wholly-owned Subsidiary					0.006	0.007+
					(0.005)	(0.004)
Subsidiary	0.001	-0.015				
	(0.001)	(0.009)				
Ownership			0.000	-0.008		
			(0.000)	(0.005)		
Wholly-owned Subsidiary					0.000	-0.013
					(0.001)	(0.009)
Non-wholly-owned Subsidiary					0.001	-0.015
					(0.001)	(0.009)
Abnormal Returns (t–1)		-0.126***		-0.126***		-0.126***
		(0.017)		(0.017)		(0.017)
(Intercept)	-0.001	0.005	-0.001+	0.005	-0.001	0.005
	(0.001)	(0.007)	(0.001)	(0.007)	(0.001)	(0.007)
Event FE		Yes		Yes		Yes
Num.Obs.	3579	3422	3579	3422	3579	3422
R2	0.005	0.113	0.003	0.112	0.005	0.114
R2 Adj.	0.004	0.038	0.003	0.037	0.004	0.039
F	5.594	1.513	4.115	1.490	3.830	1.515

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

Table C.1: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Data limited to 10 days before - 10 days after the *Event*

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

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

Table C.2: Heterogeneous effects of corruption scandals on parent companies' stocks, conditional on involved entity nature. Data limited to 10 days before the *Event* and the event day

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 3.1 yielding an R-squared lower than 0.10. This restricts the analysis to a subset of 189 companies involved in 235 events. I replicate my entire analysis and verify results are consistent (Table C.3). I also report event-analysis estimates using a sparse and full model (Figures C.7 and C.8).

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

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

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

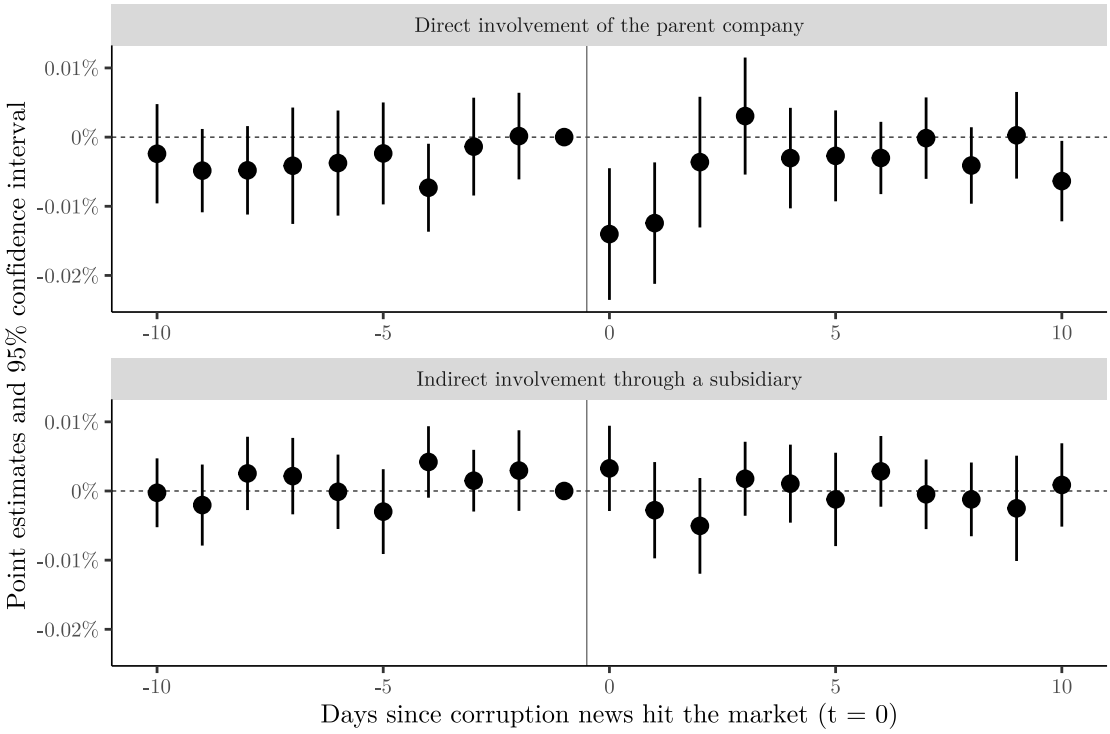


Figure C.7: Event-analysis design in the 20 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Data limited to events with precise counterfactual estimation. Full model

