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

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Abstract

Countries prosecute domestically-incorporated firms for foreign crime. By threatening prosecution at home, states conscript companies to diffuse regulated business models abroad. These policies are criticized for deterring investment into countries with lax regulatory standards, where financial crime is more likely. Yet, cleaner business models should also cut informal costs and favor investment. I reconcile these opposite expectations and show they are special cases of a single argument. I study the impact of anti-bribery policies on firms' cross-border investment. I claim that their effect depends on the level of corruption of the host economy. It is null in clean countries. It is positive where corruption is moderate: here laws provide a legal leverage to refuse paying bribes. Where corruption is endemic, the effect is negative: anti-bribery laws expose firms to additional regulatory costs. Multilevel models test the argument studying the 1997 OECD Anti-Bribery Convention. I study investment from 3871 firms between 2006 and 2011. Regulated companies 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-dyad investment flows corroborates this finding. Thus, corporate regulation can favor investment, but in economies with weakest regulatory standards.

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

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Introduction

Firms can commit economic crimes when they invest into foreign economies. A company can bribe in a foreign market to circumvent competition and derive rents (Malesky et al., 2015). Or, it can extract natural resources from protected areas, perhaps even employing prohibited pollutants. These felonies can then be linked to yet other nefarious transactions, like laundering of illicitly obtained money (Findley et al., 2015). Examples of corporate crime also include fraud, human or labor rights violations, tax evasion, and financing of transnational terrorism.

Home states have adopted corporate criminal regulations to prosecute companies for such misconduct in foreign markets. This strategy can be seen as a form of policy diffusion (Simmons and Elkins, 2004) that substitutes for lax regulatory standards in host jurisdictions. By threatening legal repercussions at home, countries effectively conscript domestically-incorporated companies to diffuse cleaner behaviors abroad. This regulatory strategy was shown to be effective (Jensen and Malesky, 2018).

These policies are often blamed for a potential side effect. They would raise additional costs for companies, increasing risk of investing in host countries where economic crime is common in the conduct of business. This would deter investment towards these destinations and favor unregulated competitors. Yet, an opposite hypothesis can be advanced alongside this *deterrence* argument. Corporate policies enforced by the home country can also *empower* firms abroad. They do not only impose legal costs. They also assist companies in devising more efficient business models in host countries where informal transactions raise costs of doing business (Davis, 2002). According to this argument, regulated companies are offered a legal leverage to operate more efficiently and cut down costs induced by uncertainty of criminal business practices. This prerogative is, instead, precluded to unregulated competitors, who then operate at a disadvantage. Regulated companies should therefore be incentivized to invest into foreign 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 can be seen as special cases of a single general dynamic. I study home country regulations that prosecute foreign bribe payments by multinational corporations (MNCs). Anti-bribery policies are often criticized for deterring investment into corrupt countries (Gutterman, 2015). Yet, they also help companies cutting down informal costs of corruption (Perlman and Sykes, 2017).

My argument unifies the *deterrence* and *empowerment* hypotheses by claiming that the two opposed mechanisms are simultaneously at play. I claim that the direction of the resulting effect depends on the *level* of corruption of the host economy. Anti-bribery policies provide firms with a legal leverage which is strong enough to refuse bribe requests from public officials only where their bargaining power is relatively low. 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 negatively affect investments only in *extremely* corrupt economies, where public officials' bargaining power is larger. Policies will favor investments in

moderately corrupt host economies, instead. I expect the effect to be null in non-corrupt countries.

Empirically, I study laws under the 1997 Organization of Economic Cooperation and Development (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 mildly 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 an event analysis framework. I use a generalized synthetic control design to achieve a more robust identification of the proposed effect. Results confirm findings from the firm-level analysis. Overall, empirics show that home anti-bribery policies affect firms' investment into corrupt economies, but not necessarily in a negative way. Concerns on the anti-business nature of these laws should be taken with skepticism.

Implications travel beyond the anti-bribery regime. The paper speaks to a literature in international political economy that has studied the diffusion of corporate policies across borders, addressing their determinants (Putnam, 2009), effects on other states (Kaczmarek and Newman, 2011), and on private companies (Findley et al., 2015; Jensen and Malesky, 2018; Kalyanpur and Newman, 2019).

To the best of my knowledge, this paper represents the first attempt to reconcile two radically opposed expectations on the effects of regulation for businesses. I show that home countries can leverage firms to diffuse economic regulation across borders, by threatening legal domestic repercussion for foreign wrongdoing, without necessarily undermining their economic activity. These are good news for their home states. Yet, my conclusions are bad news for host countries with *extremely* weak regulatory standards, where diffusion of better business practices would be perhaps most needed. Here the strategy backfires. These host countries are left exposed to investments from unregulated companies, who can arguably commit corporate felonies while remaining unpunished. This pessimistic conclusion adds to existing findings on the perverse effects of corporate policies induced by the existence of different standards among competitors (Brazys and Kotsadam, 2020; Chapman et al., 2020).

The study also speaks to an important question in international political economy: whether, and to what extent, international institutions can affect behaviors of private transnational companies (Gray, 2009). A vast scholarship has addressed this question studying the effect of international law regulating licit transactions on foreign investment. Typical examples include institutions creating fora for arbitration

[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).

in investment disputes or those protecting investors' rights (Allee and Peinhardt, 2011; Neumayer and Spess, 2005; Skovgaard Poulsen, 2014). I address the same question from a different angle. I study what effect international *criminal* laws, and international institutions aimed at keeping economic exchanges above board, have on private cross-border investment, if any.

1 FDI, corruption, and regulation

A foreign direct investment (FDI) is a long-term initiative with productive goals. A company in the headquarter or home country – called "parent"– projects its presence abroad by establishing ownership[2] of a firm in the host country – called "subsidiary" (Jensen et al., 2012). The transaction rests on a cost-benefit evaluation. Firms go multinational if advantages in ownership, location, or internalization terms[3] outweigh costs (Dunning, 1980) and political risk (Jensen, 2008). The resulting investment can have positive effects in the host country in terms of job creation, technology spillovers, economic growth (Borensztein et al., 1998), and democratization (Eichengreen and Leblang, 2008). However, investment can also have adverse effects for the host country. Firms often resort to bribery[4] as a strategy to enter foreign markets (Søreide, 2006). Bribe payments are documented in registration of an MNC and procurement in corrupt economies (Gueorguiev and Malesky, 2012).

Corruption does not have a univocal effect on the investment cost-benefit calculus. On the one hand, it reduces the probability that an investment will take place (Barassi and Zhou, 2012) because it increases its costs, like a tax (Treisman, 2007; Wei, 2000). Corrupt contracts are uncertain and inefficient, since they lack systems to be enforced (Lambsdorff, 2002; Rose-Ackerman, 1975). On the other hand, bribery can be a profitable strategy to crowd out competitors and establish oligopolies from which firms can extract large rents (Zhu, 2017). MNCs from developed countries active in less-advanced economies often adopt this strategy (Pinto and Zhu, 2016). Bribery is used in extractive industries, where the existence of natural barriers facilitates market exclusion (Knutsen et al., 2017), but also in markets artificially restricted by institutions (Malesky et al., 2015).

Home states have adopted corporate policies to intervene in this calculus and univocally increase costs of corruption to firms (Cuervo-Cazurra, 2008). The US unilaterally[5] passed a legislation to prohibit foreign bribery in 1977: the Foreign Corrupt Practices Act (FCPA). Similar policies were then adopted

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

[4] 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).

