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

Lorenzo Crippa†

September 7, 2022

Word count: 10802

Abstract

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

Keywords: Foreign direct investment; International corporate regulations; Anti-bribery; OECD Anti-Bribery Convention; OECD

*I am thankful to Federica Genovese, Han Dorussen, Nicole Baerg, Martin Steinwand, Laura Saavedra-Lux, James Wood, Alina Mungiu-Pippidi, Ina Kubbe, Carolina Garriga, Rabia Malik, Kristian Gleditsch, Brian Phillips, Miranda Simon, Sara Polo, Calvin Thrall, Cleo O'Brien-Udry, Alex Kirss, Yunsieg Kim, Nina Obermeier, Frederik Heitmüller, Elizabeth Meehan, Lucio Picci, Boram Lee, Karen Nershi, Celeste Beesley for precious feedback on various versions of this work. I also thank Lucio Picci and Laarni Escresa for sharing material necessary to compute the PACI measure. Versions of the paper were circulated at ECPR 2020, IASOC 2020, University of Essex, Department of Government internal PhD colloquium and IR PhD workshop, at a Spring 2021 GSIPE Workshop, PSA 2021, ISA 2021, and MPSA 2021.

†PhD student, Department of Government, University of Essex. l.crippa@essex.ac.uk

1 Introduction

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

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

Research showed that corporate regulations embedded in IOs can effectively curb criminal behavior (Jensen and Malesky, 2018; Morse, 2019). But how do they affect firms' legitimate activity, such as their foreign investment? Political economy research advances two opposite expectations. First, policies would raise additional costs for regulated companies, thus *deterring* investment. For instance, anti-bribery laws would increase risk of investing into corrupt countries for regulated companies, because exposure to bureaucrats' bribe requests is higher there (Gueorguiev and Malesky, 2012) and thus the risk of facing prosecution under IO-negotiated rules (Cuervo-Cazurra, 2008). Yet, an opposite hypothesis expects that regulatory policies *empower* firms' foreign investment. They would force companies to devise business models that cut costs induced by uncertainty of criminal practices in countries that otherwise lack regulatory standards. For instance, anti-bribery provisions can tie companies' hands, allowing them to refuse bribe-requests and cut down costs of corruption (Davis, 2002). IOs would thus offer regulated companies an advantage when investing into countries with lax standards of economic activity.

How to reconcile these opposite expectations? In this paper I propose a single argument to unify them. I show that the two claims, which I label respectively *deterrence* and *empowerment*, are observable special cases of a single general dynamic. I study regulations that prosecute foreign bribe payments by MNCs adopted under a common IO umbrella. My argument unifies the *deterrence* and *empowerment* hypotheses, by claiming that IO-negotiated anti-bribery policies subject regulated companies to both mechanisms simultaneously, when deciding on a foreign investment in a corrupt economy. I claim that their net effect on investment depends on the level of corruption of the host economy. Anti-bribery

policies provide regulated firms with a legal leverage which is strong enough to refuse bribe requests only where public officials' bargaining power is relatively weak. Since public officials' power to demand bribes increases in the level of corruption of a country (Ades and Di Tella, 1999), I expect anti-bribery policies will favor investments in moderately corrupt host economies. Policies will deter investments only in *extremely* corrupt economies, where companies cannot refuse bribe requests. In other words, I claim that regulatory policies adopted under an IO umbrella do not necessarily disadvantage firms in international competition. Rather, they alter investment conditions faced by companies and lead them to restructure foreign investment choices accordingly.

Empirically, I study laws under the 1997 OECD Anti-Bribery Convention[1], that criminalized foreign bribe payments by companies from 44 ratifier countries. Two exercises support my argument. First, I leverage data from Beazer and Blake (2018) and model individual decisions by 3871 firms to invest in a foreign location between 2006 and 2011. I show that firms under OECD anti-bribery policies make investment decisions conditionally on the level of corruption of the host economy, non-linearly. I find that firms from ratifiers are no more likely than their unregulated competitors to invest in non-corrupt economies. They are up to 31% more likely to invest in moderately corrupt host economies. Instead, they are 58% less likely to invest in *extremely* corrupt destinations. This exercise provides insights in support of my argument at the level of investment decision-makers. Second, I employ time-varying country-dyadic data in a generalized synthetic control design to achieve a more robust identification of the proposed effect. Results confirm findings from the firm-level analysis. Empirics show that multilateral anti-bribery policies affect firms' investment into corrupt economies, but not necessarily in a negative way. In fact, they favor investment of regulated companies into a range of moderately corrupt countries.

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

The study concludes that IO-negotiated corporate regulations do not univocally disadvantage or favor

[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).

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

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

2 FDI, bribery, and the anti-bribery regime

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

Companies can bribe local public officials in the conduct of international business (Pinto and Zhu, 2016). Bribery[3] is documented in the registration of an MNC and procurement in corrupt economies (Gueorguiev and Malesky, 2012). It does not univocally affect the cost-benefit evaluation made by firms. On the one hand, corruption offers advantages to foreign companies, "greasing the wheels of commerce": it allows firms to enter local markets, overcome competitors, and extract rents (Knutsen et al., 2017; Malesky et al., 2015; Søreide, 2006; Zhu, 2017). On the other, it increases costs faced by a firm, because

[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]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).

it works as a tax demanded by public officials (Treisman, 2007; Wei, 2000) and it creates uncertain and inefficient contracts (Lambsdorff, 2002; Rose-Ackerman, 1975).

Home states have adopted corporate policies to intervene in this calculus and deter foreign bribe-payments made by their firms (Cuervo-Cazurra, 2008). By threatening judicial repercussions at home, these policies conscript domestically-incorporated firms to diffuse anti-bribery standards abroad. In order to be viable, anti-bribery policies have historically been harmonized under the umbrella of international organizations[4]. United States history offers a meaningful example. The US was the first country to prohibit foreign bribery by American companies in 1977, when Congress passed the Foreign Corrupt Practices Act (FCPA). Notwithstanding the powerful provisions in this Act, which established civil and criminal responsibility for foreign corruption, enforcement of the FCPA lagged for decades due to the absence of similar regulations among non-US competitors (Tarullo, 2004). Enforcing the FCPA would have tilted the playing field of international competition against US-based companies. It was only when similar policies were adopted at the OECD in 1997, with an Anti-Bribery Convention[5], that US authorities could start to enforce their own anti-bribery law against multinational companies (Brewster, 2017). Ratifier home states now scrutinize and prosecute bribery perpetrated beyond national borders by their companies, foreign employees, or subsidiaries[6].

Crucially, enforcement of this anti-bribery regime does not rest solely on individual countries' efforts. Rather, other member countries can (and do) substitute for weak enforcement by partners. The most notable country applying this strategy is arguably the US: with the ratification of the 1997 OECD Convention, the United States expanded significantly the extraterritorial provisions of its own anti-bribery policy, the FCPA (Leibold, 2014). As a result, since the early 2000s US regulatory agencies – the Department of Justice (DOJ) and the Securities and Exchange Commission (SEC) – could threaten firms headquartered in OECD Convention's ratifiers of prosecution *in the US* for violating the FCPA (Spahn, 2013), a strategy that some legal scholars describe as "international-competition neutral" (Brewster, 2017, 1615). Cooperation among anti-corruption IO members is well-documented (Gest and Grigorescu, 2010). Since the early 2000s, such mutual assistance allowed US prosecutors vast support from authorities of other signatories to prosecute foreign companies in the US (Kaczmarek and Newman, 2011). Figure 1 describes this approach by plotting the yearly number of FCPA cases enforced by the DOJ and SEC involving US and non-US companies and the total fines levied. Starting from 2005, US federal agencies enforced the FCPA vigorously against non-US based companies. Choi and Davis (2014) study these

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

[5]The Convention is considered among the strongest anti-corruption regulations. MNCs under this regulatory umbrella account for more than 80% of global outbound foreign direct investment stocks and include 95 of the 100 largest non-financial enterprises (OECD, 2018). As of August 2022, 44 ratifier countries include all current OECD members and 7 non-member states: Argentina, Brazil, Bulgaria, Costa Rica, Peru, South Africa, and Russia.

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

patterns and find that, even when holding constant the severity of the criminal conduct, US agencies levy disproportionally larger fines from non-US companies.

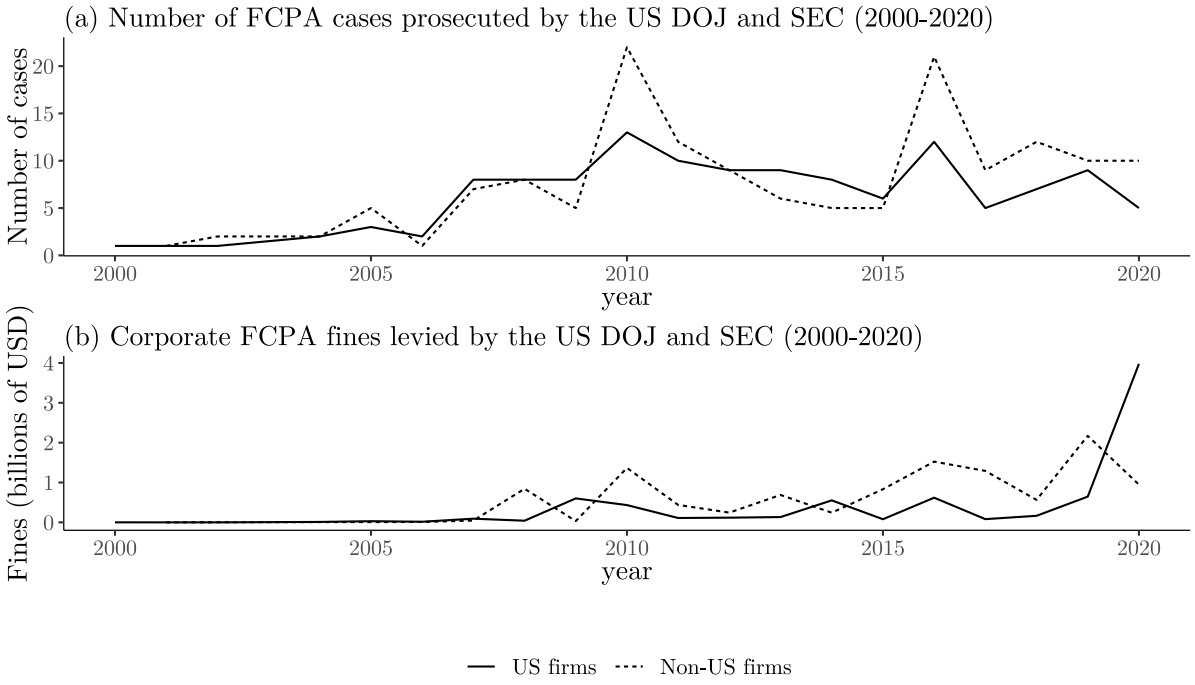


Figure 1: FCPA cases enforced by the DOJ and SEC, number of cases and amount of fines. Data from the Violation Tracker, Good Jobs First: https://violationtracker.goodjobsfirst.org

3 The conditional effect of anti-bribery regulations on FDI

Recent evidence shows that multilateral anti-bribery policies can effectively curb regulated companies' foreign corrupt behaviors (Jensen and Malesky, 2018). How do they affects firms' *legitimate* economic activity, instead? Answers from political economy are divided in two camps, which currently lack unification. In this section, I first resume mechanisms and expectations generated by each camp, before arguing that their implications are observable in host economies with different levels of corruption.

