The conditional arm of the law. The effect of the OECD Anti-Bribery Convention on foreign direct investment*

Lorenzo Crippa†

October 16, 2020

Abstract

Firms' foreign investment depends on an assessment of political risk, and corporate criminal regulations are often criticized for increasing it and deterring investment. Yet, evidence is scant. This paper studies the effect of policies imposed by states under the 1997 OECD Anti-Bribery Convention on their firms' outward investment to corrupt economies. It argues that their effect is non-linear in the level of corruption of the host economy. It is null in clean countries. Where corruption is mild it is positive: anti-bribery policies give firms a leverage to refuse paying costly bribes. Where corruption is endemic, instead, these policies have a negative effect on investment: they expose firms to the risk of prosecution without providing an effective leverage. Multilevel logit models test the argument, explaining investment decisions of 3871 individual firms between 2006 and 2011. Companies from signatories have a 40% higher probability of investing in mildly corrupt economies, which plummets to −50% in extremely corrupt ones. This exercise shows that the effect of corporate regulations on political risk depends on characteristics of the host country. A following country-level difference-in-differences exercise also shows that the Convention has not affected aggregate outward investments of signatories. Results inform a re-evaluation of current anti-bribery policies.

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

*I thank Federica Genovese, Han Dorussen, Nicole Baerg, Martin Steinwand, Laura Saavedra-Lux, James Wood, Alina Mungiu-Pippidi, Ina Kubbe, and participants to the panel "The International Factor in Promoting Good Governance" at the European Consortium for Political Research 2020 General Conference, for thoughtful and precious comments on various versions of this work. I thank Lucio Picci and Laarni Escresa for sharing material necessary to compute the PACI measure. Needless to say, all mistakes are mine.

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

Introduction

The decision of a firm to invest overseas results from an assessment of long-term foreseeable costs and profits. Such investment creates jobs, technological spillovers, and favors growth of the host country. Yet, investors often resort to nasty business practices in host economies. Bribery is among them (Barassi and Zhou, 2012; Malesky et al., 2015; Zhu, 2017). Home states have committed to fight such wrongdoing, with cross-border regulations that oversee their firms' foreign-owned branches and make bribery a costlier strategy. Whatever the effect on propensity to bribe, these "extraterritorial" policies risk increasing costs to firms to the point of deterring foreign investment altogether, undermining its positive effects. They are also criticized for disadvantaging firms in corrupt host economies *vis-à-vis* competitors without such standards: in 2012 future President Donald Trump called the U.S. anti-bribery policy "a horrible law" which "should be changed"[1]. Similar statements worry anti-corruption practitioners. Yet, studies in political economy offer no conclusive evidence about such alleged effect. They rest on analyses of country-aggregated investment flows, a choice flawed by methodological shortcomings which prevents to trace how anti-bribery policies affect individual firms' decisions.

This article studies the effect of home countries' anti-bribery policies on foreign investments by their multinational corporations (MNCs). I focus on the 1997 OECD Anti-Bribery Convention[2] with which 44 signatory countries have made it a crime for their firms to pay bribes abroad. I account for shortcomings of previous research studying the effect of anti-bribery policies on individual firm's investment choices. I argue that the impact need not be negative in corrupt economies. Rather, its direction depends non-linearly on the *level* of corruption of the host economy: it is null in very clean countries, positive in mildly corrupt ones, and negative only in extremely corrupt destinations. Hence, these policies need not have a univocal negative effect on country-aggregated outward FDI.

Two empirical exercises support my argument. First, I model individual decisions

[1]See: https://fcpaprofessor.com/donald-trump-the-fcpa-is-a-horrible-law-and-it-should-be-changed/ (accessed on September 23rd, 2020).

[2]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).

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 squared level of corruption of the host economy. Firms from signatories are no more likely than their competitors to invest in very clean economies. They have a higher probability of investing in mildly corrupt host economies, up to a peak of 40%. Instead, they are 50% less likely to invest in *extremely* corrupt destinations. Results are robust to a vast series of tests. Second, I employ country-level data to show that no causal effect whatsoever can be attributed to the OECD Convention on *aggregate* outward foreign direct investment (FDI) flows and stocks of signatories. Findings show that home extraterritorial anti-bribery policies affect firms' investment choices, but not aggregate flows. Concerns about the anti-business nature of these laws should therefore be taken with skepticism.