[5] The US was the first country in the world to adopt a similar regulation, pushed by the immediate post-Watergate outrage.

under the 1997 OECD Convention[6]. Ratifier home states scrutinize and prosecute bribery perpetrated beyond national borders by their companies, foreign employees, or entities they own abroad[7] (Brewster, 2017). They effectively conscript domestically-incorporated firms to diffuse anti-bribery standards abroad, under the threat of fines and judiciary consequences at home for foreign corrupt payments[8].

How do companies respond to such policies? Academic answers are divided in two camps. I label the argument from the first one *deterrence*. Its proponents expect firms subject to anti-bribery standards will invest less in corrupt economies, where they are imposed additional legal anti-corruption costs. The expectation finds empirical support (Cuervo-Cazurra, 2008; Hines, 1995). The second camp expects the opposite effect. I label its argument *empowerment*. It expects anti-corruption policies will favor investment of regulated companies into corrupt economies. Regulations reduce corruption-induced uncertainty and ensure cleaner business models without off-the-record expenditures (Lambsdorff, 2002), which should make regulated companies operate at an advantage (Davis, 2002; Perlman and Sykes, 2017).

These two camps currently lack unification. Empirical studies on the matter have not managed to settle the controversy. They all impose the conditional effect of anti-bribery policies is linear in corruption, a choice which prevents from studying complex ways in which the two opposite pulls interact (Hainmueller et al., 2019). Some studies conclude no effect of anti-bribery policies on investment can be detected (Hakkala et al., 2008; Smarzynska and Wei, 2000; Wei, 2000).

The controversy reaches beyond the evaluation of anti-bribery policies. Political economy hotly debates companies' response to corporate policies. Some argue regulations intrude into companies' activity, adding unnecessary costs and frictions that distort incentives and cause inefficiencies (Baily et al., 1993; Bruno and Claessens, 2010; Wintoki, 2007). Others point out that regulated companies gain advantageous positions over their competitors (Genovese, 2020; Kennard, 2020), as indicated by the fact that they often lobby for regulations and voluntarily participate to regulatory efforts (Ahlquist and Mosley, 2021; Thrall, 2021).

I claim that the case of anti-corruption policies offers a chance to rejoin the two camps. Next section advances a single general argument that justifies both claims, by specifying conditions for each of them. I argue that the level of corruption of the host country plays a moderator role. The *deterrence* claim will be justified as a special case relative to countries with extreme levels of corruption. The *empowerment* expectation, instead, will apply to mildly corrupt host economies.

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

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

[8]The strategy of conscripting domestically-incorporated firms to diffuse policies abroad is also adopted to prevent money laundering (Sharman, 2011) or financing of terrorism (Findley et al., 2014). Similar efforts have also been recently initiated at the OECD level to curb a phenomenon known as "base erosion and profit shifting" (BEPS) by corporate tax payers. Applications of domestic laws against foreign misbehavior are also used to fight violations of human rights.

2 The conditional effect of anti-bribery laws on investment

Imagine two identical firms faced a decision to invest in a foreign economy. An investment is followed by competition for securing public contracts in the host country. The company offering the cheaper product or service wins the competition. Imagine companies only differed in their country of origin. The first company is British. Because the UK is a ratifier of the OECD Convention, the British company is subject to anti-corruption provisions. The second company is Indian. India is not a ratifier, therefore the company is subject to no anti-bribery rules from its home economy. A firm decides to invest only if expected profits outweigh expected costs. Suppose the investment entailed the same cost to both enterprises. Is the regulated company more or less likely to invest than the unregulated one? In this section I provide an answer that unifies the *deterrence* and the *empowerment* arguments. I claim that regulation affects the investment probability differently in countries with different levels of corruption.

I distinguish three investment scenarios based on the level of corruption of the host economy. First, a scenario where investors consider the host economy to be extremely corrupt. For instance, the business community typically regards Nigeria as a country where corruption is endemic and corrupt fees are expected for the conduct of business[9]. In similar economies bribery is often an expected business custom and sometimes even a necessary condition to entry (Zhu, 2017). In such a scenario, both the British and the Indian company include potential bribery costs in their expectation. They know an investment in the country would make them interact with corrupt local public officials. Competition for the provision of public contracts will likely entail demands for bribes, that increase inefficiency and costs to both companies (Beck and Maher, 1986). In such a scenario, it is unlikely that either company will manage to refuse paying such bribes (Ades and Di Tella, 1999; Svensson, 2003).

In case bribes were paid, however, the British company would face additional regulatory costs not incurred by its Indian competitor. Home country judicial authorities apply anti-bribery provisions and regularly levy blockbuster fines from guilty companies under their jurisdiction[10]. Monetary disbursements are also not limited to penalties. Authorities often mandate costly re-structure of corporate organization and culture to ensure future compliance with anti-bribery standards[11] (Garrett, 2011). Financial markets also impose reputational costs for corporate crime (Sampath et al., 2018). When discovered, foreign bribery turns into scandals with international resonance that make the first page of newspapers. Markets react to these stories. It is estimated that on average 80% of every lost dollar in a firm's share value,

[9] See: https://www.pwc.com/ng/en/press-room/impact-of-corruption-on-nigeria-s-economy.html (last accessed on January the 11th, 2022).

[10] Penalties have increased consistently over the years, to reach records in the order of billions of US dollars in recent judiciary cases. For a top-ten of disbursements under the US FCPA see: https://fcpablog.com/2020/02/03/airbus-shatters-the-fcpa-top-ten/ (last accessed on July the 28th, 2021).

[11] Terms often include turning executive offices inside out; setting up systems of internal investigations; having third-parties monitoring activities of a firm for a probation period; and periodically rotating international offices to avoid managers established personal connections with local authorities. For a textbook example, see the drastic changes implemented by Siemens AG after an infamous worldwide bribery scandal: https://www.complianceweek.com/how-siemens-worked-to-fix-a-culture-of-institutionalized-corruption/14915.article (last accessed on July the 28th, 2021).

following bribery scandals, comes from market-imposed penalties (Karpoff et al., 2008).

The British company is therefore likely to expect an uneven playing field with its Indian competitor in the high-corruption scenario. More generally, regulated investors expect higher costs than their unregulated competitors in extremely corrupt economies. They know they will operate at a disadvantage and deem the investment less profitable. They are therefore less likely to invest in such a scenario than their unregulated competitors, all else equal. Here, *deterrence* prevails.

A second scenario is represented by a host economy that is considered to be only moderately corrupt. This scenario entails costs for the fictitious British and Indian companies that differ from those in the first one. 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[12]. However, instances of corruption in public procurement exist[13]. Bribery here is unlikely to be a necessary business custom and public officials' room to expect bribes is limited.

In a similar context, firms subject to anti-bribery standards find they can leverage these rules to enhance their bargaining power *vis-à-vis* local public officials (Hakkala et al., 2008; Kaufmann and Wei, 1999). Anti-bribery policies here work as a hand-tying mechanism, forcing regulated companies to operate more efficient models (Davis, 2002). Perlman and Sykes (2017) collect extensive qualitative evidence of this mechanism through US Congressional hearings with MNCs' Chief Executive Officers (CEOs) and interviews with corporate and legal practitioners. They find that public officials in similar settings are more likely to switch their bribe requests to *unregulated* companies, because they know regulated firms have a legal leverage to refuse such requests. This qualitative insight is backed by large-N evidence (Brazys and Kotsadam, 2020). Perlman and Sykes (2017) even point out that corporate requests for tighter anti-bribery regulation can be explained as a desire to enhance bargaining position *vis-à-vis* foreign public officials, a point also made by others (Gutterman, 2015). Regulated companies can thus cut corruption-induced costs: bribe fees, uncertainty, and transaction costs (Rose-Ackerman, 1975). This possibility is precluded to their unregulated competitors, who operate at higher corruption-induced costs, therefore at a disadvantage.