I label the first argument *deterrence*. Its proponents stress that anti-bribery standards impose costs for regulated firms that deter investment into corrupt economies. In these countries, the likelihood that a bureaucrat will demand a bribe in the conduct of business is higher. If they pay bribes, regulated companies are at risk of legal costs. Home-country judicial authorities (or those who enforce anti-bribery policies extraterritorially, *e.g.* the US) levy blockbuster fines from guilty companies. But monetary disbursements are not limited to penalties. Authorities, especially in the US, often mandate costly re-structure of corporate organization to ensure compliance with anti-bribery standards in the future[7]

[7]Authorities often turn executive offices of guilty companies inside out. Suspect firms usually set up systems of internal investigations, employ third-parties for monitoring internal activities (at least for a probatory period), and periodically rotate international offices to avoid managers established personal connections with local authorities. For a textbook example, see the measures implemented by Siemens AG after an infamous worldwide bribery scan-

(Garrett, 2011). Finally, markets impose reputational costs for corporate corruption. When discovered, foreign bribery turns into scandals with wide international resonance. Markets react to these stories: following a bribery scandal, about 80% of every lost dollar in a firm's share value is due to market-based sanctions (Sampath et al., 2018).

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

I argue that, in fact, both pulls simultaneously affect the conditions that regulated companies face when deciding whether to invest into a foreign corrupt economy. Before investing into such countries, any company assesses the likelihood that its subsidiaries will have to pay bribes to local public officials in the conduct of business, whether at the startup phase of an investment or *ex-post* (Barassi and Zhou, 2012). Assessing this likelihood means evaluating the relative bargaining power between the firm's subsidiaries and local bureaucrats. As argued by the *empowerment* claim, regulated firms might evaluate that their subsidiaries in a corrupt country could resist bribe requests, enabling them to operate more efficiently. However, this can only be successful where public officials' bargaining power is relatively weak. In countries where corruption is endemic, instead, public officials' bribe requests cannot be easily turned down (Ades and Di Tella, 1999; Svensson, 2003). In these contexts, regulation does not provide an effective legal leverage. Rather, it exposes companies to the risk of facing further legal costs at home. Here, regulated firms expect they would operate at higher costs, as expected by *deterrence*.

These evaluations, then, translate into actual investment decisions. This argument therefore maps onto three possible comparisons of investment choices made by regulated and unregulated firms, corresponding to three levels of host-country corruption. In a first scenario, firms face the decision of investing into a non-corrupt host economy[8]. Here, regulated firms do not face different investment conditions than

dal: https://www.complianceweek.com/how-siemens-worked-to-fix-a-culture-of-institutionalized-corruption/14915.article (last accessed on July the 28th, 2021).

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

(comparable) unregulated competitors, because bureaucrats would be very unlikely to demand bribe payments. Regulated and unregulated firms would make similar investment decisions, thus anti-bribery regulations do not affect investment decisions.

In a second scenario, firms evaluate investing into a moderately corrupt host economy[9]. Here, local public officials could demand bribes to investors, either in the process of starting up a subsidiary or *ex-post*. However, regulated firms could leverage provisions they are subject to, under anti-corruption IOs, in order to turn such requests down and operate more efficiently. They will be more likely to invest in these destinations than similar unregulated competitors, who cannot exploit the same legal leverage to cut costs. That is, anti-bribery policies should favor investment of regulated firms into moderately corrupt economies.

Finally, in a third scenario firms evaluate an investment into an extremely corrupt host country[10]. In contexts like this, firms would not be able to turn down bribe requests, which are an expected business custom and sometimes even a necessary condition to entry (Zhu, 2017). This is true for regulated and unregulated companies. However, those under the umbrella of anti-corruption IOs are exposed to additional legal costs at home if they are caught paying bribes. They will be less likely to invest here than their unregulated competitors. Anti-bribery policies therefore deter investment of regulated companies into extremely corrupt destinations.

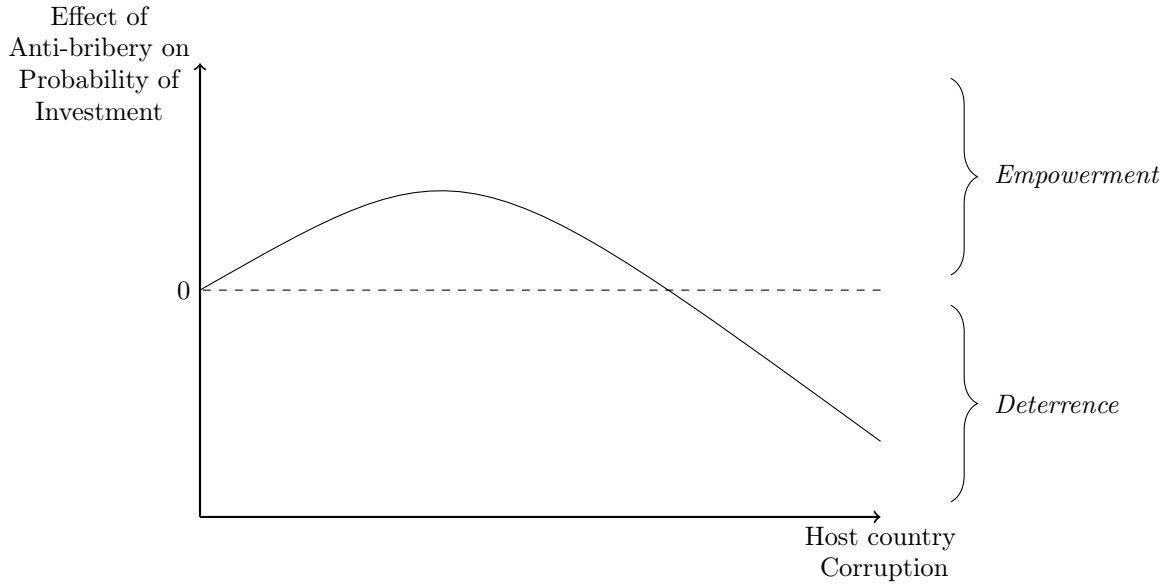


Figure 2: Expected effect of anti-bribery laws on investments, conditional on host country corruption

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

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

Figure 2 generalizes my expectation beyond these discrete scenarios. It sketches the effect of anti-bribery policies on the probability of a foreign investment (y-axis), at given levels of corruption of the host country (x-axis). It shows that *deterrence* and *empowerment* can be seen as special cases of the same argument. For low levels of corruption of the host country, the effect should be null. As the host economy becomes more corrupt, regulation advantages firms and increases their probability to invest. *Empowerment* prevails here. As the host country becomes more corrupt, this effect reaches a maximum, decreases, and reverses. In extremely corrupt host countries, anti-bribery policies disadvantage firms due to higher risk of prosecution. Here, *deterrence* dominates and the effect is negative. I thus expect the effect of anti-bribery policies on investment to depend non-linearly on the level of corruption of the host economy.

4 Empirical analysis

I claim that anti-bribery policies affect foreign investment decisions by companies conditional on the level of corruption of host economies, in the non-monotonic manner sketched by Figure 2. I test my argument focusing on anti-bribery policies under the OECD Convention. I propose two empirical exercises to test my argument. The first one uses firm-level data on foreign investment decisions. It offers insights into the micro-level decision-making process of investors. Results are based on a selection on observables design and lend broad support to my theoretical claim. The second empirical exercise uses country-level data on dyadic FDI flows. Its goal is to provide a more solid identification on top of the micro-level evidence from the first exercise. It employs a generalized synthetic counterfactual design to corroborate expectations from the theory and results from the firm-level study.

4.1 Firm-level data

I estimate a firm-level model of investment decisions conditional on corruption. A firm f from country i is observed to invest in country j ($I_{fij} = 1$) only if its latent propensity to invest, I^*_{fij}, is greater than 0. Equation 1 expresses the latent variable I^*_{fij}. It is a function of whether country i is a ratifier of the OECD Convention ($S_i = 1$), and of a continuous measure for the level of corruption of the host country (C_j). Corruption also appears as a squared term (C_j^2). Both C_j and C_j^2 are multiplied by S_i. This represents the statement that the effect of the OECD Convention on the propensity to invest abroad is non-linear in the level of corruption of the host country, as in Figure 2. Matrix $\mathbf{X_{fij}}$ includes covariates while u_{fij} is the idiosyncratic error term.

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

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:

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

Equation 2 represents the non-linear effect of the OECD Convention on the propensity of a firm to invest abroad, conditional on the level of corruption of the host country. It is a parabola with an expected inverted-U shape, as in Figure 2. Therefore, β_1 is expected to be negative, β_2 positive, and β_3 null.