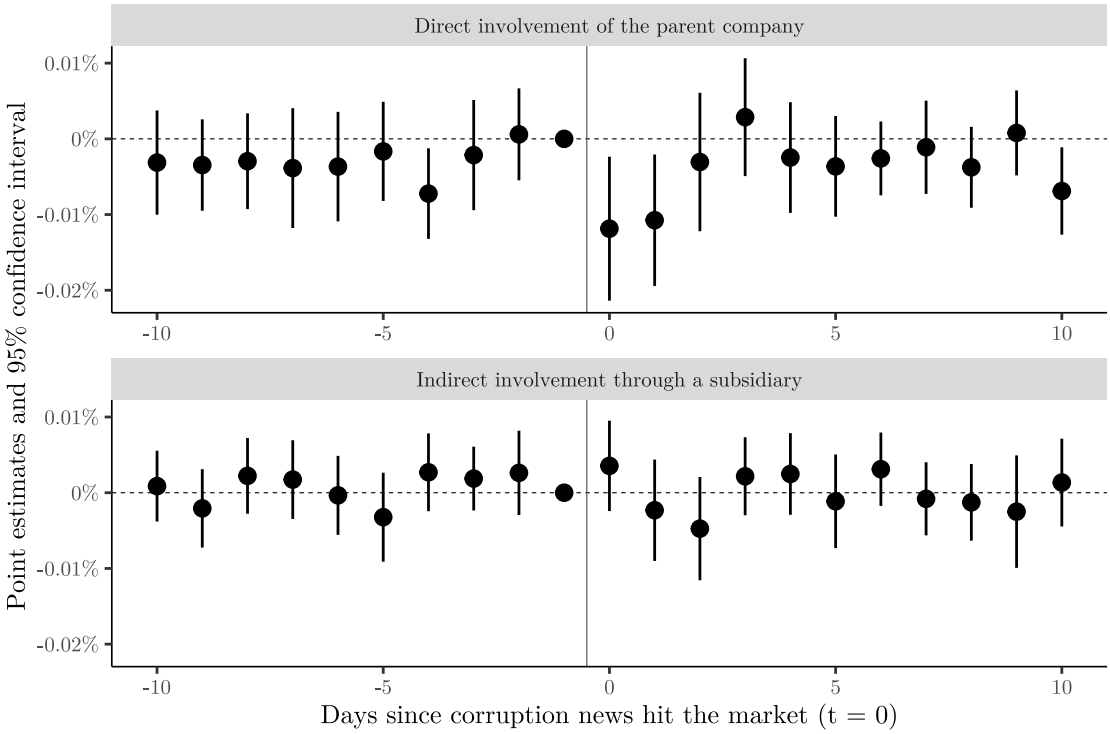


Figure C.8: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Data limited to events with precise counterfactual estimation. Sparse model