The British company therefore expects more favorable conditions than its Indian counterpart in a moderately corrupt scenario. More generally, regulated firms are more likely to invest in moderately corrupt economies than their unregulated competitors because they can cut down costs faced by their competitors. *Empowerment* prevails in such a scenario.

Finally, a third scenario is represented by a non-corrupt host economy. An example is Denmark, regularly placed at the top of charts for perception of corruption from the business community[14]. Here,

[12] See: https://www.transparency.org/en/countries/taiwan (last accessed on January the 12th, 2022).

[13] 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).

[14] See: https://www.ganintegrity.com/portal/country-profiles/denmark/ (last accessed on January the 12th, 2022).

anti-bribery policies should neither advantage nor disadvantage firms, as it is unlikely that corruption fees are expected at all. The regulated and the unregulated companies should therefore operate at the same cost, all else equal. No company should be more likely than the other to invest in such a scenario.

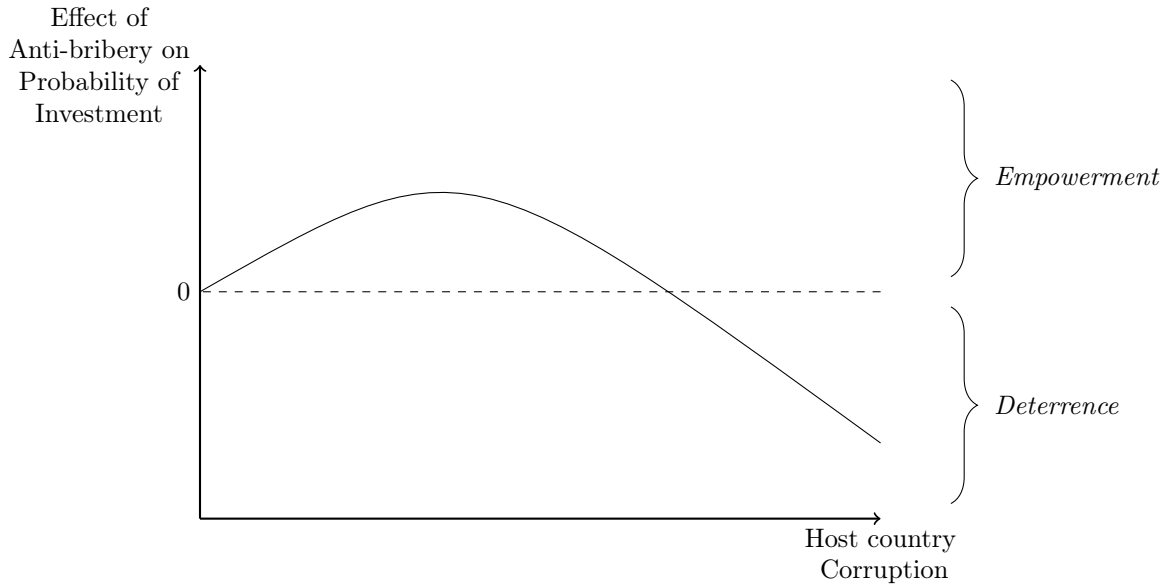


Figure 1: Expected effect of anti-bribery laws on investments, conditional on host country corruption

Figure 1 generalizes my expectation beyond these three dichotomous 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 then 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.

3 Empirical analysis

I claim that anti-bribery policies affect foreign investment decisions by companies conditional on the level of corruption of host economies, in the non-monotonic manner sketched by Figure 1. 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 show expectations from the theory and results from the firm-level study are confirmed.

3.1 Firm-level data

I estimate a firm-level model of investment decisions conditional on corruption (Barassi and Zhou, 2012; Hakkala et al., 2008; Smarzynska and Wei, 2000). A firm f from country i is observed to invest in country j ($I_{fij} = 1$) only if the value of a latent variable I^*_{fij}, representing its propensity to invest, is greater than 0. Equation 1 expresses the latent variable I^*_{fij}. It is a function of whether country i is a 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 (S_i) on the propensity to invest abroad (I^*_{fij}) depends on the level of corruption of the host country (C_j) in a non-linear manner, as in Figure 1. Matrix $\mathbf{X_{fij}}$ includes covariates explaining I^*_{fij} 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. Therefore parameter β_1 is expected to be negative, β_2 positive, and β_3 null.

I estimate Model 1 with firm-level data drawn from the Orbis Corporate Ownership Database[15], retrieved from Beazer and Blake (2018). This dataset reports information on foreign subsidiary incorporation from 3871 individual parent firms between 2006 and 2011. It reports the country of origin of the parent firm (home country) and that of the subsidiary (host country) for each incorporation. Represented home economies are 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[16]. These selections ensure the sample represents a population composed of large MNCs,

[15] 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.

[16] 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

and investments represent long-term foreign productive enterprises and not speculative ventures[17]. Orbis data have a two-year lag between the moment firms' information is disclosed and the moment it is reported in the data, and present various problems when year-specific information is used to obtain time-series (Kalemli-Ozcan et al., 2015). Both issues are avoided here employing a cross-section of observations between 2006 and 2011. Figure 2 breaks down firms in the resulting database according to their North American Industry Classification System (NAICS)-2 industrial code. The majority of firms are active in Manufacturing (43%), followed by Management of Companies and Enterprises (17.7%) and Professional, Scientific and Technical Services (8.89%).

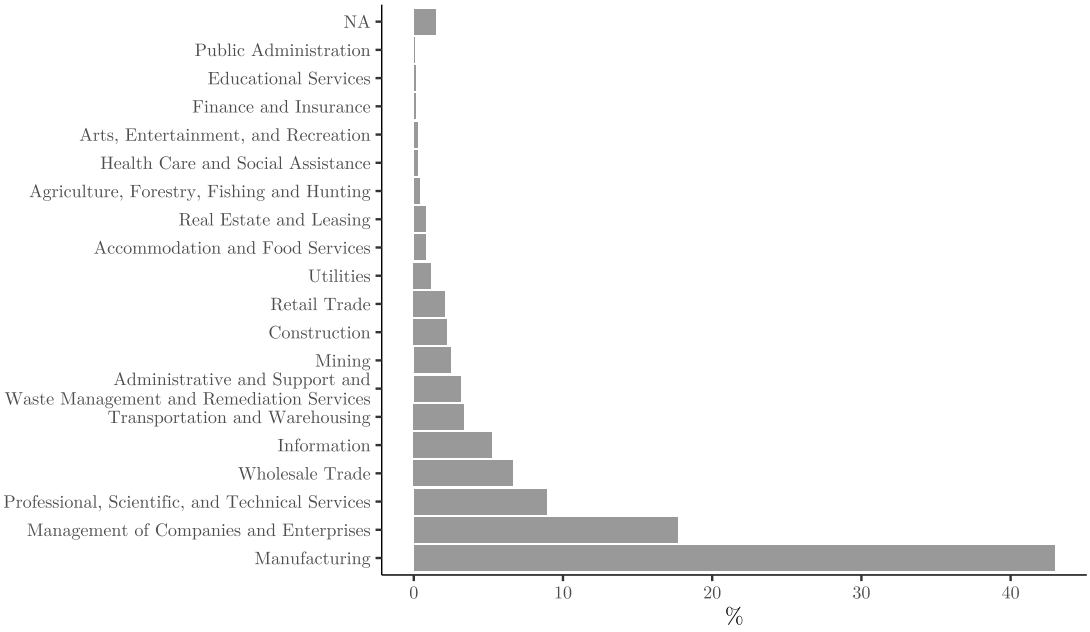


Figure 2: Firm-level database description: Percentage of firms in the database by NAICS-2 code

Three reasons make this dataset an optimal source to test my expectation at the level of the actors who face investment decisions. 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. Third, information spans only until 2011. Enforcement of the OECD Convention has improved after 2010 (Jensen and Malesky, 2018). Using information that extends only one year after that turning point allows me to test if there is any effect even at a time when enforcement was arguably still lax, thus representing a hard

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

[17]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.

test for my theory[18].

