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

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Abstract

Countries prohibit firms' transnational financial crime by coordinating their regulations under international organizations (IOs). Under these IOs, states threaten to prosecute firms' foreign misconduct at home. Such threats can help conscript companies to diffuse sustainable business models abroad. This paper studies the effect of corporate criminal regulations on firms' foreign direct investment (FDI). Critics of these policies claim they push firms' investment away from host economies where financial crime is more likely to happen. Yet, regulations should also cut informal costs of crime and favor investment. I reconcile these opposed expectations and show they are special cases of the same argument. I claim that the effect of multilateral anti-bribery policies on FDI 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 legal leverage to refuse paying bribes and cut corruption costs. The effect is negative where corruption is endemic: here, anti-bribery laws expose firms to additional regulatory costs. I support the argument with multiple evidence. Company-level data on investment by 3871 firms between 2006 and 2011 show that regulated corporations have a 27% higher probability of investing in moderately corrupt economies than unregulated firms, which plummets to −52% in extremely corrupt countries. A synthetic counterfactual design using country-dyadic FDI flows corroborates this finding. Results show that regulatory policies harmonized by IOs change international competition for FDI in ways that do not necessarily harm regulated firms.

Keywords: Foreign direct investment; corporate regulations; international organizations; corruption; OECD Anti-Bribery Convention

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1 Introduction

Corporate crime consists of complex transactions taking place across borders. A multinational company (MNC) can bribe in a market outside its home country's jurisdiction to circumvent local competition and extract rents (Malesky et al., 2015). Bribe payments can be paid through bank accounts located in several countries (Cooley and Sharman, 2017). Criminals, then, can conceal illicit funds in jurisdictions with poor money-laundering standards (Sharman, 2011).

Countries coordinate the regulation of such complex transnational flows by adopting common rules under international organizations (IOs) (Keohane, 1984). Members of the Organization for Economic Cooperation and Development (OECD), for instance, agreed on common anti-bribery policies in 1997 (Abbott and Snidal, 2002). Similarly, the Financial Action Task Force coordinates 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). By creating these IOs, states extend the arm of their laws beyond borders (Kaczmarek and Newman, 2011) to prohibit foreign misconduct by companies incorporated in their jurisdictions. They conscript firms under their regulatory umbrella to diffuse sustainable corporate standards, often by threatening prosecution at home.

How do corporate criminal regulations affect firms' legitimate activity such as foreign direct investment (FDI)? This question remains unanswered. Political economy expectations are twofold. First, policies would raise additional costs for regulated companies—*i.e.*, firms whose home countries impose regulations against foreign crime—thus *deterring* investment. For instance, home countries' anti-bribery laws would increase risk of investing into corrupt host countries, where exposure to bureaucrats' bribe requests is higher (Gueorguiev and Malesky, 2012) and so is the risk of prosecution (Cuervo-Cazurra, 2008). Yet, an opposite hypothesis expects that corporate criminal regulations *empower* firms' foreign investment. They would force companies to keep business above board thus cutting costs induced by uncertainty of criminal practices in countries that otherwise lack regulatory standards. For instance, anti-bribery provisions can tie companies' hands and force them to refuse bribe requests, cut down costs of corruption, and operate more efficiently (Perlman and Sykes, 2017). IO-sponsored regulations would thus offer companies an advantage when investing into countries with lax business standards.

In this paper I propose a single argument to unify these two expectations which I label, respectively, *deterrence* and *empowerment*. I study the effects that corporate regulations against foreign bribery have on FDI. I argue that *deterrence* and *empowerment* are observable when considering investment into host countries with different corruption levels. Whereas *deterrence* is observable in extremely corrupt hosts, *empowerment* dominates in moderately corrupt ones. Anti-bribery policies reduce firms' incentives to participate in bribery deals (Jensen and Malesky, 2018) by adding regulatory costs to such transactions. In doing so, regulations tie firms' hands and incentivize them to secure business opportunities legally at lower costs (Davis, 2011). This effect modifies firms' expected utility in a potential host country—and

thus the likelihood that they will decide to invest there—in a direction that depends on its corruption level. This is so because bribery provides firms with greater rents in more corrupt economies (Ades and Di Tella, 1999). In moderately corrupt hosts, bribery offers perks that are not more lucrative than opportunities achieved legally. Thus regulation ties firms' hands to refuse bribe requests, and operate more efficiently, without losing business. In extremely corrupt hosts, instead, bribery is frequent (Zhu, 2017) and refusing to take part in it means that regulated firms lose access to exclusive rents (Malesky et al., 2015). Here, regulated firms expect a lower utility and are less likely to invest.

Empirically, I study laws under the 1997 OECD Anti-Bribery Convention 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 to invest in a foreign location between 2006 and 2011. I show that firms under the OECD Convention make investment decisions that depend non-linearly on the level of corruption of the host economy. Firms from ratifiers are no more likely than their unregulated competitors to invest in non-corrupt economies. They are up to 27% more likely to invest in moderately corrupt host economies. Instead, they are 52% less likely to invest in *extremely* corrupt destinations. This offers evidence 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 identify the proposed effect more credibly. Results confirm findings from the firm-level analysis.

The paper offers three distinct contributions. First, I show that regulations do not necessarily place a burden on companies' FDI. I therefore speak to research on the effects of corporate regulations for international business. In the first place, this contributes to studies about the effect of anti-corruption policies on FDI. To the best of my knowledge, I offer the first attempt at reconciling two competing expectations (see Cuervo-Cazurra, 2008; Davis, 2011). I show that home countries' anti-corruption policies create complex interactions with host markets' institutional characteristics—as argued by Beazer and Blake (2018)—and alter firms' business conditions: they can be both a liability and an asset. Beyond anti-corruption, this conclusion contributes to studies of firms' regulatory preferences (Ahlquist and Mosley, 2021; Genovese, 2020; Kalyanpur and Newman, 2019; Kennard, 2020).

Second, I use the anti-corruption case to show that IOs can alter the behavior of MNCs. This offers the chance to renew academic interest in classic international political economy questions: whether, and by what means, international institutions affect behaviors of private transnational companies (Gray, 2009). Vast scholarship has addressed these questions by studying the effectiveness of IOs regulating licit transactions on foreign investment. Studied examples include arbitration in investment disputes or institutions protecting investors' rights (Allee and Peinhardt, 2011; Betz et al., 2021; Biglaiser and DeRouen, 2010; Neumayer and Spess, 2005; Skovgaard Poulsen, 2014; Tobin and Rose-Ackerman, 2011). I approach this topic from a different angle and study the effect on foreign investment generated by

international corporate *criminal* laws and IOs keeping economic exchanges above board. In doing so, I document a form of policy diffusion where home countries negotiate common corporate standards under an IO umbrella and conscript domestically-incorporated companies to diffuse them abroad. Areas of global governance where states operate similarly include the prevention of money laundering, tax evasion, human rights violation, or environmental degradation (Putnam, 2009).

Finally, I contribute to the literature on IO effectiveness in pursuing global governance goals. I study IO effectiveness by looking at the behavior of firms under their umbrella (see Abbott and Snidal, 2010; Baradaran et al., 2012; Findley et al., 2015; Morse, 2019; Thrall, 2021). I offer a faceted perspective on whether anti-corruption IOs can reduce corruption. Ultimately, I find that effectiveness depends on the institutional context of the countries where firms under the IO umbrella operate. Findings that anti-corruption IOs favor companies' investment into mid-range corrupt economies are good news for ensuring sustainable business models through multilateral negotiations. Because firms under anti-bribery laws are deterred from offering bribes (Jensen and Malesky, 2018), this motivates some optimism for the prospect of curbing (the international supply of) corruption without undermining investment.

However, conclusions are pessimistic for host countries with severe corruption levels which would perhaps need anti-corruption IOs to elicit a positive effect the most. I find that regulated firms tend not to invest, here. These countries are left exposed to investments from unregulated firms who can arguably commit felonies, remain unpunished, and reinforce existing levels of corruption. 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 poor practices among member states (Ferry et al., 2020; Hafner-Burton and Schneider, 2019).

2 The effect of anti-bribery laws on foreign investment

2.1 The effect of foreign bribery on foreign investment

The literature on FDI and political risk argues that a "parent" firm decides to invest abroad—and establish ownership of a foreign "subsidiary"—if doing so maximizes its expected utility. Expected utility maximization drives investment choices because an investment is relatively immobile *ex-post*, thus the firm evaluates expected future benefits against sunk costs (Dunning, 1980). Economic, political, and institutional features of the host drive firms' utility expectations (Danzman and Slaski, 2022; Jensen, 2008; Jensen et al., 2012; Pandya, 2016; Pond, 2018). Bribery is among them (Busse and Hefeker, 2007).

Bribery[1] is an informal exchange between a firm and a public official. In a stylized bribery deal, the public official demands a payment in exchange for the discretionary award of a service—*e.g.*, a contract

[1]In the article, I consider exclusively *foreign* bribery, where the bribe payer and payee are of different nationalities. Foreign bribery is documented in the registration of MNCs and procurement (Gueorguiev and Malesky, 2012) and is frequent when investing into corrupt host economies (Chen et al., 2008; Della Porta and Vannucci, 1999; Zhu, 2017).

in public procurement (Rose-Ackerman, 1975). The firm would leave the deal if it could obtain the same service by not bribing. The bureaucrat wants to maximise the fee and would award the service to the firm's competitors if the offered bribe were too small. Each actor's power to extort or turn down bribe requests increases if it can leave the deal while still deriving what the counterpart was offering.[2]

Firms have a weaker power to turn down bribe requests in more corrupt countries.[3] Two corruption-induced factors result in this outcome: exclusive access to rents and number of potential bribers. First, in more corrupt economies bribe-payer firms gain a monopolistic position (Malesky et al., 2015) which allows them to extract rents that could not be derived by other means (Pinto and Zhu, 2016; Zhu, 2017). This is so because corrupt economies are more concentrated and public officials can offer exclusive deals to bribe-payers (Ades and Di Tella, 1999). Bribery ensures the monopolistic access to public contracts such as the exploitation of natural resources (Knutsen et al., 2017) or construction deals.[4] Second, in more corrupt economies bribery is more frequent (Treisman, 2007). If a firm refused to take part in a corrupt deal, bureaucrats would more easily find alternative bribers (Lambsdorff, 2002). Thus, firms enjoy a weaker power to turn down bribe requests.

When deciding whether to invest in a corrupt country, a firm evaluates the expected utility deriving from taking part in such deals. It invests if expected bribery costs are smaller than bribery-induced benefits. A large literature has debated whether corruption favors or harms FDI, with very mixed results (Barassi and Zhou, 2012; Egger and Winner, 2005; Zhu and Shi, 2019). In this paper, I remain agnostic on this debate and simply claim that the firm expects higher utility if it anticipates that it will manage to turn down bribe requests—while obtaining the same service it would have bribed for—or if it will bribe and extract rents larger than what it paid. Rather than discussing the effect of corruption on FDI, per se, my goal is to derive testable implications on how home countries' anti-bribery laws *change* firms' foreign investment choices in corrupt economies. I derive these implications in the next section.

[2]This description is certainly very abstract: parties' choices to take part in the deal are likely affected by other, more complex factors. For instance, the firm might find it easier to turn down bribe requests if it threatened to divest and if the host country's divestment costs were large. Viceversa, the public official might find it easier to advance bribe requests if the firm could not find comparable alternative hosts. I do not include these relevant factors in my argument in order to keep my focus on the key explanatory variables here (home anti-bribery laws and host corruption). This choice also ensures consistency with my analysis, which cannot properly model these other factors. Ultimately, this means that I assume that divestment costs for the firm and host country, or the presence of alternative hosts, do not vary with the home country's adoption of anti-bribery laws. However, note that Perlman and Sykes (2017, 166-168) discuss how the effect of anti-bribery regulations might be conditional on the existing investment position of the firm.

[3]In the text, I adopt a very narrow definition of what constitutes a more or less corrupt country. I define a country's level of corruption solely in terms of how frequent bribe payments are in its economy.

[4]An example is offered by the alleged $180 million in bribe payments paid between 1995 and 2004 by the TSKJ joint venture to Nigerian government officials in order to obtain $6 billion worth in contracts for the construction of natural gas facilities on Bonny Island in Nigeria. Involved companies that formed the consortium were Technip (French), Snamprogetti (Dutch, but owned by the Italian ENI), KBR (owned by Halliburton, US), and JGC (Japanese). All companies have settled allegations for bribery with US and other national authorities between 2004 and 2011. See: https://fcpaprofessor.com/jgc-of-japan-formally-joins-the-bonny-island-bribery-club/ and https://www.traceinternational.org/TraceCompendium/Detail/192?type=1. Both accessed on May 18, 2023.

2.2 The effect of anti-bribery laws on FDI

How do anti-bribery policies imposed by the home country affect firms' expected utility in a corrupt economy? I present my answer in this section. Corruption affects the expected utility (thus, the investment decision) depending on whether the company can turn down bribe requests or pay the bribe cost and extract rents. My argument considers exclusively how anti-bribery laws alter a firm's expected bribery cost and power to turn down bribe requests because rents from corrupt deals depend on institutions of the host country—most importantly, its level of corruption (Ades and Di Tella, 1999; Malesky et al., 2015)—and should not change with the adoption of anti-bribery laws by the home country.

In a nutshell, I argue that anti-bribery policies deter firms' participation to bribery deals by increasing their cost. They tie firms' hands and offer regulated firms an option to secure business opportunities legally, at lower costs. This leverage improves firms' expected utility only where bribery offers relatively poorer perks than those that can be achieved legally, *i.e.*, in less-corrupt economies. Here, refusing to bribe does not prevent access to profitable opportunities. The opposite occurs in extremely corrupt hosts, where bribing guarantees exclusive rents. Regulated firms' investment decisions in host countries with different corruption levels reflect this change in expected utility.

With anti-bribery policies, home countries threaten prosecution at home for companies under their jurisdiction engaging in foreign bribery. They increase costs of bribing in all settings (Cuervo-Cazurra, 2008). Next section describes the large fines and settlements levied by law enforcers since the 2000s. But costs are not limited to penalties.[5] For instance, authorities mandate costly plans to restructure firms' organization and monitor compliance with anti-bribery standards[6] (Garrett, 2011).

I describe how these costs factor into investment choices in three possible scenarios. In each scenario a firm evaluates whether to set up an investment in a foreign country. The first scenario considers a highly corrupt economy. The second describes a moderately corrupt economy. The final scenario considers a non-corrupt economy. I theorize the effect of anti-bribery regulations on the probability of an investment by comparing the utility expectation of a regulated firm and that of its unregulated counterfactual.

Scenario 1: High corruption levels in host countries. The home country's anti-bribery policies deter the investment choice in the scenario of a highly corrupt host.[7] The regulated firm would risk anti-bribery prosecution at home if it took part in bribery deals, resulting in costs that it would not have faced without regulation. The expected corruption perks, instead, are the same. In expectation,

[5] Markets impose additional sanctions. Foreign bribery scandals have wide international resonance. An estimated 80% of every lost dollar in a firm's share value is due to market-based responses to such stories (Sampath et al., 2018). However, such reputational costs need not vary with the OECD Convention signatory status of involved companies' home countries. That is, a firm headquartered in a non-signatory might be exposed to reputational costs for involvement into bribery as much as a comparable firm headquartered in a ratifier of the Convention, if news of corruption are made public.

[6] Suspect firms usually have to set up internal monitoring systems run by third-parties (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.

[7] According to my data, highly corrupt economies include countries like Kazakhstan, Nigeria, Vietnam, or Russia.

therefore, bribery is less profitable for a regulated firm. Refusing to bribe would save corruption and regulatory costs. However, refusing to bribe in very corrupt economies would also likely imply the loss of access to the rents offered by corruption (Malesky et al., 2015) which would not be easily obtained by legal means. Thus the regulated firm expects either to pay higher cost for the same gains or to lose access to corrupt rents. As such, regulated firms expect a significantly lower utility than they would have without regulation when evaluating whether to invest in very corrupt economies. Regulated firms will thus be less likely to invest in countries where bribery is entrenched in the business opportunities.

Scenario 2: Mid corruption levels in host countries. The regulated firm is, instead, more likely to invest in the second scenario—that of a moderately corrupt[8] host—than its unregulated counterfactual. Here, too, the regulated investor expects regulatory costs for partaking in bribery deals. However, because bribery here does not provide significantly more lucrative opportunities than those achieved legally, anti-bribery policies put the regulated firm in a position to resist possible bribe requests without losing business (Davis, 2011). Home-country regulations thus enhance the expected utility of regulated firms because they remove bribery costs. In the absence of regulation, the firm would not have enjoyed this prerogative and it would have experienced a lower utility.