Appendix 3.D Typologies of indirect involvement

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

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

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

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

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

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

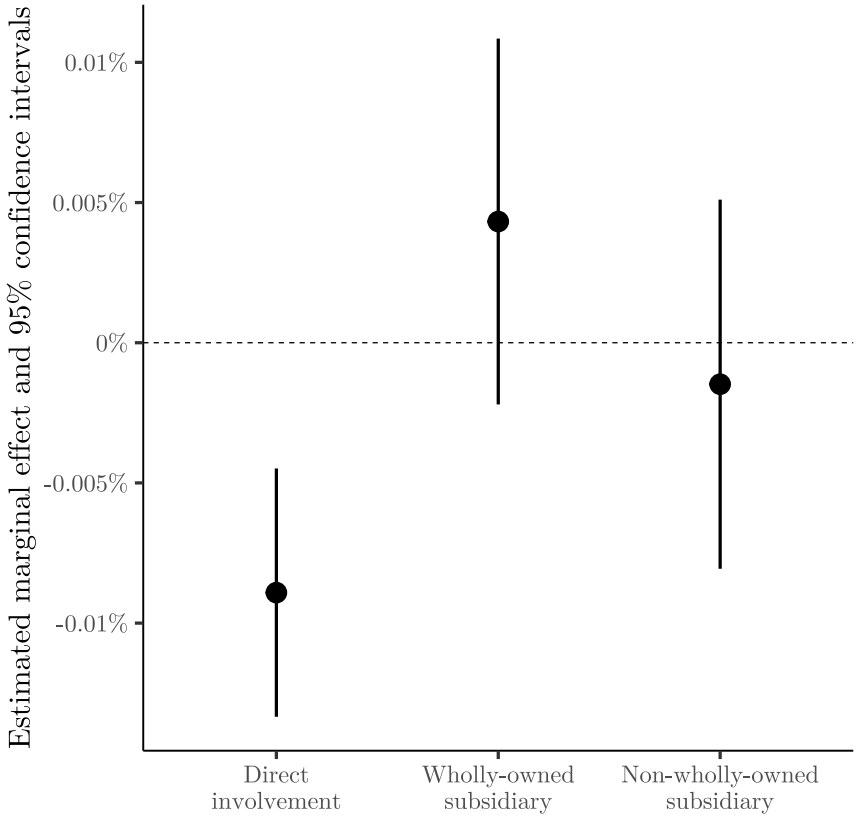


Figure D.1: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly, through a wholly-owned subsidiary, or through a majority-owned subsidiary

Note: Results from sparse model of Table D.2

Appendix 3.E OLS-estimated synthetic counterfactuals

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

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

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

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

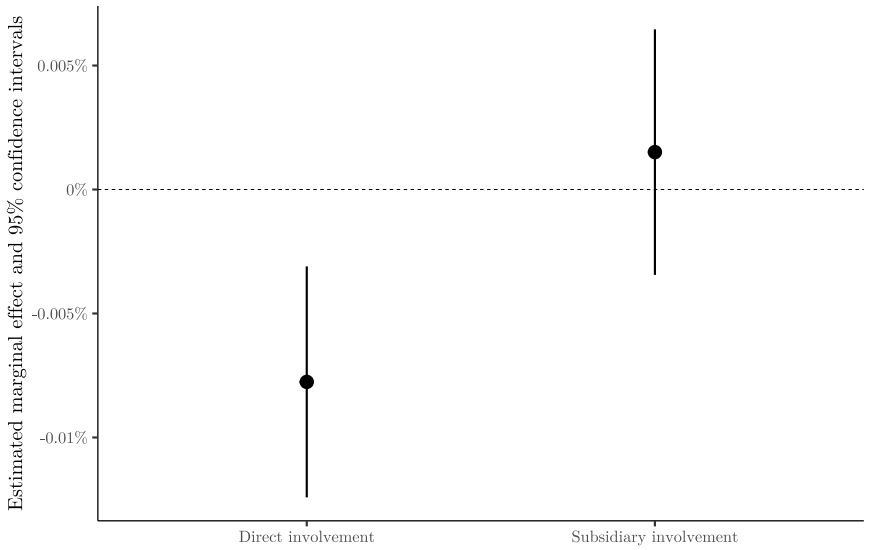


Figure E.1: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on whether the company is involved directly or through a subsidiary. OLS-estimated counterfactuals