Findings offer relevant contributions beyond anti-corruption studies. Methodologically, they give yet more reasons urging scholars of FDI to study individual firms' behaviors, rather than aggregate data. Substantively, they show that home countries' cross-border regulations and international policies have no univocal effect on foreign investment. Rather, they interact with host fundamentals to affect privates' perceptions of political risk (Beazer and Blake, 2018). Home states oversee their firms' foreign activities in such cases as merges (Damro, 2001; Griffin, 1999), trade with sanctioned countries (Rodman, 1995), and money laundering (Sharman, 2011). I contribute to a study in international political economy that has assessed domestic causes of these policies (Putnam, 2009), and effects on other states (Kaczmarek and Newman, 2011).

Finally, the study informs a nuanced view of anti-corruption policies. Bribery has wide and well-known detrimental effects for a society (Rose-Ackerman and Palifka, 2016). Anti-bribery regulations intend to reduce the supply-side of corruption and undermine kleptocracies. Yet, they are blamed for jeopardizing firms' competitiveness in corrupt economies. I document that this need not always be true, as firms in fact *gain* from these policies in a range of mid-level corrupt countries. This is good news for their home states, since their aggregated outward FDI is not impacted. Instead, it is bad news for the most corrupt host countries. Anti-bribery policies pull firms away from these

destinations, which are thus left exposed to investments from companies with allegedly no similar standards.

The next section expands on the puzzle motivating this study, building on previous literature and anecdotal evidence. Then, an argument is introduced for the non-monotonic effect of OECD anti-bribery policies on firms' investment choices, conditional on the level of corruption of the host economy. Two empirical exercises follow, and support the argument. The final section concludes discussing limitations of the study.

1 The puzzle: What effect for extraterritorial anti-bribery regulations on foreign investment?

Foreign investment is made of individual decisions of private or public-owned firms to project their presence abroad. It differs from international trade, in that it entails the actual ownership[3] of a firm in the host country (called "subsidiary") from another firm in the headquarter or home country (called "parent")[4]. Firms decide to go multinational if expected advantages in ownership, location or internalization (OLI)[5] terms outweigh costs (Caves, 1971; Dunning, 1977, 1980). The resulting foreign investment has positive effects in the host country. It creates jobs and technology spillovers which foster economic growth (Borensztein et al., 1998; De Soysa and Oneal, 1999), and it favors democratization (Eichengreen and Leblang, 2008; Li and Reuveny, 2003). It also has detrimental consequences. Foreign investors often adopt unfair competition practices to crowd out local competitors (Görg, 2000), and promote inequality (Bornschier et al., 1978).

Firms can resort to bribery[6] as a business strategy in foreign markets (Søreide, 2006).

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

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

[5]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).

[6]I abide by a traditional definition of bribery as a specific instance of corruption (Heywood, 1997).

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

Home states have adopted international regulations to try and limit such heinous behaviors by their firms in host economies. "Extraterritorial" policies (Putnam, 2009) under the 1997 OECD Convention[7] grant home states jurisdiction to scrutinize and prosecute bribery perpetrated beyond national borders by their nationals or by foreign entities they own (Brewster, 2017; Jensen and Malesky, 2018). Signatory home states thus regularly impose fines on firms for corrupt payments made by their foreign subsidiaries or employees. For instance, in June 2019 the U.S. corporation Walmart Inc. disbursed $282 million to the U.S. Department of Justice (DOJ) and Securities and Exchange Commission (SEC) in admission of corrupt payments made by its Brazilian subsidiary[8].

These policies have received criticisms over time for their intrusion into overseas busi-

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).

[7]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).

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

ness. The U.S. unilaterally adopted a legislation to prohibit foreign bribery in 1977: the Foreign Corrupt Practices Act (FCPA)[9]. Immediately, U.S. firms protested the FCPA was an anti-business policy imposing too large a burden on them, which turned into a disadvantage in international markets *vis-à-vis* competitors with no such standards (Brewster, 2017). An early report found U.S. foreign investment suffered from this law in the 1980s (Hines, 1995). The very adoption of a common OECD anti-bribery regulation was delayed for 20 years precisely due to resistances among U.S. partners about its possible deterrent effect on foreign investment (Brewster, 2017; Gutterman, 2015; Tarullo, 2004).