Perlman and Sykes (2017) studied the US anti-bribery policy—the Foreign Corrupt Practices Act (FCPA)—by conducting interviews with corporate and legal practitioners that offer perhaps the most complete description of how invoking anti-bribery laws can help companies avoid bribe requests. They report that "Chinese government agents know about the FCPA and know that bribes requested from American companies will not be provided. [...] Similarly, [...] in Russia [...] the FCPA made it easier to avoid bribes by explaining to corrupt officials that it would be impossible to withdraw the necessary cash without detection" (Perlman and Sykes, 2017, 170). Thus anti-bribery laws improve firms' power to turn down bribe requests by making them less frequent and by providing them a "hands-tied" argument out of a bribe request.

Of course, regulated firms' expected utility improves only if they do not risk losing business opportunities by not taking part in bribery deals. Because public officials can award the corruption perks to alternative bribers, if a regulated firm refused to pay the bribe, regulation increases utility only if the size of the exclusive advantages offered by corruption is low. I argue that a similar condition occurs in moderately corrupt economies. Here, corruption offers comparatively less lucrative perks than what can be obtained by legal means (Ades and Di Tella, 1999). Thus, regulated firms expect larger investment benefits than they would have experienced in the absence of regulation: they would find it easier to turn down bribe requests without losing significant business opportunities.

[8]Moderately corrupt economies in my data include Italy, Mexico, Singapore, or Taiwan.

Scenario 3: Low corruption levels in host countries. Finally, in a scenario where the potential host country is non-corrupt[9] the regulated firm is no more or less likely to invest than its unregulated counterfactual. Here, it is unlikely that local public officials will demand bribe payments at all. Because bribe requests here do not likely occur, the regulated firm would not expect different investment conditions than it would have without regulation.

So far, I have assumed vigorous law enforcement. In fact, some evidence suggests that anti-bribery laws might yield effects on FDI independent of that. Using 1980s FDI data, Hines (1995, 19) found a "relative decline of American business activity in the more corrupt countries after 1977" that is, right after the adoption of the US FCPA. As described in the next section, the FCPA was significantly underenforced in the 1980s: prosecutors brought just 40 cases in the twenty years after 1977 "and settled these charges on sympathetic terms" (Brewster, 2017, 1614). The fact that the FCPA had negative effects on US companies' business into more corrupt economies even in a period of "enforcement silence" (Brewster, 2017, 1645) suggests that the adoption of anti-bribery laws might still change companies' investment decisions, regardless of enforcement levels. Perhaps, the uncertainty about future intensity of law enforcement and the long time-horizon entailed by an FDI change regulated firms' investment *as if* the law were vigorously enforced.

In general, however, expectations of future law enforcement are an important condition determining whether policies have an effect on investment. If firms expected lax anti-bribery law enforcement, a regulated firm would not anticipate higher bribery costs. Thus, it would be no more or less likely to engage in bribery than without regulation. Similarly, expectation of lax enforcement would make anti-bribery regulations a weak hand-tying leverage. Thus, an expected lax enforcement should weaken the effect of anti-bribery on FDI. Conversely, expectations of vigorous law-enforcement should reinforce the effects of anti-bribery laws.

Figure 1 generalizes my expectations beyond the three scenarios. It sketches the effect of anti-bribery policies on the probability of a foreign investment (y-axis) at increasing levels of corruption of the host (x-axis). For low levels of corruption of the host, the effect should be null. As the host economy becomes more corrupt, regulation *empowers* firms and increases their probability to invest. When the level of corruption increases, this effect reaches a maximum, decreases, and reverses. In extremely corrupt host countries, anti-bribery policies *deter* firms' investment. *Deterrence* and *empowerment* are thus observable at different levels of corruption of the host.

Research on the political economy of FDI typically explains the choice of investment locations as a function of their political characteristics (Pandya, 2016) and my argument abides by this tradition. It describes how home corporate regulations interact with host country corruption to determine the decision to invest in a location.

[9] In my data these countries include Canada, Denmark, or Sweden.

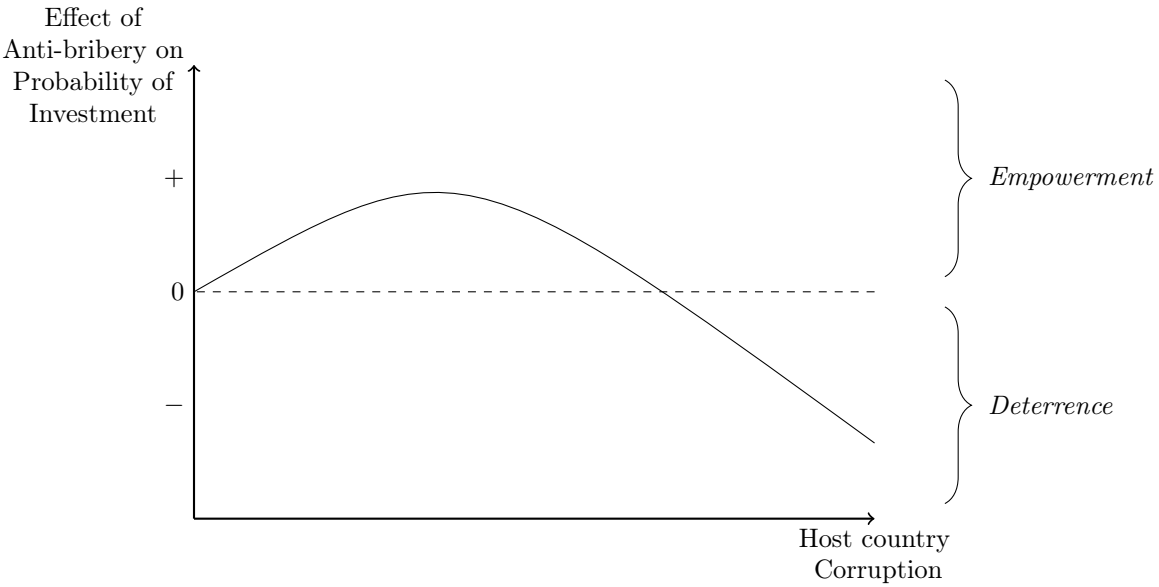


Figure 1: Expected effect of anti-bribery laws on investments, conditional on host country corruption

To some extent, anti-bribery laws can also similarly affect firms' decisions on the investment *amount*. A firm would be willing to invest more if it expected larger returns, net of foreseeable costs (Dunning, 1980). The risk of anti-bribery prosecution increases the expected costs of corruption for regulated companies investing in very corrupt economies, whereas rents offered by corruption likely do not vary with regulations adopted at home. Expected returns, net of foreseeable costs, will thus be lower for regulated firms investing in very corrupt economies and so will be the size of their investment. Conversely, in moderately corrupt economies regulated firms expect larger returns because they can cut down on costs induced by corruption without losing profits. Here, regulated investment should be larger.

This should also hold if firms already have investments in a foreign corrupt country, representing sunk costs. Market exit is costly for firms with existing sunk costs (Barkema et al., 1996; O'Brien and Folta, 2009), so investing larger amounts might be relatively cheaper. This might make these companies relatively indifferent to the presence of home anti-bribery regulations. However, Perlman and Sykes (2017) notice that anti-bribery laws work similarly to investment treaties in that they protect investors who already have sunk costs in a country from opportunistic attempts by host bureaucrats to expropriate bribes. This seems to imply that even firms with existing investment in a country would make decisions on investment amounts that are influenced by anti-bribery laws consistent with my argument.

3 The OECD Anti-Bribery Convention

Although laws against domestic corruption have a long history, policies that forbid companies from paying bribes abroad are more recent. The US was the first country to prohibit foreign bribery when, in 1977, Congress passed the Foreign Corrupt Practices Act (FCPA) as a response to the discovery of bribe

payments made by several major US companies abroad.[10] However, enforcement of the FCPA lagged for two decades because non-US competitors lacked similar regulations (Brewster, 2017). Until 1997, about half of the OECD countries even endorsed such payments by making them tax-deductible (Gutterman, 2015). In this context US administrations feared enforcing the FCPA would have tilted the playing field of international competition against US-based companies.

In the late 1970s and 1980s the US attempted to secure an anti-corruption international treaty in order to level the playing field of international business. Attempts made at the United Nations, the International Chamber of Commerce, the OECD, and the Tokyo Round of the General Agreement on Tariffs and Trade failed to secure anything more than non-binding recommendations (Brewster, 2017): "Because other governments understood that Congress could not undo the FCPA, the United States had no interest-based leverage" (Abbott and Snidal, 2002, 162).

Western countries' reluctance to adopt anti-bribery rules failed in the 1990s, when a shift in norms and corruption salience made an OECD anti-bribery treaty inevitable[11] (Abbott and Snidal, 2002). In this decade, scandals of corruption hit Western public opinion (Tarullo, 2004) and "various NGOs [...] demanded that OECD governments confront the consequences of their policies of tolerance of [...] foreign corruption by their multinational corporations" (Brewster, 2017, 1641). The US strategically used issue salience to secure a major international anti-corruption treaty at the OECD by leveraging European officials' fear of public criticism. Allegedly, the US Assistant Secretary of State for Economic and Business Affairs at the time "carried with him (or told people he did) a list of the 10 largest bribe-paying companies in the world. When officials became recalcitrant, he would tap his jacket pocket, suggesting he could make the list public." (Abbott and Snidal, 2002, 164).

The OECD Anti-Bribery Convention was ratified in 1997, initially signed by 34 countries including five non-OECD members.[12] Ten more countries have ratified the treaty since 1997.[13] Membership covers a disproportionate share of the global economy: MNCs under the regulatory umbrella of the Convention account for more than 80% of global outbound foreign direct investment stocks. They include 95 of the 100 largest non-financial enterprises and the 50 largest financial companies (OECD, 2018).[14]

The Convention is an instrument of hard-law (Abbott and Snidal, 2000). Articles 1 through 4 legally bind ratifiers to adopt policies that prohibit companies, foreign employees, subsidiaries, and individuals

[10] Arguably, the most notorious case involved Lockheed: https://www.washingtonpost.com/archive/business/1977/05/27/lockheed-paid-38-million-in-bribes-abroad/800c355c-ddc2-4145-b430-0ae24afd6648/.

[11] Because a change in norms and values was crucial in determining ratification of the Convention, I claim that participation in the treaty can be considered exogenous to considerations related to firms' international business position, at least for its first ratifiers. To back up this claim, in Appendix B, I investigate determinants of ratification of the OECD Convention. I find no evidence that country-level factors such as the level of outward FDI in more or less corrupt economies delayed or fast-tracked treaty adoption.

[12] Original signatories were: Argentina, Australia, Austria, Belgium, Brazil, Bulgaria, Canada, Chile, Czechia, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Luxembourg, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, South Korea, Spain, Sweden, Switzerland, Turkey, the UK, and US.

[13] In chronological order: Slovenia (2001), Estonia (2004), South Africa (2007), Israel (2009), Colombia and Russia (2012), Latvia (2014), Costa Rica and Lithuania (2017), and Peru (2018).

[14] Such disproportion is reflected in my firm-level data, see Appendix C.

under their jurisdiction to pay bribes in international business. To date, all parties have adopted the necessary laws to implement the Convention. Under Article 5, the Convention mandates countries to enforce laws against domestic subjects suspected of foreign bribery.

How have treaty members complied with the OECD Convention over time? Answering this question is important in order to understand companies' expectations of future law enforcement. During the first decade member countries mostly focused on the adoption of laws required under the Convention. They achieved two crucial goals. First, members harmonized corporate criminal laws (Brewster, 2017; Spahn, 2013). Far from being formal, these achievements included the removal of favorable tax treatments for companies engaging in foreign bribery (Gutterman, 2015). It is likely that companies regarded such legalization process as a credible commitment against corporate corruption (Abbott and Snidal, 2002). Second, in early years member countries developed networks of mutual legal assistance (MLA) and cooperation defined under Article 9.

Enforcement of the Convention was mostly done by the US in this first decade. The OECD Convention had "levelled the playing field" of international competition and extended anti-bribery laws to US firms' competitors. The US Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) could thus significantly fast-track enforcement from the end of the 1990s (Brewster, 2017; Leibold, 2014). Crucially, such significant increase in enforcement did not only target US-based companies. Under the FCPA *extraterritorial* provisions, the DOJ and the SEC enforce the US anti-bribery law against non-US companies headquartered in OECD Convention signatories by leveraging MLA networks (Spahn, 2013). Figure 2 describes this "international-competition neutral" strategy (Brewster, 2017, 1615). Since the early 2000s, the number of FCPA cases and fines levied from US and non-US firms significantly increased, experiencing a peak in 2010. Importantly, this time of growing enforcement overlaps with the period considered by the analysis in the next section.

Such intense US-lead enforcement of the Convention stimulated OECD Convention members to start applying their own anti-bribery policies (Kaczmarek and Newman, 2011). Excluding the US, the median ratifier of the OECD Convention brought its first anti-bribery case in 2005 (earliest enforcement in 1998, latest in 2008).[15] By 2010, judicial authorities in treaty members like France, Germany, Italy, Norway, Switzerland, the UK, and the US fully cooperated in important and highly publicized cases. They involved US-based companies (*e.g.*, Baker Hughes, Monsanto, Halliburton, and the Titan Corporation), but also large non-US companies—including ABB Ltd., BAE Systems, ENI, Innospec, JGC Corp, Siemens, Statoil, and Technip (Spahn, 2012).

Have law enforcement actions been biased against certain countries or industries in a way that might alter firms' expectations? Scholarly answers are mixed. McLean (2012) finds that FCPA enforcement between 2000 and 2011 was mainly determined by the level of corruption of the host country. Early law

[15] My own computation using replication data from Kaczmarek and Newman (2011).

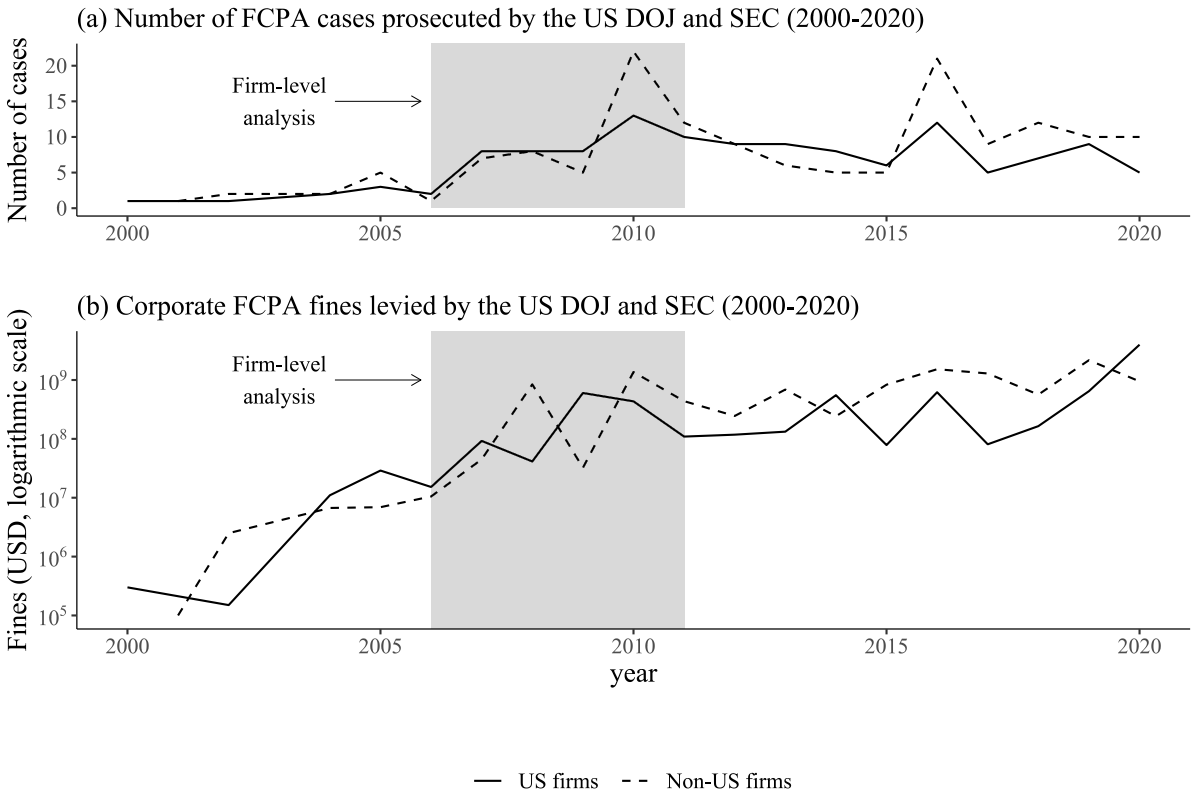


Figure 2: FCPA enforcement. Number of cases and fines. Shaded area reports the time-period of the firm-level analysis. Data from the Violation Tracker, Good Jobs First

enforcement, it seems, just followed the bribes where they were more frequent. However, Tomashevskiy (2021) extends the data to consider more recent FCPA cases and finds that US agencies disproportionately target "unfriendly" countries to the US. Work by Garrett (2020) also suggests that, in recent years, corporate law enforcement might have followed political objectives rather than the occurrence of crime. Choi and Davis (2014) find that US agencies levy disproportionally larger fines from non-US companies, even when holding constant the size of the bribe, perhaps as a way to set an example.