Note: Results from model 1 of Table E.1

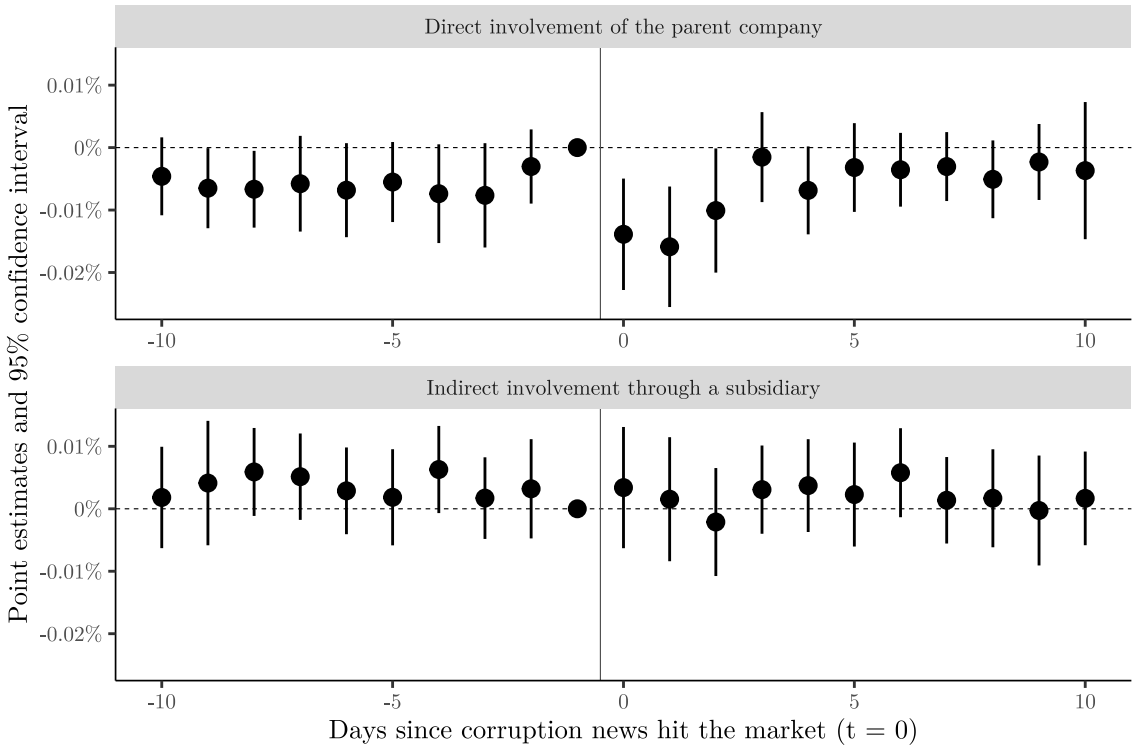


Figure E.2: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Full model. Full *event window*. OLS-estimated counterfactuals

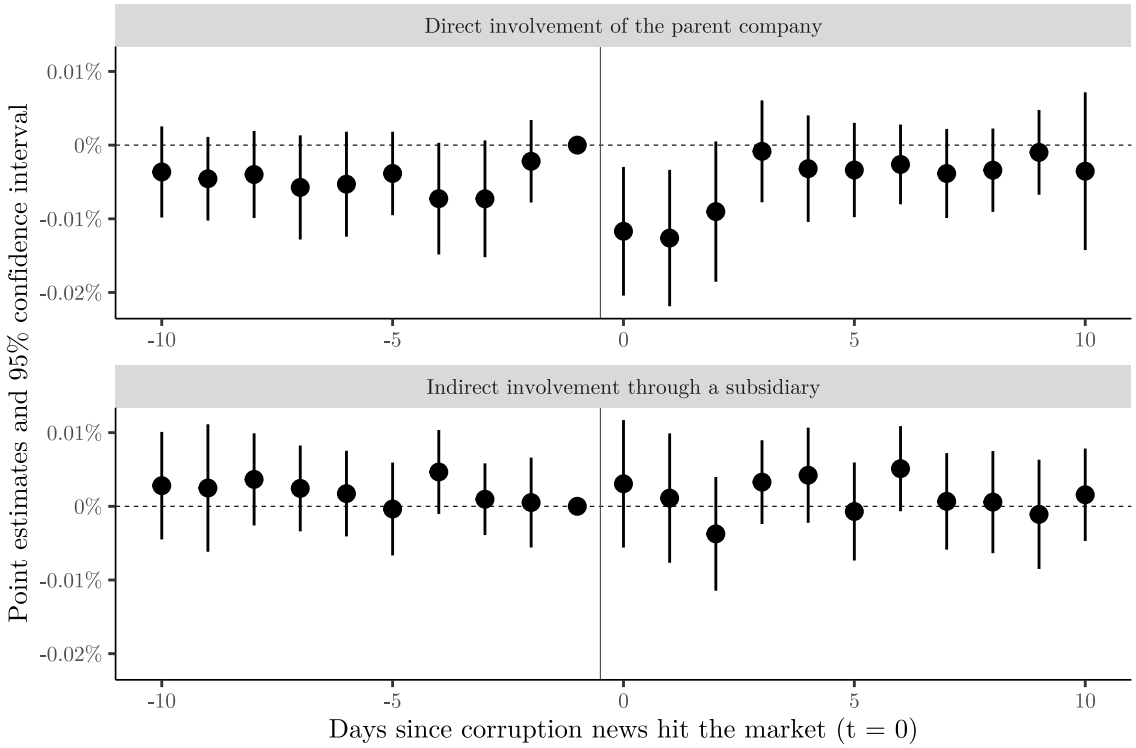


Figure E.3: Event-analysis design in the 60 days around the publication of corruption news, conditional on direct or indirect involvement of the parent company in the scandal. Sparse model. Full *event window*. OLS-estimated counterfactuals