I estimate equation 1 with firm-level data drawn from the Orbis Corporate Ownership Database[11], retrieved from Beazer and Blake (2018). This dataset reports information on foreign subsidiary incorporations made by 3871 individual parent firms between 2006 and 2011. It reports the country of origin of the parent firm (home country) and that of the subsidiary (host country) for each incorporation. Represented home economies are 62, host countries are 85. Data also include firm-level, country-level, and dyadic covariates.

The dataset reports the "ultimate parent" of each foreign subsidiary. It excludes financial investments and small firms[12]. These selections ensure the sample represents a population composed of large MNCs and investments represent long-term foreign productive enterprises, rather than speculative ventures[13]. Orbis data have about a two-year lag between the disclosure of firms' information and its reporting in the data. They are also not suited to derive firm-level time-series (Kalemli-Ozcan et al., 2015). Both issues are avoided here employing a cross-section of observations reporting *any* investment of these companies between 2006 and 2011. Figure B.1 in Appendix describes industry classification of these companies.

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

Skeptical readers might be concerned that, between 2006 and 2011, the OECD Convention had no dent against foreign bribery, because many signatories began enforcing their laws in later years. However, my argument focuses on the way adoption of anti-bribery laws affects investment conditions, regardless of

[11] 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.

[12] 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.

[13] 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.

their level of enforcement. Moreover, even though OECD Convention ratifiers laxly enforced their policies in the early 2000s, US authorities exploited ratification of the Convention to vigorously apply the FCPA against non-US companies headquartered in signatories (Brewster, 2017; Leibold, 2014), as described by Figure 1 and anecdotal evidence[14]. Finally, lax enforcement should draw towards the null equally the *deterrence* effect (because companies would not be discouraged from bribing) and the *empowerment* effect (because companies would not have a legal leverage to refuse bribe requests). If enforcement strengthened the two mechanisms, any significant finding would likely be larger in magnitude in periods with stronger enforcement. In appendix, I address these concerns empirically too by extending data from Beazer and Blake (2018) to consider investments made by these very firms until 2018. Results hold[15].

I follow Beazer and Blake (2018) and code a binary outcome variable, called *Subsidiary*, representing whether a firm f from country i has incorporated a subsidiary in country j between 2006 and 2011. The binary dependent variable has a dyadic form. For each parent company f from country i it is assigned a 1 if the firm is reported to have set up a subsidiary in the host country j in the time period of interest. It is assigned a 0, instead, if no subsidiary was established in the (potential) host country j[16]. 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 the binary indicator *OECD Ratifier*. It represents whether the home country i of a parent firm f has ratified the OECD Anti-Bribery Convention by 2005[17]. As for all my covariates I consider 2005, for it is the year before the start of my cross-section.

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

Alternative solutions leverage observable information. Measures like the number of bribery cases,

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

[15]See Table B.4.

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

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

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

I adopt this measure of corruption and discuss its assumptions in Appendix. In a robustness test, I substitute it with traditional perception-based measures and verify that my results hold[18]. 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 non-corrupt economies) to a maximum of 8.90.

I follow Beazer and Blake (2018) and explain my binary outcome variable in a multilevel logit model[19]. This is a forced choice to specify the cross-level interaction in equation 1 (Bell and Jones, 2015). This model choice is also suited to the dataset structure, where a firm investing abroad is cross-nested in a directed dyad, and in its home and host countries. Multilevel unobserved heterogeneity in this complex nesting can easily confound the explanation of the outcome variable, therefore it must be properly modelled. For instance, research found that companies gain from investing in the same destinations as their co-nationals (Johns and Wellhausen, 2016). To account for similar clustering, all specifications include random intercepts at the dyad, home, and host countries-level. A further specification adds industry-level intercepts to account for sector-specific heterogeneity. Since no clear hierarchy can be discerned in the data structure, I employ a cross-classified random effect model. A multilevel regression also correctly models the thousands of repeated observations generated by the dyadic structure of the dataset. If their correlation were not properly accounted for, this large number of repeated observations would artificially reduce standard errors and produce unreliable tests of hypotheses.

[18] See Table B.3 in Appendix.
[19] I maximize the log-likelihood function of this model with a Gauss-Hermite Quadrature method.

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

4.1.1 Results

Table 1 presents my results relative to the variables of interest only[21]. All models condition the effect of *OECD Ratifier* on the first and second-degree terms of the host country's corruption measure *Host PACI*. In order to prevent suppression effects (Lenz and Sahn, 2021), Table 1 first includes only the variables of interest (1). Then, it adds controls at the level of host and home countries (2). Then it adds firm-level covariates (3), then dyadic controls (4). Finally, it adds industry-level intercepts (5).

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

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

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

[20] See Table B.1. 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.2.

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

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

I compute the marginal effect of anti-bribery policies at given levels of corruption to evaluate if the argument represented by Figure 2 is supported. I compute the change in predicted probability when *OECD Ratifier* varies from 0 to 1 holding everything else at its mean. I compute this effect for given levels of *Host PACI*. This is equivalent to measuring the change in the predicted probability of an investment when comparing an average regulated company to an average unregulated company, conditional on observables, at given values of *Host PACI*. I compute 95% confidence intervals of this estimated difference simulating 1000 draws from its sampling distribution (King et al., 2000).

Figure 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 non-corrupt host economies where data support is limited. When *OECD Ratifier* changes from 0 to 1, the predicted probability that a firm will incorporate a subsidiary changes conditionally on the level of corruption of

the host economy, in a non-monotonic way.

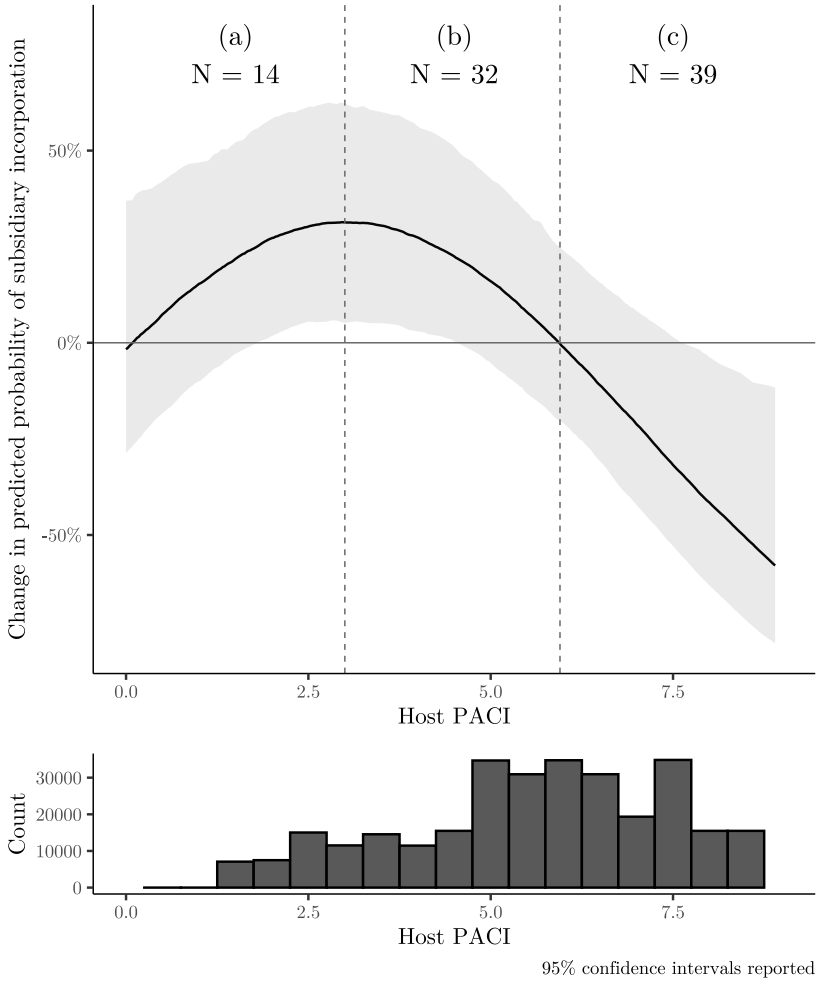


Figure 3: The non-linear effect of *OECD Ratifier* on *Subsidiary*, conditional on *Host PACI*

The effect can be roughly divided in panels (a), (b), and (c). Figure 3 reports the number of host countries included in each panel, under the corresponding label. In panel (a) the change in predicted probability is close to zero for least corrupt host economies (*e.g.*: Canada, Denmark, Sweden). Then, it increases as the host country becomes more corrupt, indicating that firms from countries with anti-bribery policies have a higher probability of investing here. At its maximum, firms from ratifiers have a 31% higher probability of investing than their competitors. Host countries corresponding to the maximum of the *empowerment* effect are Singapore and Taiwan. In panel (b), as the host country becomes more corrupt, the effect of regulation remains positive but declines in size. This indicates that OECD anti-bribery policies still benefit regulated firms investing in economies like Brazil, China, Indonesia, Italy, Mexico, and the United Arab Emirates, but to a lesser extent. For extreme levels of corruption, as in panel (c), firms from ratifier countries are worse off. They have a lower probability of investing here than their unregulated counterparts, a quantity that reaches a lowest point of -58% for host countries at the right-end of the corruption scale like Egypt, India, Kazakhstan, Nigeria, or Russia.

I propose extensive tests to show robustness of these results in Appendix. I first show that an interaction of *OECD Ratifier* with a first-degree polynomial of *Host PACI* produces insignificant estimates. This provides confidence that the effect of the Convention on investment is not linear in corruption. I then substitute the PACI with traditional, perception-based indexes of corruption and show that results hold. I also use the original PACI measure in Escresa and Picci (2017) to enlarge the set of host countries included in the analysis. Then, I exclude outlier countries or firms from signatories that ratified the Convention in the 2006-2011 cross-section. Finally, I replicate results for an extended dataset considering additional investments made by these firms until 2018. Results hold to all such tests. In Appendix, I also offer a placebo test that confirms my expectations. I show that the effect of the OECD Convention is detected only for firms active in industries where bribes are typically paid. I find no effect in sectors where bribery is not customary.