Because studying determinants of enforcement exceeds my scope, I address the issue in two ways. First, I focus on the early years of the Convention when companies likely had limited observations of past law enforcement to expect biases. Second, within this narrow time-frame I investigate whether past enforcement actions by country or sector factor differently into investment choices (Appendix F).

4 Empirical analysis

I propose two empirical exercises to test my argument that anti-bribery policies affect foreign investment decisions non-linearly in the level of corruption of host economies. The first one applies a selection on observables design on firm-level data about foreign investment decisions. The second employs a generalized synthetic counterfactual design on dyadic country-level FDI data.

4.1 Firm-level analysis

My firm-level analysis models binary investment choices. A firm f from country i invests in country j if its propensity to invest, I^*_{fij}, is greater than 0. In Equation 1, I^*_{fij} is a function of whether country i is a signatory 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.[16] Matrix $\mathbf{X_{fij}}$ includes covariates and u_{fij} is the idiosyncratic error term.

$$I^*_{fij} = \beta_1 S_i \times C_j^2 + \beta_2 S_i \times C_j + \beta_3 S_i + \beta_4 C_j^2 + \beta_5 C_j + \mathbf{X'_{fij}}\boldsymbol{\gamma} + u_{fij} \tag{1}$$

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, is derived in Equation 2. It is a parabola with an expected inverted-U shape, as in Figure 1. Therefore, β_1 is expected to be negative, β_2 positive, and β_3 null.

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

I estimate Equation 1 with Orbis[17] firm-level data retrieved from Beazer and Blake (2018). This dataset reports information on the portfolio of foreign subsidiary incorporations by 3871 parent firms between 2006 and 2011 *i.e.*, in early years of the Convention. It reports the home country[18] of the parent (62 in total) and that of the subsidiary (host country, 86 in total) for each incorporation.[19]

The dataset is a cross-section of the investment choices of the firms between 2006 and 2011, where each company is paired with each potential host country. 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.[20] The binary outcome variable (*Subsidiary*) codes whether a firm f is the "ultimate parent" of a subsidiary in host j between 2006 and 2011. Subsidiary incorporations do not include financial investments and small firms are excluded from the sample which thus represents a population composed of large MNCs embarking in long-term foreign productive enterprises, rather than speculative ventures.[21]

[16] In Appendix E, I show that the effect of the OECD Convention is not significant with a linear interaction with corruption.

[17] Orbis 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.

[18] In Appendix (Table E.2), I show that results are robust when excluding firms from countries that joined the Convention between 2006 and 2011 or from likely outliers.

[19] Appendix C discusses selection into the sample and balance in covariates.

[20] 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.

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

Skeptical readers might be concerned that, between 2006 and 2011, the OECD Convention had no dent against foreign bribery because many signatories fast-tracked enforcement in later years. This is a relevant concern: as argued in the previous section, credibility of enforcement is necessary for anti-bribery rules to have an effect on firms' investment decisions.

However, as argued above, lax enforcement should draw towards the null equally *deterrence* and *empowerment*. Any significant finding I observe from a period of lax enforcement should therefore be larger in magnitude in times of stronger enforcement. Moreover, US authorities vigorously applied the FCPA against non-US companies headquartered in OECD Convention signatories in the time-period considered (Figure 2). Finally, as argued above, early 2000s did not represent a period of non-compliance with the OECD Convention by other members: ratifiers signalled commitments to anti-bribery standards by adopting significant legal tools and by beginning to enforce their anti-bribery laws in a series of largely publicized cases (Spahn, 2012).

In Appendix I propose some tests to probe this concern empirically. I show that results are robust to the extension of data from Beazer and Blake (2018) to consider investments made by these very firms until 2018 thus considering a period of stronger enforcement.[22] Moreover, a placebo test shows that the effect is detected only for firms in countries and industries that had experienced enforcement by 2005 and that could therefore reasonably expect future actions (Appendix F).

My main explanatory variable is the binary *OECD Ratifier*. It codes whether the OECD Anti-Bribery Convention was into force for home country i of a parent firm f by 2005. I consider the 2005 value for all variables on the right-hand side of equation 1, for it is the year before the start of my cross-section.

Next, I need a measure of the moderator: host-country level of corruption. Well-known limitations of perception-based measures of corruption (Gueorguiev and Malesky, 2012; Olken, 2009) make them a non-appealing choice. These measures are built by surveying experts or the general population about perceptions or experiences of corruption. Social desirability biases answers about first-hand experiences (Treisman, 2007). Annual 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 that of respondents or researchers (Heywood, 1997). Alternative solutions leverage observable information. Measures like the observed number of bribery cases, however, are no reliable indicators of corruption since effective crime takes place out of sight. In my case they might reflect enforcement of the Convention, 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 leverages variation in the *geographic distribution* of observable cases of cross-border corruption to derive a measure of relative corruption among countries. It is based on the following intuition. Suppose we observed that a large share of foreign bribery cases exported from home country

[22] Table E.3.

i involved host country j but j made up for a relatively modest share of i's economic outflows. This would be evidence that j is relatively more corrupt than other partners of i because it attracts relatively more bribes. The PACI generalizes this intuition: it measures corruption of each host country as the deviation between the *observed* geographic distribution of cross-border bribes and the distribution that could be *expected* if all countries were equally corrupted and bribes followed economic flows.

I adopt this measure of corruption.[23] I also test robustness of my results to traditional perception-based measures and verify that my results hold.[24] Escresa and Picci (2017) compute the PACI employing information between 1997 and 2012. For each host country in my dataset, I re-compute the index using software and data provided by the authors. I consider bribes paid between 1997 and 2005 (included). The resulting measure *Host PACI* is my main indicator of corruption of the host economy. It ranges from 0 to 8.90, with higher values indicating more corrupt countries.

I explain my binary outcome variable in a multilevel logit model. This choice allows to specify the cross-level interaction in equation 1 (Bell and Jones, 2015). It also models the complex structure of the data (home-, host-, and dyad-level cross-nesting) accounting for unobserved clustering—*e.g.*, companies' advantage when investing in the same destinations as co-nationals (Johns and Wellhausen, 2016). All specifications include cross-classified random intercepts at the dyad, home, and host countries-level.

I include covariates to control for potential confounders. 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 GDP percentages). I include its Political Constraint (POLCON) III index, a binary indicator for democracy from Cheibub et al. (2010), and a measure for judicial independence (Linzer and Staton, 2015). Next, I control for home country features that could correlate with *OECD Ratifier*: wealth (logged GDP and GDP growth rate) and level of judicial independence. Then, I control for dyadic covariates. First, a measure of the distance in kilometres between capitals of the home and host. Second, dummies for: whether they signed a bilateral investment treaty (BIT); whether they 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 for each firm, its age, and its total assets

[23] I present the intuition supporting the measurement and its two assumptions in Appendix A. The reader should keep in mind one important assumption that supports the validity of the PACI. This index draws on observable enforced cases of bribery to measure corruption of a country j. It assumes that "the probability of observing a corrupt transaction involving firms from country i [...] and public officials in country j does not depend on the identity of country j" (Escresa and Picci, 2017, 211). This means that the PACI assumes that other countries do not discriminate when enforcing anti-corruption cases based on characteristics of country j other than its level of corruption. The index uses only cases of cross-border corruption first-enforced in other countries (meaning, not in country j itself), to mitigate violations of this assumption. The authors draw on results from McLean (2012) to support the validity of a no-discrimination assumption in anti-bribery law enforcement. However, recently Tomashevskiy (2021) argued that US authorities use anti-corruption enforcement as a way to further political goals against specific states. If this is taking place, the PACI might return a biased measure of corruption: a PACI score would in part reflect the discriminatory enforcement of anti-bribery cases, rather than the genuine propensity of its official to obtain bribes. Since studying the determinants of enforcement goes beyond the scope of this article, I caution the reader against this potential violation of the index validity. However, results presented in the next section hold when using traditional perception-based measures of corruption that do not assume no discriminatory enforcement. This reassures against such violation of the PACI assumption, at least in this case.

[24] See Table E.1.

(all logged).[25] Summary statistics are reported in Appendix.[26]

4.1.1 Results

Table 1 presents my results relative to the variables of interest only.[27] In order to prevent suppression effects (Lenz and Sahn, 2021), Table 1 first includes only the variables of interest (1). Then, it adds host and home countries' controls (2). Then, firm-level covariates (3), dyadic controls (4), and industry-level intercepts (5). Model 6 estimates the full model but updates the dependent variable from Beazer and Blake's (2018) data with Orbis data on additional investments made by these 3781 companies from 2012 until 2018. Extensive robustness and placebo tests are presented in Appendix E and F.

	Dependent variable:					
	Subsidiary					
	(1)	(2)	(3)	(4)	(5)	(6)
OECD Ratifier × Host PACI^2	−0.033**	−0.038**	−0.024$^+$	−0.032*	−0.034*	−0.034**
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
OECD Ratifier × Host PACI	0.212*	0.240**	0.169$^+$	0.210*	0.224*	0.262**
	(0.089)	(0.091)	(0.090)	(0.096)	(0.096)	(0.083)
OECD Ratifier	−0.103	−0.124	−0.210	−0.273	−0.285	−0.317
	(0.158)	(0.184)	(0.245)	(0.200)	(0.200)	(0.224)
Host PACI^2	−0.040	0.013	0.003	0.010	0.013	0.021
	(0.033)	(0.029)	(0.026)	(0.027)	(0.028)	(0.028)
Host PACI	−0.130	−0.022	0.018	−0.010	−0.037	−0.053
	(0.285)	(0.241)	(0.219)	(0.230)	(0.231)	(0.241)
Random intercepts	Yes	Yes	Yes	Yes	Yes	Yes
Country-level controls		Yes	Yes	Yes	Yes	Yes
Dyad-level controls			Yes	Yes	Yes	Yes
Firm-level controls				Yes	Yes	Yes
Industry intercepts					Yes	Yes
Extended dep. var. data						Yes
N. of host countries	86	85	85	85	85	85
N. of home countries	62	61	61	58	57	57
Observations	324,526	319,253	319,253	293,013	288,528	288,528
Log Likelihood	−33,166.090	−33,003.980	−32,839.340	−26,652.560	−26,303.350	−39,281.510
Akaike Inf. Crit.	66,350.180	66,045.960	65,724.690	53,357.120	52,660.690	78,617.030
Note:						$^+$ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table 1: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models

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

[25] In Appendix, I also show that results are robust when including the original term of interest from Beazer and Blake (2018): the interaction between home and host country's judiciary independence (Table E.2).

[26] Table C.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.

[27] Full disclosure of estimates from models 1–5 is provided in Table D.1. Full results for model 6 are in Table E.3.

3 (p-value: 0.06). Coefficients are estimated with more precision when updating investment data to consider subsidiaries established until 2018.

I compute the marginal effect of anti-bribery policies at given levels of corruption to evaluate if the argument represented by Figure 1 is supported (Brambor et al., 2006). I compute the percentage change in predicted probability of an investment when *OECD Ratifier* varies from 0 to 1, for given levels of *Host PACI*, holding everything else at its mean. I draw 95% confidence intervals from 1000 simulations of the sampling distribution of the estimated effect (King et al., 2000).

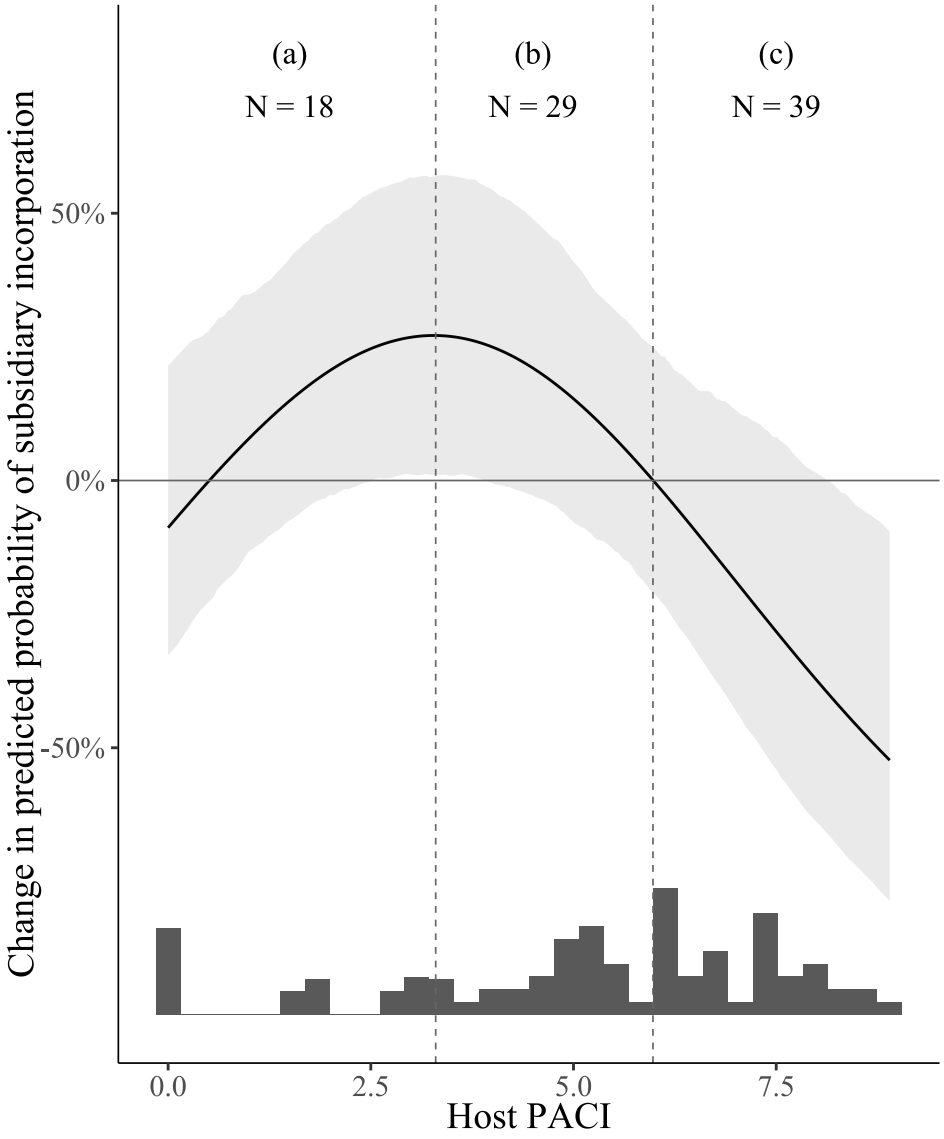


Figure 3: The non-linear effect of ratifying the OECD Convention on companies' subsidiary incorporation, conditional on host-country corruption

Figure 3 shows results obtained when considering model 1 from Table 1. 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 non-linearly. The effect can be roughly divided

in panels (a), (b), and (c). Figure 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 hosts (*e.g.*: Canada, Denmark, Sweden). As the host country becomes more corrupt, firms from countries with anti-bribery policies have a higher probability of investing. At its maximum, firms from ratifiers have a 27% higher probability of investing than their competitors (hosts at the peak are Singapore and Taiwan).[28] As the host country becomes more corrupt (b), 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 -52% for host countries at the right-end of the corruption scale like Egypt, India, Kazakhstan, Nigeria, Russia, or Vietnam.[29]

4.2 Country-dyadic analysis

The previous section provides micro-level evidence that firms subject to OECD anti-bribery policies make investment choices non-linear in the level of corruption of the host country. Yet, the analysis has two limitations. First, it cannot study changes in investment behavior, as it focuses on cross-sectional information. Thus, it cannot control for companies' existing investments into countries with different corruption levels which might subject firms to different investment conditions (Perlman and Sykes, 2017). Second, selection under OECD policies is not random. OECD Convention ratifiers have characteristics that distinguish them from non-ratifiers (Table C.1). 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 the effect of these idiosyncrasies to anti-bribery policies. Time-varying data would provide a solution to both issues and allow to hold time-invariant characteristics constant.

In order to test internal validity of my estimates, I use country-level FDI data from the United Nations Conference on Trade and Development (UNCTAD). My argument is at the firm-level: it predicts the probability of an investment and, to some extent, its size. When aggregated up to the country-level, individual investment and size choices should still be detected albeit in a noisy manner. I intend this test as a solution of the identification problems highlighted above.

I use UNCTAD country-level data on foreign investment in directed dyads because my theory claims that the effect of a home-country anti-bribery policy is conditional on host-country corruption. I retrieve UNCTAD data, country-, and dyad-level covariates from Beazer and Blake (2018). My dependent variable is the logarithm of dyad-level net FDI flows. Information ranges from 1985 to 2006 included.[30]

[28] The baseline probability of investment in these countries is 0.02.
[29] The baseline probability of investment in these countries is 0.001.
[30] Attempting to extend this time-series proved surprisingly difficult. At the time of writing, the UNCTAD data portal does not report bilateral FDI statistics anymore. Alternative common sources of FDI bilateral data are not appropriate

It thus covers the period preceding and shortly 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 109. Descriptive statistics are in Table G.1.