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

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

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

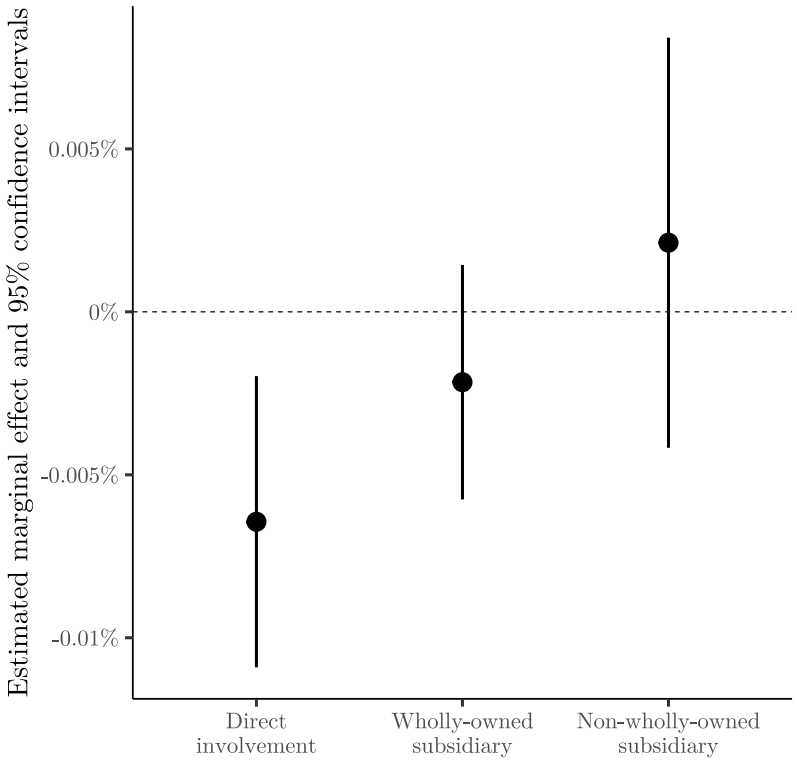


Figure E.4: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on the degree of ownership by the company of the subsidiary

Note: Results from model 1 in Table E.2. OLS-estimated counterfactuals

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

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

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

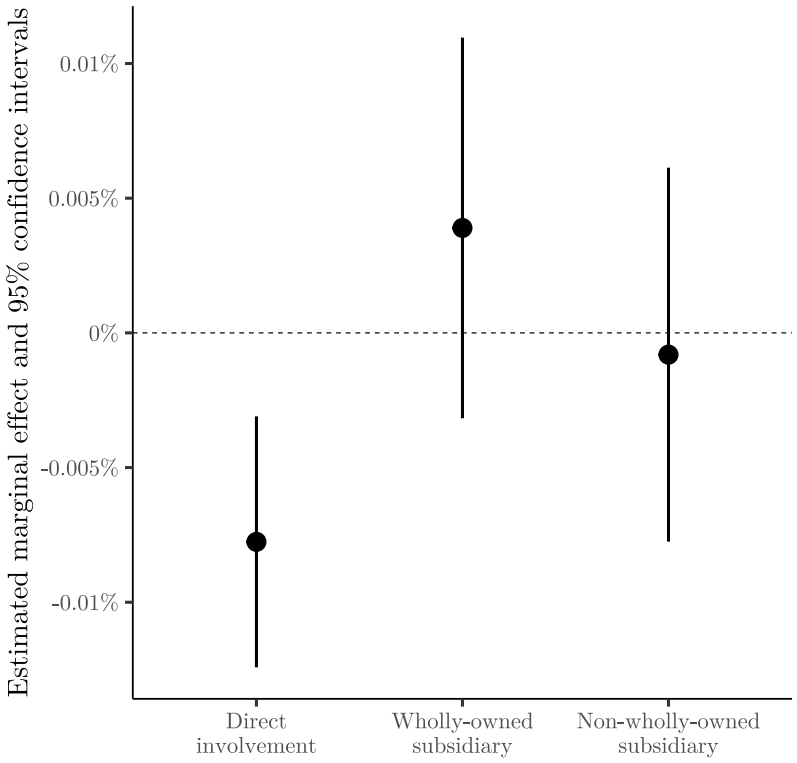


Figure E.5: Marginal effect of a corporate corruption scandal on the involved parent company's *Abnormal Returns*, conditional on the degree of ownership by the company of the subsidiary

