The Conditional Arm of the Law. The Effect of the OECD Anti-Bribery Convention on Foreign Direct Investment*

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Abstract

Countries have adopted policies to prevent their domestically-incorporated firms from violating environmental, social, and governance (ESG) values abroad. By threatening judicial prosecution at home, countries conscript domestic companies to diffuse cleaner business models abroad. Evidence shows these efforts are often effective. Yet, these policies are often criticized for increasing costs to firms and deterring their investment in countries with poor ESG records. On the other hand, cleaner business models are also expected to cut down informal costs and *favor* investment. This paper studies the impact of corporate criminal policies home states adopted with the 1997 OECD Anti-Bribery Convention on their firms' outward investment. It argues that their effect on investment depends on the level of corruption of the host economy. The effect is null in clean countries. Where corruption is mild, anti-bribery laws empower firms: they provide a legal leverage to refuse paying bribes and cut related costs. The effect on investment is positive. Where corruption is endemic, instead, these policies deter investment: they expose firms to the risk of prosecution without providing any effective leverage. The effect is negative. Multilevel logit models test the argument, explaining investment decisions of 3871 individual firms between 2006 and 2011. Companies from signatories have a 40% higher probability of investing in mildly corrupt economies than those from non-signatories, which plummets to −50% in extremely corrupt countries. Difference-in-differences models of country-dyad investment flows and a generalized synthetic control design corroborate this finding. Results show that corporate policies do not have a univocally negative effect on investment, although they pull firms away from economies with extremely poor ESG records.

Keywords: Foreign direct investment; multinational corporations; corporate regulations; anti-bribery; OECD Anti-Bribery Convention

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Introduction

Firms inform their decision of a foreign investment by assessing long-term risks and benefits (Jensen, 2008). Investments create jobs, have technological spillovers, and favor growth of the host country. Yet, companies can resort to nasty business practices when advancing in host economies. Home states have adopted corporate criminal regulations to curb wrongdoing in violation of clean environmental, social, and governance (ESG) practices by their firms' foreign-owned branches. By threatening judiciary repercussions at home, countries are able to conscript domestically-incorporated companies to diffuse policies abroad. This regulatory strategy is a powerful tool to advance ESG values and was shown to be effective (Jensen and Malesky, 2018; Kaczmarek and Newman, 2011). Yet, it is also often blamed for a potential side effect. It would increase risk to companies, raising costs of investing in host countries with poor ESG records. This would undermine their investment and favor unregulated competitors.

The anti-bribery regime offers perhaps the best example of the problem. Bribery is a documented strategy that firms can resort to when investing abroad to circumvent competition and extract rents (Malesky et al., 2015). Its detrimental effects are well known (Rose-Ackerman, 1975). In the 1990s the main exporters of investment have adopted policies to prevent their firms from engaging in such behaviors abroad. These anti-bribery policies have been frequently criticized for disadvantaging companies in corrupt host economies *vis-à-vis* unregulated competitors (Gutterman, 2015; Tarullo, 2004).

Do such policies deter foreign investment in countries with poor ESG records? Political economy still offers no conclusive answer. An opposite hypothesis can be advanced alongside the often-cited *deterrence* argument. Policies enforced by the home country can also *empower* firms abroad, providing them with cleaner and more efficient business models to cut costs induced by poor local ESG levels. Again, bribery provides a good example. Anti-corruption policies imposed by home countries can provide regulated firms with a legal leverage to resist bribe requests from public officials in corrupt economies. Companies would then cut bribery-induced costs (Davis, 2019). This prerogative is precluded to competitors with no such legal standards, who then operate at a disadvantage. Empirical studies on the matter offer no conclusive evidence, with studies finding a negative, positive, or null effect of anti-bribery policies on investment. Almost all of them rely on country-aggregated data. The choice prevents to trace existing heterogeneous effects of anti-bribery policies on individual firms' decisions, thus making it impossible to disentangle the opposite effects of competing mechanisms (Kerner, 2014; Zhu and Shi, 2019).

In this article I study the effect of home countries' corporate criminal policies on foreign investments by their multinational corporations (MNCs), in the case of anti-bribery. I propose an argument to unify the *deterrence* and *empowerment* hypotheses. I argue that both pulls are simultaneously at play. The direction of the resulting effect depends on the *level* of corruption of the host economy. Anti-bribery policies provide firms with a legal ground which is strong enough to refuse bribe requests from public officials only where their bargaining power is relatively low. Public officials' power to demand bribes increases in the level of corruption of a country (Ades and Di Tella, 1999). I thus claim the resulting effect of the policies is null in very clean countries, positive in mildly corrupt ones, and negative only in extremely corrupt destinations. I focus on laws under the 1997 OECD Anti-Bribery Convention[1], that made bribe payments abroad a criminal offence in 44 signatory countries.

Two empirical exercises support my argument. First, I leverage on 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 OECD anti-bribery policies make investment decisions conditionally on the level of corruption of the host economy, non-linearly. Firms from signatories are no more likely than their unregulated competitors to invest in clean economies. They are up to 40% more likely to invest in mildly corrupt host economies. Instead, they are 50% less likely to invest in *extremely* corrupt destinations. This exercise provides insights in support of my argument at the very micro level of the investment decision-makers. Second, I corroborate these findings employing country-dyadic data. I employ difference-in-differences and a generalized synthetic control design to achieve a more robust identification of the effect advanced. Results confirm findings from the firm-level analysis. Overall, empirics show that home anti-bribery policies affect firms' investment, but not necessarily in a negative way. Concerns on the anti-business nature of these laws should be taken with skepticism.

Implications travel beyond the anti-bribery regime. Home states regulate foreign behaviors by domestically-incorporated companies in areas including money laundering, human rights protection, taxes, labor rights, and environmental impact. I show that home countries can leverage on companies to diffuse better ESG performance across borders, by threatening domestic repercussion for wrongdoing, without undermining their foreign economic activity. This is good news for their home states. At the same time, my conclusions are bad news for host countries with *extremely* poor ESG records, where regulations to advance such values would be perhaps most needed. Here, home states' strategy backfires. These host countries are left exposed to investments from unregulated companies, that are not constrained by ESG standards. This pessimistic conclusion adds to previous findings on the perverse effects of corporate policies induced by the existence of different standards among competitors (Brazys and Kotsadam, 2020; Chapman et al., 2020). Findings contribute to a literature in international political economy that has studied the diffusion of corporate policies across borders, addressing their determinants (Putnam, 2009), effects on states (Kaczmarek and Newman, 2011), and on non-state actors (Findley et al., 2015; Jensen and Malesky, 2018; Kalyanpur and Newman, 2019).

The next section expands on the puzzle using past literature and anecdotal evidence. Then, I propose an argument for the effect of OECD anti-bribery policies on investment, conditional on the level of corruption of the host. Two empirical exercises follow. I conclude discussing limitations.

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

1 What effect for anti-bribery policies on foreign investment?

Foreign investment is made of individual decisions of private or public-owned firms to project their presence abroad (Jensen et al., 2012). In contrast to international trade, it entails the ownership[2] of a firm in the host country (called "subsidiary") from another firm in the headquarter or home country (called "parent")[3]. In comparison to "portfolio investment", made for speculative purposes, a foreign direct investment (FDI) is typically a long-term initiative with productive goals. Firms decide to go multinational if expected advantages in ownership, location or internalization[4] terms outweigh costs (Dunning, 1980) and political risk (Jensen, 2008) in the productive process. The resulting investment can have positive effects in the host country in terms of job creation, technology spillovers, economic growth (Borensztein et al., 1998), and democratization (Eichengreen and Leblang, 2008; Li and Reuveny, 2003).

However, investment can also have adverse effects for the host country. Firms can resort to bribery[5] as a business strategy in foreign markets (Søreide, 2006). Bribe payments are documented in procurement and registration of an MNC in corrupt economies (Gueorguiev and Malesky, 2012). The effect of corruption on investments has been subject to intense scrutiny. Corruption generally deters business (Habib and Zurawicki, 2002). It reduces the probability that an investment will take place (Barassi and Zhou, 2012) because it increases its costs, like a tax (Treisman, 2007; Wei, 2000). Corrupt contracts are also uncertain and inefficient, since they lack systems to be enforced (Lambsdorff, 2002; Rose-Ackerman, 1975). Yet, in specific markets bribery is a profitable strategy to crowd out competitors and establish oligopolies, from which firms can extract considerable rents (Zhu, 2017). In this case it is often worth its price. This strategy has been shown to work quite well if played by MNCs from developed countries active in less-advanced economies (Pinto and Zhu, 2016). It is observed in extractive industries, where the existence of natural barriers facilitates market exclusion (Knutsen et al., 2017), but also in markets artificially restricted by institutions (Malesky et al., 2015).

Home states have adopted corporate policies to intervene in this calculus and univocally increase costs of corruption to firms (Cuervo-Cazurra, 2008). Policies under the 1997 OECD Convention[6] grant

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

[3] Foreign ownership can occur in the form of a "greenfield" – setting up an entirely new overseas branch – or a "brownfield" investment – acquisition of already-existing facilities. The two strategies present different advantages to firms (Görg, 2000) but I equate them in the argument presented here.

[4] Ownership advantages include access to foreign patents or technologies. Location advantages cover proximity to strategic foreign markets or cheaper factors of production, and the possibility of bypassing trade barriers. Internalization advantages include incentives to keep strategic assets and information within the firm (Jensen, 2008). This framework thus combines elements from the so-called "horizontal integration" and "vertical integration" theories (Barassi and Zhou, 2012).

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

[6] The Convention is among the strongest anti-corruption regulations (Bukovansky, 2006; Spahn, 2013). MNCs under this regulatory umbrella account for more than 80% of global outbound foreign direct investment stocks and include 95 of the 100 largest non-financial enterprises (OECD, 2018). As of September 2020, 44 signatory countries include all current OECD members and 7 non-member states: Argentina, Brazil, Bulgaria, Costa Rica, Peru, South Africa, Russia.

home states jurisdiction to scrutinize and prosecute bribery perpetrated beyond national borders by their companies, foreign employees, or entities they own abroad (Brewster, 2017). Signatory home states thus effectively *conscript* domestically-incorporated firms to diffuse anti-bribery standards abroad, under the threat of fines and judiciary repercussions at home for foreign corrupt payments. To take a recent example, in June 2019 the U.S. corporation Walmart Inc. disbursed $282 million to U.S. federal authorities in admission of corrupt payments made by its Brazilian subsidiary[7].

The strategy of conscripting domestically-incorporated firms to diffuse policies abroad has not been adopted only in the area of anti-corruption. In fact, states regulate foreign operations of firms and attempt to curb such activities as money laundering (Sharman, 2011) or financing of terrorism (Findley et al., 2014). Similar efforts have also been recently initiated at the OECD level to curb phenomena like base erosion and profit shifting (BEPS) by corporate tax payers, or violation of human and labor rights.

An alleged side effect of these policies has caused them to receive harsh criticism over time. These policies would intrude into firms' overseas business, compromising it. The U.S. unilaterally[8] adopted a legislation to prohibit foreign bribery in 1977: the Foreign Corrupt Practices Act (FCPA). Immediately, U.S. firms lamented the FCPA was an anti-business policy that increased their cost and disadvantaged them in international markets *vis-à-vis* competitors with no such standards (Brewster, 2017). An early report found U.S. foreign investment suffered from this law in the 1980s (Hines, 1995). A common OECD anti-bribery regulation was aimed at mitigating these concerns, but its adoption was delayed for 20 years precisely due to resistances among U.S. partners about its possible deterrent effect on investment (Gutterman, 2015; Tarullo, 2004).

European and U.S. anti-bribery policies have kept receiving criticisms even after they were eventually coordinated at the OECD[9] (Gutterman, 2017). The existence of two legal standards, with some firms who are bound by anti-bribery policies and some who are not, is an ongoing concern not only in the opinion of politicians or reporters. Recent studies pointed out that bribery by MNCs outside the umbrella of the OECD Convention has *increased*, somehow filling a gap left by their competitors (Chapman et al., 2020).

Empirical evidence about the effect of these policies on investment, yet, is not conclusive. Some studies find firms subject to anti-bribery policies invest less in corrupt economies, because they risk prosecution (Blundell-Wignall and Roulet, 2017; Cuervo-Cazurra, 2008; Hines, 1995). Others argue public officials are more likely to demand bribes from firms that are *not* subject to these policies, because they lack

[7] See statements from the DOJ: https://www.justice.gov/opa/pr/walmart-inc-and-brazil-based-subsidiary-agree-pay-137-million-resolve-foreign-corrupt and the SEC: https://www.sec.gov/news/press-release/2019-102 (both accessed on September 23rd, 2020).

[8] In the middle of the post-Watergate outrage, the U.S. was the first country in the world to do so and it would take 20 years for its OECD partners to follow the lead.

[9] In 2012 Donald Trump called the U.S. anti-bribery policy "a horrible law" which "should be changed". See: https://fcpaprofessor.com/donald-trump-the-fcpa-is-a-horrible-law-and-it-should-be-changed/ (accessed on September 23rd, 2020). European anti-bribery laws have received no milder judgment: See: https://www.theguardian.com/law/2011/apr/01/revamped-bribery-act-firms-jitters (accessed on September 24th 2020).

a legal ground to oppose the request (Kaufmann and Wei, 1999), an argument which finds evidence to support it (Jensen and Malesky, 2018; Brazys and Kotsadam, 2020). All else equal, this should *favor* firms with anti-bribery standards in international competition. Still others find no evidence for any effect (Hakkala et al., 2008; Smarzynska and Wei, 2000; Wei, 2000).