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 measure does not represent the size of an investment, but this is consistent with my argument predicting its probability. The binary dependent variable has a dyadic form. For each parent company f from country i it is assigned a 1 if the firm is reported to have set up a subsidiary in the host country j in the time period of interest. It is assigned a 0, instead, if no subsidiary was established in the (potential) host country j[19]. Potential host countries are all economies where a subsidiary has been established by at least one firm in the dataset. This is supposed to represent all attractive host countries.

My main explanatory variable is *OECD Ratifier*. It represents whether the home country i of a parent firm f has ratified the OECD Anti-Bribery Convention by 2005[20]. The variable is binary: it takes value 1 if this condition is met, 0 otherwise.

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

An increasingly popular alternative is represented by so-called "objective" measures, that rely upon observable information. These measures have the obvious downside that observed cases of corruption are no good measure of corruption, since when it is most effective it takes place out of sight. The Public Administration Corruption Index (PACI) advanced by Escresa and Picci (2017) proposes a solution. Intuitively, the index compares the *observed* number of cross-border cases of bribery with those that could be *expected* if countries were all equally corrupted, based upon commercial ties. It then measures corruption of a country as the deviation between observed and expected corruption cases. The PACI is suited to measure cross-border bribery as I define it (see Footnote 4). I adopt the index and discuss it in Appendix. However, in a robustness test I substitute this index with traditional perception-based

[18] In a robustness test I then extend data on companies from Beazer and Blake (2018) to information until 2018. Results hold, see Table B.4.

[19] 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.

[20] 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).

measures, choosing among the most reliable ones, and verify that my results hold[21].

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

I follow Beazer and Blake (2018) and explain my binary outcome variable employing a multilevel logit model[22]. This is a forced choice to correctly specify cross-level interaction effects (Bell and Jones, 2015) like those implied by Model 1. This model choice is also suited to the dataset structure, where a firm investing abroad is cross-nested in a directed dyad, and in its home and host countries. Multilevel unobserved heterogeneity in this complex nesting can easily confound the explanation of the outcome variable, therefore it must be properly modelled. To this aim, all specifications include random intercepts at the dyad-level, and at the level of home and host countries. A further specification also includes industry-level intercepts to account for sector-specific heterogeneity. Since no clear hierarchy can be discerned in the data structure, I employ a cross-classified random effect model. A multilevel 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 to zero and produce unreliable tests of hypotheses.

Finally, I include a series of covariates to control for potential confounders and increase precision of my estimates. I consider the 2005 value for all. First, I control for economic and institutional features of the host country: its (logged) Gross Domestic Product (GDP), per capita GDP, 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).

[21] See Table B.3 in Appendix.

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

Summary statistics are reported in Appendix[23].

3.1.1 Results

Table 1 presents my results relative to the variables of interest only[24]. All models include random effects at the dyad, home and host country level. 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).

	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

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).

[23] 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.

[24] Full disclosure of all estimates is provided in Table B.2.

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.

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

Interpretation of results is particularly complex in multiplicative models (Brambor et al., 2006). I compute the marginal effect of anti-bribery policies at given levels of corruption to evaluate if the argument represented by Figure 1 is supported. 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 clean host economies where data support is limited. When *OECD Ratifier* changes from 0 to 1, the predicted probability that a firm will incorporate a subsidiary changes conditionally on the level of corruption of the host economy, in a non-monotonic way.

The effect can be roughly divided in panels (a), (b), and (c). Figure 3 reports the number of host countries 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, which have a baseline investment probability of 0.008. 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

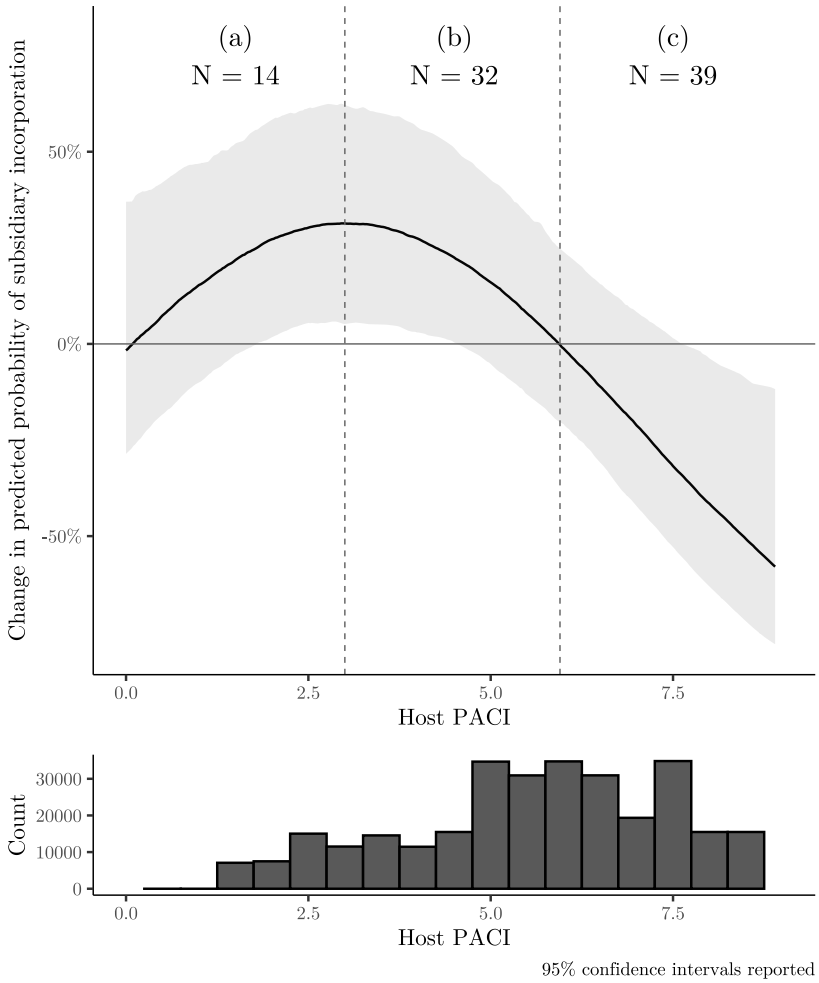


Figure 3: The non-linear effect of *OECD Ratifier* on *Subsidiary*, conditional on *Host PACI*

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. Baseline probability of investment for unregulated companies in this interval is 0.003.