I test my non-linear conditional argument by adopting a binning approach.[31] 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.[32] The five subsamples are defined based on quintiles of the *Host PACI* distribution.[33] A total of 1765 directed dyads report information for the dependent variable and *Host PACI*. I estimate the effect of the OECD Convention for dyads in each of the five bins to study the impact of anti-bribery policies conditionally on the level of corruption of the host economy.

I identify the average treatment effect of the treated (ATT) dyads with a generalized synthetic control approach.[34] A dyad whose home country ratified the Convention is considered treated after the treaty has entered into force. Control dyads are those whose home countries did not ratify the agreement. My identification strategy draws on control dyads to impute one synthetic counterfactual for each treated dyad. It does so by maximizing similarity in pre-treatment trends between treated and synthetic controls. I choose this solution over a standard two-way fixed-effect (2FE) model because the treatment timing (entry into force of the OECD Convention) is staggered over the years 1999-2001 for early signatories and my panel dataset is unbalanced. I adopt the model proposed by Xu (2017), which allows for heterogeneity in treatment effects, staggered treatment timing, and unbalanced data.[35]

4.2.1 Results

Figure 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

to the case studied here. OECD statistics only report bilateral FDI data where origin and destinations of investments are OECD countries, thus they would only include information for treated units in the dataset. Moreover the moderator would not have enough variation. A valid alternative would be represented by the IMF Coordinated Direct Investment Survey (CDIS) data. Unfortunately, CDIS time series start only in 2009 and they measure FDI stocks, whereas the UNCTAD time-series consider FDI flows. I return on the issue of time coverage in the conclusion.

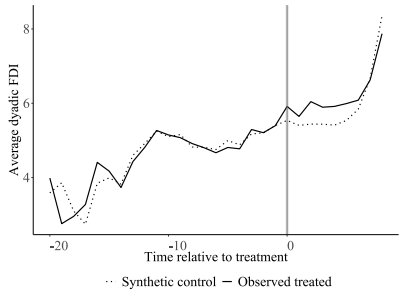
[31] In Appendix J, I substitute my binning strategy with a traditional interaction of the treatment variable with the linear and squared measure of *Host PACI*, in various model specifications.

[32] I choose the 2005 value for consistency with the firm-level analysis. The choice is 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 rather than within countries, especially in a short time window as the one of my analysis.

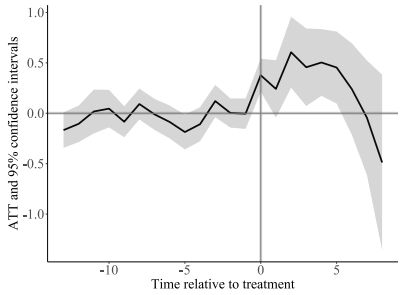
[33] The choice of quintiles is purely empirical, as it guarantees enough observations in each bin. Alternatives (using tertiles and quartiles) provided consistent results, see Appendix H.

[34] In Appendix, I show results are very similar when adopting a 2FE design (Figure I.1). This design includes all dyads, therefore ensuring results do not depend on excluding dyads without enough pre-treatment observations.

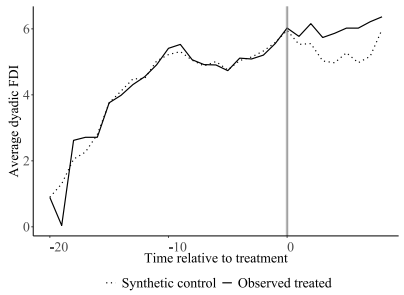
[35] In the estimation procedure I impose a 2FE model specification. I employ time-varying covariates at the level of the host country, home country, and dyad that are also adopted in the firm-level analysis. This is done to improve the synthetic counterfactual imputation. I drop all treated dyads without 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 between 0 and 3. Non-parametric standard errors are estimated with 1000 bootstrap iterations blocked at the dyad-level.



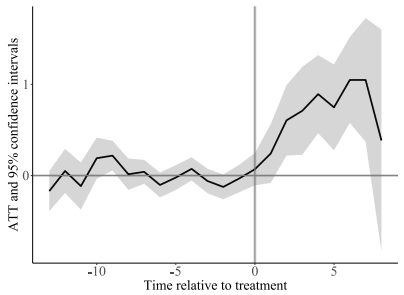
(a) Bin 1: Average dyadic flows



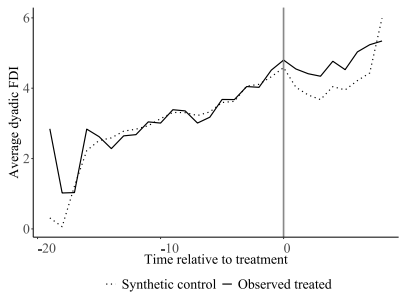
(b) Bin 1: Dynamic ATT



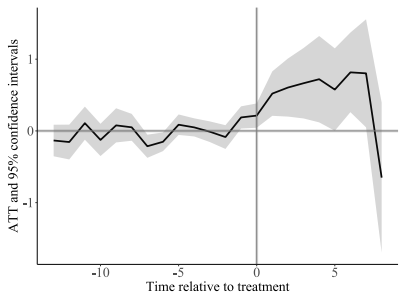
(c) Bin 2: Average dyadic flows



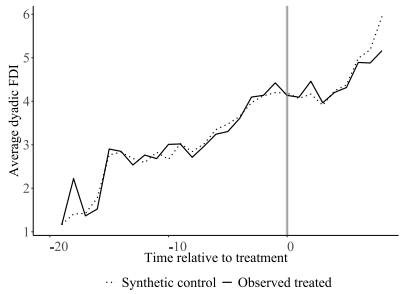
(d) Bin 2: Dynamic ATT



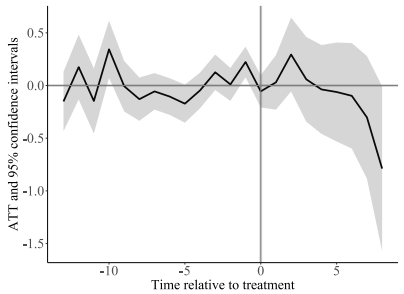
(e) Bin 3: Average dyadic flows



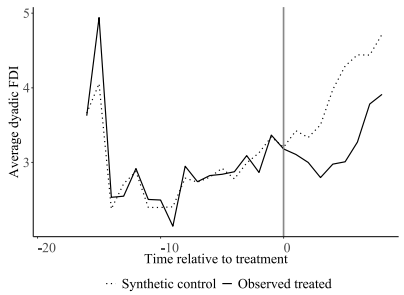
(f) Bin 3: Dynamic ATT



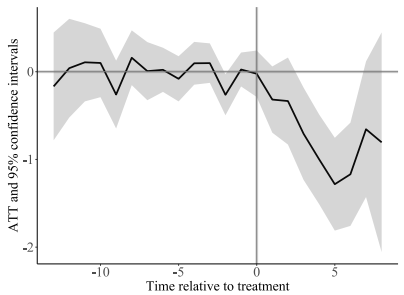
(g) Bin 4: Average dyadic flows



(h) Bin 4: Dynamic ATT



(i) Bin 5: Average dyadic flows



(j) Bin 5: Dynamic ATT

Figure 4: Synthetic counterfactuals for treated dyadic FDI flows. Average trends by host corruption level

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 hosts (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).

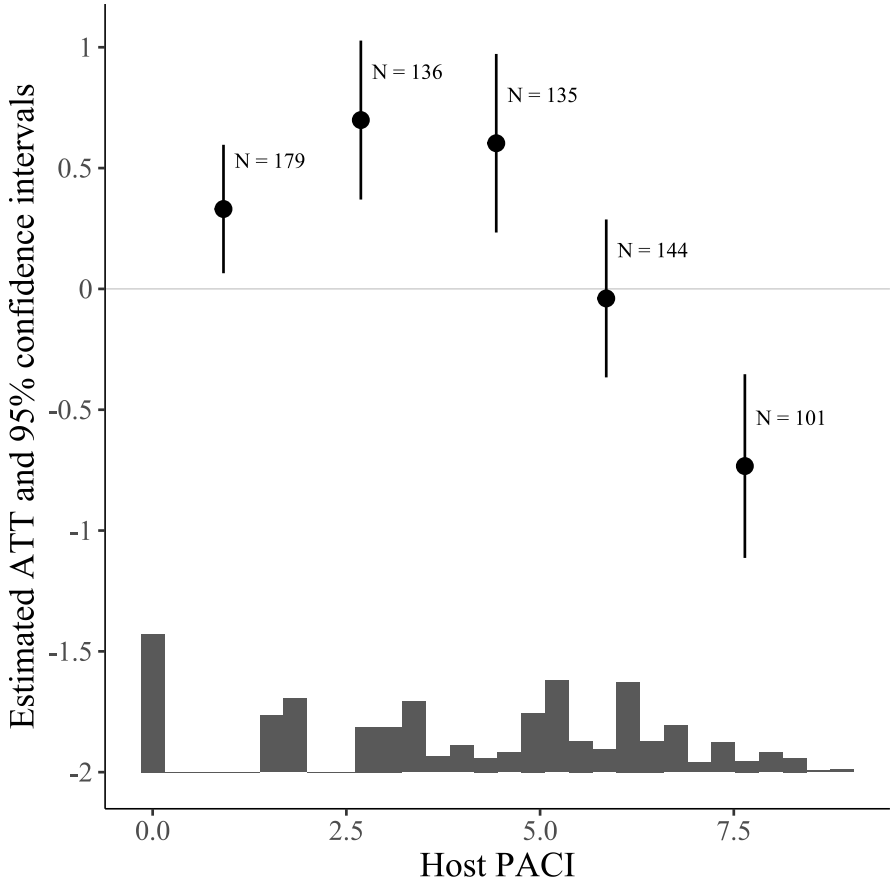


Figure 5: The effect of the OECD Convention on dyadic FDI flows at given host-country corruption levels. Estimates from synthetic counterfactual designs

Figure 5 reports the aggregated ATT 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. The Convention had a small effect on investment for dyads in the first bin (hosts include Australia, Canada, Denmark, and Sweden). As the host economy in a dyad gets moderately corrupt, the estimated effect is positive and statistically significant. This is true for dyads in the second and third bins, whose hosts include Brazil, Italy, Mexico, Singapore, and Taiwan. When converted from the logarithmic scale, estimates inform us that ratification increased net FDI flows from an average treated dyad, on an average post-

treatment year, by about 2.01 million constant US dollars (second bin) and 1.83 million (third bin). These effects amount to an increase in FDI flows to dyads in the second and third bin of about 2.11% and 8.83% respectively, over the pre-treatment average. 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 like Kazakhstan, Nigeria, and Uzbekistan. In this bin, the estimated ATT is a reduction of about $2.08 million in net FDI flows, equal to a 22.09% reduction in net FDI flows from the average. Robustness of these results is tested in Appendix H through J.

5 Concluding remarks

This article has studied the effect of multilateral corporate regulations on companies' foreign investments. I have focused on regulations imposed by firms' home states to prohibit bribe payments abroad under the 1997 OECD Anti-Bribery Convention. Political economy advances two opposed expectations about the direction of the effect of such regulations on FDI. I contributed to the debate by attempting to rejoin the two claims. I argued that the two expectations are observable in host countries with different corruption levels. In moderately corrupt countries, regulated companies can leverage regulations to turn bribe requests down and secure business opportunities by other means than bribing. This favors their investment. In extremely corrupt hosts, instead, regulated companies find it harder to secure business opportunities without bribing and operate at higher costs induced by anti-bribery. Here, laws deter investment. I find empirical support of this argument at the firm- and country-dyad levels.

Limitations of the study open up various possible lines of future inquiry. My argument rests on regulated companies' leverage to refuse bribe requests, which I claim operates differently in countries with different corruption level. I have not tested this mechanism directly although previous studies lend plausibility to it (Svensson, 2003). It might be possible that anti-bribery policies affect the behavior of public officials, too (Perlman and Sykes, 2017). The overall observed effect on investment might thus be the compounded result of these different mechanisms. Future work could disentangle the role of these regulations in affecting bribery by firms *vs* foreign public officials.

An important limitation concerns modes of entry in a foreign market. I did not consider other strategies than establishing ownership. Licensing and joint ventures, yet, are potential alternatives to invest abroad. These strategies cannot be studied with the type of FDI data I used (Kerner, 2014). They can expose firms from ratifiers of the Convention to a lower risk of interaction with corrupt public officials (see Chapman et al., 2020; Zhu and Shi, 2019). By entering a market using these alternative modes, firms might circumvent anti-bribery policies. Future work could investigate this possibility.

A final limitation concerns the time coverage of the country-level analysis: as UNCTAD data used extend only until 2006, the analysis did not consider more recent time periods when some countries

under the OECD Convention reinforced their commitments to enforce anti-bribery standards—roughly since 2010 (Jensen and Malesky, 2018). An example is the UK which, in 2010, passed the Bribery Act, considered by many as an improvement over its previous anti-bribery policy. Future work could extend the time-series considered by the current study and evaluate whether more vigorous enforcement, or the adoption of more recent anti-bribery policies, have produced different or comparable effects on regulated companies' investments.

My study on the effectiveness of the OECD Convention rests on scope conditions that could limit the generalizability of the results to other anti-bribery IOs. A crucial scope condition is represented by the role of US prosecutors in sustaining enforcement. Because of the leading role played by the US in its ratification, the OECD Convention almost represents the extension of the FCPA to OECD partners. A similar outcome would have been more difficult to achieve in larger venues such as the UN Convention Against Corruption (Spahn, 2013). Due to the tight connection between provisions in the OECD Convention and the FCPA, US prosecutors can easily leverage the treaty to prosecute foreign companies for the very type of misconduct prohibited under FCPA terms. This has two effects. First, it compensates for weak levels of compliance by other parties making the US the "global policeman" of the Convention (Choi and Davis, 2014; Crippa, 2021; Tomashevskiy, 2021). Second, it stimulates other treaty member to initiate their own anti-bribery actions (Kaczmarek and Newman, 2011). Future studies could investigate more systematically differences in effectiveness for anti-corruption IOs where there is no leading state *de facto* in charge of enforcement.

The study shows that a multilateral approach to the diffusion of sustainable business models can facilitate companies in a range of countries where financial crime would otherwise be common. These findings are 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 regulation.

A caveat concerns host countries with *extremely* weak regulatory standards. Here, the strategy backfires. Regulated firms are more likely to abandon these economies. Although a rigorous welfare analysis goes beyond the scope of this article, this might look like a desirable outcome: if not from the perspective of firms, at least from that of extremely corrupt host economies, given that firms from OECD countries are among the main exporters of bribery (Picci, 2018). Cutting down on the supply of bribery, one could argue, is one effective way to reduce corruption in places that need it the most.

However, a reduction in the supply of bribes by regulated companies could be met by an increase in bribe-payments from *un*regulated competitors (Jensen and Malesky, 2018). To the extent that unregulated firms can violate business standards, bribe in the conduct of business, and remain unpunished by their home economies, standards of economic activity in host economies with already high corruption

levels might not improve—or even decline further. 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) and on the limited effectiveness of anti-corruption IOs (Ferry et al., 2020; Hafner-Burton and Schneider, 2019).

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Supplemental Material

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.[36] 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 (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.

[36] 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).

B Determinants of the ratification of the OECD Convention

In this section, I offer some descriptive evidence suggesting that ratification of the OECD Convention by home countries of MNCs was not significantly affected by economic factors related to their firms' existing foreign investment positions. This section provides some confidence that selection into the treatment (ratification of the Convention) was not endogenous to considerations related to firm pressure, at least for the early ratifiers of the Convention. Results are consistent with qualitative accounts claiming that ratification was the result of a normative global shift around corruption, exploited by the second Clinton administration to secure a treaty (Abbott and Snidal, 2002; Brewster, 2017).

The goal of this exercise is to show that early ratifiers of the OECD Convention were no more or less likely to sign the treaty based on their existing foreign investment or their investment into more corrupt economies. On the contrary, if considerations about firm pressure determined ratification of the OECD Convention, we should expect that countries with larger investment into more corrupted economies would be significantly less likely to ratify the Convention.

In order to provide insights into the process of ratification of the OECD Convention, I first build a country-year level panel dataset comprising the 63 home countries of MNCs documented in the firm-level analysis. I observe them between 1977 (the year the US first adopted the FCPA) and 2005 (the year the firm-level cross-section starts). For each country, I code a binary dependent variable representing whether and when it ratified the OECD Convention.

I explain this dependent variable with a number of relevant covariates, all one-year lagged. First, I measure the existing investment into low-, mid- and high-corruption countries. To obtain this variable, I draw on the same UNCTAD dyadic data employed in the country-dyadic analysis reported in the main text. For each country, in each year, I aggregate this variable based on terciles[37] of the distribution of the *Host PACI* measure. This results in a variable representing, for each home country, the size of its yearly investment into host countries with low, medium, and high levels of corruption. I express this measure as GDP percentage.