4.2 Country-dyadic data

The previous section provides micro-level evidence that investment behaviors of firms who are subject to OECD anti-bribery policies depend non-linearly on the level of corruption of the host country. Yet, the analysis suffers from two issues. First, it focuses on cross-sectional information between 2006 and 2011. It cannot study change in investment behavior after ratification of the Convention. Second, selection under OECD policies is not random. Firms under OECD policies have very specific characteristics that distinguish them from those who are not subject to such policies. If random effects and controls did not account for such differences, the conditional independence assumption would be violated and the previous analysis would wrongly attribute the effect of these idiosincrasies to anti-bribery policies. For instance, economies adopting the OECD Convention generally belong to the Global North and corrupt host economies tend to be concentrated in the Global South. Do results in Table 1 represent the deterrent effect of institutions in these host countries for companies headquartered in the Global North (see Beazer and Blake, 2018), rather than a genuine effect of the Convention in corrupt host economies?

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

flows in directed dyads should therefore be able to capture this effect.

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

I test my conditional argument by adopting a binning approach, which allows to study non-linear conditional effects. I divide dyads in five subsamples depending on the level of corruption of the host country in the dyad. I measure corruption using the same 2005 *Host PACI* index computed for the firm-level analysis[22]. The five subsamples are defined based on quintiles of the *Host PACI* distribution. The choice to divide the distribution in five parts is purely empirical, as it guarantees enough observations in each bin. Alternative feasible choices (using tertiles and quartiles) provide consistent evidence of a non-linear effect of the Convention on investment, conditional on the corruption level of host economies[23]. A total of 1679 directed dyads report information for the dependent variable. I estimate the effect of the OECD Convention within each of the five bins to study the impact of anti-bribery policies conditionally on the level of corruption of the host economy.

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

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

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

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

usually require balanced panel datasets, whereas mine is unbalanced.

Given that treatment timing is staggered, and my dataset is unbalanced, I follow a different approach. I draw on information from the control group to synthesize counterfactuals for dyads in the treatment group. A synthetic counterfactual is a weighted average of a single treated unit imputed using available information from untreated units in the sample (Abadie et al., 2015). The algorithms building such synthetic unit aim at maximizing similarity in pre-treatment trends between the treated unit and the counterfactual. This allows to make more credible inferences on estimated ATTs. Xu (2017) proposed a generalization of the approach that extends to a panel with several treated units and a sufficient number of untreated ones. The methodology takes into account potential heterogeneity in treatment effects and treatment timing. It also works with unbalanced panel data. It is thus an appealing choice for my case study. The method imputes one synthetic counterfactual for each treated unit, and derives an average effect from this data. It also computes measures of uncertainty around the estimated ATTs. I apply this methodology to impute an untreated counterfactual for each treated dyad in each of the five bins[24].

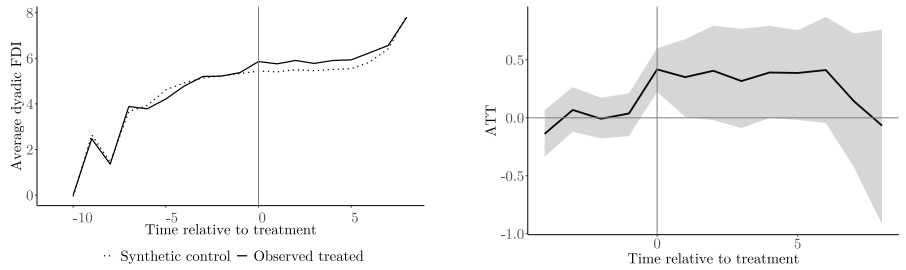
This methodology results in a much smaller sample, because only dyads with enough pre-treatment observations are kept, to ensure estimates do not depend on extrapolations. Excluding some dyads might introduce a bias if missingness correlates with outcome and treatment grouping. In a robustness test reported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

4.2.1 Results

Figure 4 reports results obtained in each corruption bin. Average pre-treatment trends of the synthetic counterfactuals closely approximate observed average trends of treated dyads in all bins. This lends confidence that synthetic control units were properly imputed. Post-treatment differences in average flows between observed and synthetic controls confirm expectations from the theory. On average, dyads with extremely clean host economies (first bin) saw a small or insignificant increase in their FDI flows in the post-treatment period. A positive effect, instead, is detected for dyads with moderately corrupt host economies (second and third bins). Post-treatment differences between observed and synthetic FDI dyadic flows are not significant for units in the fourth bin. Finally, FDI flows from ratifiers to the Convention were negatively affected for dyads with extremely corrupt host countries (fifth bin).

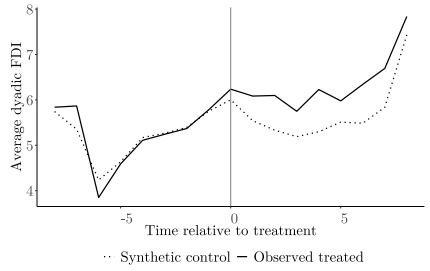
Figure 5 reports the average effect over the entire time period for each of the five bins. It also reports the distribution of the *Host PACI* variable and the number of dyads in each bin. Estimates across the fives bins reproduce the inverted-U pattern seen in the firm-level analysis. For dyads in the

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

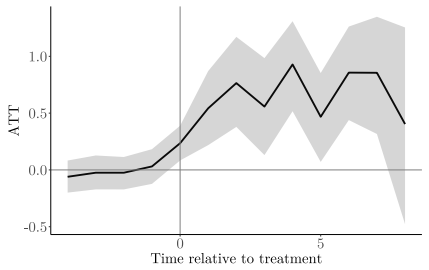


(a) Bin 1: Average dyadic flows

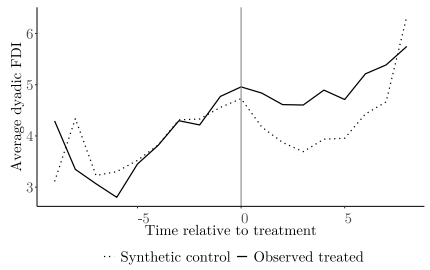
(b) Bin 1: Estimated ATT



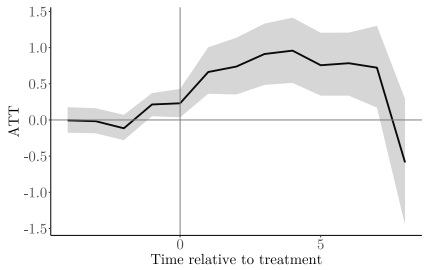
(c) Bin 2: Average dyadic flows



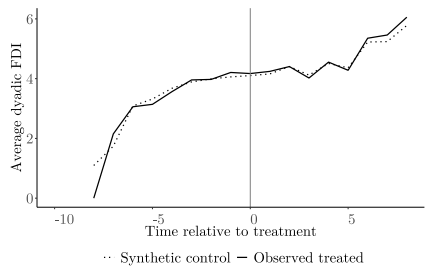
(d) Bin 2: Estimated ATT



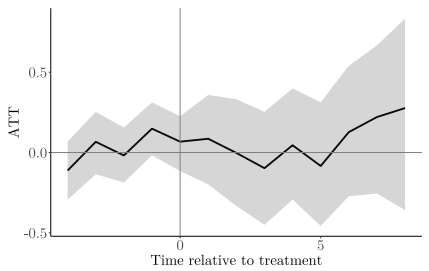
(e) Bin 3: Average dyadic flows



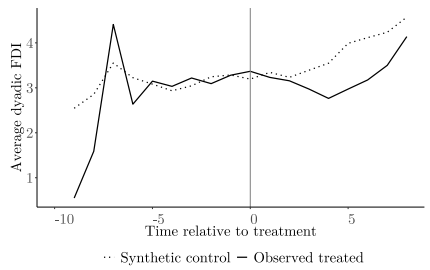
(f) Bin 3: Estimated ATT



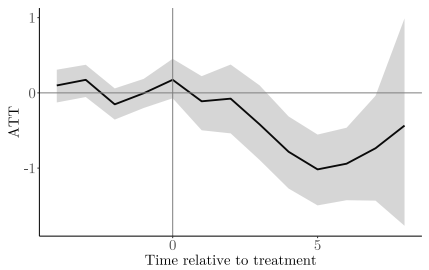
(g) Bin 4: Average dyadic flows



(h) Bin 4: Estimated ATT



(i) Bin 5: Average dyadic flows



(j) Bin 5: Estimated ATT

Figure 4: Country-level data. Generalized synthetic control method. Average trends and estimated ATT in the five bins

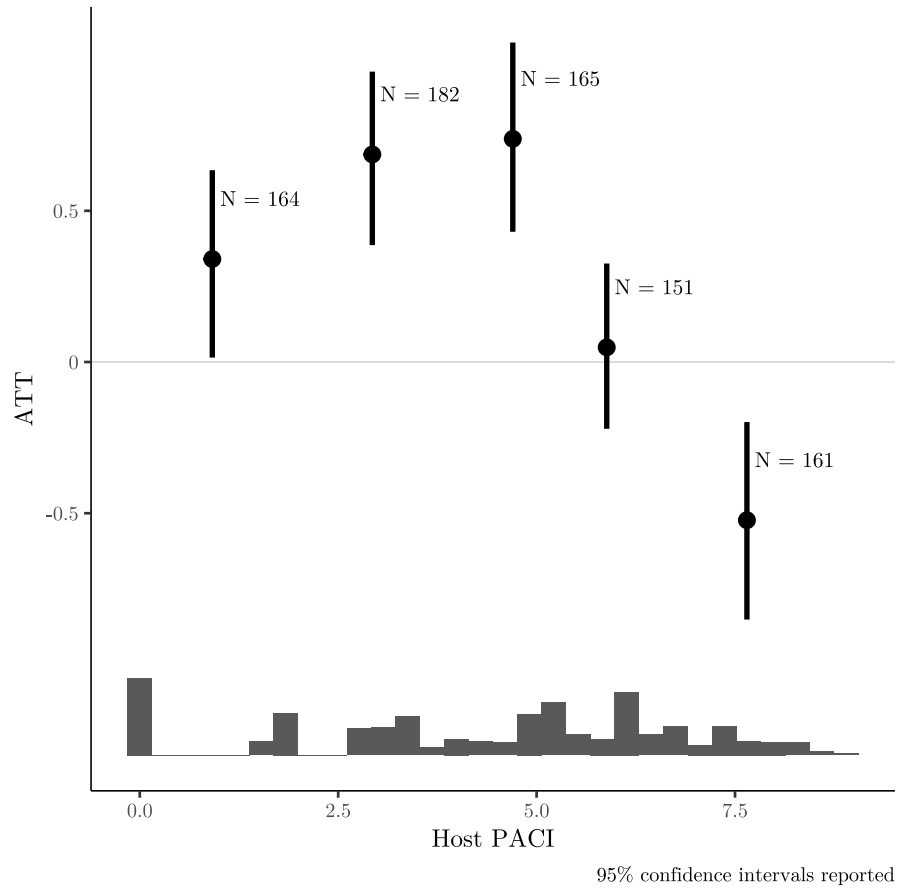


Figure 5: Country-level data. Effect estimates from synthetic counterfactual designs