I propose extensive tests to show robustness of these results in Appendix (Table B.3). 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. I finally exclude firms from countries that ratified the Convention in the 2006-2011 cross-section. 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 expected. I find no effect in sectors where bribery is not customary. I leverage data from Escresa and Picci (2017) to distinguish industries where at least one case of corruption is reported between 1997 and 2005.

One final concern with results in Table 1 relates to the time-period data from Beazer and Blake (2018) cover. As claimed above, data spanning until 2011 fit my theoretical needs: they allow me to perform a hard test where the effect of anti-bribery regulations is studied in a period of arguably lax enforcement. Yet, skeptical readers can worry that the Convention did not have any real deterrent effect on companies' investment before 2010 (Jensen and Malesky, 2018). Using information until 2011 might thus downplay the effect of the Convention. I extend the available information for companies in the dataset from Beazer and Blake (2018). I follow the same procedure adopted by the authors to get information on subsidiaries incorporated by these same companies until 2018. I then replicate models presented in Table 1 on this extended dataset. Estimates are consistent with previous findings (Table B.4).

3.2 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 distinguish if investment behavior changed after ratification of the Convention. Second, selection under OECD policies is not random. Firms under OECD policies have very specific characteristics that distinguish them from those who are not subject to such policies. The previous analysis cannot disentangle these characteristics from the effect of anti-bribery policies themselves. To name one of such potential sources of endogeneity, economies adopting the OECD Convention generally belong to the

Global North and corrupt host economies tend to be concentrated in the Global South. Do results in Table 1 represent the deterrent effect of institutions in these host countries for companies headquartered in the Global North (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. 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 data 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 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[25]. The five subsamples are defined based on quintiles[26] of the *Host PACI* distribution. 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

[25]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.

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

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 identifying assumption is that trends in FDI flows of the treatment and the control group would have been parallel, after the treatment, had the treatment not taken place.

The assumption is non-testable, as it refers to unobservable post-treatment untreated outcomes for the treatment group. However, here a glance at trends in FDI flows in the five bins warrants skepticism on its plausibility. Figure 4 reports average logarithmic dyadic FDI flows for the treatment and control group in each bin, and indicates the period countries ratified the Convention (1999–2001) with a shaded area. Pre-treatment trends seem to diverge for most of the bins. A second concern here has to do with the timing of ratification. Countries ratified the OECD Convention at staggered times, thus a two-way fixed-effect (2FE) estimation strategy would have to be adopted. However, a recent literature points out the estimator can produce wrong comparisons between groups at different times of their treatment in this kind of staggered-treatment situation, if heterogeneity in effect is in place (Goodman-Bacon, 2018). Estimators that address this issue and do not assume homogeneous effects usually require balanced panel datasets, whereas mine is unbalanced.

Given that trends are likely not parallel and treatment timing is staggered, 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[27].

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

[27]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.

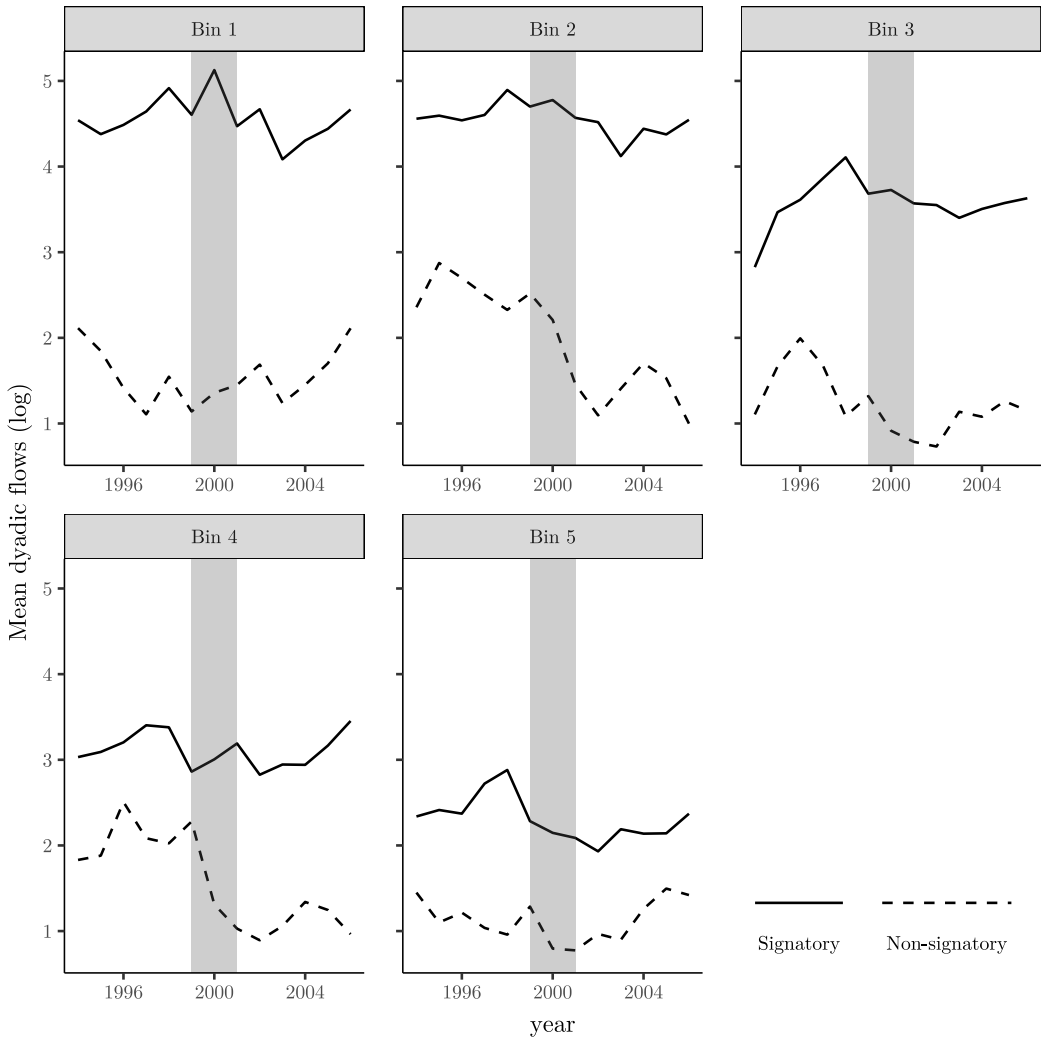


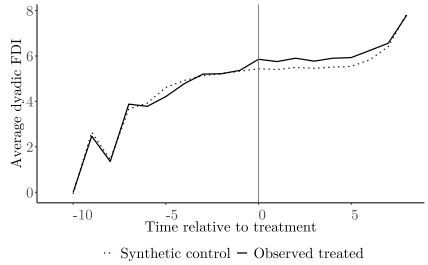
Figure 4: Country-level data: Trends of the dependent variable in treatment and control group.

dyads, and show my results do not hinge on my synthetic estimation strategy and its data selection.

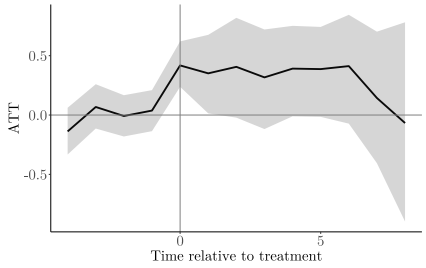
3.2.1 Results

Figure 5 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 no significant 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).