Anti-bribery policies have kept receiving criticisms after they were coordinated at the OECD. The comment made by Donald Trump in 2012 exemplifies this, but European anti-bribery laws have received no milder judgments[10] (Gutterman, 2017). The existence of two legal standards, with some firms that are and others that are not subject to anti-bribery policies, is an ongoing concern not only in the opinion of politicians and reporters. Recent studies pointed out that MNCs outside the umbrella of the OECD Convention have *increased* their propensity to bribe, somehow filling a gap left by their competitors (Chapman et al., 2019; Jensen and Malesky, 2018).

Notwithstanding these criticisms, the quest about the effect of anti-bribery laws on foreign investment choices remains open. Previous studies are not conclusive, and offer substantively mixed findings. Some find countries with anti-bribery standards have invested less in corrupt countries (Blundell-Wignall and Roulet, 2017; Cuervo-Cazurra, 2006, 2008; Hines, 1995), others argue the effect is positive (Kaufmann and Wei, 1999), while still others find no evidence for any effect at all (Hakkala et al., 2008; Smarzynska and Wei, 2000; Wei, 2000).

One explanation for such mixed evidence is the use of country-level data, as argued by Zhu and Shi (2019). This methodological custom turns into a twofold problem. First, as next section shows, two competing arguments can be advanced about how anti-bribery extraterritorial policies affect individual firms' investment decisions in corrupt countries.

[9]The U.S. was the first country in the world to do so and it would take 20 years for its OECD partners to follow the lead.

[10]See: https://www.theguardian.com/law/2011/apr/01/revamped-bribery-act-firms-jitters (accessed on September 24th 2020).

They cannot be explicitly disentangled using country-level information.

Second, the use of country-level data turns into a severe problem of selection bias that is regularly overlooked: only investments that have been decided upon are observable (Barassi and Zhou, 2012). This prevents from understanding how anti-bribery policies affect the choice for an investment in the first place. The only study that, to my knowledge, makes use of individual firms' data and explicitly accounts for this problem find no effect of OECD anti-bribery policies on firms' decisions (Hakkala et al., 2008). Yet, it relies on observations from Swedish companies only, a sample with very specific characteristics which severely undermine the external validity of the results. Moreover, its observations stretch until 1998, *i.e.* only one year after the ratification of the Convention, and in fact one year *before* its entry into force. It is not surprising that no effect is observed.

Anecdotal evidence also does not suggest that MNCs from countries with anti-bribery regulations are necessarily penalized in corrupt countries. In fact, firms have not univocally opposed them. In the 1990s a coalition of anti-corruption non-governmental organizations *and* businesses emerged first in the U.S., then in other OECD countries, to lobby for the adoption of global anti-corruption policies (Gutterman, 2015). In 1997 U.S. Senator Sarbanes, who was working on a revision of corporate laws, even received a letter signed by the CEOs of 35 major corporations, expressing their wish for a quick ratification and implementation of the Convention (US Senate, 1998). At the very least this suggests that anti-bribery laws are not necessarily disadvantageous to companies. What is their effect on investment decisions, then? Are concerns about their anti-business effect in corrupt countries justified? Next section proposes an answer to these questions.

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

On December 17th, 1997, representatives of 34 countries convened at the OECD offices in Paris, and agreed on the terms of an Anti-Bribery Convention. They committed to make it a criminal offence for their firms to pay bribes abroad, and to prosecute such foreign

wrongdoing. This group of countries included all members of the club at the time, plus five non-member states. It would still take two years before the Convention entered into force, but an exhausting 20-year long process of discussion on a supra-national anti-bribery agreement was finally over. Until today, 10 more countries have joined the Convention and agreed on its terms[11]. The ratification process is now complete for all 44 signatory home countries. All of them have also adopted the required implementing legislations.