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

In Appendix, I show results are similar when binning *Host PACI* using tertiles or quartiles (Figures C.1 and C.2). I also test robustness of these estimates by adopting a 2FE design (Figure C.3). The design uses all dyads, therefore ensuring results from Figure 5 do not depend on dropping dyads without enough pre-treatment observations. In Appendix, I also tackle the rollout of treatment timing issue by introducing a "buffer" for the interval of years 1999–2001, when countries were ratifying the OECD Convention at staggered times. I exclude observations in these years to force the analysis back to a

traditional two groups and two time-periods difference-in-differences. Results are consistent with those presented here. Finally, I fit traditional alternatives to the binning estimator for capturing the conditional effect. I interact the binary *OECD Ratifier* 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.

5 Concluding remarks

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

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

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

criticized of an ecological fallacy for using aggregated data to test a firm-level theory. Their estimates, yet, appear more credible in light of the micro-level evidence from the first empirical exercise.

Limitations of the study open up to various future lines of inquiry. I argue that home countries' adoption of anti-bribery laws makes firms better or worse-off in international business depending on the leverage available to refuse bribe requests. This mechanism is not tested by the present analysis, although previous studies lend plausibility to it (Ades and Di Tella, 1999; Perlman and Sykes, 2017; Svensson, 2003). It might be possible that anti-bribery policies affect the behavior of public officials too. The overall observed effect on investment might thus be the compounded result of these different mechanisms, which the study cannot disentangle[25]. Future qualitative work could further investigate this mechanism against possible alternatives. Finally, the study explicitly does not consider strategies to invest in a foreign market other than establishing ownership. Licensing and joint ventures, yet, are potential ways for firms to invest in a foreign economy. They can expose firms from ratifiers of the Convention to a lower risk of interaction with corrupt public officials, and might therefore be a preferred strategy (see Chapman et al., 2020; Zhu and Shi, 2019). A future study could therefore investigate the effect of the Convention on these alternative investment strategies.

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

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

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

[25]Sector-specific evidence presented in Appendix (Table B.5) suggests that the effect in place involves only industries where bribes are a custom, and not the rest. This is consistent with the mechanism advanced, which should not hold in industries where bribes are no usual custom, but does not allow a final word on the matter.

References

Abadie, A., Diamond, A., and Hainmueller, J. (2015). Comparative politics and the synthetic control method. *American Journal of Political Science*, 59(2):495–510.

Abbott, K. W. and Snidal, D. (2002). Values and interests: International legalization in the fight against corruption. *The Journal of Legal Studies*, 31(S1):S141–S177.

Ades, A. and Di Tella, R. (1999). Rents, competition, and corruption. *American Economic Review*, 89(4):982–993.

Allee, T. and Peinhardt, C. (2011). Contingent credibility: The impact of investment treaty violations on foreign direct investment. *International Organization*, 65(3):401–432.

Barassi, M. R. and Zhou, Y. (2012). The effect of corruption on FDI: A parametric and non-parametric analysis. *European Journal of Political Economy*, 28(3):302–312.

Beazer, Q. H. and Blake, D. J. (2018). The conditional nature of political risk: How home institutions influence the location of foreign direct investment. *American Journal of Political Science*, 62(2):470–485.

Bell, A. and Jones, K. (2015). Explaining fixed effects: Random effects modeling of time-series cross-sectional and panel data. *Political Science Research and Methods*, 3(1):133–153.

Biglaiser, G. and DeRouen, K. (2010). The effects of IMF programs on US foreign direct investment in the developing world. *The Review of International Organizations*, 5(1):73–95.

Brambor, T., Clark, W. R., and Golder, M. (2006). Understanding interaction models: Improving empirical analyses. *Political Analysis*, 14(1):63–82.

Brazys, S. and Kotsadam, A. (2020). Sunshine or curse? foreign direct investment, the OECD Anti-Bribery Convention, and individual corruption experiences in Africa. *International Studies Quarterly*.

Brewster, R. (2017). Enforcing the FCPA: International resonance and domestic strategy. *Virginia Law Review*, pages 1611–1682.

Bruno, V. and Claessens, S. (2010). Corporate governance and regulation: can there be too much of a good thing? *Journal of Financial Intermediation*, 19(4):461–482.

Bukovansky, M. (2006). The hollowness of anti-corruption discourse. *Review of International Political Economy*, 13(2):181–209.

Chapman, T. L., Jensen, N. M., Malesky, E. J., and Wolford, S. (2020). Leakage in international regulatory regimes: Did the OECD Anti-Bribery Convention increase bribery? *Quarterly Journal of Political Science*.

Cheibub, J. A., Gandhi, J., and Vreeland, J. R. (2010). Democracy and dictatorship revisited. *Public choice*, 143(1-2):67–101.

Choi, S. J. and Davis, K. E. (2014). Foreign affairs and enforcement of the Foreign Corrupt Practices Act. *Journal of Empirical Legal Studies*, 11(3):409–445.

Cooley, A. and Sharman, J. C. (2017). Transnational corruption and the globalized individual. *Perspectives on Politics*, 15:732–753.

Coppedge, M., Gerring, J., Knutsen, C. H., Lindberg, S. I., Teorell, J., Altman, D., Bernhard, M., Fish, M. S., Glynn, A., Hicken, A., Lührmann, A., Marquardt, K. L., McMann, K., Paxton, P., Pemstein, D., Seim, B., Sigman, R., Skaaning, S.-E., Staton, J., Wilson, S., Cornell, A., Alizada, N., Gastaldi, L., Gjerløw, H., Hindle, G., Ilchenko, N., Maxwell, L., Mechkova, V., Medzihorsky, J., von Römer, J., Sundström, A., Tzelgov, E., Wang, Y.-t., Wig, T., and Ziblatt, D. (2020). V-dem country-year dataset v10.

Cuervo-Cazurra, A. (2008). The effectiveness of laws against bribery abroad. *Journal of International Business Studies*, 39(4):634–651.

Das, S. P. (1999). Direct foreign investment versus licensing. *Review of Development Economics*, 3(1):86–97.

Davis, K. E. (2002). Self-interest and altruism in the deterrence of transnational bribery. *American Law and Economics Review*, 4(2):314–340.

Della Porta, D. and Vannucci, A. (1999). *Corrupt Exchanges. Actors, Reources and Mechanisms of Political Corruption*. Aldine de Gruyter.

Dunning, J. H. (1980). Toward an eclectic theory of international production: Some empirical tests. *Journal of International Business Studies*, 11(1):9–31.

Escresa, L. and Picci, L. (2017). A new cross-national measure of corruption. *The World Bank Economic Review*, 31(1):196–219.

Findley, M. G., Nielson, D. L., and Sharman, J. C. (2015). Causes of noncompliance with international law: A field experiment on anonymous incorporation. *American Journal of Political Science*, 59(1):146–161.

Garrett, B. L. (2011). Globalized corporate prosecutions. *Virginia Law Review*, 97:1775.

Genovese, F. (2020). Market responses to global governance: International climate cooperation and Europe's carbon trading. *Business and Politics*, pages 1–33.

Gest, N. and Grigorescu, A. (2010). Interactions among intergovernmental organizations in the anti-corruption realm. *The Review of International Organizations*, 5(1):53–72.

Goodman-Bacon, A. (2018). Difference-in-differences with variation in treatment timing. Technical report, National Bureau of Economic Research.

Gray, J. (2009). International organization as a seal of approval: European union accession and investor risk. *American Journal of Political Science*, 53(4):931–949.

Gueorguiev, D. and Malesky, E. (2012). Foreign investment and bribery: a firm-level analysis of corruption in vietnam. *Journal of Asian Economics*, 23(2):111–129.

Hafner-Burton, E. M. and Schneider, C. J. (2019). The dark side of cooperation: International organizations and member corruption. *International Studies Quarterly*, 63(4):1108–1121.

Hainmueller, J., Mummolo, J., and Xu, Y. (2019). How much should we trust estimates from multiplicative interaction models? simple tools to improve empirical practice. *Political Analysis*, 27(2):163–192.

Hakkala, K. N., Norbäck, P.-J., and Svaleryd, H. (2008). Asymmetric effects of corruption on FDI: evidence from Swedish multinational firms. *The Review of Economics and Statistics*, 90(4):627–642.

Heywood, P. M. (1997). Political corruption: Problems and perspectives. *Political Studies*, 45(3):417–435.

Imai, K. and Kim, I. S. (2020). On the use of two-way fixed effects regression models for causal inference with panel data. *Political Analysis*.

Jensen, N. M. (2008). *Nation-states and the multinational corporation: A political economy of foreign direct investment*. Princeton University Press.

Jensen, N. M. and Malesky, E. J. (2018). Nonstate actors and compliance with international agreements: An empirical analysis of the OECD Anti-Bribery Convention. *International Organization*, 72(1):33–69.