Note: Results from model 1 in Table E.3. OLS-estimated counterfactuals

Conclusion

This dissertation moves from the acknowledgment that crime committed by multinational companies (MNCs) challenges state sovereignty, as the exercise of coercive powers within a territory (Thomson, 1995). Corporations can fragment their operations in networks of cross-border subsidiaries and shell companies that conceal illicit transactions from state regulators (Cooley and Sharman, 2017; Findley et al., 2015; Sharman, 2010) or exploit loopholes between legal regimes (Arel-Bundock, 2017; Chapman et al., 2020). Can states wield regulatory tools over crime committed across elusive corporate networks? The dissertation addresses this question. It analyzes the anti-corruption international regime and proposes a tentative conclusion that sovereign states can wield regulatory tools to prevent multinational companies' crime, even when it takes place across borders and in complex corporate networks. Broadly speaking, this result is in line with a body of literature claiming that states can cast important rules over the globalization of business transactions (Frieden, 1991; Hirst et al., 2015; Jensen, 2005). It also aligns with studies showing that countries can exploit cross-border networks of private economic actors for coercive purposes (Crasnic et al., 2017; Farrell and Newman, 2019, 2022; Kalyanpur and Newman, 2019). If anything, the main regulatory failure documented in the dissertation is on the side of markets, who are often presumed to behave as a "global civil society" and complement states' regulatory action (Fukuyama, 2016; Ruggie, 2018).

The first paper investigates whether criminal regulations imposed by states on their companies' foreign activities undermine their investment abroad, a position that would imply regulation is politically challenging (Kapstein, 1989). It shows that, in fact, states can diffuse regulations abroad, by leveraging connections between domestically incorporated companies and their foreign

subsidiaries, without necessarily undermining their business. In the case of anti-corruption policies, regulated companies are shown to benefit from regulations imposed by their home country. Anti-bribery policies cut down corruption-induced costs and enhance investment in countries with mid-levels of corruption such as Brazil, China, Indonesia, Italy, Mexico, and the United Arab Emirates. Regulated firms' investment is undermined only in extremely corrupt economies like Egypt, India, Kazakhstan, Nigeria, or Russia. Therefore, home-imposed regulations do not necessarily undermine foreign business and are politically sustainable.

The second paper focuses on one specific tool sovereign states can weild to address multinational corporate crime: extraterritorial regulations. It asks why states with powerful extraterritorial regulations (that is, states that have the authority to prosecute foreign subjects) apply these policies against specific foreign companies (Putnam, 2009). It argues that regulators leverage *territorial* connections of foreign companies to their country in order to apply *extra*territorial legislation. It claims that foreign companies with a physical presence on the soil of regulators' countries are more easily targeted by authorities because they are more exposed to the local public. Therefore, they are incentivized to cooperate with authorities upon discovery of misconduct in order to mitigate reputational damage. The paper focuses on US-based regulators applying the main anti-bribery US statute, the Foreign Corrupt Practices Act (FCPA), against non-US firms' corruption. It finds that foreign companies with an investment on US soil are almost twice as likely to be investigated by US agencies than comparable foreign firms with no similar presence. Therefore, MNCs' fragmentation of operations across borders is not necessarily a regulatory liability. In fact, it can be leveraged as a coercive asset.

Finally, the third paper studies one tool that potentially complements states' regulatory action: informal reputational penalties imposed by markets upon firms for involvement in criminal scandals. Similar penalties are documented for firms involved in money laundering (Morse, 2022), negative environmental, social, and governance news (Capelle-Blancard and Petit, 2019), and human right violations (Kreitmeir et al., 2020). However, investors fragment operations in complex corporate networks so as to conceal criminal conduct from the eyes of regulators (Sharman, 2010). The

paper studies whether such networks conceal criminal behavior from investors, too. I answer that fragmented corporate structures shield a parent company's reputation from that of its subsidiaries. Whereas direct involvement of a company in a scandal damages its reputation, involvement through a subsidiary generates negligible losses. I study stock market performances of firms involved in corporate scandals for violations of the US FCPA. I find that companies involved directly in anti-corruption scandals suffer financially relevant and sustained damages to their stock prices. However, no effect is found when they are involved in scandals through subsidiaries. The paper thus concludes the dissertation by noting that informal penalties imposed by markets work only as a partial complement to states' regulation, given that they do not seem to bite against the main tool to further corporate crime: fragmentation of ownership.

Methodologically, the dissertation proposes an important novelty against shortcomings found in studies of corruption that rely on perception-based measures (see critiques in Gueorguiev and Malesky, 2012; Heywood and Rose, 2014; Olken, 2009; Treisman, 2007). In the three papers, I rely on data about observable cases of corruption to support my arguments. In paper 1, the main index of corruption – the Public Administration Corruption Index (PACI, see Escresa and Picci, 2017) – is computed by leveraging a dataset on observable cases of cross-border bribery. This measure exploits differences in the *distribution* of corruption cases involving the set of partners of country x to infer their relative corruption level.