The literature reviewed in the previous section shows corruption has no univocal effect on investment decisions. While it certainly increases costs, it can be a viable strategy to firms in certain markets or industries. Home countries designed anti-bribery tools under the OECD Convention to intervene in this uncertain calculus and deter a corrupt investment of their MNCs abroad. The idea was to unequivocally increase costs of wrongdoing (Cuervo-Cazurra, 2008; Davis, 2019). Costs to firms for foreign corruption increase from various points of view. First, judicial authorities can (and do) levy blockbuster fines. Penalties have increased consistently over the years, to reach records in the order of billions of U.S. dollars in recent judiciary cases[12].

Monetary disbursements are not limited to fines. A common practice in anti-bribery law enforcement, particularly in the U.S., consists in reaching out-of-court agreements between prosecutors and the firm. The company thus avoids perils of a judiciary prosecution. Yet, it undertakes a painful and costly process. It admits guilt, cooperates with prosecutors, pays an expensive monetary settlement, and pledges to undertake a severe re-structure of its corporate organization and culture to ensure future compliance with anti-bribery standards (Garrett, 2011). That often implies turning executive offices inside out; setting up systems of internal investigations; having third-parties monitoring activities of a firm for a probation period; and implementing strategies to avoid managers established personal connections with local foreign authorities[13].

[11] As of September 2020, 44 signatory countries include all current OECD members and 7 non-member states: Argentina, Brazil, Bulgaria, Costa Rica, Peru, South Africa, Russia.

[12] For a top-ten of disbursements under the U.S. FCPA see: https://fcpablog.com/2020/02/03/airbus-shatters-the-fcpa-top-ten/ (accessed on September the 25th, 2020).

[13] See the drastic changes implemented by Siemens AG after an infamous worldwide bribery scandal: https://www.complianceweek.com/how-siemens-worked-to-fix-a-culture-of-institutionalized-corruption/14915.article (accessed on September the 25th, 2020).

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

How do these polices affect decisions of a firm to invest in a corrupt economy? First, they enter firms' calculus about the OLI advantages of a foreign investment as a potential cost, and can deter it. Yet, they can also prove advantageous. Common international regulatory regimes level the playing field of economic competition imposing the same regulation to companies (Vogel, 1995). This reduces costs to firms that already observe corporate standards. The mechanism has been observed in the environmental (Kennard, 2020; Perkins and Neumayer, 2012; Prakash and Potoski, 2007) and financial regulations (Kalyanpur and Newman, 2019).

Cross-border anti-bribery policies enter firms' calculus not only by imposing costs or benefits, but also by providing information about their home states' behaviors. A foreign investment entails a long-term commitment to the host economy. Uncertainty about its future can deter it. Institutions reduce this "shadow of the future", providing credible information which improves predictability (Axelrod, 1984; Keohane, 1984). This effect is documented for economic policies (Alesina and Dollar, 2000; Li and Resnick, 2003) and political institutions (Jensen, 2003, 2008) in the host country. International institutions, too, regularize expectations about state behavior, reducing risk (Bodea and Ye, 2017; Gray, 2009; Skovgaard Poulsen, 2014).

Based on these effects, two competing arguments can be advanced about the effect of

anti-bribery policies on firms' foreign investment decisions. The first expects firms subject to anti-bribery standards invest less in corrupt economies than competitors without similar regulations. The second predicts the opposite. To see how they are justified, consider that a foreign investment entails a principal-agent problem for a firm (Das, 1999). A parent firm (the principal) delegates most of its business decisions to local offices when it incorporates a subsidiary (the agent) in a host country. Decisions to pay bribes in a host country usually result from the agency of local employees and managers, without the headquarter necessarily having information about it.

Imagine two identical firms, respectively from countries A and B, faced decisions to invest in some foreign country C which is known to have corrupt bureaucracies. Country A is a ratifier of the OECD Convention, and has anti-bribery regulations in place. Country B is not. Investments in a corrupt host country like C often look like a bid between competing firms, where public officials can demand competitors a bribe to facilitate business (Tarullo, 2004). Agents in C have an incentive to pay the bribe: winning such bids have licit rewards (such as career advancements, or prestige), and often illicit ones (such as kickbacks agents can re-direct to their own personal accounts).