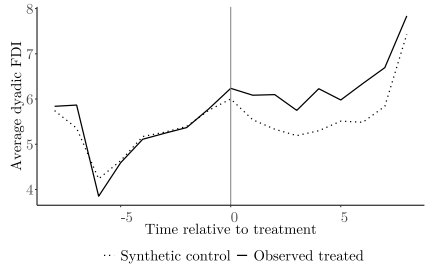
Figure 6 reports the average effect over the entire time period for each of the five bins. It also reports



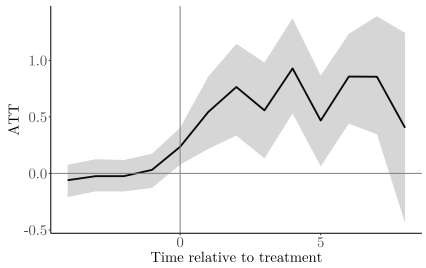
(a) Bin 1: Average dyadic flows



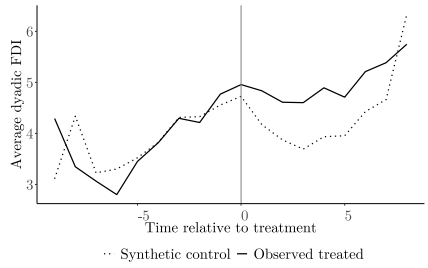
(b) Bin 1: Estimated ATT



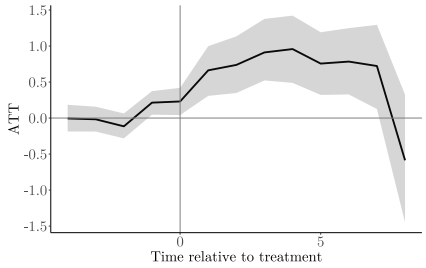
(c) Bin 2: Average dyadic flows



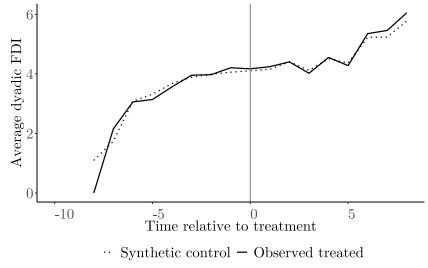
(d) Bin 2: Estimated ATT



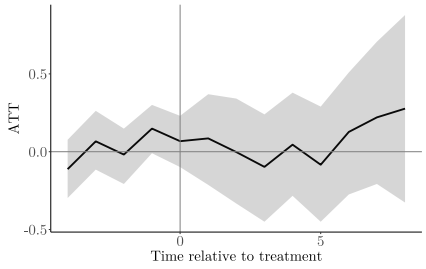
(e) Bin 3: Average dyadic flows



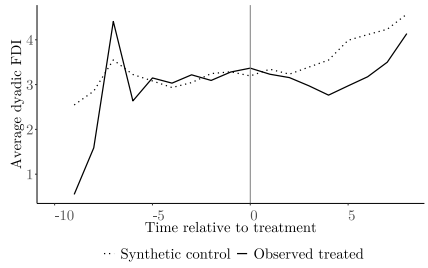
(f) Bin 3: Estimated ATT



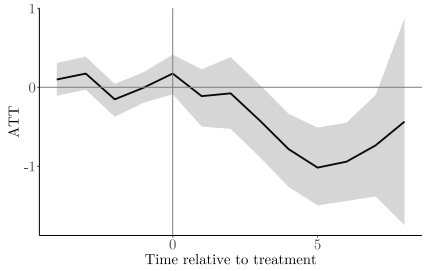
(g) Bin 4: Average dyadic flows



(h) Bin 4: Estimated ATT



(i) Bin 5: Average dyadic flows



(j) Bin 5: Estimated ATT

Figure 5: Country-level data. Generalized synthetic control method. Average trends and estimated ATT in the five bins

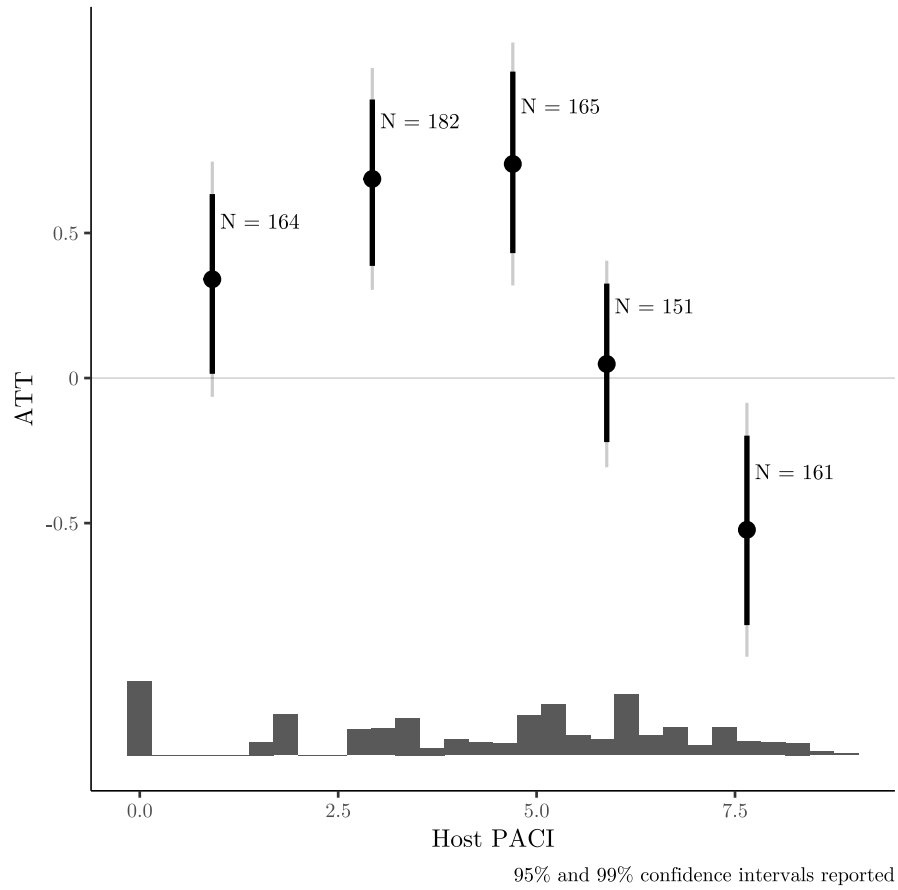


Figure 6: Country-level data. Effect estimates from synthetic counterfactual designs

the distribution of the *Host PACI* variable and the number of dyads in each bin. Estimates across the fives bins reproduce the inverted-U pattern seen in the firm-level analysis. For dyads in the first bin (those with host countries like Australia, Canada, Denmark, and Sweden) entry into force of the Convention seems to have had a small to no-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 for dyads with extremely corrupt host economies including Bulgaria, 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.

I test robustness of these estimates in Appendix by adopting a 2FE design (Figure C.1). The design uses all dyads, therefore ensuring results from Figure 6 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[28].

4 Discussion

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

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

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

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

[28] I also try more 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. Results are not reported to keep the Appendix compact but are available upon request.

an individual firm-level theory. Their estimates, yet, appear more credible in light of the micro-level evidence provided by the first exercise.