Similarly, papers 2 and 3 propose a novel dataset on publicly known judicial cases under the international anti-bribery regime. To my knowledge, this represents the first automatically-scraped dataset of judicial cases not limited to a single country[27]. This granular dataset is defined at a violation-firm level, allowing information to be combined with widely used firm-level data sources – such as Orbis, Bloomberg, fDi Markets, or similar. This represents a valuable contribution. It could support further studies on the political economy of bribery and anti-bribery at the micro-level of the very actors involved in such criminal transactions and their prosecution[28].

[27]For examples of datasets limited to the application of the US FCPA, see `https://fcpa.stanford.edu` or `https://violationtracker.goodjobsfirst.org/prog.php?primary_offense_sum=Foreign+Corrupt+Practices+Act`.

[28]I plan on making this dataset, and the code that automatically compiles it, a publicly accessible resource through

Paper 2 exemplifies the potential of this data source. It uses this novel dataset to explain how US authorities selected which foreign cases of corruption to investigate, applying their extraterritorial prerogatives, out of the set of publicly known violations of the anti-bribery regime. It explains selection as a function of the foreign company's presence in the US, by leveraging connections between this novel data and a widely used source of firm-level information such as Orbis. The use of public cases of corruption is also central in paper 3, whose interest is the study of investors' response to public corruption events. This paper connects the dataset with granular information on companies' daily stock prices (source: Refinitiv Workspace) and ownership structure (source: Orbis).

At the same time, data on observable cases of corruption pose some limitations to the study, that circumscribe its validity and indicate potential novel avenues for future research. For instance, the PACI index in paper 1 rests on the assumption that observing a case of cross-border corruption involving companies from country x and public officials in y does not depend on features of country y other than its level of corruption and its bilateral economic relation to country x. This assumption is mitigated by not considering cases that are brought to the public only by country y, but is nevertheless heavy. The paper acknowledges this assumption and shows results are robust to the usage of traditional perception-based indexes – such as that proposed by V-Dem (Coppedge et al., 2020). Similarly, empirics in paper 2 assume that cases selecting out of the data because all informed authorities chose not to investigate them are not systematically different in terms of companies' US presence. That is, willing selection of non-US cases out of the set of observed ones is independent of the US presence of involved companies.

This limitation represents a fruitful potential avenue for future research. Future investigations could bolster the conclusions from the three papers by supplying list experiments and survey experiments to company representatives or judicial authorities, so as to overcome issues of social desirability bias and lack of information on corruption. The technique has been recently proposed

its GitHub repository. Possibly, I would associate it with packages in R and Python to automatically import information in users' sessions. As a name for the dataset and the project, I propose to use the acronym DETrACT, as in: Dataset on the Enforcement of Transnational Anti-Corruption Treaties.

by studies specifically in the case of corporate corruption, with promising results (see Jensen and Malesky, 2018; Malesky et al., 2015)

Furthermore, important scope conditions of the study define its external validity. An important condition concerns the type of criminal transactions studied. The dissertation has focused exclusively on bribery, which is but one of the many forms corruption can take (Heywood and Rose, 2014). The study has also not focused on other forms of corporate crime such as tax evasion, money laundering, environmental crime, or violations of human or labor rights. It also did not focus on negative corporate social responsibility actions that do not amount to criminal behaviors.

The generalizability of my conclusions to other forms of crime or misbehavior must be therefore considered carefully. Namely, the anti-bribery regime presents features that other regimes might lack. First, an international anti-bribery regime has now been in existence for almost three decades. Other regimes, such as the anti-tax evasion, are much younger[29]. Moreover, the anti-bribery regime is sustained by a range of international agreements at various multilateral levels (including the OECD, UN, and EU) where common anti-bribery principles, rules, and norms (Krasner, 1982) have been adopted. These agreements include the world's main headquarters of MNCs, therefore the main exporters of corruption (Picci, 2018). The regime is also supported by the vigorous regulatory action of US agencies (Kaczmarek and Newman, 2011; Tomashevskiy, 2021). These agencies have, by now, an experience with anti-bribery laws that dates back to almost 50 years. They have specialized units entirely dedicated to the enforcement of anti-corruption policies (Brewster, 2014). Other regimes lack the vigorous action performed by a similar "global sheriff", located at the core of its design. Finally, fines and monetary penalties imposed under this regime are a concerning expected cost for investors who hold stocks of companies in violation of anti-corruption standards (Garrett, 2011; Sampath et al., 2018). At least under US-security law, firms are mandated to report investigations to their investors, thus enabling the naming-and-shaming mechanism to operate, within certain limits. A similar mechanism of disclosure is just being proposed by the US

[29]Consider, for instance, the much more recent OECD/G20 Inclusive Framework on Base-Erosion and Profit-Shifting (BEPS), aimed at fighting corporate tax avoidance.