The headquarter office of the firm from A anticipates its agents would operate in these conditions, were an investment made. Being subject to OECD anti-bribery policies regularizes its expectations about the behavior of its home state's judiciary in that event. In case its agents in C committed bribery, prosecution would be a likely risk. Anti-bribery policies turn a possible scandal of corruption into a significant expected cost for the entire company. The firm from B, instead, does not face the same type of concern: its agents in C can bribe to secure contracts without the principal risking judiciary repercussions. All else equal anti-bribery standards thus deter the parent firm from A to invest in C, while the firm from B is not deterred. Thus, one should expect firms subject to anti-bribery standards would invest *less* than competitors in corrupt countries.

Yet, a second argument justifies the opposite expectation. Anti-corruption policies are not intended to jeopardize firms' position in international business. States agreed on regulations that would reduce corruption-induced uncertainty and ensure cleaner business

models without off-the-record expenditures (Lambsdorff, 2002). Davis (2019) argues that anti-bribery policies help companies put in place internal systems of compliance to oversee their foreign agents, and deter them from paying bribes. They also provide foreign agents and branches a legal ground to refuse paying extra fees, which increases their bargaining power *vis-à-vis* public officials (Hakkala et al., 2008; Kaufmann and Wei, 1999). Public officers are less likely to expect fees from firms in a stronger bargaining position (Svensson, 2003), in particular knowing that they could be involved in international scandals with worldwide resonance.

If anti-bribery rules play this role, firms under their jurisdiction can leverage on them to cut corruption-induced expenses: bribe fees, uncertainty, and transaction costs (Rose-Ackerman, 1975). In the investment decision stilized before, this argument claims anti-bribery policies do not dis-incentivize the principal of the firm from A about investing in C. On the other hand, the firm from B is not subject to the same regulations. Its agents in C cannot leverage on these policies to refuse paying bribes, and have no enhanced bargaining power. Brazys and Kotsadam (2020) argue public officials demand more bribes from unconstrained firms. Their operations will therefore be more inefficient and costly, and they will be worse off in the competition. According to this argument, firms subject to anti-bribery policies would benefit from them and would invest *more* in corrupt economies.

How to reconcile these opposite expectations? I argue that both pulls are at play. Their net effect on investment depends on the level of corruption of the host economy, because public officials' power increases in it (Ades and Di Tella, 1999; Svensson, 2003), and so does their likelihood of effectively demanding bribes. The first mechanism, whereby firms from countries with anti-bribery standards are disadvantaged in international business, prevails when the host country is extremely corrupt. In these economies bribery is perceived as an expected business custom. This gives public official a strong position to demand fees, and makes it highly unlikely that they refrain from doing it. All parent firms include these fees in expected costs of investing in these economies. However, principal firms subject to anti-bribery standards face greater costs than their competitors, because

they risk prosecution in their home country if their agents misbehave. Anticipating these conditions, they are less likely to invest in very corrupt economies.

In economies where corruption is diffused but milder, instead, the second mechanism prevails. Bribery here is not the necessary way to conduct business and public officials' room to demand bribes is limited. Firms subject to anti-bribery standards find they can leverage on these rules and enhance their bargaining power. They have the legal ground to refuse paying bribes to public officials, reducing costs associated with an investment. This possibility is precluded to their competitors, which operate at a disadvantage. As a result, firms from signatories are more likely to invest in these economies.

In very clean economies, finally, anti-bribery policies should neither advantage nor disadvantage firms, as it is very unlikely that corrupt fees are expected at all. Overall, a home state adopting anti-bribery policies does not necessarily penalize its foreign investment. Rather, its companies find themselves advantaged in some economies and at a disadvantage in others, and choose investment destinations accordingly. In this argument the effect of anti-bribery policies on the probability that a firm invests abroad depends non-linearly on the level of corruption of the host economy, which proxies for the power of public officials. Next section presents firm-level evidence to support this expectation.