One explanation for such mixed evidence is the use of country-level data. This methodological custom turns into a twofold problem. First, it obscures heterogeneous effects (Zhu and Shi, 2019). It makes it impossible to explicitly disentangle two competing mechanisms on the effect of anti-bribery policies on individual firms' investment decisions in corrupt countries. The problem might be rather easily solvable with the use of dyadic country-level data, which still is not customary in this literature.

Yet, a second issue emerges. The use of country-level data turns into a problem of selection bias that is usually overlooked: only investments that have been decided are observable (Barassi and Zhou, 2012). This prevents from understanding how anti-bribery policies affect the choice of individual firms to begin an investment in corrupt countries in the first place. The only study on the matter that, to my knowledge, employs firm-level data and explicitly accounts for this issue of selection finds no effect of OECD anti-bribery policies on firms' decisions (Hakkala et al., 2008). Yet, it relies on observations from Swedish companies only, a sample with very specific characteristics which severely undermine the external validity of the results. Moreover, its observations stretch until 1998, *i.e.* only one year after the ratification of the Convention, and in fact one year *before* its entry into force. The lack of an effect cannot be taken as conclusive.

Finally, anecdotal evidence does not suggest that MNCs from countries with anti-bribery regulations are necessarily penalized by them. In fact, firms have not univocally opposed their introduction. In the 1990s a coalition of anti-corruption non-governmental organizations *and* businesses emerged first in the U.S., then in other OECD countries, to lobby for the adoption of global anti-corruption policies (Gutterman, 2015). In 1997 U.S. Senator Sarbanes, who was working on a revision of corporate laws, even received a letter signed by the CEOs of 35 major corporations, expressing their wish for a quick ratification and implementation of the OECD Convention (US Senate, 1998). At the very least this anecdote suggests that anti-bribery laws are not necessarily disadvantageous to companies.

2 The argument: The conditional effect of anti-bribery laws on foreign investment

What is the effect of home state policies to diffuse ESG values abroad on foreign investment decisions? Are concerns about their anti-business side effect in countries with poor ESG records justified? In this section I provide a theoretical answer specific to the case of laws under the OECD Anti-Bribery Convention.

Policies under the OECD Convention impose various types of costs to firms for foreign bribery. Judicial authorities can (and do) levy blockbuster fines[10]. Monetary disbursements are not limited to fines. A common practice in corporate law enforcement, particularly in the U.S., consists in reaching out-of-court agreements between prosecutors and the firm. This prevents the perils of a judiciary prosecution for the company and its officers. Yet, the process is painful and costly for a company. Usually the firm admits guilt, cooperates with prosecutors, pays an expensive monetary settlement, and pledges to undertake a severe re-structure of its corporate organization and culture to ensure future compliance with anti-bribery standards (Garrett, 2011). Terms often include turning executive offices inside out; setting up systems of internal investigations; having third-parties monitoring activities of a firm for a probation period; and periodically rotating offices around the world to avoid managers established personal connections with local authorities[11].

Financial markets also impose reputational costs for corporate crime (Karpoff et al., 2008). When discovered, foreign bribery easily turns into a scandal with wide international resonance. Inquiries unveil corrupt deals where large sums of money are secretly channeled to personal accounts of dirty public officials, and where the bribe-payer firm extracts huge illicit revenues. Sometimes they involve public figures or politicians in high and visible places in the host country, or companies involved in extensive business around the globe. This is enough material for stories that regularly make the first pages of newspapers and generate public outrage. Markets react to these stories. It is estimated that on average 80% of every lost dollar in share value, following anti-bribery prosecution of a firm, comes from the effect of these scandals on markets, rather than from fines or monetary settlements (Sampath et al., 2018).

I claim that these costs translate into credible information to investors about the legal risk at home in case of bribery abroad, thus regularizing their long-term expectations. Information is key for an investor. A foreign investment entails a long-term commitment to the host economy. Uncertainty about its future can deter it. Political science scholarship has shown that institutions can provide information that improves predictability (Axelrod, 1984). This effect is documented for host country (Alesina and Dollar, 2000; Jensen, 2003, 2008; Li and Resnick, 2003), home country (Beazer and Blake, 2018), and international (Bodea and Ye, 2017; Gray, 2009; Skovgaard Poulsen, 2014) institutions and policies.

How will expectations of anti-bribery costs affect decisions to invest in a corrupt country? Two competing arguments can be advanced. I label the first one *deterrence*. The argument expects firms subject to anti-bribery standards invest less in corrupt economies. Imagine two identical firms, respectively from the U.K. and India, faced decisions to invest in a country like Nigeria, which is known to have corrupt bureaucracies. The U.K. is a ratifier of the OECD Convention, and has anti-bribery regulations in place.

[10] Penalties have increased consistently over the years, to reach records in the order of billions of U.S. dollars in recent judiciary cases. For a top-ten of disbursements under the U.S. FCPA see: https://fcpablog.com/2020/02/03/airbus-shatters-the-fcpa-top-ten/ (accessed on September the 25th, 2020).

[11] For a textbook example, see the drastic changes 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 (accessed on September the 25th, 2020).

India is not. Investments in a corrupt host country like Nigeria often look like a bid between competing firms, where public officials can demand competitors a bribe to facilitate business (Beck and Maher, 1986). Firm agents in Nigeria have an incentive to pay the bribe: winning such bids have licit rewards (such as career advancements, or prestige), and often illicit ones (such as kickbacks agents can re-direct to their own personal accounts).

The headquarter office of the British firm anticipates its agents would operate in these conditions, were an investment made. Being subject to OECD anti-bribery policies regularizes expectations about legal costs at home in that event. In case agents in Nigeria committed bribery, prosecution in the U.K. would be a likely risk. Anti-bribery policies turn a possible scandal of corruption by a Nigerian subsidiary into a significant cost for the entire company. The firm from India, instead, does not face the same concern: its agents in Nigeria can bribe to secure contracts without the parent risking judiciary repercussions. All else equal anti-bribery standards thus deter the parent firm from the U.K. to invest in Nigeria, while the firm from India is not deterred. *Deterrence* expects firms subject to anti-bribery standards would invest *less* than competitors in corrupt countries.

Yet, a second argument justifies the opposite expectation. I label it *empowerment*. Anti-corruption policies are intended to reduce corruption-induced uncertainty and ensure cleaner business models without off-the-record expenditures (Lambsdorff, 2002). Anti-bribery policies help companies put in place internal systems of compliance to oversee their agents abroad, and deter them from paying bribes (Davis, 2019). They also provide foreign agents and branches a legal ground to refuse paying extra fees, which increases their bargaining power *vis-à-vis* public officials (Hakkala et al., 2008; Kaufmann and Wei, 1999). Public officers are also less likely to expect fees from regulated firms in the first place, given that they themselves could end up being involved in scandals with worldwide resonance.

Regulated firms can thus leverage on anti-bribery standards to cut corruption-induced expenses: bribe fees, uncertainty, and transaction costs (Rose-Ackerman, 1975). In the investment decision stilized before, this argument claims anti-bribery policies empower the British firm in Nigeria. On the other hand, the Indian firm is not subject to the same regulation. Its agents in Nigeria cannot leverage on these policies to refuse paying bribes, and have no enhanced bargaining power. As a consequence, public officials demand more bribes from unregulated firms (Brazys and Kotsadam, 2020). Their operations will therefore be more inefficient and costly, and they will be worse off in competition. According to the *empowerment* argument, regulated firms would invest *more* in corrupt economies.

How to reconcile these opposite expectations? I argue that both pulls are at play. Their net effect on investment depends on the level of corruption of the host economy, because public officials' power increases in it (Ades and Di Tella, 1999; Svensson, 2003), and so does their operating space for demanding bribes. *Deterrence* prevails when the host country is extremely corrupt. In these economies bribery is perceived as an expected business custom, and often a necessary condition to entry (Zhu, 2017). This gives public official a strong position to demand fees, and makes it highly unlikely that they refrain from doing it. All parent firms include these fees in expected costs, however firms subject to anti-bribery standards face greater costs than competitors, since they risk prosecution in their home country. Anticipating these conditions, they are less likely to invest in very corrupt economies.

In economies where corruption is diffused but milder, instead, *empowerment* prevails. Bribery here is not the necessary way to conduct business and public officials' room to demand bribes is limited. Also, bribery is unlikely to represent a condition for firms to enter the economy. Firms subject to anti-bribery standards find they can leverage on these rules and enhance their bargaining power. They have the legal ground to refuse paying bribes, thus they can cut costs. This possibility is precluded to their unregulated competitors, who operate at a disadvantage. As a result, firms from signatories are more likely to invest in these economies.

In very clean economies, finally, anti-bribery policies should neither advantage nor disadvantage firms, as it is very unlikely that corrupt fees are expected at all. Overall, anti-bribery corporate policies do not necessarily penalize foreign investment. Rather, their effect depends on characteristics of the host country. Regulated companies find themselves advantaged in some economies and at a disadvantage in others, and choose investment destinations accordingly. In this argument the effect of anti-bribery policies on the probability that a firm invests abroad depends non-linearly on the level of corruption of the host economy, which proxies for the bargaining power of public officials.

I formalize this expectation drawing from political economic models of investment decisions conditional on corruption (Barassi and Zhou, 2012; Hakkala et al., 2008; Smarzynska and Wei, 2000). A firm f from country i is observed to invest in country j ($I_{fij} = 1$) only if the value of a latent variable I^*_{fij}, representing its propensity to invest, is greater than 0. Equation 1 expresses the latent variable I^*_{fij}. It 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 (S_i) on the propensity to invest abroad (I^*_{fij}) depends on the level of corruption of the host country (C_j), in a non-linear manner[12].

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

From equation 1, the effect of the OECD Convention on the propensity of a firm to invest equals the partial derivative of I^*_{fij} with respect to S_i:

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

[12]When the model is estimated, in the next section, other factors explaining I^*_{fij} are summarized in the matrix $\mathbf{X_{fij}}$, while u_{fij} is the idiosyncratic error term.

Equation 2 tells us that the effect of the OECD Convention on the propensity of a firm to invest abroad, conditional on the level of corruption of the host country, should trace a parabola. Figure 1 illustrates my expectation on its shape. It reports the level of corruption of the host economy C_j on the x axis. The y axis, instead, reports the marginal effect that ratifying the OECD Convention has on the propensity for a home country's firms to invest overseas ($\frac{\partial I^*}{\partial S}$). For low levels of corruption of the host country, the effect should be zero. As the host economy becomes more corrupt, a home country ratifying the Convention advantages its firms and increases their propensity to invest. *Empowerment* prevails here. As the host country becomes more corrupt, this effect reaches a maximum, then decreases. In extremely corrupt host countries, ratifying the OECD Convention disadvantages firms due to higher risks of prosecution. Here *deterrence* dominates and the effect on the propensity to invest is negative. In terms of equation 2, this means that parameter β_1 is expected to be negative, β_2 positive and β_3 null.

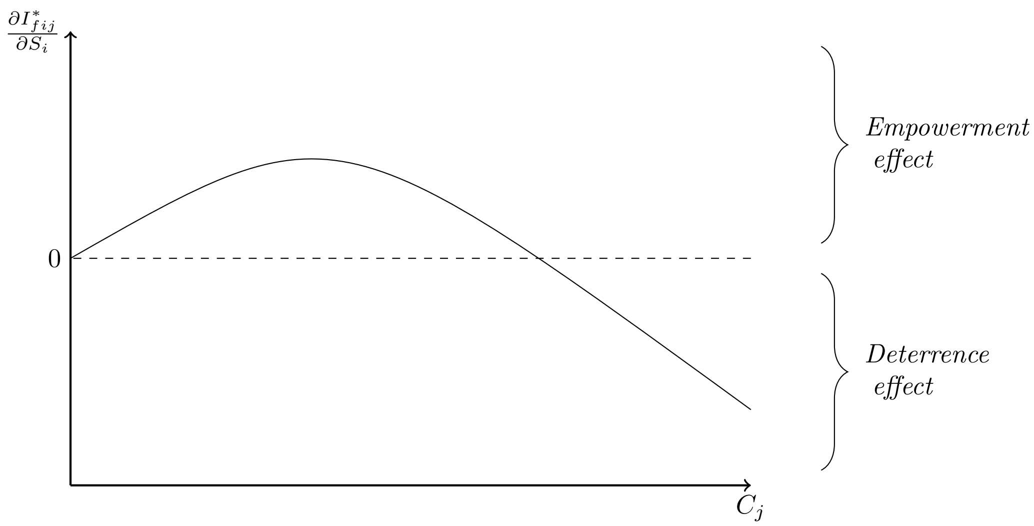


Figure 1: Non-monotonic marginal effect of the OECD Convention on investments, conditional on host country corruption

3 Empirical analysis

I claim that the OECD Convention affected foreign investment decisions by companies conditional on the level of corruption of host economies, in the non-monotonic manner sketched by figure 1. I propose two empirical exercises to test my argument. The first one uses firm-level data on foreign investment decisions. It offers insights into the very micro-level decision-making process of actors making an investment. Results are based on a selection on observables design and lend broad support to my theoretical claim. The second empirical exercise uses country-level data on dyadic FDI flows. Its goal is to provide a more solid identification on top of the micro-level evidence from the first exercise. It employs difference-in-differences and a generalized synthetic counterfactual design to show expectations from the theory and micro results from the firm-level study are confirmed.