Johns, L. and Wellhausen, R. L. (2016). Under one roof: Supply chains and the protection of foreign investment. *American Political Science Review*, 110(1):31–51.

Kaczmarek, S. C. and Newman, A. L. (2011). The long arm of the law: Extraterritoriality and the national implementation of foreign bribery legislation. *International Organization*, 65(4):745–770.

Kalemli-Ozcan, S., Sorensen, B., Villegas-Sanchez, C., Volosovych, V., and Yesiltas, S. (2015). How to construct nationally representative firm level data from the orbis global database: New facts and aggregate implications. Technical report, National Bureau of Economic Research.

Kalyanpur, N. and Newman, A. L. (2019). Mobilizing market power: Jurisdictional expansion as economic statecraft. *International Organization*, 73(1):1–34.

Kaufmann, D. and Wei, S.-J. (1999). Does "grease money" speed up the wheels of commerce? Technical report, National bureau of economic research.

Kennard, A. (2020). The enemy of my enemy: When firms support climate change regulation. *International Organization*, pages 1–35.

Keohane, R. O. (1984). *After hegemony: Cooperation and discord in the world political economy*. Princeton University Press.

Kerner, A. (2014). What we talk about when we talk about foreign direct investment. *International Studies Quarterly*, 58(4):804–815.

King, G., Tomz, M., and Wittenberg, J. (2000). Making the most of statistical analyses: Improving interpretation and presentation. *American Journal of Political Science*, 44(2):347–361.

Knutsen, C. H., Kotsadam, A., Olsen, E. H., and Wig, T. (2017). Mining and local corruption in Africa. *American Journal of Political Science*, 61(2):320–334.

Lambsdorff, J. G. (2002). Making corrupt deals: contracting in the shadow of the law. *Journal of Economic Behavior & Organization*, 48(3):221–241.

Leibold, A. (2014). Extraterritorial application of the FCPA under international law. *Willamette Law Review*, 51:225.

Lenz, G. S. and Sahn, A. (2021). Achieving statistical significance with control variables and without transparency. *Political Analysis*, 29(3):356–369.

Linzer, D. A. and Staton, J. K. (2015). A global measure of judicial independence, 1948–2012. *Journal of Law and Courts*, 3(2):223–256.

Malesky, E. J., Gueorguiev, D. D., and Jensen, N. M. (2015). Monopoly money: Foreign investment and bribery in Vietnam, a survey experiment. *American Journal of Political Science*, 59(2):419–439.

Morse, J. C. (2019). Blacklists, market enforcement, and the global regime to combat terrorist financing. *International Organization*, 73(3):511–545.

Neumayer, E. and Spess, L. (2005). Do bilateral investment treaties increase foreign direct investment to developing countries? *World Development*, 33(10):1567–1585.

OECD (2018). Fighting the crime of foreign bribery. the Anti-Bribery Convention and the OECD Working Group on Bribery. Technical report, Organization for the Economic Cooperation and Development.

Olken, B. A. (2009). Corruption perceptions vs. corruption reality. *Journal of Public Economics*, 93(7-8):950–964.

Perlman, R. L. and Sykes, A. O. (2017). The political economy of the Foreign Corrupt Practices Act: An exploratory analysis. *Journal of Legal Analysis*, 9(2):153–182.

Pinto, P. M. and Zhu, B. (2016). Fortune or evil? the effect of inward foreign direct investment on corruption. *International Studies Quarterly*, 60(4):693–705.

Putnam, T. L. (2009). Courts without borders: Domestic sources of us extraterritoriality in the regulatory sphere. *International Organization*, 63(3):459–490.

Rose-Ackerman, S. (1975). The economics of corruption. *Journal of Public Economics*, 4(2):187–203.

Sampath, V. S., Gardberg, N. A., and Rahman, N. (2018). Corporate reputation's invisible hand: Bribery, rational choice, and market penalties. *Journal of Business Ethics*, 151(3):743–760.

Sharman, J. C. (2011). *The money laundry: Regulating criminal finance in the global economy*. Cornell University Press.

Simmons, B. A. and Elkins, Z. (2004). The globalization of liberalization: Policy diffusion in the international political economy. *American Political Science Review*, 98(1):171–189.

Skovgaard Poulsen, L. N. (2014). Bounded rationality and the diffusion of modern investment treaties. *International Studies Quarterly*, 58(1):1–14.

Søreide, T. (2006). Corruption in international business transactions: the perspective of norwegian firms. *International handbook on the economics of corruption*, pages 381–417.

Spahn, E. K. (2013). Implementing global anti-bribery norms: from the Foreign Corrupt Practices Act to the OECD Anti-Bribery Convention to the UN Convention Against Corruption. *Ind. Int'l & Comp. L. Rev.*, 23:1.

Svensson, J. (2003). Who must pay bribes and how much? evidence from a cross section of firms. *The Quarterly Journal of Economics*, 118(1):207–230.

Tarullo, D. K. (2004). The limits of institutional design: Implementing the OECD Anti-Bribery Convention. *Virginia Journal of International Law*, 44(3):665–710.

Teorell, J., Dahlberg, S., Holmberg, S., Rothstein, B., Alvarado Pachon, N., and Axelsson, S. (2020). The quality of government standard dataset, version jan20.

Tobin, J. L. and Rose-Ackerman, S. (2011). When BITs have some bite: The political-economic environment for bilateral investment treaties. *The Review of International Organizations*, 6(1):1–32.

Treisman, D. (2007). What have we learned about the causes of corruption from ten years of cross-national empirical research? *Annual Review of Political Science*, 10:211–244.

Wei, S.-J. (2000). How taxing is corruption on international investors? *Review of Economics and Statistics*, 82(1):1–11.

Wintoki, M. B. (2007). Corporate boards and regulation: The effect of the Sarbanes–Oxley Act and the exchange listing requirements on firm value. *Journal of Corporate Finance*, 13(2-3):229–250.

Xu, Y. (2017). Generalized synthetic control method: Causal inference with interactive fixed effects models. *Political Analysis*, 25(1):57–76.

Zhu, B. (2017). MNCs, rents, and corruption: Evidence from China. *American Journal of Political Science*, 61(1):84–99.

Zhu, B. and Shi, W. (2019). Greasing the wheels of commerce? corruption and foreign investment. *The Journal of Politics*, 81(4):1311–1327.

Appendix

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

A The *Host PACI* measure

In this section I present the Public Administration Corruption Index (PACI), proposed by Escresa and Picci (2017) and adopted in this study. The PACI relies on the following intuition: suppose all countries were equally corrupt. Then the number of observed cases of cross-border bribery occurring in a country should be proportional to its economic inflows: bribery would simply be more likely to occur where more funds were inflowing. Imagine in fact we observed that a large share of bribes paid by firms from country x abroad are paid in country y, but country y is not a major commercial partner of x. This is evidence that public officials in country y are more corrupt than those in the other partners of x, because they attract more bribes than what could be expected by simply looking at economic flows. The PACI generalizes and formalizes this intuition. For each country y, it is computed as the ratio between the number of observed cross-border bribes paid by firms from the set of all countries X ($X \not\supset y$) 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[26]. 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

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

Table B.1 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)[27]. Data on Host CCE and Host V-Dem Bribery have been retrieved respectively from the Quality of Governance dataset (Teorell et al., 2020) and from the Varieties of Democracy (V-Dem) core database, version 10 (Coppedge et al., 2020).

[26] 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).

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

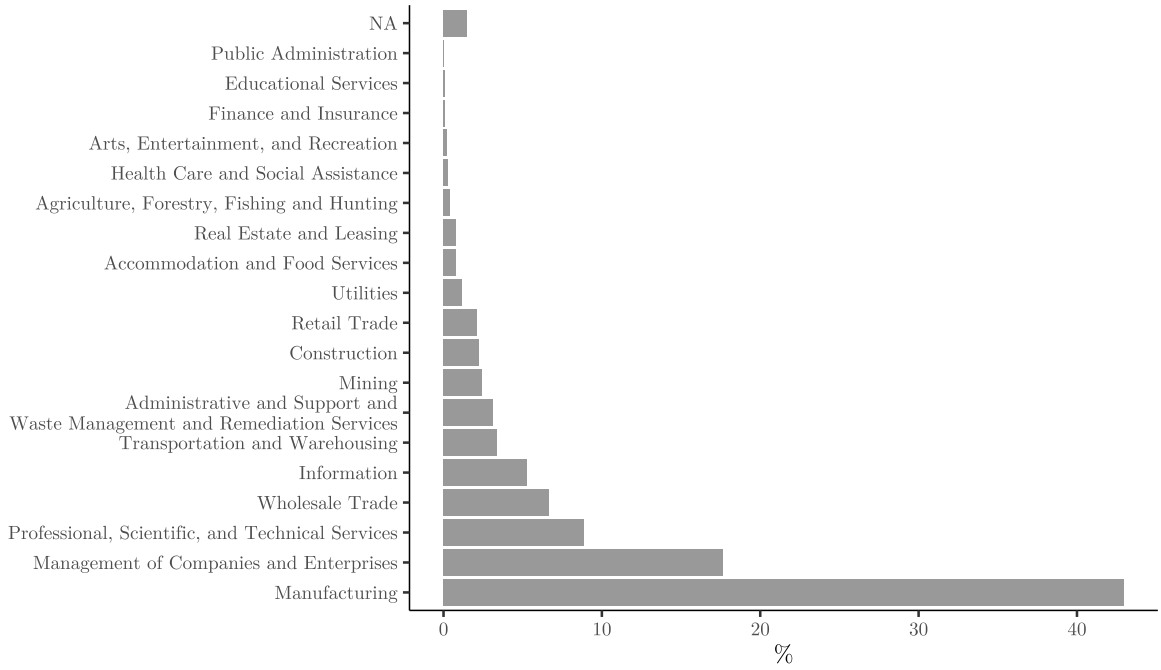


Figure B.1: Firm-level database description: Percentage of firms in the database by NAICS-2 code

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

Table B.1: Firm-level data. Summary statistics

B.2 Full disclosure of results

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

Table B.2: 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 the first tests are reported in Table B.3. In model 1 I replicate the full specification of model 5 in Table 1 excluding the squared measure of *Host PACI* and its interaction with *OECD Ratifier* to show that the effect of the OECD Convention on *Subsidiary* is not conditional on a linear measure of corruption. No term involved in the interaction is found to be statistically significant. I then replicate model 5 of Table 1 using more traditional, perception-based indexes of corruption. First, I use the "Executive bribery and corrupt exchanges" measure from V-Dem (Coppedge et al., 2020). The measure is a Bayesian-based index that relies on both objective and survey information, and is generally considered an improvement of traditional perception-based indexes. Next, I employ the World Bank's CCE, rescaled so as to range from 0 to 5. In both cases, lower values indicate higher levels of corruption. Results obtained remain substantively the same.