I obtained further covariates by drawing on the World Development Indicator (Arel-Bundock, 2022) to obtain data about a number of covariates: the yearly total outward FDI (as GDP percentage); the total value of merchandise trade export (as GDP percentage); GDP (logged); GDP per capita; and GDP growth (in percentage). Next, I draw on the same sources described in the main text for measuring the country's level of judiciary independence and the POLCON III index.

I explain my binary dependent variable in a linear model including country and year fixed effects, with standard errors clustered at the country-level. In order to allow a comparison of estimates across different covariates, I standardize all variables so that coefficients represent the effect of a one-standard deviation increase in the covariate. Table B.1 reports estimated results. Model 1 shows that variables about the size of FDI in countries with different levels of corruption bear a small and insignificant effect on the probability of ratifying the OECD Convention. A barely significant effect is only detected for FDI into mid-corruption economies, which is however not significant when controlling for the rest of the covariates (model 4). In models 2, 3, and 4 we observe that the level of outward FDI is associated with a slightly larger probability of ratifying the OECD Convention. However, such effect is only significant at the 0.10 conventional level. Moreover, the size of the estimate is remarkably small. A one-standard deviation increase in Home FDI—which would move a country significantly along the distribution of the variable: from the first to more than the third quartile—is associated with an increase in the probability of ratifying the Convention by just 0.036.

Results are very similar when estimating the same specifications in a survival analysis, which models the time-to-ratification for each country—similarly to what Von Stein (2008) does. I adopt a Cox proportional hazard model and replicate previous specifications in Table B.2.

[37] I adopted terciles, in this exercise, so as to estimate more parsimonious models. Attempts performed when using different quantiles (*e.g.*, quartiles, quintiles) yielded similar results.

	Dependent variable:			
	OECD Ratification			
	(1)	(2)	(3)	(4)
Home FDI in low-corruption countries (GDP %)	0.013			-0.007
	(0.010)			(0.007)
Home FDI in mid-corruption countries (GDP %)	0.022+			0.002
	(0.013)			(0.008)
Home FDI in high-corruption countries (GDP %)	-0.012			0.005
	(0.026)			(0.011)
Home FDI (GDP %)		0.036+	0.035+	0.036+
		(0.021)	(0.018)	(0.018)
Home Trade (GDP %)			-0.005	-0.557***
			(0.066)	(0.141)
Home GDP (log)			-0.357**	-0.129
			(0.132)	(0.267)
Home GDP per capita			0.310***	0.009
			(0.038)	(0.080)
Home GDP Growth (%)			0.007	0.099**
			(0.014)	(0.033)
Home Judiciary Indep.			0.121+	-0.045
			(0.069)	(0.065)
Home POLCON III			0.004	-0.050
			(0.026)	(0.034)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Country-clustered Std. Err.	Yes	Yes	Yes	Yes
Num.Obs.	367	1473	1287	363
R2	0.886	0.665	0.764	0.946

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

Table B.1: Economic determinants of ratification of the OECD Convention. Linear models

	Dependent variable:			
	OECD Ratification			
	(1)	(2)	(3)	(4)
Home FDI in low-corruption countries (GDP %)	0.432			0.270
	(0.357)			(0.432)
Home FDI in mid-corruption countries (GDP %)	0.229			-0.014
	(0.406)			(0.506)
Home FDI in high-corruption countries (GDP %)	-0.670			0.055
	(0.452)			(0.606)
Home FDI (GDP %)		-0.182	0.299	0.283
		(0.191)	(0.283)	(0.413)
Home Trade (GDP %)			-0.209	-1.216
			(0.349)	(0.970)
Home GDP (log)			0.338	-0.021
			(0.267)	(0.423)
Home GDP per capita			0.176	-0.056
			(0.272)	(0.554)
Home GDP Growth (%)			0.359	1.142
			(1.091)	(3.183)
Home Judiciary Indep.			0.978*	1.210
			(0.387)	(0.785)
Home POLCON III			0.118	-0.054
			(0.343)	(0.696)
N	21	58	54	21
Log.Lik.	-42.01	-121.95	-97.86	-37.55
Wald χ^2	2.87	0.91	22.32	7.54

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

Table B.2: Economic determinants of ratification of the OECD Convention. Cox PH models

C Firm-level analysis: Descriptive statistics

The usage of Orbis data potentially introduces an issue of selection into the sample. Orbis data are obtained from compulsory reports that companies file with their home countries' authorities. This typically over-represents OECD economies (Beazer and Blake, 2018; Kalemli-Ozcan et al., 2015). In my case, about 94.4% of firms are headquartered in signatories of the OECD Convention. This disproportion is, however, not a surprise: the Convention covers a large proportion of MNCs—95 of the 100 largest non-financial enterprises—and of FDI—about 80% of global outbound stocks (OECD, 2018). In light of this, the split between the treatment and control group in the sample seems to represent that in the relevant population of MNCs.

However, such treatment-control group split means there is only a limited number of control-group firms to operate a comparison. I evaluate the balance in covariates at the firm-level and home country-level in Table C.1, Figure C.1, and Figure C.2.

Firm-level features are rather balanced. Treated firms only tend to be smaller by assets, but the difference appears substantively minimal when converted from the logarithmic scale (about 2500 USD). They are not dissimilar with respect to their age, total number of host countries they operate in, and with respect to whether their industry experienced at least one case of corporate corruption before 2005. Figure C.1 breaks down companies in the dataset according to their North American Industry Classification Standard (NAICS) 2-digit code and treatment status. Distributions of sectors seem comparable between treatment and control group, although treated firms have more representatives in services related to management of enterprises. This reassures that treated and control firms in the sample are similar with respect to fundamental economic features.

However, treated and control firms' home countries' features largely differ. Figure C.2 describes the distribution of headquarter countries for firms in each of the two groups. More than half of the treated firms are headquartered in the United States, Germany, France, United Kingdom, and Japan. More than half of the control firms are headquartered in India, Israel, Singapore, Taiwan, and Russia. Unsurprisingly, OECD economies are, on average, larger by GDP (although they grow more slowly) and experience higher levels of judicial independence. The limited number of observations in the control group and the imbalances in home-country covariates suggest caution against the causal interpretation of findings from these data. Readers should keep in mind that countries, especially late-ratifiers self-selected into ratifying the OECD Convention and this determined their companies' treatment status. The problem of self-selection into the treatment is mitigated in the country-dyadic empirical exercise, which exploits within-dyad variation to estimate the conditional effect of the OECD Convention. Between-country differences are therefore removed in that analysis.

	Control (N=218)		Treatment (N=3653)			
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Firm Age (log)	3.31	0.73	3.31	0.96	-0.0009	0.05
Firm Assets (log)	14.73	2.05	13.82	2.11	-0.91	0.15
Firm Host Countries (log)	0.60	0.70	0.68	0.72	0.08	0.05
Industry with enforcement	0.75	0.43	0.76	0.43	0.005	0.03
Home GDP (log)	24.89	1.49	25.63	1.53	0.74	0.11
Home GDP Growth (%)	5.74	2.91	1.79	0.93	-3.95	0.21
Home Judiciary Indep.	0.59	0.22	0.91	0.10	0.32	0.01

Table C.1: Description of covariates for treated and control firms

Table C.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 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).[38] Data on Host CCE and Host V-Dem Bribery have been retrieved

[38] 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.

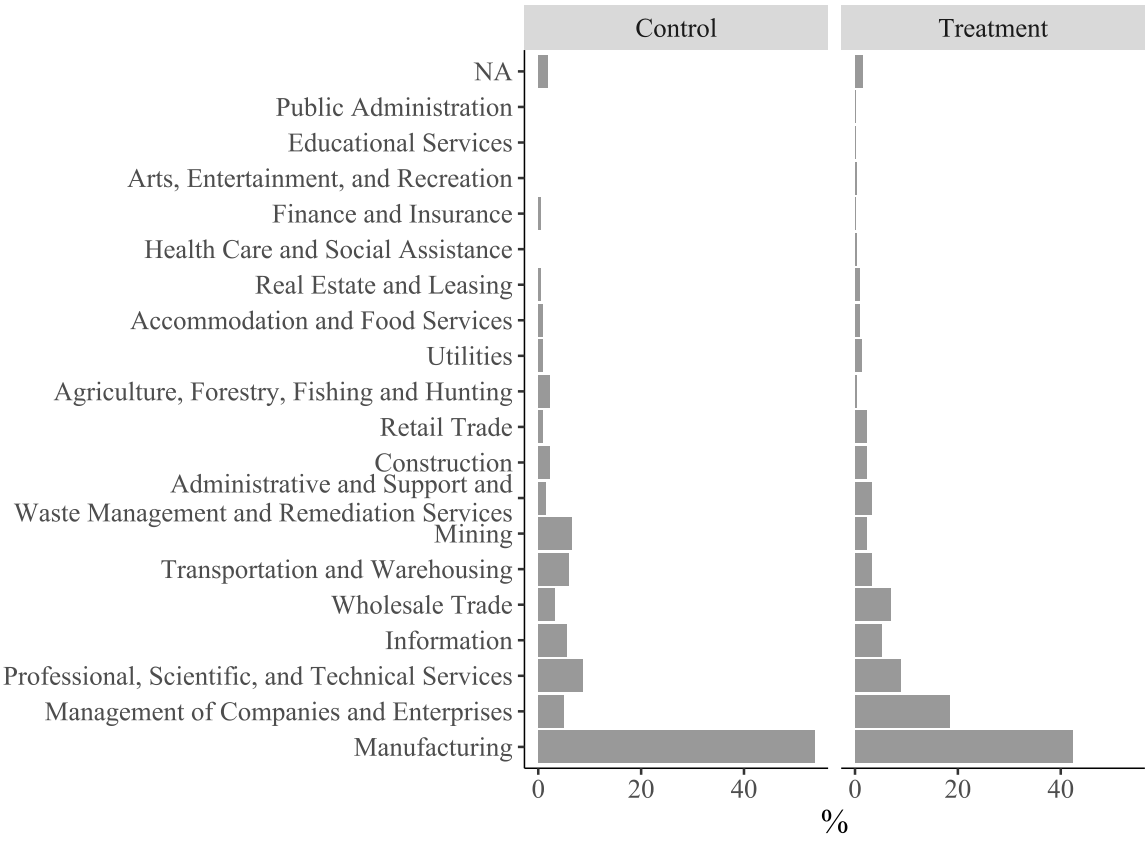

Figure C.1: Firm-level database description: Percentage of firms in the database by NAICS-2 code and treatment status

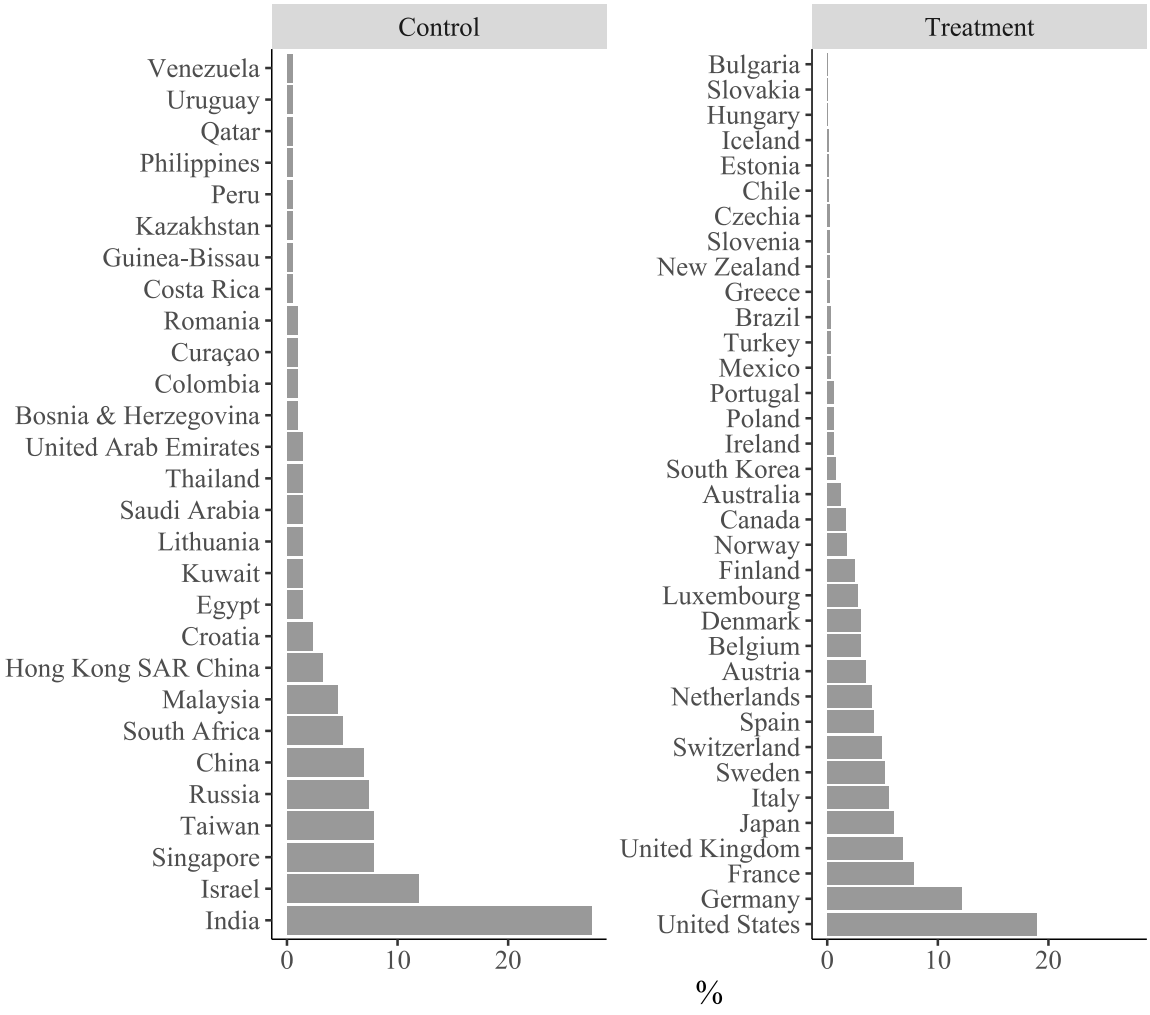

Figure C.2: Firm-level database description: Percentage of firms in the database by headquarter country and treatment status

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	Max
Subsidiary	402,589	0.026	0.159	0	1
Subsidiary (extended)	402,589	0.047	0.212	0	1
OECD Ratifier	402,589	0.944	0.231	0	1
Host PACI	329,155	5.162	2.333	0.000	8.901
Host PACI (2012)	332,971	5.030	2.261	0.000	8.755
Host Bribery V-Dem	398,720	0.197	1.556	−2.838	3.363
Host CCE	398,720	2.661	1.069	1.082	4.825
Industry with enforcement	402,589	0.757	0.429	0	1
Host with enforcement	402,589	0.579	0.494	0	1
Home with enforcement	402,589	0.860	0.347	0	1
Home GDP (log)	399,885	25.594	1.540	18.750	27.859
Host GDP Growth (%)	399,885	1.987	1.408	−6.272	10.647
Home Judiciary Indep.	402,381	0.895	0.133	0.167	0.988
Host GDP (log)	383,260	23.196	1.717	19.414	27.859
Host FDI (GDP %)	383,260	6.533	17.617	−4.258	172.716
Host GDP per capita	383,260	1.430	1.445	0.028	6.829
Host Trade (GDP %)	383,260	0.876	0.533	0.265	4.299
Host Judiciary Indep.	394,849	0.555	0.280	0.018	0.988
Host Democracy	390,985	0.703	0.457	0	1
Host POLCON III	383,243	0.311	0.198	0.000	0.692
Dyad BIT	402,589	0.384	0.486	0	1
Dyad Common Language	386,205	0.113	0.316	0	1
Dyad Colonial Relation	386,205	0.051	0.219	0	1
Dyad Distance (hundreds of km)	386,205	0.656	0.422	0.006	1.995
Firm Age (log)	396,349	3.312	0.948	0.000	5.897
Firm Assets (log)	375,755	13.875	2.115	4.025	20.181
Firm Host Countries (log)	402,589	0.678	0.721	0.000	3.714

Note:

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

D Firm-level analysis: Full disclosure of results

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Ratifier × Host PACI^2	-0.033^{**}	-0.038^{**}	-0.024^{+}	-0.032^{*}	-0.034^{*}
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
OECD Ratifier × Host PACI	0.212^{*}	0.240^{**}	0.169^{+}	0.210^{*}	0.224^{*}
	(0.089)	(0.091)	(0.090)	(0.096)	(0.096)
OECD Ratifier	-0.103	-0.124	-0.210	-0.273	-0.285
	(0.158)	(0.184)	(0.245)	(0.200)	(0.200)
Host PACI^2	-0.040	0.013	0.003	0.010	0.013
	(0.033)	(0.029)	(0.026)	(0.027)	(0.028)
Host PACI	-0.130	-0.022	0.018	-0.010	-0.037
	(0.285)	(0.241)	(0.219)	(0.230)	(0.231)
Host GDP (log)		0.596^{***}	0.652^{***}	0.674^{***}	0.680^{***}
		(0.127)	(0.114)	(0.119)	(0.120)
Host GDP per capita		-0.003	-0.043	-0.023	-0.056
		(0.179)	(0.162)	(0.169)	(0.172)
Host FDI (GDP %)		0.010	0.010	0.010	0.010
		(0.009)	(0.008)	(0.009)	(0.009)
Host Trade (GDP %)		-0.242	-0.193	-0.187	-0.171
		(0.333)	(0.301)	(0.314)	(0.316)
Host Judiciary Indep.		3.757^{**}	3.571^{***}	3.711^{***}	3.755^{***}
		(1.144)	(1.029)	(1.076)	(1.081)
Host POLCON III		0.503	0.095	0.147	0.190
		(0.957)	(0.860)	(0.900)	(0.903)
Host Democracy		-0.135	-0.005	-0.026	-0.032
		(0.460)	(0.414)	(0.433)	(0.434)
Home GDP (log)		0.062^{*}	0.136^{**}	0.051^{+}	0.054^{+}
		(0.027)	(0.046)	(0.030)	(0.030)
Home GDP Growth (%)		-0.018	-0.031	-0.006	-0.006
		(0.018)	(0.026)	(0.020)	(0.020)
Home Judiciary Indep.		-0.192	-0.303	-0.392	-0.391
		(0.233)	(0.383)	(0.258)	(0.256)
Dyad BIT			0.102	0.088	0.092
			(0.069)	(0.073)	(0.074)
Dyad Common Language			0.700^{***}	0.765^{***}	0.756^{***}
			(0.091)	(0.100)	(0.100)
Dyad Colonial Relation			0.765^{***}	0.785^{***}	0.780^{***}
			(0.112)	(0.123)	(0.123)
Dyad Distance			-1.216^{***}	-1.062^{***}	-1.061^{***}
			(0.093)	(0.093)	(0.094)
Firm Assets (log)				0.008	0.009
				(0.008)	(0.008)
Firm Age (log)				0.010	0.009
				(0.014)	(0.014)
Firm Host Countries (log)				1.270^{***}	1.271^{***}
				(0.019)	(0.019)
Constant	-3.190^{***}	-5.520^{***}	-5.539^{***}	-6.089^{***}	-6.032^{***}
	(0.596)	(0.637)	(0.604)	(0.608)	(0.610)
Random intercepts	Yes	Yes	Yes	Yes	Yes
Industry intercepts					Yes
N. of host countries	86	85	85	85	85
N. of home countries	62	61	61	58	57
Observations	324,526	319,253	319,253	293,013	288,528
Log Likelihood	$-33{,}166.090$	$-33{,}003.980$	$-32{,}839.340$	$-26{,}652.560$	$-26{,}303.350$
Akaike Inf. Crit.	66,350.180	66,045.960	65,724.690	53,357.120	52,660.690
Note:					$^{+}$ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table D.1: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models (full disclosure)

E Firm-level analysis: Robustness tests

Results for the first tests are reported in Table E.1. In model 1 I replicate the full specification of model 5 in Table 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 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 1 using the version of the index computed and published by Escresa and Picci (2017), which employs information until 2012 and includes more host countries.[39] Results obtained when using this version of the index are substantively the same as the ones discussed before.

Next, I show that results are robust to the inclusion of the interaction originally proposed by (Beazer and Blake, 2018) between *Home Judiciary Independence* and *Host Judiciary Independence*. Excluding this interaction might introduce omitted variable bias, if the interaction term correlated with the outcome and with the interactions of interest in this paper (those between *OECD Ratifier* and *Host PACI* or *Host PACI*^{2}). This is a plausible concern: home country's levels of judicial independence likely correlate with the adoption of anti-bribery policies; and host country's levels of judicial independence likely correlates with host country's level of corruption. Model 1 in Table E.2 introduces the original interaction term in the fullest model from Table 1. Results for the coefficients of interest are consistent with those presented in the main results table, in the same direction, and statistically significant. Coefficient for β_1 is negative and significant at the 0.10 level. Estimate of β_2 is positive and significant at the 0.05 level. As a next 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 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 updating the dataset to include all investment made by these same companies until 2018. I follow the same procedure in Beazer and Blake (2018) to update investment data from Orbis. Results are reported in Table E.3. Point estimates of variables of interest are substantively unchanged and statistically significant.

[39] 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.

	Dependent variable:			
	Subsidiary			
	(1)	(2)	(3)	(4)
OECD Ratifier	−0.066	0.019	−2.040*	−0.258
	(0.184)	(0.181)	(0.847)	(0.194)
OECD Ratifier × Host PACI	−0.004			
	(0.033)			
OECD Ratifier × Host Bribery V-Dem		0.134+		
		(0.071)		
OECD Ratifier × Host Bribery V-Dem^2		−0.074*		
		(0.032)		
OECD Ratifier × Host CCE			1.367*	
			(0.543)	
OECD Ratifier × Host CCE^2			−0.214**	
			(0.082)	
OECD Ratifier × Host PACI (2012)				0.277**
				(0.097)
OECD Ratifier × Host PACI^2 (2012)				−0.048***
				(0.014)
Host PACI	0.038			
	(0.105)			
Host Bribery V-Dem		−0.090		
		(0.159)		
Host Bribery V-Dem^2		0.133*		
		(0.062)		
Host CCE			−0.674	
			(1.012)	
Host CCE^2			0.156	
			(0.148)	
Host PACI (2012)				−0.058
				(0.233)
Host PACI^2 (2012)				0.020
				(0.028)
Host GDP (log)	0.723***	0.751***	0.737***	0.626***
	(0.106)	(0.087)	(0.088)	(0.115)
Host GDP per capita	−0.065	−0.171	−0.184	0.220
	(0.173)	(0.165)	(0.187)	(0.225)
Host FDI (GDP %)	0.011	0.013+	0.014+	0.006
	(0.009)	(0.008)	(0.008)	(0.009)
Host Trade (GDP %)	−0.119	−0.152	−0.172	−0.370
	(0.310)	(0.281)	(0.292)	(0.315)
Host Judiciary Indep.	3.695***	3.205**	2.453+	2.926**
	(1.080)	(1.065)	(1.373)	(1.082)
Host POLCON III	0.241	0.454	0.421	0.025
	(0.902)	(0.819)	(0.828)	(0.891)
Host Democracy	0.006	0.006	0.068	0.039
	(0.431)	(0.397)	(0.413)	(0.411)
Home GDP (log)	0.054+	0.048	0.048	0.052+
	(0.030)	(0.035)	(0.035)	(0.030)
Home GDP Growth (%)	−0.005	−0.003	−0.002	−0.003
	(0.020)	(0.022)	(0.022)	(0.020)
Home Judiciary Indep.	−0.398	−0.366	−0.375	−0.321
	(0.260)	(0.294)	(0.293)	(0.258)
Dyad BIT	0.075	0.093	0.081	0.107
	(0.073)	(0.071)	(0.071)	(0.073)
Dyad Common Language	0.762***	0.793***	0.790***	0.710***
	(0.100)	(0.098)	(0.098)	(0.100)
Dyad Colonial Relation	0.781***	0.757***	0.757***	0.759***
	(0.123)	(0.120)	(0.120)	(0.123)
Dyad Distance	−1.082***	−1.242***	−1.238***	−1.062***
	(0.094)	(0.090)	(0.090)	(0.093)
Firm Assets (log)	0.009	0.008	0.008	0.006
	(0.008)	(0.007)	(0.007)	(0.008)
Firm Age (log)	0.009	0.010	0.009	0.009
	(0.014)	(0.014)	(0.014)	(0.014)
Firm Host Countries (log)	1.271***	1.274***	1.274***	1.277***
	(0.019)	(0.019)	(0.019)	(0.019)
Constant	−6.115***	−6.209***	−5.352**	−5.999***
	(0.587)	(0.258)	(1.639)	(0.629)
Random intercepts	Yes	Yes	Yes	Yes
N. of host countries	85	100	100	86
N. of home countries	57	57	57	57
Observations	288,528	340,553	340,553	291,944
Log Likelihood	−26,306.690	−27,685.150	−27,684.540	−26,637.240
Akaike Inf. Crit.	52,663.380	55,424.300	55,423.080	53,328.490
Note:				+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table E.1: Firm-level data. Robustness tests of multilevel logit models to different corruption specifications

	Dependent variable:		
	Beazer and Blake (2018)	Subsidiary Exclude China	Exclude Israel and South Africa
	(1)	(2)	(3)
OECD Ratifier × Host PACI^2	-0.022^{+}	-0.029^{*}	-0.024^{+}
	(0.014)	(0.012)	(0.014)
OECD Ratifier × Host PACI	0.215^{*}	0.166^{+}	0.165^{+}
	(0.096)	(0.089)	(0.096)
OECD Ratifier	-0.454^{*}	-0.180	-0.216
	(0.203)	(0.195)	(0.225)
Host PACI^2	0.003	0.016	0.012
	(0.028)	(0.026)	(0.027)
Host PACI	-0.037	-0.068	-0.098
	(0.230)	(0.221)	(0.230)
Host Judiciary Indep. × Home Judiciary Indep.	3.119^{***}		
	(0.663)		
Host Judiciary Indep.	3.904^{***}	3.724^{***}	4.412^{***}
	(1.076)	(1.034)	(1.110)
Home Judiciary Indep.	-0.990^{***}	-0.258	-0.286
	(0.283)	(0.275)	(0.298)
Host GDP (log)	0.679^{***}	0.679^{***}	0.687^{***}
	(0.119)	(0.121)	(0.112)
Host GDP per capita	-0.060	-0.041	-0.143
	(0.171)	(0.167)	(0.169)
Host FDI (GDP %)	0.010	0.011	0.012
	(0.009)	(0.008)	(0.008)
Host Trade (GDP %)	-0.176	-0.220	-0.226
	(0.314)	(0.303)	(0.297)
Host POLCON III	0.199	0.182	0.356
	(0.898)	(0.864)	(0.848)
Host Democracy	-0.038	-0.051	-0.389
	(0.432)	(0.418)	(0.459)
Home GDP (log)	0.055^{+}	0.077^{*}	0.070^{*}
	(0.030)	(0.033)	(0.030)
Home GDP Growth (%)	-0.006	-0.005	-0.008
	(0.020)	(0.023)	(0.021)
Dyad BIT	0.120	0.197^{***}	0.184^{***}
	(0.074)	(0.049)	(0.049)
Dyad Common Language	0.708^{***}	0.687^{***}	0.696^{***}
	(0.100)	(0.043)	(0.043)
Dyad Colonial Relation	0.760^{***}	0.350^{***}	0.341^{***}
	(0.122)	(0.048)	(0.049)
Dyad Distance	-1.047^{***}	-1.091^{***}	-1.035^{***}
	(0.093)	(0.059)	(0.057)
Firm Assets (log)	0.009	0.009	0.009
	(0.008)	(0.008)	(0.008)
Firm Age (log)	0.009	0.005	0.007
	(0.014)	(0.014)	(0.014)
Firm Host Countries (log)	1.271^{***}	1.256^{***}	1.254^{***}
	(0.019)	(0.019)	(0.019)
Constant	-5.881^{***}	-5.871^{***}	-5.647^{***}
	(0.608)	(0.585)	(0.617)
Random intercepts	Yes	Yes	Yes
N. of host countries	85	84	83
N. of home countries	57	56	55
Observations	288,528	283,987	278,905
Log Likelihood	−26,292.270	−26,184.200	−26,131.620
Akaike Inf. Crit.	52,640.530	52,420.400	52,315.230
Note:			$^{+}$ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table E.2: Firm-level data. Further robustness tests of multilevel logit models

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Ratifier × Host PACI^2	-0.038^{***}	-0.046^{***}	-0.025^{*}	-0.031^{**}	-0.034^{**}
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
OECD Ratifier × Host PACI	0.282^{***}	0.328^{***}	0.209^{**}	0.251^{**}	0.262^{**}
	(0.075)	(0.077)	(0.077)	(0.082)	(0.083)
OECD Ratifier	0.065	−0.176	−0.211	−0.305	−0.317
	(0.190)	(0.243)	(0.255)	(0.222)	(0.224)
Host PACI^2	−0.036	0.029	0.012	0.018	0.021
	(0.034)	(0.029)	(0.027)	(0.028)	(0.028)
Host PACI	−0.207	−0.103	−0.002	−0.031	−0.053
	(0.302)	(0.247)	(0.231)	(0.241)	(0.241)
Host GDP (log)		0.719^{***}	0.783^{***}	0.828^{***}	0.835^{***}
		(0.132)	(0.122)	(0.127)	(0.128)
Host GDP per capita		−0.048	−0.095	−0.091	−0.109
		(0.184)	(0.171)	(0.178)	(0.179)
Host FDI (GDP %)		0.006	0.004	0.004	0.004
		(0.010)	(0.009)	(0.009)	(0.009)
Host Trade (GDP %)		−0.027	0.057	0.060	0.070
		(0.344)	(0.320)	(0.332)	(0.333)
Host Judiciary Indep.		4.833^{***}	4.707^{***}	4.905^{***}	4.924^{***}
		(1.194)	(1.108)	(1.153)	(1.156)
Host POLCON III		0.262	−0.147	−0.149	−0.127
		(1.005)	(0.933)	(0.971)	(0.973)
Host Democracy		−0.346	−0.208	−0.236	−0.239
		(0.477)	(0.443)	(0.461)	(0.462)
Home GDP (log)		0.106^{*}	0.151^{**}	−0.005	−0.025
		(0.048)	(0.052)	(0.042)	(0.042)
Home GDP Growth (%)		0.005	−0.001	0.040	0.029
		(0.028)	(0.029)	(0.025)	(0.025)
Home Judiciary Indep.		0.539	0.390	0.786^{*}	0.712^{*}
		(0.409)	(0.434)	(0.351)	(0.355)
Dyad BIT			0.007	−0.012	−0.004
			(0.063)	(0.068)	(0.068)
Dyad Common Language			0.752^{***}	0.818^{***}	0.810^{***}
			(0.091)	(0.100)	(0.101)
Dyad Colonial Relation			0.797^{***}	0.861^{***}	0.876^{***}
			(0.109)	(0.120)	(0.121)
Dyad Distance			-1.186^{***}	-1.290^{***}	-1.306^{***}
			(0.087)	(0.093)	(0.094)
Firm Assets (log)				0.186^{***}	0.208^{***}
				(0.006)	(0.006)
Firm Age (log)				0.129^{***}	0.071^{***}
				(0.011)	(0.011)
Firm Host Countries (log)				0.900^{***}	0.893^{***}
				(0.014)	(0.015)
Constant	-2.825^{***}	-5.300^{***}	-5.400^{***}	-5.913^{***}	-6.110^{***}
	(0.645)	(0.691)	(0.654)	(0.661)	(0.667)
Random intercepts	Yes	Yes	Yes	Yes	Yes
Industry intercepts					Yes
N. of host countries	86	85	85	85	85
N. of home countries	62	61	61	58	57
Observations	324,526	319,253	319,253	293,013	288,528
Log Likelihood	−49,284.060	−49,000.120	−48,801.010	−40,045.040	−39,281.510
Akaike Inf. Crit.	98,586.120	98,038.240	97,648.030	80,142.070	78,617.030
Note:					+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001

Table E.3: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models. Extended data

F Firm-level analysis: Placebo tests using past enforcement

I further test my argument by constructing three placebo tests. In the theory section, I argued that the effect of anti-bribery laws should be reinforced if firms have credible expectations of law enforcement. It should not hold if firms believe anti-bribery laws to be "empty paper" with no real application. Because past enforcement actions against bribery are a strong predictor of future enforcement actions against the same targets (see Tomashevskiy, 2021, table 1, p. 394), I use available information on past enforcement events (pre-2005) to discriminate between firms with and without likely expectations of future anti-bribery law enforcement.

In my first placebo test, I distinguish between host countries involved in past anti-bribery law enforcement actions. In the second, I distinguish between firms in industries that were targeted by previous events of enforcement. And in the third, I distinguish between firms from the same home countries of those that were prosecuted for foreign bribery. If my argument is correct, in all three placebo tests the effect of anti-bribery laws should be observable only for firms that are linked to host countries, industries, or home countries that experienced past enforcement events.

In order to identify hosts, industries, and home countries with past enforcement events, I exploit information in the database from Escresa and Picci (2017). I remove from their records of cross-border bribery enforcement events all observations before 2005 and the few cases of FCPA enforcements that pre-date 1997.[40] Next, I use the dataset to obtain three lists: *i*) a list of host countries where bribes were paid in the selected pre-2005 enforcement events; *ii*) a list of home countries whose firms were involved in the pre-2005 enforcement events; and *iii*) a list of industries (defined at the NAICS-2 level) of firms that were involved in such actions. In each placebo test, I use these lists to subset my whole firm-level database in two groups: one including observations whose host/home/industries were targeted by at least one enforcement action (which I call "test"), and one including the rest ("placebo").