3.1 Firm-level data

I estimate model 2 with firm-level data drawn from the Orbis Corporate Ownership Database[13], retrieved from Beazer and Blake (2018). This dataset reports information on foreign subsidiary incorporation from 3871 individual parent firms between 2006 and 2011. It reports the country of origin of the parent firm (home country) and that of the subsidiary (host country) for each incorporation. Represented home economies are 61, host countries are 84. Data also include firm-level, country-level, and dyadic covariates.

The dataset reports the "ultimate parent" of each foreign subsidiary. It excludes financial investments and small firms[14]. These selections ensure the sample represents a population composed of large MNCs, and investments represent long-term foreign productive enterprises and not speculative ventures[15]. Orbis data have a two-year lag between the moment firms' information is disclosed and the moment it is reported in the data, and have various problems when year-specific information is used to obtain time-series (Kalemli-Ozcan et al., 2015). Both issues are avoided here employing a cross-section of observations between 2006 and 2011. Figure 2 breaks down firms in the resulting database according to their NAICS-2 industrial code. The majority of firms is active in Manufacturing (43%), followed by Management of Companies and Enterprises (17.7%) and Professional, Scientific and Technical Services (8.89%).

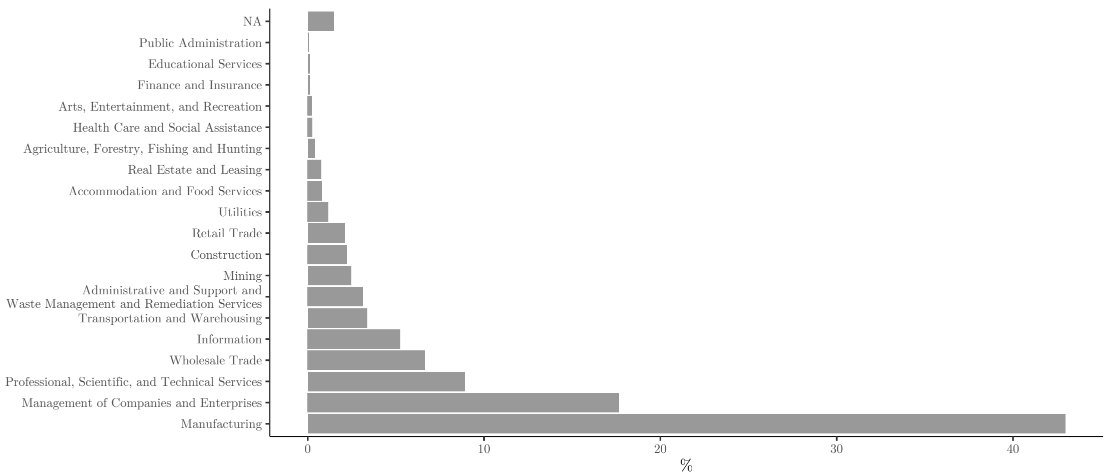


Figure 2: Database description: Percentage of firms in the database by NAICS-2 code

Three reasons make this dataset an optimal source to test my expectation at the level of the actors who face the investment decision. First, it reports firm-level information on the home and host country of each company. This allows me to study my conditional argument where the effect of a home country policy is mediated by host country characteristics. Second, it comprises only large companies involved

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

[14]The "ultimate parent" is defined as the firm owning more than 25% in stakes of the foreign subsidiary. Financial companies, insurance firms, hedge funds, and investment banks are excluded. Small firms have less than one million euros in operating revenues a year, total assets less than two million euros, and less than 15 employees.

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

in long-term foreign investments for productive purposes. Smaller companies or enterprises with short-term investment goals would likely not face the same type of decision problem my theory advances. They should be excluded from the study. Third, information spans only until 2011. Enforcement of the OECD Convention has improvd after 2010 (Jensen and Malesky, 2018). Using information that extends only one year after that turning point allows me to test if there is any effect at a time when arguably the only role played by these regulations was due to their adoption, regardless of their enforcement[16].

I follow Beazer and Blake (2018) and construct a binary outcome variable, called *Subsidiary*, representing whether a firm f from country i has incorporated a subsidiary in country j between 2006 and 2011. The measure does not represent the size of an investment, but this is consistent with my argument predicting its *probability*. The binary dependent variable has a dyadic form. For each parent company f from country i it is assigned a 1 if the firm is reported to have set up a subsidiary in the host country j in the time period of interest. It is assigned a 0, instead, if no subsidiary was established in the (potential) host country j[17]. 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.

My main explanatory variable is *OECD Signatory*. It represents whether the home country i of a parent firm f has ratified the OECD Anti-Bribery Convention by 2005[18]. The variable is binary: it takes value 1 if this condition is met, 0 otherwise.

Next, I need a measure of the moderator: corruption of the host country. Measuring corruption is notoriously difficult. The most common indexes are survey-based and include the World Bank Control of Corruption Estimate (CCE) or Transparency International's Corruption Perception Index. These indicators are typically built surveying the general population or experts (usually businessmen) about perceptions or first-hand experiences of corruption. They are criticized for being weak indicators of the real level of corruption in a country (Olken, 2009). Social desirability biases answers about first-hand experiences (Treisman, 2007). Annual survey-based measures, moreover, are subject to confirmation bias if respondents' answers are informed by previous releases. Finally, they often implicitly adopt a definition of corruption which might not align with respondents' or researchers' (Heywood, 1997). These issues are a notorious source of inconsistency in empirical studies on corruption (Gueorguiev and Malesky, 2012).

An increasingly popular alternative is represented by so-called "objective" measures, that rely upon observable information. These measures have the obvious downside that observed cases of corruption are no good measure of corruption, since when it is most effective it takes place out of sight. The Public Administration Corruption Index (PACI) advanced by Escresa and Picci (2017) proposes a solution.

[16] In a robustness test I then extend data on companies from Beazer and Blake (2018) to information until 2018 and verify that my results hold.

[17] 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 relaxing this condition.

[18] I consider only countries for which the Convention had entered into force by 2005, to make sure that anti-bribery legislations under the OECD Convention were in place at the time my cross-section starts. Information on ratification status was retrieved from the OECD website: http://www.oecd.org/daf/anti-bribery/WGBRatificationStatus.pdf (accessed on September the 26$^{\text{th}}$, 2020). Table B.1 in appendix reports which home countries belong to each group in the sample.

Intuitively, the index compares the *observed* number of cross-border cases of bribery with those that could be *expected* if countries were all equally corrupted, based upon commercial ties. I present the index in more details in appendix. The PACI is suited to measure specifically cross-border bribery as it is defined in this study (see footnote 5).

Escresa and Picci (2017) compute a PACI measure employing information between 1997 and 2012. For each host country j in my dataset I re-compute the index using only information relative to bribes paid between 1997 and 2005 included, since my cross-section starts in 2006. To do so, I draw on the database provided by the authors about observed cases of cross-border bribery. I follow the authors' suggestion and take the natural logarithm of the PACI measure +1 to reduce the skewness of its distribution, and exclude countries for which information is not sufficient to compute a reliable index. The resulting measure *Host PACI* is my main indicator of corruption of the host economy. It ranges from a minimum of 0 (corresponding to very clean economies) to a maximum of 8.90. In a series of robustness tests I substitute it with more traditional perception-based indexes, choosing among the most reliable ones, and verify that my results hold.

I follow Beazer and Blake (2018) and explain my binary outcome variable employing a multilevel logit model[19]. This is a forced choice to correctly specify cross-level interaction effects (Bell and Jones, 2015) like those implied by model 1. This model choice is also suited to the dataset structure, where a firm investing abroad is cross-nested in a directed dyad, and in its home and host countries. Multilevel unobserved heterogeneity in this complex nesting can easily confound the explanation of the outcome variable, therefore it must be properly modelled. To this aim, all specifications include random intercepts at the dyad-level, and at the level of home and host countries. A further specification also includes industry-level intercepts to account for sector-specific heterogeneity. Since no clear hierarchy can be discerned in the data structure, I employ a cross-classified random effect model. A multilevel logit also correctly models the thousands of repeated observations generated by the dyadic structure of the dataset. If their correlation were not properly accounted for, this large number of repeated observations would artificially reduce standard errors to zero and produce unreliable tests of hypotheses.

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

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

distance in kilometres between capitals of the home and host country, and binary indicators measuring whether a bilateral investment treaty (BIT) was signed by the dyad, whether the two countries have a past colonial relationship, and whether they have a common first or official language. Finally, I control for firm-level features: the number of host countries each firm operates in, its age, and its total assets (all logged). Summary statistics are reported in appendix[20] (table B.2).

3.1.1 Results

Table 1 presents my results relative to the variables of interest only[21]. The first four models include random effects at the dyad, home and host country level. The fifth one also includes industry-specific intercepts. All models condition the effect of *OECD Signatory* on the squared and first-degree terms of the host country's corruption measure (*Host PACI*). To ensure that data are not being overfitted, table 1 first includes only the variables of interest. Then, it adds controls at the level of host and home countries (Model 2). Then it adds firm-level covariates (Model 3), then dyadic controls (Model 4). Finally, it adds industry-level intercepts (Model 5).

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Signatory × Host PACI^2	-0.033^{***}	-0.038^{***}	-0.023^{*}	-0.031^{**}	-0.034^{**}
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)
OECD Signatory × Host PACI	0.197^{**}	0.225^{**}	0.163^{*}	0.206^{**}	0.220^{**}
	(0.090)	(0.092)	(0.090)	(0.096)	(0.096)
OECD Signatory	−0.016	−0.034	−0.213	−0.267	−0.282
	(0.165)	(0.192)	(0.246)	(0.205)	(0.205)
Host PACI^2	−0.041	0.013	0.003	0.011	0.013
	(0.033)	(0.029)	(0.026)	(0.027)	(0.028)
Host PACI	−0.097	−0.007	0.023	−0.008	−0.036
	(0.286)	(0.242)	(0.221)	(0.230)	(0.231)
Dyad, country intercepts	✓	✓	✓	✓	✓
Industry intercepts					✓
Country-level controls		✓	✓	✓	✓
Dyad-level controls			✓	✓	✓
Firm-level controls				✓	✓
N. of host countries	84	83	83	83	83
N. of home countries	61	60	60	57	56
Observations	320,913	315,657	315,657	289,732	285,295
Log Likelihood	−31,266.030	−31,117.490	−30,957.630	−25,107.560	−24,775.210
Akaike Inf. Crit.	62,550.060	62,272.990	61,961.250	50,267.110	49,604.410
Note:					*p<0.1; **p<0.05; ***p<0.01

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 associated with the interaction between *OECD Signatory* and the squared *Host PACI* is negative in size and estimated with precision. It is

[20]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.

[21]Full disclosure of all estimates is provided in table B.3.

distinguishable from zero at the 0.01 or 0.05 conventional levels of significance in all specifications but model 3. Here the estimation is less precise, but the coefficient is still significant for conventional levels (p-value: 0.06). Estimates of the coefficient of the interaction with the linear *Host PACI* term are also positive and statistically significant at the 0.05 conventional level, but for Model 3 (p-value: 0.07).

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

The coefficients associated with the un-interacted corruption measures are also not statistically significant. This result informs us that corruption is not a significant determinant of investment decisions for firms that are *not* subject to anti-bribery standards (*OECD Signatory* = 0). It is consistent with concerns expressed in other studies about the perverse effects of anti-bribery regulations on firms outside their jurisdictions (Brazys and Kotsadam, 2020; Chapman et al., 2020).

Interpretation of results is particularly complex in multiplicative models, and requires to compute substantive quantities of interest (Brambor et al., 2006). I then compute the marginal effect of anti-bribery policies to evaluate if the argument formalized by equation 2 is supported. Here, the probability of an investment by a firm is modelled (rather than its propensity). The marginal effect can therefore be computed evaluating the *change* in predicted probability of a firm's *Subsidiary* incorporation when *OECD Signatory* changes from 0 to 1, for given levels of *Host PACI*.

In non-linear specifications marginal effects cannot be computed as with linear models (Ai and Norton, 2003). I follow Beazer and Blake (2018) and compute the change in predicted probability when *OECD Signatory* varies from 0 to 1 holding everything else at its mean. This is equivalent to measuring the change in the predicted probability of an investment when comparing an average regulated company to an average unregulated company, conditional on observables, at given values of *Host PACI*. I compute 95% confidence intervals of this estimated difference simulating 1000 draws from its sampling distribution (King et al., 2000).

Figure 3 shows the results obtained when considering the estimates of model 1 in table 1. It also reports data support for the mediator variable, to ensure results do not depend on extrapolation or interpolation (Hainmueller et al., 2019). Results obtained using the estimates of the other models are consistent with these ones, although confidence intervals become larger, especially for very clean host economies where data support is limited. When *OECD signatory* changes from 0 to 1, the predicted probability that a firm will incorporate a subsidiary changes conditionally on the level of corruption of the host economy, in a non-monotonic way.