Next, I consider the possibility that the main measure of corruption I adopt restricts the sample excessively and introduces a source of selection. Computing the 2005 version of *Host PACI* reduces the number of host countries in the analysis because it relies on fewer observations of the dataset from Escresa and Picci (2017). To test whether results hold with an extended sample of host countries, I replicate model 5 of Table B.2 using the version of the index computed and published by Escresa and Picci (2017), which employs information until 2012 and includes more host countries[28]. 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 Ratifier* assigns them a value of 0. I therefore replicate the analysis excluding them. Results, again, do not change significantly.

Finally, I replicate results from Table 1 extending the dataset to include all investment made by these same companies until 2018. I follow the same procedure in Beazer and Blake (2018) to extend investment data from Orbis. Results are reported in Table B.4. Point estimates of variables of interest are substantively unchanged and statistically significant.

[28] 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:					
	First degree	V-Dem	CCE	Subsidiary PACI (2012)	No China	No Israel No South Africa
	(1)	(2)	(3)	(4)	(5)	(6)
OECD Ratifier × Host PACI^2					-0.029^{*}	-0.024^{+}
					(0.012)	(0.014)
OECD Ratifier × Host PACI	-0.007				0.168^{+}	0.168^{+}
	(0.034)				(0.089)	(0.096)
OECD Ratifier × Host V-Dem Bribery^2		-0.075^{*}				
		(0.032)				
OECD Ratifier × Host V-Dem Bribery		0.133^{+}				
		(0.071)				
OECD Ratifier × Host CCE^2			-0.213^{**}			
			(0.082)			
OECD Ratifier × Host CCE			1.360^{*}			
			(0.543)			
OECD Ratifier × Host PACI^2 (2012)				-0.048^{***}		
				(0.014)		
OECD Ratifier × Host PACI (2012)				0.277^{**}		
				(0.097)		
OECD Ratifier	-0.067	0.021	-2.023^{*}	-0.260	-0.188	-0.197
	(0.191)	(0.181)	(0.847)	(0.193)	(0.196)	(0.225)
Host PACI^2					0.016	0.012
					(0.026)	(0.027)
Host PACI	0.039				-0.069	-0.101
	(0.106)				(0.221)	(0.230)
Host V-Dem Bribery^2		0.133^{*}				
		(0.062)				
Host V-Dem Bribery		-0.089				
		(0.159)				
Host CCE^2			0.154			
			(0.148)			
Host CCE			-0.660			
			(1.012)			
Host PACI^2 (2012)				0.020		
				(0.028)		
Host PACI (2012)				-0.057		
				(0.234)		
Host GDP (log)	0.723^{***}	0.751^{***}	0.737^{***}	0.627^{***}	0.678^{***}	0.687^{***}
	(0.107)	(0.087)	(0.088)	(0.115)	(0.121)	(0.113)
Host GDP per capita	-0.065	-0.173	-0.185	0.218	-0.044	-0.145
	(0.173)	(0.165)	(0.187)	(0.225)	(0.167)	(0.169)
Host FDI (GDP %)	0.010	0.013^{+}	0.014^{+}	0.006	0.011	0.012
	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Host Trade (GDP %)	-0.105	-0.148	-0.168	-0.366	-0.210	-0.217
	(0.311)	(0.281)	(0.292)	(0.315)	(0.304)	(0.298)
Host Judiciary Indep.	3.640^{***}	3.205^{**}	2.453^{+}	2.930^{**}	3.685^{***}	4.367^{***}
	(1.084)	(1.066)	(1.374)	(1.084)	(1.036)	(1.115)
Host POLCON III	0.248	0.455	0.422	0.028	0.201	0.366
	(0.904)	(0.820)	(0.829)	(0.892)	(0.865)	(0.850)
Host Democracy	0.015	0.005	0.068	0.038	-0.050	-0.382
	(0.431)	(0.397)	(0.413)	(0.412)	(0.418)	(0.460)
Home GDP (log)	0.058^{+}	0.048	0.048	0.052^{+}	0.080^{*}	0.074^{*}
	(0.031)	(0.035)	(0.035)	(0.030)	(0.032)	(0.029)
Home GDP Growth (%)	-0.005	-0.002	-0.002	-0.002	-0.001	-0.003
	(0.021)	(0.022)	(0.022)	(0.020)	(0.023)	(0.021)
Home Judiciary Indep.	-0.398	-0.372	-0.382	-0.328	-0.253	-0.333
	(0.263)	(0.293)	(0.292)	(0.257)	(0.271)	(0.289)
Dyad BIT	0.065	0.078	0.064	0.092	0.189^{***}	0.166^{***}
	(0.073)	(0.070)	(0.070)	(0.072)	(0.049)	(0.049)
Dyad Common Language	0.748^{***}	0.791^{***}	0.787^{***}	0.707^{***}	0.651^{***}	0.657^{***}
	(0.101)	(0.098)	(0.098)	(0.100)	(0.045)	(0.045)
Dyad Colonial Relation	0.734^{***}	0.759^{***}	0.759^{***}	0.761^{***}	0.304^{***}	0.292^{***}
	(0.127)	(0.120)	(0.120)	(0.123)	(0.052)	(0.052)
Dyad Distance	-1.126^{***}	-1.241^{***}	-1.237^{***}	-1.061^{***}	-1.129^{***}	-1.069^{***}
	(0.096)	(0.090)	(0.090)	(0.093)	(0.059)	(0.058)
Firm Assets (log)	0.005	0.008	0.008	0.006	0.005	0.005
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Firm Age (log)	0.013	0.010	0.010	0.009	0.012	0.011
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Firm Host Countries (log)	1.287^{***}	1.274^{***}	1.274^{***}	1.277^{***}	1.271^{***}	1.270^{***}
	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Constant	-6.105^{***}	-6.193^{***}	-5.361^{**}	-5.984^{***}	-5.854^{***}	-5.655^{***}
	(0.589)	(0.258)	(1.642)	(0.628)	(0.585)	(0.619)
Random intercepts	Yes	Yes	Yes	Yes	Yes	Yes
N. of host countries	83	99	99	85	82	81
N. of home countries	56	56	56	56	55	54
Observations	285,295	340,554	340,554	291,945	280,767	275,705
Akaike Inf. Crit.	49,607.020	55,424.820	55,423.580	53,329.030	49,350.190	49,272.190
Note:						$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Table B.3: Firm-level data. Robustness tests of multilevel logit models

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

Table B.4: Firm-level data. The effect of the OECD Convention on probability of subsidiary incorporation. Multilevel logit models. Extended data

B.4 Placebo test: Sector-level analysis

I further investigate my argument moving to a sector-specific analysis, which also works as a placebo test. If my argument is correct, the mechanism should be observable only in industries where bribes are typically paid. In sectors where bribery is no typical custom, instead, anti-bribery policies should not enter firms' decision-making. I exploit information in the database from Escresa and Picci (2017) to test this implication. From this data I first obtain a list of industries with at least one reported case of cross-border bribery prosecution before 2005. I argue that these industries represent sectors where bribes are more often paid[29]. 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"), and one including the rest of the sectors in the sample ("placebo").

Table B.5 reports results obtained. For each subsample I replicate the model including no controls (only random effects) and all controls. Estimates of the coefficients associated with the interaction terms are consistent with the ones presented in Table 1 for the "test" subsample. Standard errors shrink in this subsample, resulting in estimates that are significant at smaller conventional levels. This indicates that coefficients are estimated with even more precision. They are never distinguishable from zero, instead, in the "placebo" subsample. This provides further confidence on my argument. The conditional effect of anti-bribery policies is observed only in industries where corruption is likely customary.

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

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

Table B.5: 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

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

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

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

C.2 Robustness tests with binning approach

C.2.1 Alternative binning choices

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

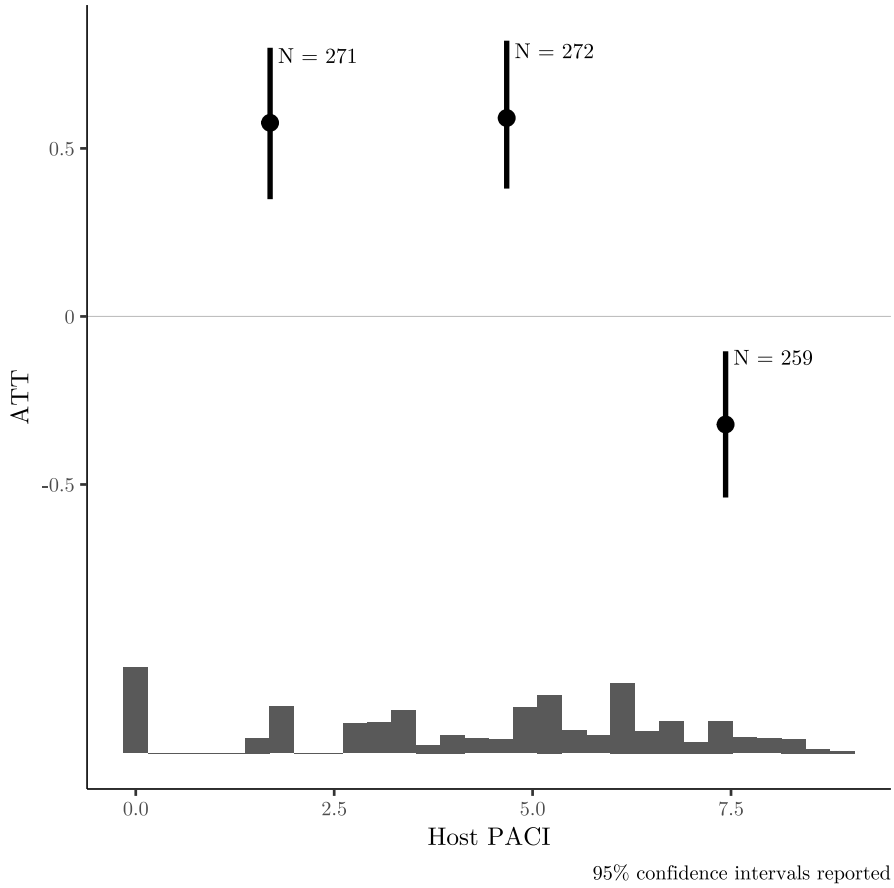


Figure C.1: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on tertiles of *Host PACI*