Securities and Exchange Commission (SEC) for reducing companies' emissions[30]. It is still too early to study its effects.

Moreover, a large part of the study focuses on the application of the US anti-bribery policy. This represents a forced choice, given the centrality of the US anti-corruption law, the FCPA, within the international anti-bribery regime. However, conclusions drawn from papers 2 and 3 might need important adjustments before travelling to other countries. US regulators can easily leverage the centrality of their economy to target foreign companies with subsidiaries on their soil (paper 2). This strategy might not be feasible for other countries, located at the margins of the international economic architecture – although recent studies point out that minor economies, *e.g.* Nigeria, might also be able to behave in a similar fashion under specific circumstances (see Crasnic et al., 2017). Similarly, US-security laws require companies to disclose information of potential relevance to investors (paper 3). Similar arrangements are not in place even in advanced economies, thus the effect might not hold in different contexts.

Despite its limitations and scope conditions, the dissertation advances an important question from three distinct but interlinked perspectives. The papers' conclusions give rise to interesting future research avenues. Future studies could draw on the regulatory mechanisms analyzed here (*e.g.* the empowerment/deterrence argument from paper 1, the liability of foreignness from paper 2, and the shield of ownership from paper 3) and generalize them beyond anti-bribery, to study if and how they apply to other forms of corporate misconduct. Important lessons could thus be drawn for literatures that deal with international regimes including anti-tax evasion (Arel-Bundock, 2017; Thrall, 2021) and anti-money laundering (Findley et al., 2015; Morse, 2019; Sharman, 2010).

Relevant further questions emerge from the study. I show that states do not lose on their sovereignty *vis-à-vis* transnational corporate crime. In fact, they command tools to prohibit it that leverage the very multinational corporate networks. However, evidence that countries defect from their duties to prevent corporate crime abounds (Jensen and Malesky, 2018; Ruggie, 2018). Why do countries fail to hold companies accountable for foreign misconduct? Answering this question

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

would allow a rare opportunity to join together a study on corporate crime with the debate on state compliance with international law (Chayes and Chayes, 1993; Simmons, 1998; Von Stein, 2005).

Secondly, the study opens up to important questions concerning reputation. Reputation plays an important role in justifying the theoretical mechanism of paper 2 and an even larger role in paper 3, where it is the central topic of study. Paper 3, in particular, shows that companies can protect their reputation by hiding misconduct behind obscure layers of corporate ownership. This provides an important contribution to a literature claiming that reputation can be an important driver of compliance with international norms (Baradaran et al., 2012; Downs and Jones, 2002; Simmons, 2000). Damages to reputation of private actors can be considered as a significant deterrent to induce compliance with international norms, but these agents seem to have tools for mitigating financial harm. Future studies could leverage recent innovations in the corporate transparency regime[31] to investigate if markets perform a more thorough regulatory function when they are provided with more detailed information on companies' corporate structures, or else whether "information overload" un-intendedly hinders this desired regulatory mechanism (as a growing business literature in accounting seems to suggest, see Beerbaum, 2016; Laud and Schepers, 2009; Neumann et al., 2012).

Finally, the study opens up to questions on other states' responses to the projection of sovereignty by leveraging private multinational networks. I show that countries can extend their legal reach abroad to affect foreign multinational companies. This demands an evaluation of how other states and their public opinions respond to similar projections of sovereignty, particularly in issue-areas where rules are not harmonized under the umbrella of common international regimes. Do standards imposed on regulated companies abroad spread to foreign unregulated competitors? Are public opinions abroad sensitive to this coercive strategy? Do "global sheriffs" substitute or complement the activity of foreign agencies devoted to similar regulatory tasks? Conclusions from the study inform a range of similar questions.

[31] See, for instance, recent innovations across the US and Europe: `https://www.whitecase.com/publications/alert/four-major-changes-corporate-transparency-2022`.

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