The effect can be roughly divided in panels (a), (b), and (c). In panel (a) the change in predicted

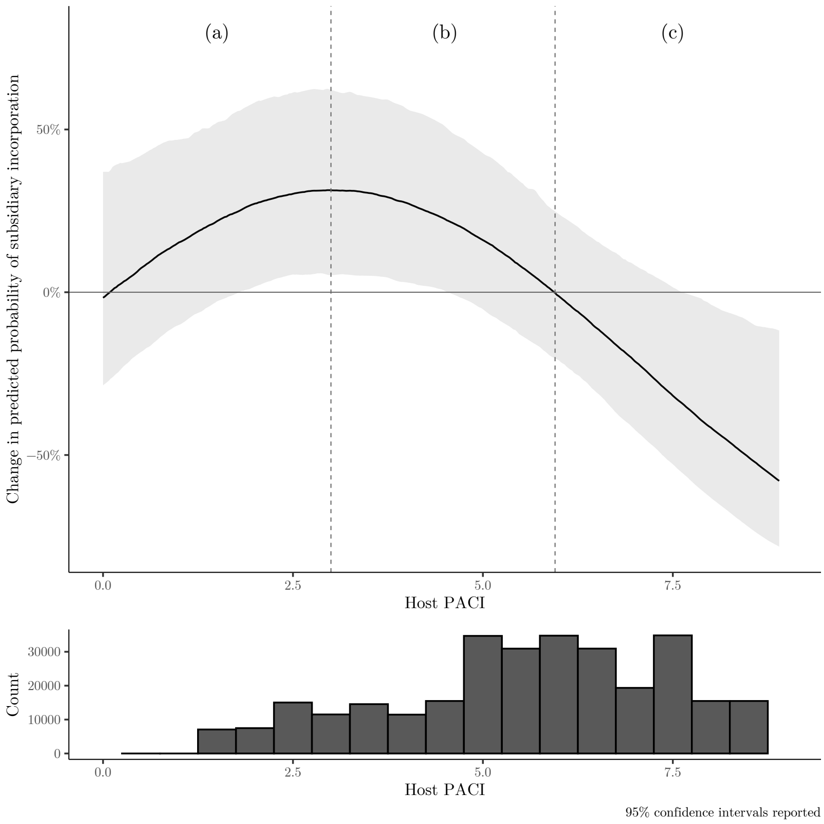


Figure 3: The non-linear effect of *OECD Signatory* on *Subsidiary*, conditional on *Host PACI*

probability is close to zero for very clean host economies (*e.g.*: Canada, Denmark, Sweden). Then it increases as the host country becomes more corrupt, indicating that firms from countries with anti-bribery policies have a higher probability of investing here. At its maximum, firms from signatories have a 40% higher probability of investing in host countries in this interval (Singapore, Taiwan) than their competitors. In panel (b), as the host country becomes more corrupt, this quantity remains positive but declines in size. This indicates that OECD anti-bribery policies still benefit 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 signatory countries are worse off. They have a lower probability of investing here than their counterparts, a quantity that reaches a lowest point of -50% for host countries at the right-end of the corruption scale like Egypt, India, Kazakhstan, Nigeria, or Russia.

I propose extensive tests to show robustness of these results in appendix (table B.4). I first show that an interaction of *OECD Signatory* with a first-degree polynomial of *Host PACI* produces insignificant estimates. This provides confidence that the effect of the OECD Convention on investment is non-linear in corruption. I then test the use of traditional, perception-based indexes of corruption and show that results hold. I also use the original PACI measure in Escresa and Picci (2017) to enlarge the set of host countries included in the analysis. Then, I exclude outlier countries. I finally exclude home countries that ratified the Convention within the 2006-2011 cross-section. Results hold to all such tests.

One final concern with results in table 1 relates to the time-period data from Beazer and Blake (2018) cover[22]. As claimed above, data spanning until 2011 fit my theoretical needs: they allow me to distinguish the effect of anti-bribery regulations from that of their enhanced enforcement. Yet, it can be argued that the Convention did not have any real deterrent effect on companies' investment before 2010 (Jensen and Malesky, 2018). Using information until 2011 might thus downplay the effect of the Convention. I tackle this concern extending the available information for companies in the dataset from Beazer and Blake (2018). I draw from the Orbis Corporate Ownership database and follow the same procedure adopted by the authors to get information on subsidiaries incorporated by these same companies until 2018 (the procedure is detailed in appendix). I then replicate models presented in table 1 on this extended dataset. Estimates are reported in appendix (table B.5). Results are consistent with my previous findings and coefficients are overall more precisely estimated.

3.1.2 Sector-specific analysis

I further investigate my argument moving to a sector-specific analysis, which also works as a placebo test. If my argument is correct, the mechanism should be observable only in industries where bribes are typically paid. In sectors where bribery is no typical custom, instead, anti-bribery policies should not enter firms' decision-making. I exploit information in the database from Escresa and Picci (2017) to perform this test. From this data I first obtain a list of industries with at least one reported case of cross-border bribery prosecution before 2005. I argue that these industries represent sectors where bribes are more often paid[23]. I then replicate the analysis proposed in table 1 within two distinct sub-samples of industries: one including those where bribes were paid at least once (which I call "test")[24], and one including the rest of the sectors in the sample ("placebo").

Figure 4 reports point estimates and confidence intervals obtained within these subsamples[25]. For each subsample I replicate the model including no controls (only random effects) and all controls. Estimates of the coefficients associated with the interaction terms are consistent with the ones presented in table 1 for the "test" subsample. They are even more significant, as standard errors shrink. This indicates that they are estimated with even more precision. They are never distinguishable from zero, instead, in the "placebo" subsample. This provides further confidence on my argument. The conditional effect of the OECD Convention is observed only in industries where corruption is a customary practice.

[22] I thank Celeste Beesley for raising this argument.

[23] 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.

[24] The list of industries in this set and their industrial classification is reported in table B.6.

[25] Full disclosure of the results is reported in table B.7.

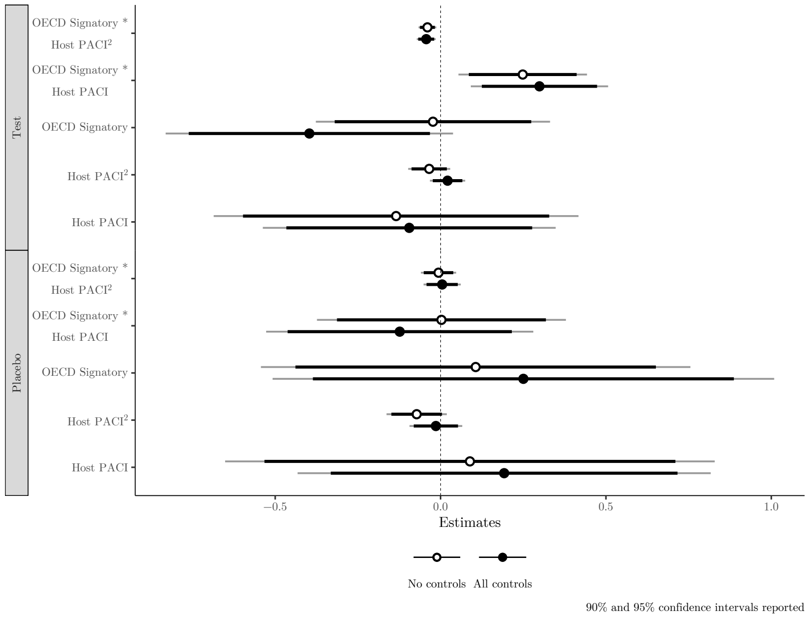


Figure 4: Coefficient plot. Market-specific results. Industries with ("test") and without ("placebo") at least one case of foreign bribery before 2006

3.2 Country-dyadic data

The firm-level exercise provides micro evidence that investment behaviors of firms who are subject to OECD anti-bribery policies depend non-linearly on the level of corruption of the host country. Yet, the analysis suffers from two issues. First, it focuses on cross sectional information between 2006 and 2011: it cannot distinguish if investment behavior changed after ratification of the Convention. Second, selection under OECD policies is not random. Firms under OECD policies have very specific characteristics that distinguish them from those who are not subject to such policies. The previous analysis cannot disentangle these characteristics from the effect of anti-bribery policies themselves. To name one of such potential sources of endogeneity, economies adopting the OECD Convention generally belong to the Global North, and corrupt host economies tend to be concentrated in the Global South. Do results in table 1 represent the deterrent effect of institutions in these host countries for companies headquartered in the Global North, rather than a genuine effect of the Convention in corrupt host economies?

Time-varying data would provide a solution to both problems. They would permit to study changes in investment behavior after the adoption of the Convention. They would also allow to hold constant characteristics that are time-invariant, at least in short time-windows, like institutional features. Unfortunately, Orbis data is not well suited to construct time-series (Kalemli-Ozcan et al., 2015). I therefore proceed differently. I leverage on country-level dyadic data about foreign direct investment from the United Nations Conference on Trade and Development (UNCTAD). My hypothesis is firm-level and predicts *probability* of an investment rather than its size. Aggregate data can obscure individual firms' investment decisions. Yet, I contend a dyadic analysis represents the best feasible solution to tackle the two problems highlighted above. My theory proposes an effect of a home-country policy is conditional on host-country characteristics. Country-level data on investment flows in directed dyads should therefore be able to capture this effect.

I retrieve UNCTAD dyadic country-level data on foreign investment, country-, and dyad-level covariates from Beazer and Blake (2018). My dependent variable is the logarithm of dyad-level FDI flows. Information ranges from 1994 to 2006, included. It thus covers the period preceding the adoption of the OECD Convention, and spans until the very beginning of my firm-level cross-section. Represented home economies are 101 and host countries are 108. The number of directed dyads included is 3591. I report signatory home economies and descriptive statistics in tables C.1 and C.2.

I leverage on time information to tackle the two problems highlighted above. I adopt a two-way fixed-effect (2FE) strategy to explain investment flows. The model includes a binary treatment variable called *OECD Convention* that takes value 1 after the Convention entered into force for dyads whose home country is a signatory. It includes fixed effects at the dyad and at the year-level. It thus removes time-invariant between-dyads variation and models within-dyad changes in investment flows after entry into force of the Convention. The inclusion of year-fixed effect also removes dyad-invariant time shocks affecting all units in the analysis. The estimate associated with *OECD Convention* thus represents the change in investment following adoption of the Convention for dyads whose home country ratified the agreement, while accounting for trends in investment common to all dyads. It can be interpreted as an average treatment effect on the treated dyads (ATT) from a difference-in-differences design under the assumption that trends in investment between dyads with and without a signatory home country would have been the same in the absence of the Convention ("parallel trends assumption").

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[26] thus the problem appears less concerning. I proceed 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 leverage on 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[27]. Assuming the effect of the Convention on investment was not extinguished in the immediate short term, the method should allow me to detect differences between the two periods.

[26] 1733 dyads out of 3591 include a home country that did not ratify the Convention.

[27] 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.

I test my conditional argument applying a binning estimator similar to that proposed by Hainmueller et al. (2019), which allows to study non-linear conditional effects. I divide my dyads in five subsamples depending on the level of corruption of the host country in the dyad. I measure corruption using the same *Host PACI* index computed for the firm-level analysis. I estimate a different 2FE model in each of these subsamples, so as to capture heterogeneity in the effect of the Convention conditional on characteristics of the host country. The five subsamples are defined based on quintiles[28] of the *Host PACI* distribution. 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.

3.2.1 Results

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 5 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 controls with year fixed effect to control for differential observable trends across dyads[29]. Panels c and d of figure 5 reproduce the same specifications in panels a and b, with the exclusion of observations in the "buffer" years 1999–2001. Standard errors are always clustered at the dyad level.

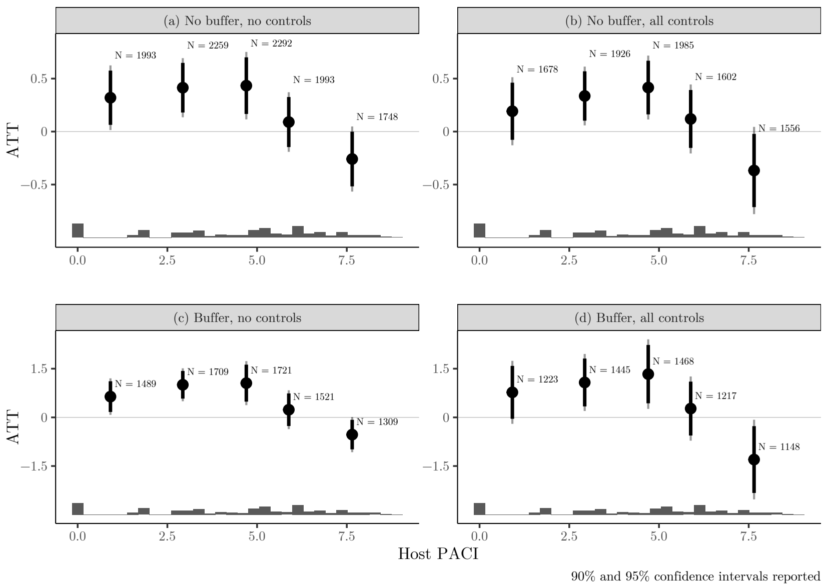


Figure 5: Country-level data: 2FE binning estimator. Models include observations in years 1999–2001.

Estimates across the fives bins reproduce the inverted-U pattern seen in the firm-level analysis. For

[28] The choice to divide the distribution in five parts is purely empirical, as it allows to have sufficient observations in each bin. Attempts with alternative feasible choices (quartiles, sextiles, septiles) provided consistent results.