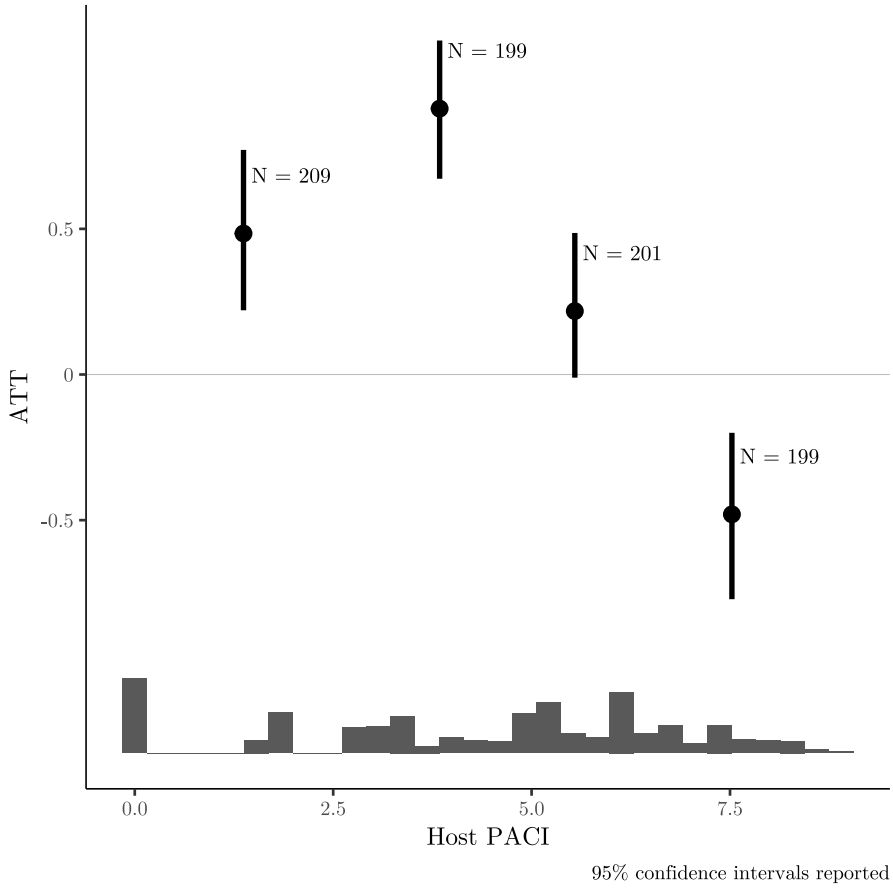


Figure C.2: Country-level data. Effect estimates from synthetic counterfactual designs. Binning based on quartiles of *Host PACI*

C.2.2 2FE design

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

A well-known problem emerges with 2FE when treatment timing varies between units. In that case the estimator produces wrong comparisons between groups at different times of their treatment (Imai and Kim, 2020). This is unfortunately the case with the OECD Convention. The problem is known to affect 2FE estimates particularly when the proportion of never-treated units is small, because of the weighting scheme implemented by 2FE (Goodman-Bacon, 2018). In my case, about half of the directed dyads were never treated[30] thus the problem appears less concerning. Nevertheless, I tackle the problem as follows. First, I estimate my 2FE models with staggered treatment and justify it based on the large share of never-treated dyads. Second, I address the potential issue of staggered treatment with a simple solution. I exploit the fact that for most economies the Convention entered into force either in 1999 or in 2001. I then exclude observations in the "buffer" years 1999–2001 and compare pre-1999 dyad-level investment flows to post-2001 observations[31]. Assuming the effect of the Convention on investment was not extinguished in the immediate short term, the method allows me to detect differences between the two periods and reduces the problem to a canonical 2-groups and 2-periods setup.

I estimate 2FE models using ordinary least squares (OLS). First, I estimate ATTs considering all observations, including those in the "buffer" years 1999–2001 when home countries ratified the Convention at staggered times. Figure C.3 reports estimates obtained in the five subsamples, their confidence intervals, number of observations in each bin, and the distribution of the moderator *Host PACI* variable. I first introduce only the *OECD Convention* variable and fixed effects (panel a). Next, I introduce all controls at the host country-, home country, and dyad-level (panel b). I interact covariates with year fixed effect to control for differential observable trends across dyads[32]. Panels c and d of Figure C.3 reproduce the same specifications of panels a and b, with the exclusion of observations in the "buffer" years 1999–2001. Standard errors are always clustered at the dyad level.

Estimates across the fives bins reproduce the inverted-U pattern seen in the synthetic counterfactual design in all panels. Entry into force of the Convention seems to have had no effect on investment for dyads in the first bin. The estimated effect is positive and statistically significant in all specifications of the second and third bin, with moderately corrupt host economies. Then, the effect declines and becomes negative for dyads with extremely corrupt host economies.

[30] 1733 dyads out of 3591 include a home country that did not ratify the Convention.

[31] 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.

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

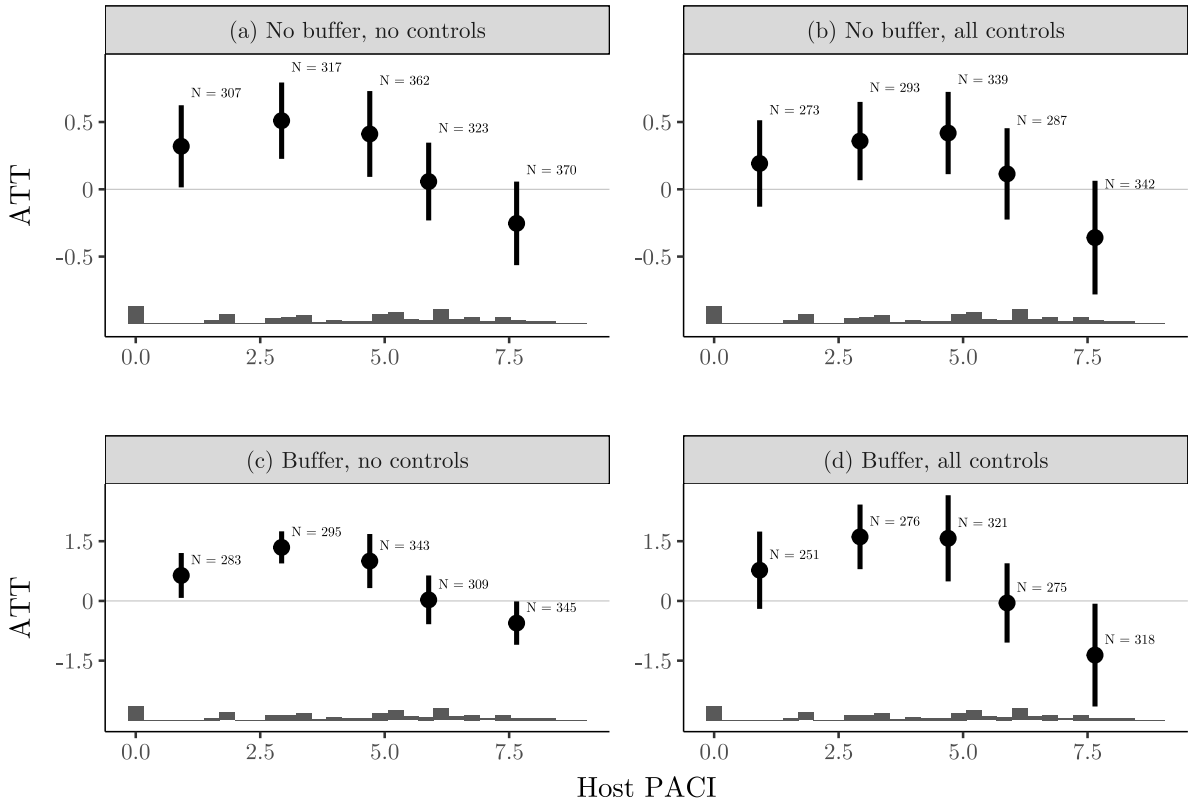


Figure C.3: Country-level data: 2FE binning estimator.

C.3 Robustness tests with interactive models

As a last robustness test, I re-estimate my 2FE models but do not employ a binning approach. I test my conditional argument fitting an interactive model. I interact my binary variable of interest *OECD Convention* with the linear and squared measure of *Host PACI*. Results are reported in table C.2 and are overall consistent with my expectation. I find the estimate of coefficient β_1 to be negative and statistically significant. The estimate of coefficient β_2 is positive but 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 C.3.

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.3 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.4 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 ×	-0.015^{*}	-0.015^{+}	-0.013^{+}	-0.013^{+}
Host PACI^2	(0.007)	(0.007)	(0.007)	(0.007)
OECD Convention ×	0.050	0.059	0.050	0.050
Host PACI	(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	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
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, *** p $<$ 0.001			

Table C.2: 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, *** p < 0.001

Table C.3: Dyadic country-level data. Multilevel models

Table C.4: 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:	$+ \text{ p} < 0.1$, * p < 0.05, ** p < 0.01, *** p < 0.001			