3 Empirical analysis

3.1 Firm-level data

The above argument predicts that, all else equal, OECD anti-bribery policies adopted by a home country affect the probability that its firms invest abroad conditionally on the level of corruption of the host economy, in a non-monotonic way. I formalize this expectation drawing from models of investment decisions conditional on corruption in Hakkala et al. (2008), Barassi and Zhou (2012) and 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:

$$\begin{cases} I_{fij} = 1, & \text{if } I^*_{fij} > 0 \\ I_{fij} = 0, & \text{otherwise} \end{cases}$$

Equation 1 expresses the latent variable I^*_{fij}. It is a function of whether country i is a signatory of the OECD Convention ($S_i = 1$), and of a continuous measure for the level of corruption of the host country (C_j). Corruption also appears as a squared term (C_j^2). Both C_j and C_j^2 are multiplied by S_i. This represents the statement that the effect of the OECD Convention (S_i) on the propensity to invest abroad (I^*_{fij}) depends on the level of corruption of the host country, in a non-linear way. Other factors explaining I^*_{fij} are summarized in the matrix $\mathbf{X_{fij}}$, 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[14] of I^*_{fij} with respect to S_i:

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

The effect of the OECD Convention on the propensity of a firm to invest abroad, conditional on the level of corruption of the host country, should therefore trace a parabola. Figure 1 illustrates my expectation on its shape. It reports the level of corruption of the host economy C_j on the x axis. The y axis, instead, reports the marginal effect that ratifying the OECD Convention has on the propensity for a home country's firms to invest overseas ($\frac{\partial I^*}{\partial S}$). For low levels of corruption of the host country, the effect should be zero. As the host economy becomes more corrupt, a home country ratifying the Convention advantages its firms and increases their propensity to invest. As the host country becomes more corrupt, this effect reaches a maximum, then decreases. In extremely corrupt

[14]Since S_i is a binary indicator for the adoption of the OECD Convention, it is also immediately verified that: $\frac{\partial I^*_{fij}}{\partial S_i} = \frac{\Delta I^*_{fij}}{\Delta S_i} = (I^*_{fij}|S_i = 1) - (I^*_{fij}|S_i = 0) = \beta_1 \, C_j^2 + \beta_2 \, C_j + \beta_3$.

host countries, ratifying the OECD Convention disadvantages firms due to higher risks of prosecution. The effect on the propensity to invest is negative. In equation 2, this means that parameter β_1 is expected to be negative, β_2 positive and β_3 null.

[Figure 1 about here.]

This expectation can be tested empirically estimating the probability to observe a firm investing abroad, as a function of terms on the right-hand side of equation 1. The following model represents the one to be estimated, where $F(\cdot)$ is the inverse of a logit function[15]:

$$P(I_{fij} = 1|S_i, C_j, \mathbf{X_{fij}}, u_{fij}) = F(I^*_{fij}) \tag{3}$$

I estimate model 3 with firm-level data drawn from the Orbis Corporate Ownership Database, which I retrieve 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. It also includes firm-level and country-level covariates. 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.

The dataset reports the "ultimate parent" of each foreign subsidiary. It excludes financial investments and small firms[16]. These are appropriate selections: the population that this sample intends to represent is composed of large MNCs, which engage in long-term productive enterprises abroad and not in speculative ventures[17]. The Orbis database has a two-year lag between the moment firms' information is disclosed and the moment it is reported in the data, and has various problems when year-specific information is used to

[15] $F(x) = \frac{e^x}{1+e^x}$.

[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, investment banks are excluded. Small firms have less than one million euros in operating revenues a year, total assets less than two million euros, and less than 15 employees.

[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. I argue it allows a conservative test of my argument.

obtain time-series (Kalemli-Ozcan et al., 2015). Both issues are avoided here employing a cross-section of observations between 2006 and 2011. Home economies represented are 61, while host countries are 84.

I follow Beazer and Blake (2018) and construct a binary outcome variable, called *Subsidiary*, representing whether a firm f from country i has incorporated a subsidiary in country j between 2006 and 2011. The measure does not represent the size of an investment, but this is consistent with my argument predicting its *probability*. The binary dependent variable has a dyadic form. For each parent company f from country i it is assigned a 1 if the firm is reported to have set up a subsidiary in the host country j in the time period of interest. It is assigned a 0, instead, if no subsidiary was established in the (potential) host country j[18]. Potential host countries are all economies where a subsidiary has been established by at least one firm in the dataset. This is supposed to represent all attractive host countries.