[29] 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.

dyads in the first bin (those with extremely clean host countries like Canada, Sweden, Denmark, and Australia) entry into force of the Convention seems to have had no effect on investment. The estimated effect here is positive and statistically significant only when controls are not introduced. As the host economy in a dyad gets moderately corrupt, the estimated effect is positive and statistically significant in all specifications. This is true for dyads in the second and third bins, whose host economies include Italy, Mexico, Taiwan, Singapore, and Brazil. When converted from the logarithmic scale, estimates inform us that ratification increased FDI flows in these dyads by about 1.65 million constant US dollars (panels a and b) and up to 3.80 million constant US dollars (panels c and d), on average. Then, the effect starts declining and becomes negative for dyads with extremely corrupt host economies including Bulgaria, Nigeria, Kazakhstan, and Uzbekistan. The negative effect is comparable in size to the one documented for moderately corrupt dyads. Estimates are overall larger in magnitude when introducing the buffer, and their statistical significance remains overall unchanged. This lends confidence that the staggered treatment assignment is not a reason of concern with the present design.

Internal validity of these estimates rests on the parallel trends assumption. The assumption states there is no time-varying unobserved confounder affecting only either the treatment or the control group. The assumption is essentially untestable. The inclusion of time-varying controls at best mitigates concerns about its violation, but cannot ensure it is met. Figure 6 provides a casual look at trends in the yearly average value of the dependent variable for the treatment and control group in each of the five subsamples composing the 2FE bin analysis. The shaded area represents the years when the Convention entered into force at staggered times for these countries. Trends do not appear to diverge significantly in the pre-treatment period between the two groups, perhaps with the exception of bins 1 and 5. This lends some confidence in favor of estimates from figure 5.

A casual look at trends, unfortunately, is no test of the assumption. I mitigate concerns about its potential violations by applying my binning approach in a synthetic counterfactual framework. A synthetic counterfactual is a weighted average of a single treated unit imputed using available information from untreated units in the sample (Abadie et al., 2015). The algorithms building such synthetic unit aim at maximizing similarity in pre-treatment trends between the treated unit and the counterfactual. This allows to make more credible inferences on estimated ATTs. Xu (2017) proposed a generalization of the approach that extends to a panel with several treated units and a sufficient number of untreated ones. It allows to impute one synthetic control for each treated unit, and to derive an average effect from this data. I apply this methodology to impute an untreated counterfactual for each treated dyad in each of the five bins[30].

[30] In the procedure I impose a 2FE model specification, consistent with my previous approach. I employ all available covariates at the home-, host-country, and dyad level to improve the synthetic counterfactual. I drop all treated dyads without at least five pre-treatment observations. This is a recommended practice to obtain reliable synthetic control units. An Expectation Maximization algorithm has been applied to obtain more precise synthetic counterfactuals. Standard errors are estimated with 1000 bootstrap iterations.

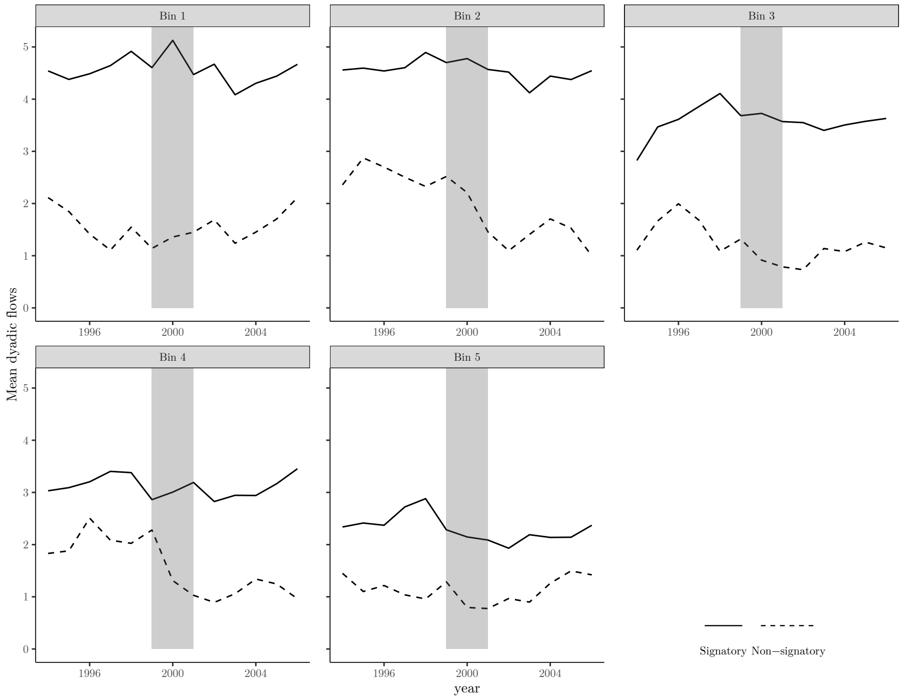
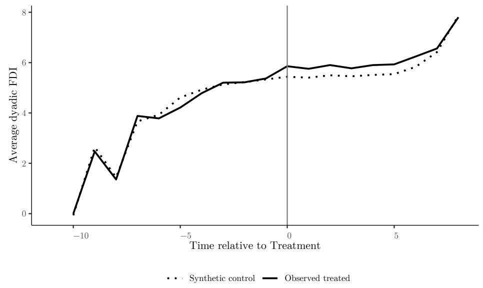


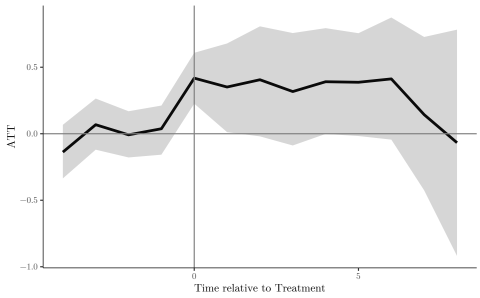
Figure 6: Country-level data: Trends of the dependent variable in treatment and control group.

Figure 7 reports results obtained for the five bins. Average pre-treatment trends of the synthetic counterfactuals seem to closely approximate observed average trends of treated dyads. This lends confidence that synthetic control units were properly imputed. Post-treatment average differences between observed and synthetic controls confirm expectations from the theory. On average, dyads with extremely clean host economies (first bin) saw no significant change in their FDI flows in the post-treatment period. A positive effect, instead, is detected for dyads with moderately corrupt host economies (second and third bins), confirming results from 2FE models. Differences between observed and synthetic FDI dyadic flows are not significant for units in the fourth bin. Finally, FDI flows from signatories to the Convention were negatively affected for dyads with extremely corrupt host countries (fifth bin). Put together, these results provide yet more evidence in support of the argument proposed. The effect of the Convention on FDI flows was null for dyads with clean host economies, positive for those with mildly corrupt countries, and negative for dyads with extremely corrupt host economies.

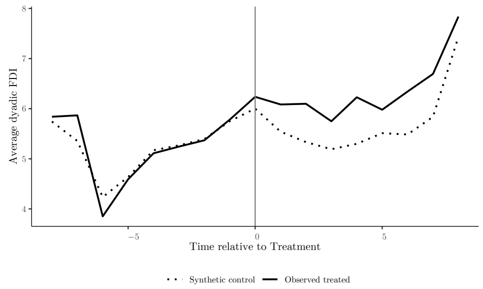
I also try more traditional alternatives to the binning estimator for capturing the conditional effect. I fit various model specifications where I interact the binary *OECD Convention* variable to the linear and squared measure of *Host PACI*. I follow this approach and model my dyadic data with 2FE, random effects, and Heckman selection models. Estimates produced are overall consistent with expectations from equation 1. Estimates of β_1 are usually negative and statistically significant. Estimates of β_2 are generally positive and significant. Coefficient β_3, instead, is often found to be positive and statistically significant. This finding runs counter the theory and suggests the Convention had a positive effect in very clean host countries. Results are reported and discussed in appendix.



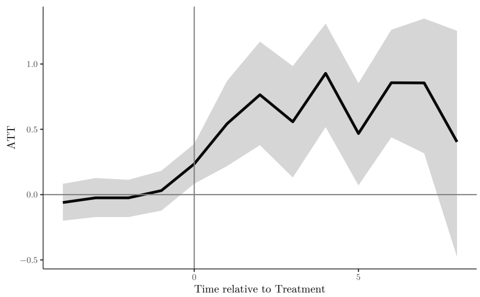
(a) Bin 1: Average dyadic flows.



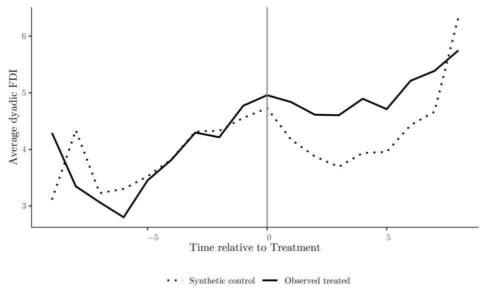
(b) Bin 1: Estimated ATT



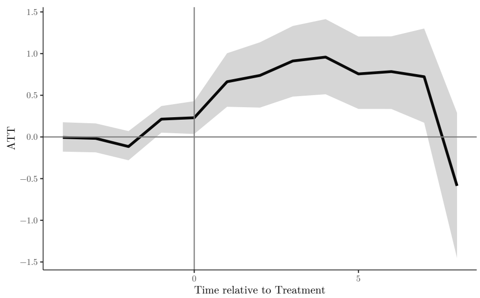
(c) Bin 2: Average dyadic flows.



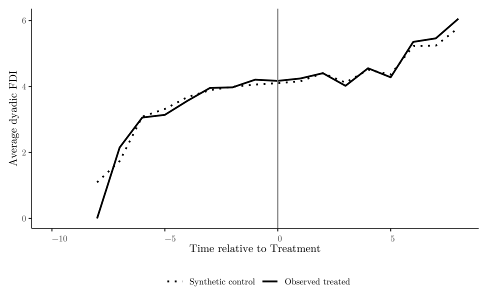
(d) Bin 2: Estimated ATT



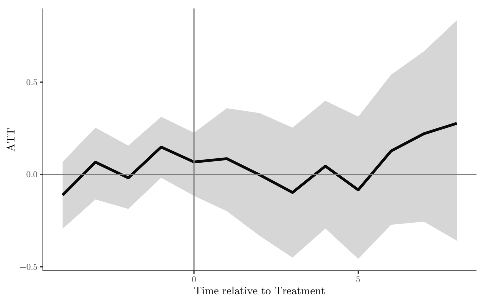
(e) Bin 3: Average dyadic flows.



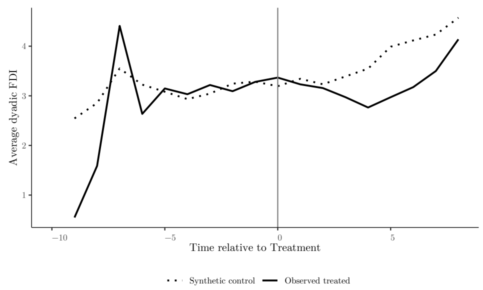
(f) Bin 3: Estimated ATT



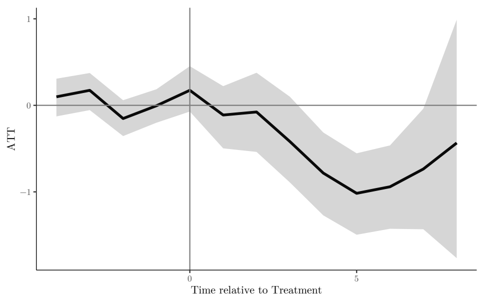
(g) Bin 4: Average dyadic flows.



(h) Bin 4: Estimated ATT



(i) Bin 5: Average dyadic flows.



(j) Bin 5: Estimated ATT

Figure 7: Generalized synthetic control method. Average trends and estimated ATT in the five bins

4 Discussion

This section discusses limitations of the study in terms of validity, generalization, and mechanisms. It also open to possible future questions based on these limitations. My argument that the effect of home country regulation on firms' investment is conditional on characteristics of the host economy finds empirical ground in firm-level and country-level evidence.

Identification in the first exercise rests entirely on a selection on observables design. Firms are not randomly assigned to the group subject to anti-bribery laws. This potentially introduces sources of endogeneity in the analysis. This plausible concern is only ruled out insofar as the factors causing endogeneity have been accounted for in the models by random effects and control variables. The lack of knowledge on the treatment assignment procedure fundamentally condemns this design to *assume* treatment is as if random, conditionally on included controls and random effects. If the assumption is violated, causality cannot be inferred from the first exercise and its estimates should be taken only as descriptive.

I claim internal validity of the second exercise is stronger. It rests on the assumption that post-treatment trends between the treatment and control group would have been parallel in the absence of a treatment. The assumption is made credible when 2FE results are confirmed by the synthetic counterfactuals approach. Compound effects and attribution are the remaining potential threats to identification. First, it is possible that the OECD Convention affected foreign investment not only through their impact on companies' decision-making, as claimed by my theory, but also through foreign officials' behavior. I return to this problem below, when discussing mechanisms. Second, it is possible that entry into force of the Convention took place at the same time of unrelated changes in FDI from signatories to corrupt economies at the end of the 1990s or early 2000s. If this were the case, 2FE and generalized synthetic counterfactuals would be wrongly attributing the effect to the Convention. I believe this problem to be less concerning here, at least in light of the inclusion of control variables generally believed to capture much of the variation in FDI flows.

I contend the value of these two exercises is in their conjunction. Firm-level estimates can be criticized for their weak internal validity. Yet, they appear stronger thanks to evidence from the country-level analysis, that support firm-level findings with a more credible identification strategy. On the other hand, country-level estimates can be criticized of an ecological fallacy for using aggregated data to test an individual firm-level theory. Their estimates, yet, appear more credible in light of the micro-level evidence provided by the first exercise.