One important limitation of the results concern their generalizability. In both studies, the estimated causal quantity is an ATT. Under the assumptions listed above, this estimate informs us about the effect of the Convention on FDI *for countries that ratified it*. It cannot tell us anything about what would happen to the outward FDI of, say, India if the country were to join the Convention. Selection into the treatment group is not random: countries self-select into it. The ATT is therefore not representative of the effect that untreated units would experience in the event of a treatment. It might as well be possible that ratifier countries chose to sign the Convention expecting the effect I document on their companies. The study simply clarifies that the effect of the Convention on regulated companies has not been univocally negative, as implied by *deterrence*, nor univocally positive as *empowerment* claims. By no means it can project what the effect would look like for a new ratifier of the OECD Convention.

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

This mechanism is not tested by the present analysis. Previous qualitative studies lend plausibility to it: Perlman and Sykes (2017) conduct interviews and archival research to document how the US anti-bribery policy altered the bargain between investors and foreign public officials. It is plausible that a similar mechanism is in place for OECD anti-bribery policies as well. However, it might as well be possible that anti-bribery policies affect investments because they change the behavior of public officials too. The overall observed effect might be the compounded result of these different mechanisms. The study cannot disentangle them. 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.

This leaves a door open for future qualitative studies to complement my analysis. They could investigate what drives the identified effects. For example, documenting negotiations of firms with foreign public officials in typically corrupt industries might enlighten this open question. This decision-making process could be studied to assess if the explanation provided here is appropriate, and to what ex-

tent competing mechanisms can be advanced, instead. Until then, the quest remains open on which mechanism ultimately explains the findings presented here.

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

Net of its limitations, the study makes valuable contributions. It provides insights into the effects of efforts to regulate the global political economy. It shows that home country corporate policies can diffuse better standards of economic activity abroad without necessarily penalizing firms in economies with weak regulatory standards.

5 Conclusion

What is the effect of corporate policies imposed by home countries to regulate economic activity abroad, on subject companies' investments? It is often argued that home states' strategy of conscripting domestically-incorporated companies to diffuse standards of economic behavior abroad disadvantages firms in countries where economic crime is common, *vis-à-vis* unregulated competitors. Such regulatory efforts would jeopardize investments. Yet, regulated business models should also be more efficient and favor companies in countries where informal costs are often expected. In this article I study the effect of home countries' corporate regulations on their companies' foreign investments, in the case of anti-bribery policies.

I first detail the two competing arguments about the effect of anti-bribery policies on foreign investment. 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.

I test my argument in two empirical exercises focusing 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 study offers insights beyond anti-bribery policies. It contributes to a literature on international regulatory regimes that assessed the effect of international institutions on transnational economic activity. I study international regimes regulating nefarious flows and keeping business above board. I focus on the side effects of home states' regulatory strategy to threaten domestic repercussion for foreign corporate misconduct. I show that this approach to policy diffusion does not penalize firms' foreign economic activity in all countries with lax regulatory standards. Rather, the cleaner business models that home countries diffuse abroad with their corporate policies can facilitate companies in a range of countries.

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. These economies are on average more likely to be abandoned by regulated firms and left exposed to companies from countries that do not impose similar rules. To the extent that such unregulated firms can violate these standards, this could lead to a further decline of records 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).

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Appendix

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

A The *Host PACI* measure

In this section I present the Public Administration Corruption Index (PACI), proposed by Escresa and Picci (2017) and adopted in this study. The PACI relies on the following intuition: suppose all countries were equally corrupt. Then the number of observed cases of cross-border bribery occurring in a country should be proportional to its economic inflows: 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[29]. A second important assumption that needs to hold is that the number of cross-border transactions is proportional to bilateral trade flows (as opposed to other economic flows like FDI). Escresa and Picci (2017) argue that many transactions are not reflected in FDI flows or stocks, and that investments eventually enable trade flows between countries. Thus, they argue, trade flows are a good proxy of economic flows between pairs of countries.

B Firm-level analysis

B.1 Descriptive statistics

Table B.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)[30]. 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).

[29] 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).

[30] 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.

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	0.000	0.000	1.000	1.000
Host POLCON III	383,244	0.311	0.198	0.000	0.127	0.468	0.692
Dyad Distance (km)	386,206	0.656	0.422	0.006	0.261	0.948	1.995
Dyad Common Language	386,206	0.113	0.316	0.000	0.000	0.000	1.000
Dyad Colonial Relation	386,206	0.051	0.219	0.000	0.000	0.000	1.000
Dyad BIT	406,454	0.376	0.484	0	0	1	1
Firm Age (log)	400,154	3.312	0.948	0.000	2.639	4.060	5.897
Firm Assets (log)	379,363	13.875	2.115	4.025	12.380	15.328	20.181
Firm Host Countries (log)	406,454	0.678	0.721	0.000	0.000	1.099	3.714

Table B.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 all 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[31]. 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.

B.3.1 Sector-specific analysis

I further investigate my argument moving to a sector-specific analysis, which also works as a placebo test. If my argument is correct, the mechanism should be observable only in industries where bribes are typically paid. In sectors where bribery is no typical custom, instead, anti-bribery policies should not enter firms' decision-making. I exploit information in the database from Escresa and Picci (2017) to 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[32]. 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.

[31] 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.