First, I subset my whole firm-level dataset based on whether the potential host country experienced at least one enforcement action by 2005. I then replicate the analysis proposed in Table 1, within the two distinct "test" and "placebo" sub-samples. This forces the analysis to explain multiple investment choices of firms among a portfolio of enforcement-vulnerable ("test") and non-enforcement vulnerable ("placebo") host countries. Results are reported in Table F.1.

Second, I replicate the same placebo test by discriminating between firms from home countries whose firms experienced at least one case of anti-bribery law enforcement and those who did not. The logic for this test is, again, to test credibility of future enforcement actions: by observing past enforcement activity against their co-nationals, firms should form more credible expectations about the likelihood of future enforcement actions. Results should hold stronger for the firms in the "test" subset. Results relative to this second placebo test are in Table F.2.

Finally, I consider the possibility that firms might be building expectations not by considering past enforcement actions against specific countries, but by considering enforcement against certain industries. Perhaps, they might expect enforcement actions are more likely to follow in industries which law enforcement agencies have previously prosecuted and where they might have developed informants. With the same logic as before, I subset the firm-level sample into the "test" subset (represented by firms in industries that experienced at least one case of law enforcement) and "placebo" subset (the rest of the sectors). Results are reported in Table F.3.

In all three tables, 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 for the "test" subsamples. In Table F.1, moreover, estimates are significantly larger in magnitude. In general, standard errors also shrink, resulting in estimates that are significant at smaller conventional levels. The exception is represented by the full model in Table F.2: here, the coefficients of interest are not significant. They are never distinguishable from zero, instead, in the "placebo" subsamples. Overall, the test provides further confidence in my argument. The conditional effect of anti-bribery policies is observed only for firms who have likely expectations of future enforcement actions based on past law enforcement events.

[40] 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 ×	-0.098^{***}	-0.086^{***}	-0.010	0.007
Host PACI^2	(0.021)	(0.022)	(0.033)	(0.035)
OECD Ratifier ×	0.812^{***}	0.715^{***}	-0.027	-0.179
Host PACI	(0.176)	(0.190)	(0.215)	(0.232)
OECD Ratifier	-1.246^{***}	-1.228^{***}	0.265	-0.019
	(0.334)	(0.373)	(0.209)	(0.283)
Host PACI^2	0.088	0.131^{**}	0.005	-0.089
	(0.062)	(0.045)	(0.047)	(0.058)
Host PACI	-1.588^{*}	-1.143^{*}	-0.322	1.022^{*}
	(0.643)	(0.451)	(0.343)	(0.505)
Host GDP (log)		0.886^{***}		0.173
		(0.145)		(0.280)
Host GDP per capita		-0.055		1.162^{+}
		(0.177)		(0.631)
Host FDI (GDP %)		0.011		0.006
		(0.009)		(0.028)
Host Trade (GDP %)		-0.190		-0.231
		(0.333)		(0.832)
Host Judiciary Indep.		3.846^{***}		1.624
		(1.156)		(2.540)
Host POLCON III		1.833		-1.969^{*}
		(1.190)		(0.985)
Host Democracy		-0.267		0.345
		(0.501)		(0.660)
Home GDP (log)		0.067^{**}		-0.013
		(0.026)		(0.037)
Home GDP Growth (%)		-0.003		-0.012
		(0.019)		(0.036)
Home Judiciary Indep.		-0.389^{+}		0.026
		(0.232)		(0.399)
Dyad BIT		0.071		0.189
		(0.080)		(0.161)
Dyad Common Language		0.631^{***}		0.879^{***}
		(0.112)		(0.171)
Dyad Colonial Relation		0.722^{***}		0.592^{*}
		(0.135)		(0.250)
Dyad Distance		-0.915^{***}		-0.857^{***}
		(0.097)		(0.165)
Firm Assets (log)		-0.00003		0.051^{**}
		(0.008)		(0.017)
Firm Age (log)		0.022		-0.044
		(0.016)		(0.031)
Firm Host Countries (log)		1.266^{***}		1.276^{***}
		(0.021)		(0.043)
Constant	0.814	-4.105^{**}	-3.951^{***}	-7.404^{***}
	(1.564)	(1.312)	(0.498)	(0.945)
Random intercepts	Yes	Yes	Yes	Yes
N. of host countries	59	59	28	27
N. of home countries	62	57	62	57
Observations	217,857	198,894	101,878	89,634
Log Likelihood	$-25{,}612.130$	$-20{,}655.770$	$-7{,}033.316$	$-5{,}623.031$
Akaike Inf. Crit.	51,244.260	41,365.540	14,086.630	11,300.060
Note:				$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Table F.1: Firm-level data. Placebo test: Effects of the OECD Convention in host countries that experienced pre-2005 enforcement events

	Dependent variable:			
		Subsidiary		
	Test	Test	Placebo	Placebo
	(1)	(2)	(3)	(4)
OECD Ratifier ×	-0.037^{*}	-0.026	-0.025	-0.036
Host PACI^2	(0.016)	(0.017)	(0.023)	(0.025)
OECD Ratifier ×	0.239^{*}	0.170	0.180	0.258
Host PACI	(0.116)	(0.120)	(0.173)	(0.187)
OECD Ratifier	-0.378^{+}	-0.194	0.019	-0.382
	(0.220)	(0.297)	(0.299)	(0.327)
Host PACI^2	-0.036	0.009	-0.056	0.007
	(0.035)	(0.030)	(0.037)	(0.032)
Host PACI	-0.186	-0.023	0.050	-0.006
	(0.303)	(0.249)	(0.309)	(0.249)
Host GDP (log)		0.702^{***}		0.581^{***}
		(0.125)		(0.123)
Host GDP per capita		-0.050		-0.014
		(0.178)		(0.178)
Host FDI (GDP %)		0.010		0.004
		(0.009)		(0.009)
Host Trade (GDP %)		-0.122		-0.338
		(0.329)		(0.394)
Host Judiciary Indep.		3.816^{***}		2.316^{*}
		(1.127)		(1.089)
Host POLCON III		0.259		0.146
		(0.939)		(0.914)
Host Democracy		-0.068		0.604
		(0.452)		(0.455)
Home GDP (log)		0.061		0.069
		(0.042)		(0.067)
Home GDP Growth (%)		-0.003		0.008
		(0.038)		(0.028)
Home Judiciary Indep.		-0.148		-0.715^{*}
		(0.359)		(0.348)
Dyad BIT		0.126		0.189
		(0.086)		(0.139)
Dyad Common Language		0.663^{***}		0.936^{***}
		(0.110)		(0.178)
Dyad Colonial Relation		0.699^{***}		0.908^{**}
		(0.134)		(0.279)
Dyad Distance		-0.918^{***}		-1.444^{***}
		(0.103)		(0.188)
Firm Assets (log)		0.006		0.027
		(0.008)		(0.020)
Firm Age (log)		0.011		-0.009
		(0.015)		(0.042)
Firm Host Countries (log)		1.277^{***}		1.252^{***}
		(0.021)		(0.055)
Constant	-2.768^{***}	-6.070^{***}	-3.522^{***}	-5.770^{***}
	(0.636)	(0.664)	(0.613)	(0.641)
Random intercepts	Yes	Yes	Yes	Yes
N. of host countries	86	85	86	85
N. of home countries	27	27	36	31
Observations	276,029	250,909	43,706	37,619
Log Likelihood	$-28{,}479.300$	$-23{,}082.170$	$-4{,}224.770$	$-3{,}240.071$
Akaike Inf. Crit.	56,978.610	46,218.350	8,469.540	6,534.142
Note:		$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001		

Table F.2: Firm-level data. Placebo test: Effects of the OECD Convention in home countries that experienced pre-2005 enforcement events

	Dependent variable:			
		Subsidiary		
	Test	Test	Placebo	Placebo
	(1)	(2)	(3)	(4)
OECD Ratifier ×	-0.042^{**}	-0.047^{**}	0.001	0.009
Host PACI^2	(0.014)	(0.015)	(0.025)	(0.027)
OECD Ratifier ×	0.290^{**}	0.327^{**}	-0.046	-0.146
Host PACI	(0.100)	(0.107)	(0.177)	(0.191)
OECD Ratifier	-0.148	-0.428^{*}	0.193	0.275
	(0.174)	(0.212)	(0.296)	(0.342)
Host PACI^2	-0.032	0.027	-0.081^{+}	-0.031
	(0.033)	(0.028)	(0.044)	(0.038)
Host PACI	-0.190	-0.117	0.130	0.251
	(0.285)	(0.230)	(0.353)	(0.299)
Host GDP (log)		0.699^{***}		0.625^{***}
		(0.118)		(0.138)
Host GDP per capita		-0.010		-0.071
		(0.168)		(0.205)
Host FDI (GDP %)		0.007		0.015
		(0.008)		(0.010)
Host Trade (GDP %)		-0.117		-0.338
		(0.308)		(0.407)
Host Judiciary Indep.		3.657^{***}		3.269^{**}
		(1.058)		(1.257)
Host POLCON III		0.348		-0.054
		(0.884)		(1.041)
Host Democracy		-0.034		0.393
		(0.426)		(0.514)
Home GDP (log)		0.053^{+}		0.049^{+}
		(0.027)		(0.029)
Home GDP Growth (%)		-0.006		0.004
		(0.020)		(0.034)
Home Judiciary Indep.		-0.344		-0.089
		(0.250)		(0.288)
Dyad BIT		0.079		0.193^{+}
		(0.079)		(0.116)
Dyad Common Language		0.728^{***}		0.645^{***}
		(0.106)		(0.127)
Dyad Colonial Relation		0.711^{***}		0.629^{***}
		(0.132)		(0.154)
Dyad Distance		-0.972^{***}		-0.718^{***}
		(0.098)		(0.116)
Firm Assets (log)		0.011		0.005
		(0.008)		(0.017)
Firm Age (log)		0.012		0.014
		(0.016)		(0.031)
Firm Host Countries (log)		1.274^{***}		1.226^{***}
		(0.022)		(0.042)
Constant	-3.069^{***}	-6.009^{***}	-3.385^{***}	-6.099^{***}
	(0.599)	(0.600)	(0.692)	(0.718)
Random intercepts	Yes	Yes	Yes	Yes
N. of host countries	86	85	86	85
N. of home countries	57	53	45	42
Observations	245,447	221,271	74,288	67,257
Log Likelihood	$-25{,}543.570$	$-20{,}536.040$	$-7{,}276.249$	$-5{,}909.923$
Akaike Inf. Crit.	51,107.140	41,126.090	14,572.500	11,873.850
Note:				$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Table F.3: Firm-level data. Placebo test: Effects of the OECD Convention in industries that experienced pre-2005 enforcement events

G Dyadic country-level analysis: Descriptive statistics

Table G.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	Max
Dyadic FDI (log)	15,074	3.218	2.561	0.000	12.056
Dyadic FDI (binary)	96,832	0.195	0.396	0	1
OECD Convention	96,832	0.172	0.378	0	1
Host PACI	80,998	4.306	2.432	0.000	8.901
Lag Host FDI (GDP %)	81,360	2.625	6.508	−54.358	172.716
Lag Host GDP per capita	83,687	16.508	13.919	0.248	86.591
Lag Host Trade (GDP %)	84,111	74.428	51.319	0.309	429.949
Lag Host POLCON III	84,771	0.328	0.213	0.000	0.720
Lag Host Democracy	88,052	0.677	0.468	0	1
Lag Host GDP (log)	83,780	25.763	1.898	18.650	30.162
Lag Host Judiciary Indep.	88,711	0.624	0.301	0.016	0.989
Home GDP per capita	89,717	15.364	11.754	0.300	86.591
Home GDP growth (%)	89,644	2.530	4.739	−45.325	90.468
Home GDP (log)	89,717	25.957	1.793	19.832	30.188
Home Judiciary Indep.	91,851	0.637	0.281	0.058	0.989
Dyad BIT	96,818	0.181	0.385	0	1

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

H Dyadic country-level analysis: Robustness tests

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 H.1 and H.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.

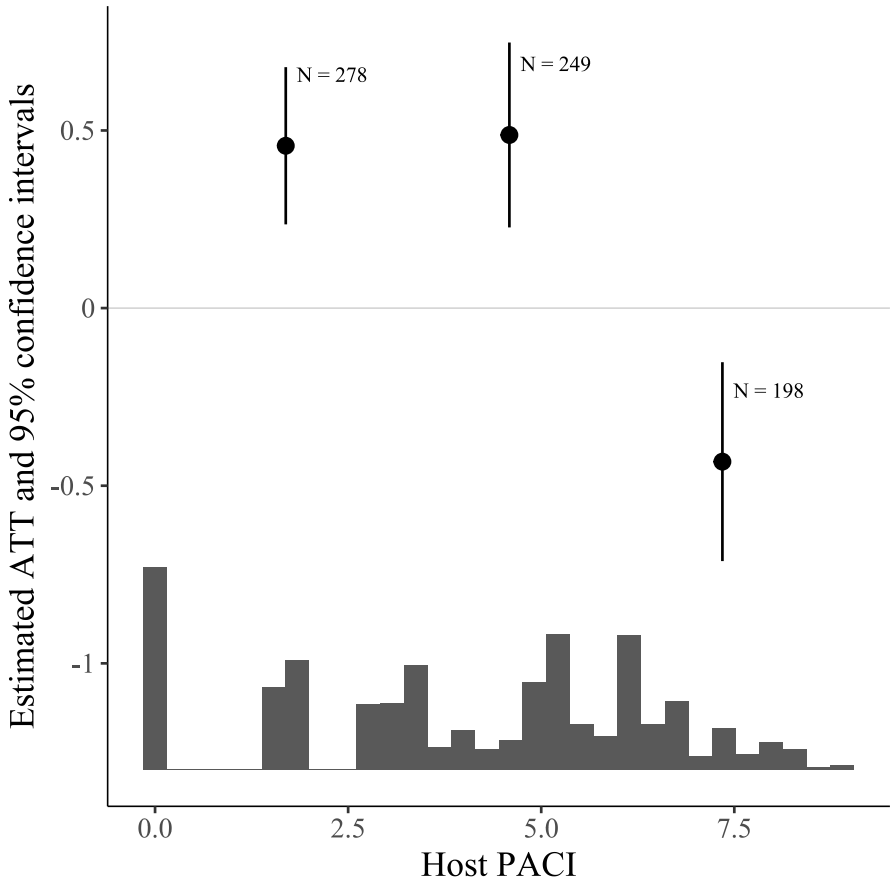

Figure H.1: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on tertiles of *Host PACI*

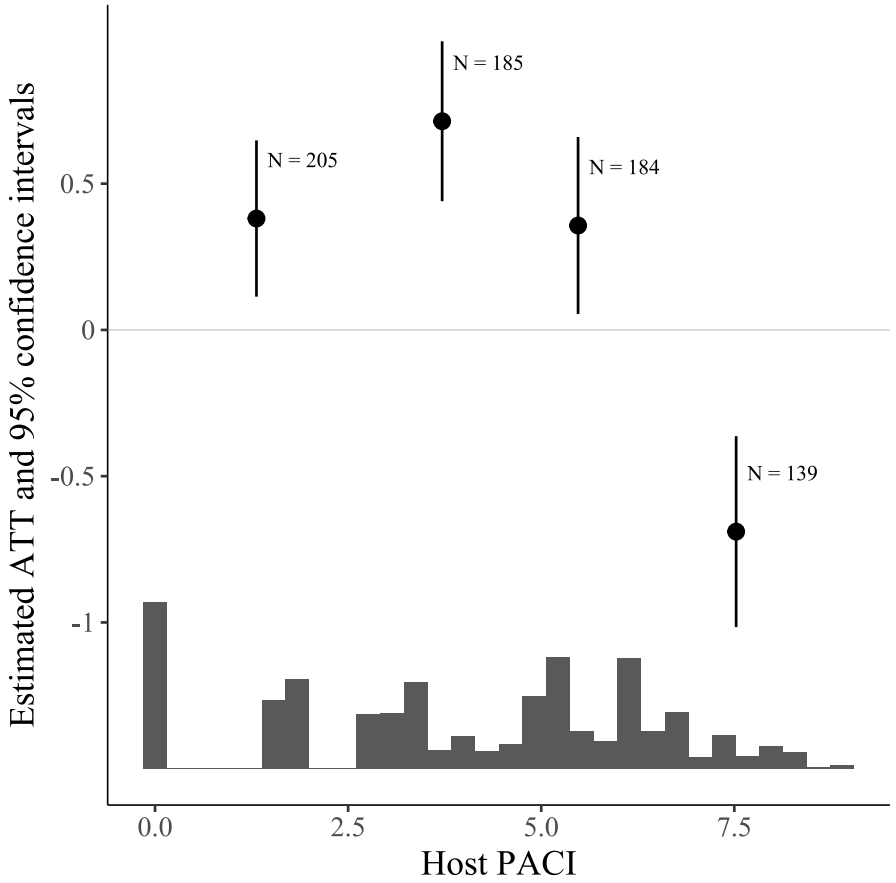

Figure H.2: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on quartiles of *Host PACI*

I Dyadic country-level analysis: 2FE designs

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[41] 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.[42] 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 all observations, including those in the "buffer" years 1999–2001 when home countries ratified the Convention at staggered times. Figure I.1 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.[43] Panels c and d of Figure I.1 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.