One important limitation of the results concern their generalizability. In both studies, the estimated causal quantity is an ATT. Under the assumptions listed above, this estimate informs us about the effect of the Convention on FDI *for countries that adopted it.* It cannot tell us anything about what would happen to the outward FDI of, say, India if the country were to join the Convention. Selection into the treatment group is not random and countries self-select into the treatment group. The ATT is therefore not representative of the effect that untreated units would experience in the event of a treatment. It might as well be possible that signatory countries chose to ratify the Convention expecting that the effect their FDI and their companies would experience were precisely this one. The value of the study is simply in clarifying what the effect of the Convention on regulated companies has been.

The question is open about the mechanisms driving the effect proposed. The argument advanced expects that home countries' adoption of anti-bribery laws makes firms better or worse-off in international business depending on the level of corruption of the host country. I explain this hypothesis based on the leverage available to firms under anti-bribery regulations to refuse bribe requests. This operating space, in turn, would depend on the power enjoyed by public officials (Ades and Di Tella, 1999; Svensson, 2003): it shrinks where corrupt public officials enjoy a disproportionately large power. Put differently, the conditional effect can be due to the different role that bribery plays in countries with extreme levels of corruption (where bribes can be a condition to entry) as opposed to those with moderate levels.

This mechanism is not tested by the present analysis. It might as well be possible that anti-bribery policies affect investments because they change the behavior *of public officials*, and not that of companies[31]. The overall observed effect can even be the compounded result of these different mechanisms. The study cannot really disentangle them. Sector-specific evidence presented in figure 4 suggests that the effect in place involves only industries where bribes are a custom, and not the rest. This is consistent with the mechanism provided, which should not hold in industries where bribes are no usual custom, but does not allow a final word on the matter.

This leaves a door open for future qualitative studies to complement my analysis. They could investigate what drives the identified effects. For example, documenting negotiations of firms with foreign public officials in typically corrupt industries might enlighten this open question. This decision-making process could be studied to assess if the explanation provided here is appropriate, and to what extent competing mechanisms can be advanced, instead. Until then, the quest remains open on which mechanism ultimately explains the findings presented here.

Finally, the study explicitly does not consider strategies to invest in a foreign market other than corporate ownership. Licensing and joint ventures, yet, are potential ways for firms to invest in a foreign economy. They can expose firms from signatories of the Convention to a lesser risk of interaction with corrupt public officials, and might therefore be a preferred strategy (Chapman et al., 2020; Zhu and Shi, 2019). A future study could therefore investigate the effect of the Convention on these alternative investment strategies.

Net of its limitations, the study makes valuable contributions. It shows that home country corporate policies to diffuse ESG standards abroad do not necessarily penalize firms in foreign economies with

[31] I am thankful to Carolina Garriga for stressing this alternative explanation.

weak ESG standards. Indeed, firms under anti-bribery policies make investment decisions that are not univocal: they depend on characteristics of the host economy. In particular, they are non-linear in its level of corruption. Although firms under anti-bribery policies are worse off in extremely corrupt economies, they are better off in a range of mildly corrupt countries.

5 Conclusion

What is the effect of corporate policies imposed by home countries to further ESG values abroad, on subject companies' investments? It is often argued that home states' strategy of conscripting domestically-incorporated companies to diffuse ESG standards abroad disadvantages firms in economies with poor ESG records, *vis-à-vis* unregulated competitors. Thus, such regulatory efforts would jeopardize investments. In this article I study this question in the case of anti-bribery policies imposed by home countries under the 1997 OECD Convention. Existing studies on the matter provide inconsistent results: while some find a negative effect of anti-bribery policies in corrupt economies, other find positive or null effects.

In fact, I contend two competing arguments can be advanced about the effect of anti-bribery policies on foreign investment. The first one, *deterrence*, argues firms under anti-bribery policies operate at a disadvantage in corrupt economies due to the expected legal costs. It claims they will invest less in these economies, as a result. The second one, *empowerment*, expects the opposite, arguing that firms under anti-bribery policies can leverage on these legal standards to refuse bribe requests and cut expenses. I argue that both mechanisms are simultaneously at play. Their net effect depends on the level of corruption of the host economy, because the bargaining power of public officials increases in it, and so does their operating space for demanding bribes.

I test my argument in two empirical exercises. First, I employ firm-level data on investment decisions by 3781 firms between 2006 and 2011. Multilevel logit models show that firms from signatories of the OECD Convention are up to 40% more likely to invest in mildly corrupt economies, a quantity that plummets to -50% in extremely corrupt countries. Results stand to a series of robustness tests. I then corroborate these findings using country-dyadic data on investment flows. The second exercise provides a more credible identification of the heterogeneous effect through the use of difference-in-differences and a generalized synthetic control design.

The study offers insights beyond anti-bribery policies, to regulatory areas that potentially include human and labor rights protection, money laundering, environmental change, and anti-corporate tax evasion. The article sheds a light on the side effects of home states' regulatory strategy to threaten domestic repercussion for foreign corporate misconduct. I show that this approach to policy diffusion does not necessarily penalize firms' foreign economic activity in countries with poor ESG records. Rather, the cleaner business models that home countries induce with their corporate policies can facilitate companies in a range of countries. This is good news for the possibility to conjugate ESG corporate regulatory efforts with economic activity. A caveat concerns host countries with *extremely* poor ESG records. In this case, the strategy backfires as these economies are on average more likely to be abandoned by regulated firms and left exposed to companies from countries that do not impose similar standards. To the extent that such unregulated firms have no need to abide by ESG standards – and can in fact violate them – this could lead to a further decline of records in these economies. This pessimistic conclusion substantively aligns with recent findings on the perverse regulatory effects of corporate policies induced by the existence of different standards among competitors (Brazys and Kotsadam, 2020; Chapman et al., 2020).

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Appendix

The Conditional Arm of the Law. The Effect of the OECD Anti-Bribery Convention on Foreign Direct Investment

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: corruption 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[32]. 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.

B Firm-level analysis

B.1 Descriptive statistics

Table B.2 presents descriptive statistics for all variables included in the firm-level models. I retrieve from Beazer and Blake (2018) data for the variables Subsidiary, Home GDP (log), Home GDP Growth (%), Home Judiciary Indep., Host GDP (log), Host GDP per Capita, Host FDI (GDP %), Host Trade (GDP %), Host Judiciary Indep., Host Democracy, Host POLCON III, Dyad 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)[33]. Data on Host CCE and Host V-Dem Bribery have been retrieved respectively from the Quality of Governance dataset (Teorell et al., 2020) and from the V-Dem core database, version 10 (Coppedge et al., 2020).

[32] 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).

[33] 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.

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

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

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Subsidiary	406,454	0.026	0.158	0	0	0	1
OECD Signatory	406,454	0.944	0.231	0	1	1	1
Host PACI	329,397	5.171	2.317	0.000	4.032	6.821	8.901
Host PACI (2012)	332,972	5.030	2.261	0.000	3.872	6.548	8.755
Host CCE	402,585	2.677	1.076	1.082	1.833	3.570	4.825
Host V-Dem	402,585	0.203	1.550	−2.838	−0.952	1.614	3.363
Home GDP (log)	403,731	25.594	1.540	18.750	24.109	26.271	27.859
Home GDP Growth (%)	403,731	1.987	1.408	−6.272	1.193	2.163	10.647
Home Judiciary Indep.	406,244	0.895	0.133	0.167	0.886	0.965	0.988
Host GDP (log)	383,261	23.196	1.717	19.414	21.822	24.229	27.859
Host GDP per capita	383,261	1.430	1.445	0.028	0.328	2.334	6.829
Host FDI (GDP %)	383,261	6.533	17.617	−4.258	1.752	5.698	172.716
Host Trade (GDP %)	383,261	0.876	0.533	0.265	0.567	1.038	4.299
Host Judiciary Indep.	398,714	0.558	0.281	0.018	0.331	0.842	0.988
Host Democracy	390,986	0.703	0.457	0.000	0.000	1.000	1.000
Host POLCON III	383,244	0.311	0.198	0.000	0.127	0.468	0.692
Dyad Distance (km)	386,206	0.656	0.422	0.006	0.261	0.948	1.995
Dyad Common Language	386,206	0.113	0.316	0.000	0.000	0.000	1.000
Dyad Colonial Relation	386,206	0.051	0.219	0.000	0.000	0.000	1.000
Dyad BIT	406,454	0.376	0.484	0	0	1	1
Firm Age (log)	400,154	3.312	0.948	0.000	2.639	4.060	5.897
Firm Assets (log)	379,363	13.875	2.115	4.025	12.380	15.328	20.181
Firm Host Countries (log)	406,454	0.678	0.721	0.000	0.000	1.099	3.714

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

B.2 Full disclosure of results

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Signatory × Host PACI^2	−0.033*** (0.012)	−0.038*** (0.013)	−0.023* (0.013)	−0.031** (0.013)	−0.034** (0.013)
OECD Signatory × Host PACI	0.197** (0.090)	0.225** (0.092)	0.163* (0.090)	0.206** (0.096)	0.220** (0.096)
OECD Signatory	−0.016 (0.165)	−0.034 (0.192)	−0.213 (0.246)	−0.267 (0.205)	−0.282 (0.205)
Host PACI^2	−0.041 (0.033)	0.013 (0.029)	0.003 (0.026)	0.011 (0.027)	0.013 (0.028)
Host PACI	−0.097 (0.286)	−0.007 (0.242)	0.023 (0.221)	−0.008 (0.230)	−0.036 (0.231)
Host GDP (log)		0.592*** (0.128)	0.652*** (0.115)	0.674*** (0.120)	0.680*** (0.120)
Host GDP per capita		0.002 (0.180)	−0.042 (0.162)	−0.023 (0.169)	−0.056 (0.172)
Host FDI (GDP %)		0.010 (0.009)	0.010 (0.008)	0.009 (0.009)	0.010 (0.009)
Host Trade (GDP %)		−0.225 (0.335)	−0.186 (0.303)	−0.172 (0.315)	−0.155 (0.316)
Host Judiciary Indep.		3.699*** (1.150)	3.537*** (1.035)	3.653*** (1.079)	3.695*** (1.085)
Host POLCON III		0.530 (0.962)	0.099 (0.865)	0.156 (0.902)	0.200 (0.905)
Host Democracy		−0.129 (0.461)	−0.001 (0.416)	−0.016 (0.434)	−0.022 (0.435)
Home GDP (log)		0.063** (0.027)	0.138*** (0.045)	0.055* (0.030)	0.057* (0.030)
Home GDP Growth (%)		−0.013 (0.019)	−0.028 (0.026)	−0.005 (0.021)	−0.006 (0.021)
Home Judiciary Indep.		−0.182 (0.241)	−0.256 (0.380)	−0.393 (0.261)	−0.391 (0.260)
Dyad BIT			0.087 (0.068)	0.079 (0.073)	0.082 (0.073)
Dyad Common Language			0.693*** (0.092)	0.751*** (0.100)	0.742*** (0.101)
Dyad Colonial Relation			0.725*** (0.116)	0.737*** (0.126)	0.732*** (0.127)
Dyad Distance			−1.229*** (0.094)	−1.102*** (0.095)	−1.105*** (0.095)
Firm Assets (log)				0.005 (0.008)	0.005 (0.008)
Firm Age (log)				0.017 (0.014)	0.013 (0.015)
Firm Host Countries (log)				1.286*** (0.020)	1.287*** (0.020)
Constant	−3.364*** (0.605)	−5.602*** (0.642)	−5.513*** (0.606)	−6.079*** (0.610)	−6.026*** (0.612)
Random intercepts	✓	✓	✓	✓	✓
Industry intercepts					✓
N. of host countries	84	83	83	83	83
N. of home countries	61	60	60	57	56
Observations	320,913	315,657	315,657	289,732	285,295
Akaike Inf. Crit.	62,550.060	62,272.990	61,961.250	50,267.110	49,604.410
Note:					*p<0.1; **p<0.05; ***p<0.01

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

B.3 Robustness tests

Results for all tests are reported in table B.4. 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 Signatory* 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 B.3 using the version of the index computed and published by Escresa and Picci (2017), which employs information until 2012 and includes more host countries[34]. Results obtained when using this version of the index are substantively the same as the ones discussed before.

As a further test I consider the hypothesis that results might be driven by some outlier countries. China figures as a very likely candidate: the country has not ratified the Convention and it is generally considered a rather corrupt bureaucracy. Yet, it is involved in the world economy as both a major importer and exporter of investments. I therefore replicate the analysis excluding observations relative to firms from this country or investing in it. Results do not change significantly with this exclusion. Next, in two countries the Convention has entered into force within the time window of the cross-section (2006-2011): Israel and South Africa. Thus, their firms might have been subject to anti-bribery policies even though *OECD Signatory* assigns them a value of 0. I therefore replicate the analysis excluding them. Results, again, do not change significantly.