[32] 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					
	First degree	V-Dem	CCE	PACI (2012)	No China	No Israel No South Africa
	(1)	(2)	(3)	(4)	(5)	(6)
OECD Ratifier × Host PACI^2					-0.029^{*}	-0.024^{+}
					(0.012)	(0.014)
OECD Ratifier × Host PACI	-0.007				0.168^{+}	0.168^{+}
	(0.034)				(0.089)	(0.096)
OECD Ratifier × Host V-Dem Bribery^2		-0.075^{*}				
		(0.032)				
OECD Ratifier × Host V-Dem Bribery		0.133^{+}				
		(0.071)				
OECD Ratifier × Host CCE^2			-0.213^{**}			
			(0.082)			
OECD Ratifier × Host CCE			1.360^{*}			
			(0.543)			
OECD Ratifier × Host PACI^2 (2012)				-0.048^{***}		
				(0.014)		
OECD Ratifier × Host PACI (2012)				0.277^{**}		
				(0.097)		
OECD Ratifier	-0.067	0.021	-2.023^{*}	-0.260	-0.188	-0.197
	(0.191)	(0.181)	(0.847)	(0.193)	(0.196)	(0.225)
Host PACI^2					0.016	0.012
					(0.026)	(0.027)
Host PACI	0.039				-0.069	-0.101
	(0.106)				(0.221)	(0.230)
Host V-Dem Bribery^2		0.133^{*}				
		(0.062)				
Host V-Dem Bribery		-0.089				
		(0.159)				
Host CCE^2			0.154			
			(0.148)			
Host CCE			-0.660			
			(1.012)			
Host PACI^2 (2012)				0.020		
				(0.028)		
Host PACI (2012)				-0.057		
				(0.234)		
Host GDP (log)	0.723^{***}	0.751^{***}	0.737^{***}	0.627^{***}	0.678^{***}	0.687^{***}
	(0.107)	(0.087)	(0.088)	(0.115)	(0.121)	(0.113)
Host GDP per capita	-0.065	-0.173	-0.185	0.218	-0.044	-0.145
	(0.173)	(0.165)	(0.187)	(0.225)	(0.167)	(0.169)
Host FDI (GDP %)	0.010	0.013^{+}	0.014^{+}	0.006	0.011	0.012
	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.008)
Host Trade (GDP %)	-0.105	-0.148	-0.168	-0.366	-0.210	-0.217
	(0.311)	(0.281)	(0.292)	(0.315)	(0.304)	(0.298)
Host Judiciary Indep.	3.640^{***}	3.205^{**}	2.453^{+}	2.930^{**}	3.685^{***}	4.367^{***}
	(1.084)	(1.066)	(1.374)	(1.084)	(1.036)	(1.115)
Host POLCON III	0.248	0.455	0.422	0.028	0.201	0.366
	(0.904)	(0.820)	(0.829)	(0.892)	(0.865)	(0.850)
Host Democracy	0.015	0.005	0.068	0.038	-0.050	-0.382
	(0.431)	(0.397)	(0.413)	(0.412)	(0.418)	(0.460)
Home GDP (log)	0.058^{+}	0.048	0.048	0.052^{+}	0.080^{*}	0.074^{*}
	(0.031)	(0.035)	(0.035)	(0.030)	(0.032)	(0.029)
Home GDP Growth (%)	-0.005	-0.002	-0.002	-0.002	-0.001	-0.003
	(0.021)	(0.022)	(0.022)	(0.020)	(0.023)	(0.021)
Home Judiciary Indep.	-0.398	-0.372	-0.382	-0.328	-0.253	-0.333
	(0.263)	(0.293)	(0.292)	(0.257)	(0.271)	(0.289)
Dyad BIT	0.065	0.078	0.064	0.092	0.189^{***}	0.166^{***}
	(0.073)	(0.070)	(0.070)	(0.072)	(0.049)	(0.049)
Dyad Common Language	0.748^{***}	0.791^{***}	0.787^{***}	0.707^{***}	0.651^{***}	0.657^{***}
	(0.101)	(0.098)	(0.098)	(0.100)	(0.045)	(0.045)
Dyad Colonial Relation	0.734^{***}	0.759^{***}	0.759^{***}	0.761^{***}	0.304^{***}	0.292^{***}
	(0.127)	(0.120)	(0.120)	(0.123)	(0.052)	(0.052)
Dyad Distance	-1.126^{***}	-1.241^{***}	-1.237^{***}	-1.061^{***}	-1.129^{***}	-1.069^{***}
	(0.096)	(0.090)	(0.090)	(0.093)	(0.059)	(0.058)
Firm Assets (log)	0.005	0.008	0.008	0.006	0.005	0.005
	(0.008)	(0.007)	(0.007)	(0.008)	(0.008)	(0.008)
Firm Age (log)	0.013	0.010	0.010	0.009	0.012	0.011
	(0.015)	(0.014)	(0.014)	(0.014)	(0.014)	(0.015)
Firm Host Countries (log)	1.287^{***}	1.274^{***}	1.274^{***}	1.277^{***}	1.271^{***}	1.270^{***}
	(0.020)	(0.019)	(0.019)	(0.019)	(0.020)	(0.020)
Constant	-6.105^{***}	-6.193^{***}	-5.361^{**}	-5.984^{***}	-5.854^{***}	-5.655^{***}
	(0.589)	(0.258)	(1.642)	(0.628)	(0.585)	(0.619)
Random intercepts	Yes	Yes	Yes	Yes	Yes	Yes
N. of host countries	83	99	99	85	82	81
N. of home countries	56	56	56	56	55	54
Observations	285,295	340,554	340,554	291,945	280,767	275,705
Akaike Inf. Crit.	49,607.020	55,424.820	55,423.580	53,329.030	49,350.190	49,272.190
Note:						$^{+}$ p $<$ 0.1, * p $<$ 0.05, ** p $<$ 0.01, *** p $<$ 0.001

Table B.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

	Dependent variable:			
		Subsidiary		
	Test	Test	Placebo	Placebo
	(1)	(2)	(3)	(4)
OECD Ratifier ×	−0.040**	−0.043**	−0.006	0.005
Host PACI^2	(0.014)	(0.015)	(0.027)	(0.029)
OECD Ratifier ×	0.248*	0.299**	0.003	−0.123
Host PACI	(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		−0.005		0.009
Growth (%)		(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		0.686***		0.762***
Language		(0.105)		(0.143)
Dyad Colonial		0.667***		0.700***
Relation		(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		1.288***		1.243***
Countries (log)		(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.

C.2 Robustness tests

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[33] 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[34]. 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.1 reports estimates obtained in the five subsamples, their confidence intervals, number of observations in each bin, and the distribution of the moderator *Host PACI* variable. I first introduce only the *OECD Convention* variable and fixed effects (panel a). Next, I introduce all controls at the host country-, home country, and dyad-level (panel b). I interact covariates with year fixed effect to control for differential observable trends across dyads[35]. Panels c and d of Figure C.1 reproduce the same specifications of panels a and b, with the exclusion of observations in the "buffer" years 1999–2001. Standard errors are always clustered at the dyad level.

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

[33] 1733 dyads out of 3591 include a home country that did not ratify the Convention.

[34] 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.

[35] 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.

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

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

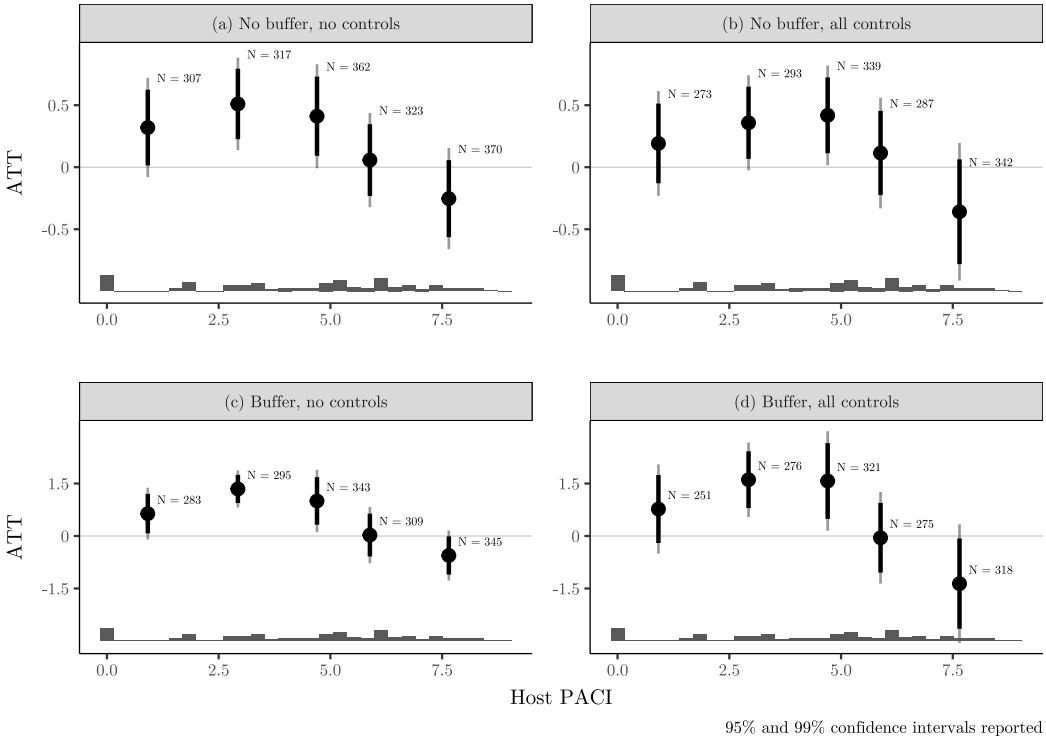

Figure C.1: Country-level data: 2FE binning estimator.