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

[41] 1733 dyads out of 3591 include a home country that did not ratify the Convention.

[42] 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.

[43] 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.

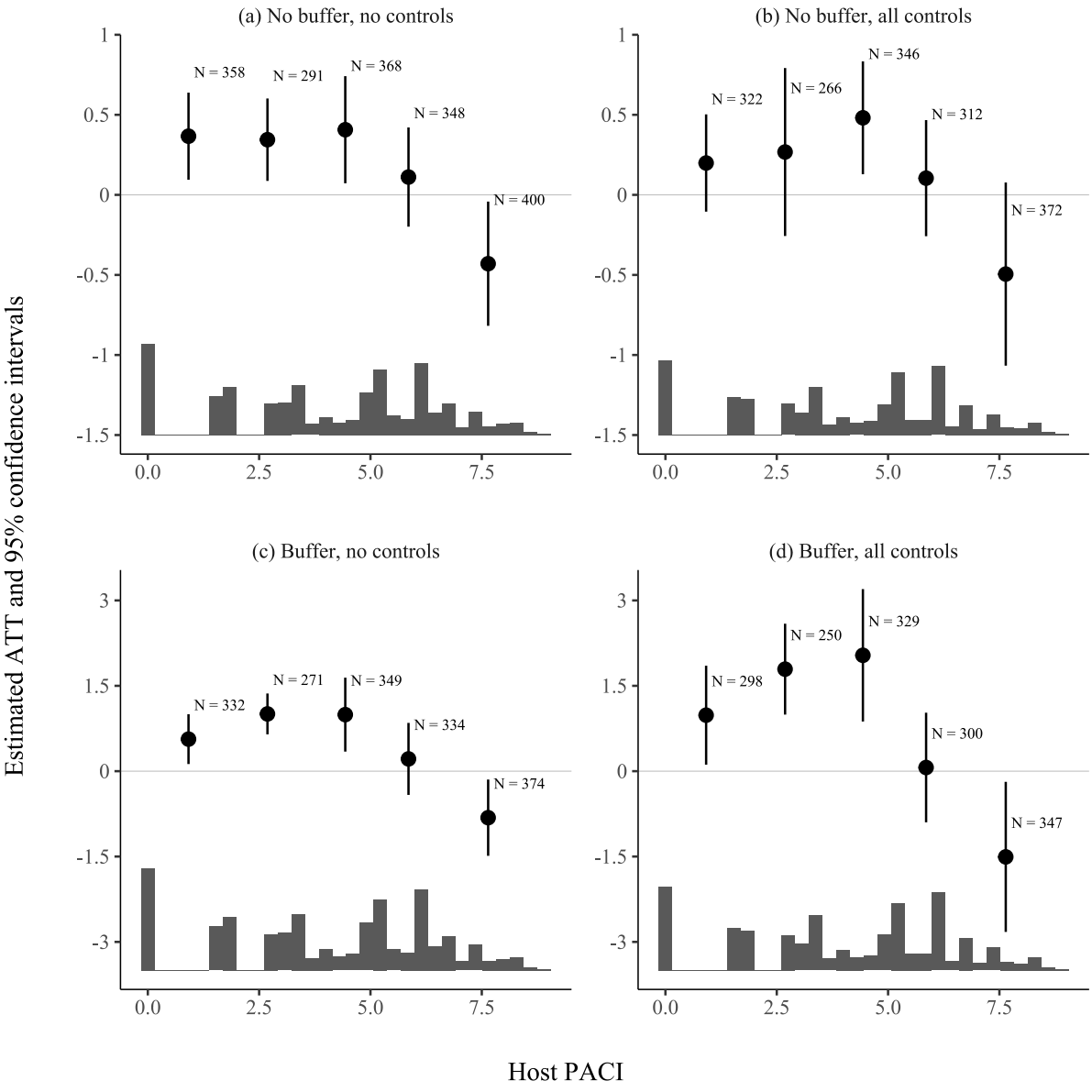

Figure I.1: Country-level data: 2FE binning estimator.

J Dyadic country-level analysis: Models with interactions

As a last robustness test, I re-estimate my 2FE models but do not employ a binning approach. I test my conditional argument fitting an interactive model. I interact my binary variable of interest *OECD Convention* with the linear and squared measure of *Host PACI*. Standard errors are clustered at the home country level (the level that receives the treatment). Results are reported in table J.1 and are overall consistent with my expectation. I find the estimate of coefficient β_1 to be negative and statistically significant. The estimate of coefficient β_2 is positive but is only statistically significant at the 0.10 conventional level. Estimate for β_3 instead, is insignificant, a finding that confirms the null-effect of the Convention for dyads with very clean host countries.

These models do not technically control for heterogeneity at the home country-level. Dyad-level fixed-effects only allow to remove idiosyncrasies at the dyad-level. Moreover, observations in the dataset are highly hierarchical and cross-nested (each dyad-year is a lower-level observation nested in a dyad, and cross-nested in home and host countries). Such structure can cause correlation between observations and make for unreliable standard errors unless properly modelled (Bell and Jones, 2015). I then re-estimate the interactive models using random effects, to account for such hierarchical structure, model home and host-country specific variation, and ensure correlation is properly accounted for in the standard errors. Table J.2 reports the results obtained. I follow the same step-wise introduction of control variables approach I adopted earlier. In all specifications I include home country, host country, and dyad-level random intercepts. Estimates of β_1 are negative and statistically significant, and those of β_2 are positive and statistically significant. Estimates of β_3 are statistically significant but inconsistent with my argument: they are positive in models 1 and 2, and negative in models 3 and 4.

I propose one last robustness test using dyadic data. I employ a Heckman selection model to account for the selection process of investment destinations for firms: only investments that have been decided-upon are observable. This is known to introduce selection bias in models that do not account for it (Barassi and Zhou, 2012). Table J.3 presents the results, where controls are introduced step-by-step as done previously. Estimates of β_1 are negative and statistically significant in the selection model and in three specifications of the outcome model. Estimates of β_2 are positive and statistically significant in various specifications of the selection and outcome models. This indicates that the Convention enters firms' decision-making process as expected. Again, estimates of β_3 are positive, contrary to my expectation. These coefficients are also similar in size and significance in the outcome model (columns 2, 3, and 4), indicating that the Convention plays a similar effect also in terms of the size of an investment, once the selection problem has been accounted for. With the exception of the parameter representing the effect of the Convention in very clean countries (β_3), results in these tests provide support for the argument advanced.

	Dependent variable:			
	Dyad FDI (log)			
	(1)	(2)	(3)	(4)
OECD Convention ×	-0.016**	-0.023**	-0.022**	-0.021**
Host PACI^2	(0.006)	(0.007)	(0.007)	(0.007)
OECD Convention ×	0.080+	0.112+	0.105+	0.101+
Host PACI	(0.043)	(0.056)	(0.055)	(0.056)
OECD Convention	0.231	0.266	0.188	0.200
	(0.175)	(0.199)	(0.180)	(0.183)
Lag Host FDI (GDP %)		0.005+	0.005+	0.005
		(0.003)	(0.003)	(0.003)
Lag Host GDP per capita		-0.005	-0.005	-0.001
		(0.014)	(0.014)	(0.013)
Lag Host Trade (GDP %)		-0.0004	-0.0003	-0.0002
		(0.002)	(0.002)	(0.002)
Lag Host POLCON III		-0.060	-0.056	-0.057
		(0.112)	(0.110)	(0.113)
Lag Host Democracy		-0.134	-0.131	-0.141
		(0.104)	(0.104)	(0.101)
Lag Host GDP (log)		1.132***	1.153***	1.134***
		(0.266)	(0.267)	(0.260)
Lag Host Judiciary Indep.		0.816+	0.724+	0.747+
		(0.433)	(0.428)	(0.421)
Home GDP per capita			0.041	0.043
			(0.038)	(0.038)
Home GDP Growth (%)			0.019	0.020
			(0.012)	(0.012)
Home Judiciary Indep.			1.438	1.421
			(1.088)	(1.080)
Dyad BIT				0.187*
				(0.077)
Dyad FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Num.Obs.	13176	11471	11460	11460
R2	0.834	0.839	0.840	0.840
Note:	+ p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001			

Table J.1: Dyadic country-level data. Twoway Fixed-Effect Models with interaction terms.

	Dependent variable:			
	Dyad FDI			
	(1)	(2)	(3)	(4)
OECD Convention × Host PACI^2	-0.017^{***}	-0.021^{***}	-0.024^{***}	-0.023^{***}
	(0.005)	(0.005)	(0.005)	(0.005)
OECD Convention × Host PACI	0.081^{*}	0.111^{**}	0.126^{**}	0.121^{**}
	(0.038)	(0.039)	(0.039)	(0.039)
OECD Convention	1.017^{***}	0.716^{***}	-0.274^{**}	-0.278^{**}
	(0.121)	(0.134)	(0.093)	(0.093)
Host PACI^2	-0.040^{***}	0.018^{*}	0.022^{*}	0.018^{*}
	(0.007)	(0.008)	(0.008)	(0.008)
Host PACI	0.036	-0.142^{*}	-0.160^{*}	-0.136^{*}
	(0.059)	(0.062)	(0.064)	(0.062)
Lag Host FDI (GDP %)		0.005^{**}	0.005^{**}	0.005^{**}
		(0.002)	(0.002)	(0.002)
Lag Host GDP per capita		0.013^{***}	0.013^{***}	0.013^{***}
		(0.004)	(0.004)	(0.004)
Lag Host Trade (GDP %)		0.004^{***}	0.004^{***}	0.004^{***}
		(0.001)	(0.001)	(0.001)
Lag Host POLCON III		−0.065	−0.069	−0.031
		(0.130)	(0.129)	(0.128)
Lag Host Democracy		−0.042	−0.070	−0.045
		(0.082)	(0.082)	(0.081)
Lag Host GDP (log)		0.480^{***}	0.490^{***}	0.514^{***}
		(0.034)	(0.034)	(0.033)
Lag Host Judiciary Indep.		0.785^{***}	0.770^{**}	0.600^{**}
		(0.234)	(0.238)	(0.232)
Home GDP per capita			0.167^{***}	0.167^{***}
			(0.007)	(0.007)
Home GDP Growth (%)			0.001	0.004
			(0.009)	(0.009)
Home Judiciary Indep.			-1.695^{***}	-1.646^{***}
			(0.308)	(0.304)
Dyad BIT				0.062
				(0.046)
Dyad Common Language				0.877^{***}
				(0.154)
Dyad Colonial Relation				1.184^{***}
				(0.193)
Dyad distance				-0.006^{***}
				(0.001)
Constant	2.758^{***}	-11.350^{***}	-13.387^{***}	-13.838^{***}
	(0.124)	(0.940)	(0.966)	(0.931)
Home-Host, year intercepts	Yes	Yes	Yes	Yes
Dyad intercepts	Yes	Yes	Yes	Yes
Observations	13,176	11,471	11,460	11,460
Log Likelihood	−22,710.600	−19,754.070	−19,387.740	−19,317.970
Akaike Inf. Crit.	45,441.190	39,542.140	38,815.470	38,683.940
Note:				$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Table J.2: Dyadic country-level data. Multilevel models

Table J.3: Dyadic country-level data. Heckman selection models

	Dependent variable:			
	Dyad FDI (log)			
	(1)	(2)	(3)	(4)
Selection model				
OECD Convention ×	-0.01^{***}	-0.01^{***}	-0.01^{***}	-0.01^{***}
Host PACI^2	(0.00)	(0.00)	(0.00)	(0.00)
OECD Convention ×	0.01	0.04^{+}	0.03	0.02
Host PACI	(0.02)	(0.02)	(0.02)	(0.02)
OECD Convention	1.16^{***}	1.11^{***}	0.49^{***}	0.49^{***}
	(0.04)	(0.04)	(0.04)	(0.04)
Host PACI^2	-0.01^{***}	0.01^{***}	0.01^{***}	0.01^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Host PACI	0.03^{**}	-0.03^{**}	-0.06^{***}	-0.08^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Lag Host FDI (GDP %)		0.00	0.00	0.00
		(0.00)	(0.00)	(0.00)
Lag Host GDP per capita		-0.01^{***}	-0.00^{***}	-0.00^{**}
		(0.00)	(0.00)	(0.00)
Lag Host Trade (GDP %)		0.00^{***}	0.00^{***}	0.00^{***}
		(0.00)	(0.00)	(0.00)
Lag Host POLCON III		0.05	0.01	0.03
		(0.05)	(0.05)	(0.05)
Lag Host Democracy		0.00	0.02	−0.01
		(0.02)	(0.02)	(0.03)
Lag Host GDP (log)		0.10^{***}	0.14^{***}	0.14^{***}
		(0.01)	(0.01)	(0.01)
Lag Host Judiciary Indep.		0.29^{***}	0.25^{***}	0.19^{***}
		(0.05)	(0.05)	(0.06)
Home GDP per capita			0.03^{***}	0.03^{***}
			(0.00)	(0.00)
Home GDP Growth (%)			0.04^{***}	0.03^{***}
			(0.00)	(0.00)
Home Judiciary Indep.			1.19^{***}	1.27^{***}
			(0.04)	(0.04)
Dyad Common Language				-0.16^{***}
				(0.02)
Dyad Colonial Relation				0.58^{***}
				(0.03)
Dyad BIT				0.41^{***}
				(0.02)
Constant	-1.11^{***}	-3.85^{***}	-6.37^{***}	-6.25^{***}
	(0.02)	(0.16)	(0.18)	(0.18)
Outcome model				
OECD Convention ×	−0.47	-0.13^{**}	-0.06^{***}	-0.05^{***}
Host PACI^2	(0.58)	(0.04)	(0.01)	(0.01)
OECD Convention ×	0.83	0.43^{+}	0.30^{***}	0.25^{***}
Host PACI	(1.67)	(0.22)	(0.07)	(0.06)
OECD Convention	79.36	10.71^{***}	0.35	0.10
	(93.42)	(2.97)	(0.25)	(0.21)
Host PACI^2	−0.57	0.06^{*}	0.05^{***}	0.04^{***}
	(0.63)	(0.03)	(0.01)	(0.01)
Host PACI	2.04	-0.39^{**}	-0.36^{***}	-0.26^{***}
	(2.37)	(0.15)	(0.05)	(0.05)

Lag Host FDI (GDP %)		0.02^{*}	0.02^{***}	0.02^{***}
		(0.01)	(0.00)	(0.00)
Lag Host GDP per capita		-0.06^{**}	0.00	0.01^{*}
		(0.02)	(0.00)	(0.00)
Lag Host Trade (GDP %)		0.02^{**}	0.01^{***}	0.01^{***}
		(0.01)	(0.00)	(0.00)
Lag Host POLCON III		0.22	0.03	0.13
		(0.51)	(0.18)	(0.16)
Lag Host Democracy		-0.18	-0.00	0.09
		(0.23)	(0.09)	(0.08)
Lag Host GDP (log)		1.28^{***}	0.86^{***}	0.76^{***}
		(0.29)	(0.07)	(0.05)
Lag Host Judiciary Indep.		3.38^{***}	1.24^{***}	0.92^{***}
		(0.98)	(0.23)	(0.19)
Home GDP per capita			0.24^{***}	0.22^{***}
			(0.01)	(0.01)
Home GDP Growth (%)			0.04^{*}	0.03
			(0.02)	(0.02)
Home Judiciary Indep.			0.75	-0.07
			(0.66)	(0.57)
Dyad Common Language				0.31^{**}
				(0.10)
Dyad Colonial Relation				2.11^{***}
				(0.23)
Dyad BIT				0.32^{+}
				(0.17)
Constant	-146.10	-52.88^{***}	-30.78^{***}	-26.05^{***}
	(178.27)	(14.10)	(3.63)	(2.86)
Inverse Mills Ratio	92.99	12.65^{***}	3.24^{***}	2.24^{***}
	(110.52)	(3.66)	(0.66)	(0.53)
Sigma	81.03	11.20	3.35	2.66
Rho	1.15	1.13	0.97	0.84
R^2	0.11	0.13	0.43	0.47
Num. obs.	77798	60668	56920	56920
Censored	64622	49197	45460	45460
Observed	13176	11471	11460	11460
Note:	$+ \text{ p} < 0.1$, * p < 0.05, ** p < 0.01, *** p < 0.001			