[34] 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			
	First degree	V-Dem	CCE	PACI (2012)	No China	No Israel No South Africa
	(1)	(2)	(3)	(4)	(5)	(6)
OECD Signatory × Host PACI^2					-0.029^{**}	-0.024^{*}
					(0.012)	(0.014)
OECD Signatory × Host PACI	-0.007				0.168^{*}	0.168^{*}
	(0.034)				(0.089)	(0.096)
OECD Signatory × Host V-Dem Bribery^2		-0.075^{**}				
		(0.032)				
OECD Signatory × Host V-Dem Bribery		0.133^{*}				
		(0.071)				
OECD Signatory × Host CCE^2			-0.213^{***}			
			(0.082)			
OECD Signatory × Host CCE			1.360^{**}			
			(0.543)			
OECD Signatory × Host PACI^2 (2012)				-0.048^{***}		
				(0.014)		
OECD Signatory × Host PACI (2012)				0.277^{***}		
				(0.097)		
OECD Signatory	-0.067	0.021	-2.023^{**}	-0.260	-0.188	-0.197
	(0.191)	(0.181)	(0.847)	(0.193)	(0.196)	(0.225)
Host PACI^2					0.016	0.012
					(0.026)	(0.027)
Host PACI	0.039				-0.069	-0.101
	(0.106)				(0.221)	(0.230)
Host V-Dem Bribery^2		0.133^{**}				
		(0.062)				
Host V-Dem Bribery		-0.089				
		(0.159)				
Host CCE^2			0.154			
			(0.148)			
Host CCE			-0.660			
			(1.012)			
Host PACI^2 (2012)				0.020		
				(0.028)		
Host PACI (2012)				-0.057		
				(0.234)		
Host GDP (log)	0.723^{***}	0.751^{***}	0.737^{***}	0.627^{***}	0.678^{***}	0.687^{***}
	(0.107)	(0.087)	(0.088)	(0.115)	(0.121)	(0.113)
Host GDP per capita	-0.065	-0.173	-0.185	0.218	-0.044	-0.145
	(0.173)	(0.165)	(0.187)	(0.225)	(0.167)	(0.169)
Host FDI (GDP %)	0.010	0.013^{*}	0.014^{*}	0.006	0.011	0.012
	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Host Trade (GDP %)	-0.105	-0.148	-0.168	-0.366	-0.210	-0.217
	(0.311)	(0.281)	(0.292)	(0.315)	(0.304)	(0.298)
Host Judiciary Indep.	3.640^{***}	3.205^{***}	2.453^{*}	2.930^{***}	3.685^{***}	4.367^{***}
	(1.084)	(1.066)	(1.374)	(1.084)	(1.036)	(1.115)
Host POLCON III	0.248	0.455	0.422	0.028	0.201	0.366
	(0.904)	(0.820)	(0.829)	(0.892)	(0.865)	(0.850)
Host Democracy	0.015	0.005	0.068	0.038	-0.050	-0.382
	(0.431)	(0.397)	(0.413)	(0.412)	(0.418)	(0.460)
Home GDP (log)	0.058^{*}	0.048	0.048	0.052^{*}	0.080^{**}	0.074^{**}
	(0.031)	(0.035)	(0.035)	(0.030)	(0.032)	(0.029)
Home GDP Growth (%)	-0.005	-0.002	-0.002	-0.002	-0.001	-0.003
	(0.021)	(0.022)	(0.022)	(0.020)	(0.023)	(0.021)
Home Judiciary Indep.	-0.398	-0.372	-0.382	-0.328	-0.253	-0.333
	(0.263)	(0.293)	(0.292)	(0.257)	(0.271)	(0.289)
Dyad BIT	0.065	0.078	0.064	0.092	0.189^{***}	0.166^{***}
	(0.073)	(0.070)	(0.070)	(0.072)	(0.049)	(0.049)
Dyad Common Language	0.748^{***}	0.791^{***}	0.787^{***}	0.707^{***}	0.651^{***}	0.657^{***}
	(0.101)	(0.098)	(0.098)	(0.100)	(0.045)	(0.045)
Dyad Colonial Relation	0.734^{***}	0.759^{***}	0.759^{***}	0.761^{***}	0.304^{***}	0.292^{***}
	(0.127)	(0.120)	(0.120)	(0.123)	(0.052)	(0.052)
Dyad Distance	-1.126^{***}	-1.241^{***}	-1.237^{***}	-1.061^{***}	-1.129^{***}	-1.069^{***}
	(0.096)	(0.090)	(0.090)	(0.093)	(0.059)	(0.058)
Firm Assets (log)	0.005	0.008	0.008	0.006	0.005	0.005
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Firm Age (log)	0.013	0.010	0.010	0.009	0.012	0.011
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Firm Host Countries (log)	1.287^{***}	1.274^{***}	1.274^{***}	1.277^{***}	1.271^{***}	1.270^{***}
	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Constant	-6.105^{***}	-6.193^{***}	-5.361^{***}	-5.984^{***}	-5.854^{***}	-5.655^{***}
	(0.589)	(0.258)	(1.642)	(0.628)	(0.585)	(0.619)
Random intercepts	✓	✓	✓	✓	✓	✓
N. of host countries	83	99	99	85	82	81
N. of home countries	56	56	56	56	55	54
Observations	285,295	340,554	340,554	291,945	280,767	275,705
Akaike Inf. Crit.	49,607.020	55,424.820	55,423.580	53,329.030	49,350.190	49,272.190

Note: *p<0.1; **p<0.05; ***p<0.01

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

	Dependent variable:				
	Subsidiary				
	(1)	(2)	(3)	(4)	(5)
OECD Signatory × Host PACI^2	−0.037***	−0.045***	−0.024**	−0.030***	−0.033***
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
OECD Signatory × Host PACI	0.261***	0.307***	0.193**	0.236***	0.247***
	(0.076)	(0.078)	(0.077)	(0.083)	(0.083)
OECD Signatory	0.126	−0.094	−0.172	−0.250	−0.265
	(0.190)	(0.243)	(0.254)	(0.223)	(0.227)
Host PACI	−0.166	−0.087	0.007	−0.024	−0.046
	(0.302)	(0.248)	(0.232)	(0.241)	(0.242)
Host PACI^2	−0.038	0.029	0.011	0.018	0.021
	(0.034)	(0.029)	(0.027)	(0.028)	(0.028)
Host GDP (log)		0.719***	0.784***	0.829***	0.837***
		(0.132)	(0.123)	(0.128)	(0.128)
Host GDP per capita		−0.038	−0.088	−0.084	−0.103
		(0.185)	(0.171)	(0.178)	(0.179)
Host FDI (GDP %)		0.005	0.004	0.004	0.004
		(0.010)	(0.009)	(0.009)	(0.009)
Host Trade (GDP %)		−0.0003	0.069	0.081	0.090
		(0.344)	(0.320)	(0.332)	(0.333)
Host Judiciary Indep.		4.697***	4.596***	4.777***	4.795***
		(1.197)	(1.112)	(1.155)	(1.158)
Host POLCON III		0.295	−0.130	−0.123	−0.101
		(1.007)	(0.935)	(0.972)	(0.973)
Host Democracy		−0.307	−0.174	−0.197	−0.200
		(0.478)	(0.444)	(0.461)	(0.462)
Home GDP (log)		0.099**	0.144***	−0.010	−0.031
		(0.048)	(0.051)	(0.041)	(0.042)
Home GDP Growth (%)		0.011	0.003	0.041*	0.029
		(0.028)	(0.029)	(0.025)	(0.025)
Home Judiciary Indep.		0.534	0.394	0.738**	0.668*
		(0.405)	(0.428)	(0.348)	(0.354)
Dyad BIT			−0.001	−0.018	−0.009
			(0.062)	(0.067)	(0.068)
Dyad Common Language			0.738***	0.794***	0.786***
			(0.092)	(0.101)	(0.102)
Dyad Colonial Relation			0.744***	0.803***	0.817***
			(0.112)	(0.123)	(0.124)
Dyad Distance			−1.189***	−1.304***	−1.322***
			(0.088)	(0.094)	(0.095)
Firm Assets (log)				0.186***	0.208***
				(0.006)	(0.007)
Firm Age (log)				0.134***	0.071***
				(0.011)	(0.012)
Firm Host Countries (log)				0.908***	0.902***
				(0.015)	(0.015)
Constant	−2.984***	−27.554***	−29.235***	−30.910***	−30.767***
	(0.653)	(3.677)	(3.471)	(3.517)	(3.530)
Random intercepts	✓	✓	✓	✓	✓
Industry intercepts					✓
N. of host countries	84	83	83	83	83
N. of home countries	61	60	60	57	56
Observations	320,913	315,657	315,657	289,732	285,295
Log Likelihood	−46,765.540	−46,500.700	−46,311.960	−37,958.790	−37,224.890
Akaike Inf. Crit.	93,549.090	93,039.410	92,669.920	75,969.590	74,503.790
Note:					*p<0.1; **p<0.05; ***p<0.01

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

B.4 Sector-specific analysis

NAICS3	NACE	NAICS3 label
111	A1	Crop Production
112	A1	Animal Production and Aquaculture
113	A2	Forestry and Logging
115	A1	Support Activities for Agriculture and Forestry
211	B6	Oil and Gas Extraction
212	B7	Mining (except Oil and Gas)
213	B9	Support Activities for Mining
221	D35	Utilities
236	F41	Construction of Buildings
237	F42	Heavy and Civil Engineering Construction
238	F43	Specialty Trade Contractors
311	C10	Food Manufacturing
312	C11	Beverage and Tobacco Product Manufacturing
315	C14	Apparel Manufacturing
323	C18	Printing and Related Support Activities
324	C19	Petroleum and Coal Products Manufacturing
325	C20	Chemical Manufacturing
326	C22	Plastics and Rubber Products Manufacturing
331	C24	Primary Metal Manufacturing
332	C25	Fabricated Metal Product Manufacturing
333	C28	Machinery Manufacturing
334	C26	Computer and Electronic Product Manufacturing
335	C27	Electrical Equipment; Appliance; and Component Manufacturing
336	C29	Transportation Equipment Manufacturing
337	C31	Furniture and Related Product Manufacturing
339	C32	Miscellaneous Manufacturing
423	G46	Merchant Wholesalers; Durable Goods
424	G46	Merchant Wholesalers; Nondurable Goods
425	G46	Wholesale Electronic Markets and Agents and Brokers
441	G45	Motor Vehicle and Parts Dealers
442	G46	Furniture and Home Furnishings Stores
443	G46	Electronics and Appliance Stores
444	G46	Building Material and Garden Equipment and Supplies Dealers
445	G47	Food and Beverage Stores
446	G46	Health and Personal Care Stores
447	G46	Gasoline Stations
448	G47	Clothing and Clothing Accessories Stores
451	G47	Sporting Goods; Hobby; Musical Instrument; and Book Stores
452	G47	General Merchandise Stores
453	G47	Miscellaneous Store Retailers
454	G47	Nonstore Retailers
483	H50	Water Transportation
491	H53	Postal Service
492	H53	Couriers and Messengers
511	J58	Publishing Industries (except Internet)
517	J61	Telecommunications
518	J63	Data Processing; Hosting; and Related Services
519	J62	Other Information Services
522	K64	Credit Intermediation and Related Activities
523	K64	Securities; Commodity Contracts; and Other Financial Investments and Related Activities
525	K64	Funds; Trusts; and Other Financial Vehicles
531	L68	Real Estate
551	M70	Management of Companies and Enterprises
561	N82	Administrative and Support Services
611	P85	Educational Services
621	Q86	Ambulatory Health Care Services
713	R92	Amusement; Gambling; and Recreation Industries
721	I55	Accommodation
921	O84	Executive; Legislative; and Other General Government Support
924	O84	Administration of Environmental Quality Programs

Table B.6: Firm-level data. Industries with at least one case of bribery between 1997 and 2005

	Dependent variable:			
	Subsidiary			
	Test		Placebo	
	(1)	(2)	(3)	(4)
OECD Signatory × Host PACI^2	−0.040***	−0.043***	−0.006	0.005
	(0.014)	(0.015)	(0.027)	(0.029)
OECD Signatory × Host PACI	0.248**	0.299***	0.003	−0.123
	(0.099)	(0.106)	(0.192)	(0.206)
OECD Signatory	−0.023	−0.397*	0.106	0.250
	(0.181)	(0.222)	(0.331)	(0.387)
Host PACI^2	−0.034	0.021	−0.072	−0.014
	(0.032)	(0.027)	(0.046)	(0.041)
Host PACI	−0.134	−0.095	0.089	0.192
	(0.281)	(0.226)	(0.378)	(0.319)
Host GDP (log)		0.667***		0.718***
		(0.115)		(0.152)
Host GDP per capita		−0.049		0.008
		(0.164)		(0.218)
Host FDI (GDP %)		0.009		0.010
		(0.008)		(0.010)
Host Trade (GDP %)		−0.160		−0.126
		(0.303)		(0.430)
Host Judiciary Indep.		3.655***		3.005**
		(1.036)		(1.370)
Host POLCON III		0.147		0.447
		(0.865)		(1.128)
Host Democracy		−0.040		0.527
		(0.416)		(0.559)
Home GDP (log)		0.063**		0.034
		(0.030)		(0.032)
Home GDP Growth (%)		−0.005		0.009
		(0.021)		(0.039)
Home Judiciary Indep.		−0.379		0.023
		(0.271)		(0.325)
Dyad BIT		0.046		0.327**
		(0.077)		(0.133)
Dyad Common Language		0.686***		0.762***
		(0.105)		(0.143)
Dyad Colonial Relation		0.667***		0.700***
		(0.132)		(0.177)
Dyad Distance		−1.138***		−0.697***
		(0.100)		(0.137)
Firm Assets (log)		0.007		0.0005
		(0.009)		(0.021)
Firm Age (log)		0.010		0.040
		(0.016)		(0.037)
Firm Host Countries (log)		1.288***		1.243***
		(0.022)		(0.051)
Constant	−3.339***	−5.885***	−3.355***	−6.463***
	(0.602)	(0.592)	(0.751)	(0.782)
Random intercepts	✓	✓	✓	✓
Industry intercepts	✓	✓	✓	✓
N. of host countries	84	83	84	83
N. of home countries	57	52	40	38
Observations	262,075	236,609	54,097	48,686
Log Likelihood	−25,757.560	−20,778.850	−5,159.393	−4,114.255
Akaike Inf. Crit.	51,535.120	41,611.710	10,338.780	8,282.511
Note:				*p<0.1; **p<0.05; ***p<0.01

Table B.7: Firm-level data. Market-specific effects of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models

C Dyadic country-level analysis

C.1 Descriptive statistics

	Signatories	Non-signatories
1	United States	Dominican Republic
2	Canada	Trinidad and Tobago
3	Mexico	Honduras
4	Brazil	El Salvador
5	Chile	Venezuela, Republica Bolivariana de
6	Argentina	Ecuador
7	United Kingdom	Bolivia
8	Ireland	Paraguay
9	Netherlands	Albania
10	Belgium	North Macedonia, Republic of
11	Luxembourg	Croatia
12	France	Bosnia and Herzegovina
13	Switzerland	Moldova
14	Spain	Romania
15	Portugal	Ukraine
16	Poland	Belarus
17	Hungary	Armenia, Republic of
18	Czech Republic	Georgia
19	Slovak Republic	Azerbaijan, Republic of
20	Italy	Cabo Verde
21	Slovenia	Nigeria
22	Greece	Uganda
23	Bulgaria	Tanzania
24	Estonia	Ethiopia
25	Finland	Mozambique
26	Sweden	Zambia
27	Norway	Malawi
28	Denmark	Namibia
29	Iceland	Botswana
30	Turkey	Eswatini, Kingdom of
31	Turkey	Swaziland
32	Korea, Republic of	Madagascar
33	Japan	Morocco
34	Australia	Algeria
35	New Zealand	Tunisia
36		Egypt
37		Syrian Arab Republic
38		Lebanon
39		Jordan
40		Saudi Arabia
41		Yemen, Republic of
42		Qatar
43		United Arab Emirates
44		Oman
45		Kyrgyz Republic
46		Kazakhstan
47		China, P.R.: Mainland
48		India
49		Pakistan
50		Bangladesh
51		Myanmar
52		Thailand
53		Cambodia
54		Lao People's Democratic Republic
55		Malaysia
56		Singapore
57		Philippines
58		Indonesia
59		Papua New Guinea
60		Fiji

Table C.1: Dyadic country-level data. Home countries

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Dyad FDI (log)	8,852	3.154	2.593	0.000	0.619	5.141	11.466
Dyad FDI (binary)	44,125	0.251	0.434	0	0	1	1
OECD Convention	44,125	0.259	0.438	0	0	1	1
Host PACI	35,910	4.381	2.418	0.000	2.740	6.147	8.901
Host FDI (GDP %)	41,812	3.567	8.203	−32.347	0.832	4.057	172.716
Host GDP per capita	42,332	17.717	14.799	0.249	4.497	28.515	74.164
Host Trade (GDP %)	42,659	80.504	52.989	0.309	50.629	95.277	437.387
Host POLCON III	41,840	0.348	0.204	0.000	0.173	0.507	0.720
Host Democracy	43,373	0.714	0.452	0.000	0.000	1.000	1.000
Host GDP (log)	42,363	25.867	1.909	18.809	24.503	27.189	30.188
Host Judiciary Indep.	44,055	0.632	0.297	0.016	0.382	0.949	0.989
Home GDP per capita	43,813	16.164	12.340	0.399	5.933	26.459	74.164
Home GDP growth (%)	43,745	3.239	4.296	−30.694	1.621	5.030	90.468
Home GDP (log)	43,813	25.980	1.823	20.205	24.704	27.148	30.188
Home Judiciary Indep.	44,125	0.645	0.276	0.074	0.405	0.944	0.989
Dyad BIT	44,125	0.254	0.435	0	0	1	1

Table C.2: Dyadic country-level data. Summary statistics

C.2 Robustness tests

As a first 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*, as proposed by equation 1. Results are reported in table C.3 and are overall consistent with my expectation. I find the estimate of coefficient β_1 to be negative and highly statistically significant. The estimate of coefficient β_2 is positive but fails to meet statistical significance. Estimate for β_3 instead, is positive and significant, a finding that confirms the positive effect of the Convention for dyads with very clean host countries that emerged from some estimates in figure 5.

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 C.4 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 in the last two models. Estimates of β_3 are statistically significant but again 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 C.5 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. Estimates of β_2 are positive and statistically significant. 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 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 × Host PACI^2	−0.015**	−0.015*	−0.013*	−0.013*
	(0.007)	(0.007)	(0.007)	(0.007)
OECD Convention × Host PACI	0.050	0.059	0.050	0.050
	(0.054)	(0.063)	(0.063)	(0.063)
OECD Convention	0.381***	0.307**	0.256*	0.258*
	(0.117)	(0.142)	(0.141)	(0.141)
Lag Host FDI (GDP %)		0.002	0.002	0.002
		(0.002)	(0.002)	(0.002)
Lag Host GDP per capita		0.036**	0.034*	0.035*
		(0.018)	(0.018)	(0.018)
Lag Host Trade (GDP %)		0.0003	0.0003	0.0004
		(0.002)	(0.002)	(0.002)
Lag Host POLCON III		−0.171	−0.151	−0.148
		(0.191)	(0.192)	(0.192)
Lag Host Democracy		−0.245*	−0.243*	−0.246**
		(0.127)	(0.125)	(0.125)
Lag Host GDP (log)		0.427	0.435	0.441*
		(0.270)	(0.265)	(0.264)
Lag Host Judiciary Indep.		0.627	0.496	0.488
		(0.707)	(0.718)	(0.715)
Home GDP per capita			0.078***	0.079***
			(0.024)	(0.024)
Home GDP Growth (%)			0.002	0.003
			(0.008)	(0.008)
Home Judiciary Indep.			1.340	1.321
			(0.922)	(0.923)
Dyad BIT				0.097
				(0.074)
Dyad FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	9,703	8,201	8,195	8,195
R^2	0.007	0.011	0.016	0.016
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table C.3: Dyadic country-level data. Twoway Fixed-Effect Models with interaction terms.

	Dependent variable:			
	Dyad FDI			
	(1)	(2)	(3)	(4)
OECD Convention × Host PACI^2	−0.018***	−0.024***	−0.025***	−0.025***
	(0.006)	(0.007)	(0.007)	(0.007)
OECD Convention × Host PACI	0.068	0.098*	0.099*	0.100*
	(0.049)	(0.052)	(0.052)	(0.052)
OECD Convention	1.126***	1.044***	−0.297**	−0.317**
	(0.187)	(0.209)	(0.135)	(0.140)
Host PACI^2	−0.039***	0.022**	0.021**	0.020**
	(0.008)	(0.009)	(0.009)	(0.009)
Host PACI	0.053	−0.120*	−0.107	−0.107
	(0.062)	(0.066)	(0.068)	(0.066)
Host FDI (GDP %)		0.003	0.003	0.004
		(0.002)	(0.002)	(0.002)
Host GDP per capita		0.012***	0.013***	0.012***
		(0.004)	(0.004)	(0.004)
Host Trade (GDP %)		0.006***	0.006***	0.006***
		(0.001)	(0.001)	(0.001)
Host POLCON III		0.086	0.089	0.108
		(0.169)	(0.169)	(0.168)
Host Democracy		−0.030	−0.059	−0.007
		(0.106)	(0.108)	(0.105)
Host GDP (log)		0.504***	0.497***	0.540***
		(0.036)	(0.037)	(0.035)
Host Judiciary Indep.		0.925***	0.869***	0.593**
		(0.280)	(0.287)	(0.276)
Home GDP per capita			0.173***	0.171***
			(0.009)	(0.009)
Home GDP Growth (%)			−0.051***	−0.061***
			(0.016)	(0.018)
Home Judiciary Indep.			−2.212***	−2.201***
			(0.411)	(0.423)
Dyad BIT				−0.055
				(0.059)
Dyad Common Language				0.728***
				(0.158)
Dyad Colonial Relation				1.151***
				(0.206)
Dyad distance				−0.008***
				(0.001)
Constant	2.725***	−12.619***	−13.571***	−14.060***
	(0.157)	(1.043)	(1.091)	(1.039)
Home-Host, year intercepts	✓	✓	✓	✓
Dyad intercepts	✓	✓	✓	✓
Observations	7,455	6,226	6,220	6,220
Log Likelihood	−13,260.950	−11,124.400	−10,948.500	−10,886.420
Akaike Inf. Crit.	26,541.900	22,282.810	21,936.990	21,820.840
Note:				*p<0.1; **p<0.05; ***p<0.01

Table C.4: Dyadic country-level data. Multilevel models

Table C.5: Dyadic country-level data. Heckman selection models

	Dependent variable:			
	Dyad FDI (log)			
	(1)	(2)	(3)	(4)
Selection model				
OECD Convention × Host PACI^2	-0.01^{***}	-0.02^{***}	-0.02^{***}	-0.02^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
OECD Convention × Host PACI	0.01	0.05^{**}	0.05^{*}	0.05^{*}
	(0.02)	(0.03)	(0.03)	(0.03)
OECD Convention	1.13^{***}	1.10^{***}	0.58^{***}	0.58^{***}
	(0.05)	(0.05)	(0.05)	(0.05)
Host PACI^2	-0.00^{**}	0.01^{***}	0.01^{***}	0.02^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Host PACI	0.03^{**}	-0.03	-0.05^{***}	-0.09^{***}
	(0.01)	(0.02)	(0.02)	(0.02)
Host FDI (GDP %)		0.00	-0.00	0.00
		(0.00)	(0.00)	(0.00)
Host GDP per capita		-0.01^{***}	-0.00^{***}	-0.00^{***}
		(0.00)	(0.00)	(0.00)
Host Trade (GDP %)		0.00^{***}	0.00^{***}	0.00^{***}
		(0.00)	(0.00)	(0.00)
Host POLCON III		-0.02	-0.02	-0.03
		(0.07)	(0.07)	(0.07)
Host Democracy		-0.05	-0.01	-0.05
		(0.03)	(0.03)	(0.03)
Host GDP (log)		0.08^{***}	0.12^{***}	0.12^{***}
		(0.01)	(0.01)	(0.01)
Host Judiciary Indep.		0.44^{***}	0.33^{***}	0.27^{***}
		(0.07)	(0.07)	(0.08)
Home GDP per capita			0.02^{***}	0.02^{***}
			(0.00)	(0.00)
Home GDP Growth (%)			0.03^{***}	0.02^{***}
			(0.00)	(0.00)
Home Judiciary Indep.			1.04^{***}	1.07^{***}
			(0.05)	(0.05)
Dyad Common Language				-0.23^{***}
				(0.03)
Dyad Colonial Relation				0.61^{***}
				(0.05)
Dyad BIT				0.38^{***}
				(0.02)
Constant	-1.06^{***}	-3.44^{***}	-5.54^{***}	-5.59^{***}
	(0.02)	(0.23)	(0.25)	(0.26)
Outcome model				
OECD Convention × Host PACI^2	-0.59	-0.07	-0.06^{***}	-0.06^{***}
	(0.77)	(0.05)	(0.01)	(0.01)
OECD Convention × Host PACI^2	0.70	0.23	0.26^{***}	0.26^{***}
	(1.66)	(0.19)	(0.08)	(0.08)
OECD Convention	64.28	4.31	-0.16	-0.11
	(80.16)	(3.09)	(0.41)	(0.34)
Host PACI^2	-0.30	0.02	0.04^{***}	0.04^{***}
	(0.32)	(0.03)	(0.01)	(0.01)
Host PACI	2.06	-0.10	-0.23^{***}	-0.17^{**}
	(2.48)	(0.11)	(0.06)	(0.07)

Host FDI (GDP %)		0.01**	0.01***	0.01***
		(0.01)	(0.00)	(0.00)
Host GDP per capita		−0.02	0.01**	0.01**
		(0.02)	(0.00)	(0.00)
Host Trade (GDP %)		0.01*	0.01***	0.01***
		(0.00)	(0.00)	(0.00)
Host POLCON III		0.09	0.41**	0.28
		(0.32)	(0.19)	(0.19)
Host Democracy		−0.44**	−0.02	0.09
		(0.20)	(0.09)	(0.09)
Host GDP (log)		0.64***	0.62***	0.62***
		(0.23)	(0.08)	(0.06)
Host Judiciary Indep.		2.05	0.78**	0.84***
		(1.31)	(0.31)	(0.26)
Home GDP per capita			0.20***	0.20***
			(0.02)	(0.01)
Home GDP Growth (%)			−0.06***	−0.05***
			(0.02)	(0.02)
Home Judiciary Indep.			−1.87**	−1.80***
			(0.84)	(0.69)
Dyad Common Language				0.46***
				(0.15)
Dyad Colonial Relation				1.75***
				(0.33)
Dyad BIT				0.05
				(0.22)
Constant	−118.89	−22.09*	−18.63***	−19.15***
	(154.74)	(12.92)	(4.56)	(3.69)
Inverse Mills Ratio	77.61	4.39	1.06	1.06
	(98.10)	(3.86)	(0.96)	(0.77)
Sigma	66.62	4.46	2.13	2.08
Rho	1.16	0.98	0.50	0.51
R^2	0.09	0.12	0.45	0.48
Total observations	34051	26908	26684	26684
Censored observations	26596	20682	20464	20464
Observed observations	7455	6226	6220	6220

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$