My main independent variable is *OECD Signatory*. It represents whether the home country i of a parent firm f has ratified the OECD Anti-Bribery Convention by 2005[19]. The variable is binary: it takes value 1 if this condition is met, 0 otherwise.

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 are informed by previous releases. Finally, they often implicitly adopt a definition of corruption which might not align with

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

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

respondents' or researchers' (Heywood, 2017). 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) proposed 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, proxied by commercial ties. I discuss the index in details in appendix. The PACI is suited to measure specifically cross-border bribery as it is defined in this study (see footnote 6).

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

I follow Beazer and Blake (2018) and explain my binary outcome variable employing a multilevel logit model[20]. This choice correctly specifies cross-level interaction effects (Bell and Jones, 2015) like the one implied by my argument. 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

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

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. The choice is also the correct way to model the thousands of repeated observations generated by the dyadic structure of the dataset. Unless properly modelled this large number of repeated and correlated observations would artificially reduce standard errors to zero and produce unreliable tests of hypotheses.

I also include control variables that, unless controlled for, can confound the analysis. For all of them I consider the 2005 value. The economy and institutions of the host country can affect its attractiveness. I control for the (logged) Gross Domestic Product (GDP), per capita GDP, and total trade and net FDI inflows (both as percentages of GDP) of the host country. I also include the POLCON III index for political constraints in the host country, a binary indicator for democracy from Cheibub et al. (2010), and a measure for judicial independence from Linzer and Staton (2015). I also control for home country features that could affect the likelihood it adopted and enforced anti-bribery policies, like: wealth (measured as logged GDP and GDP growth rate), and level of judicial independence. Then, I control for country-dyadic covariates: a measure of the distance in kilometres between capitals of the home and host country, and binary indicators measuring whether a bilateral investment treaty (BIT) was signed by the dyad, whether the two countries have a past colonial relationship, and whether they have a common first or official language. Finally, I control for firm-level features: the number of host countries each firm operates in, its age, and its total assets (all logged). Summary statistics are reported in Appendix[21].

[21]In the estimation procedure I recenter the distribution of all control variables around their means to help convergence. Descriptive statistics are reported before recentering distributions of these variables.

3.1.1 Results

Table 1 presents my results relative to the variables of interest only[22]. The first four models include random effects at the dyad, home and host country level. The fifth one also includes industry-specific intercepts. All models condition the effect of *OECD Signatory* on the squared and first-degree terms of the host country's corruption measure (*Host PACI*). To ensure that data are not being overfitted, table 1 first includes only the variables of interest. Then, it adds controls at the level of host and home countries (Model 2). Then it adds firm-level covariates (Model 3). Finally, it adds dyadic controls (Models 4 and 5).

Results are consistent with expectations. The coefficient associated with the interaction between *OECD Signatory* and the squared *Host PACI* is negative in size and estimated with precision. It is distinguishable from zero at the 1% or 5% conventional levels of significance in all specifications but model 3. Here the estimation is less precise, but the coefficient is still significant for conventional levels (p value: 0.06). Estimates of the coefficient of the interaction with the linear *Host PACI* term are also positive and statistically significant at the 5% conventional level, with the exception of Model 3 (p value: 0.07).

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

The coefficients associated with the un-interacted corruption measures are also not statistically significant. This is a surprising result, although recent findings in the literature on FDI and corruption point in the same direction (Blundell-Wignall and Roulet, 2017). It informs us that corruption is not a significant determinant of investment decisions for firms that are *not* subject to anti-bribery standards ($OECD\ Signatory = 0$).

[Table 1 about here.]

[22]Full disclosure of all estimates is provided in Appendix.

Interpretation of results is particularly complex in multiplicative models, and requires to compute substantive quantities of interest (Brambor et al., 2006). The marginal effect of anti-bribery policies is the appropriate quantity to evaluate if the argument advanced, formalized by equation 2, is supported. This means evaluating the change in predicted probability of a firm's *Subsidiary* incorporation when *OECD Signatory* passes from 0 to 1, for given levels of *Host PACI*.

In non-linear specifications marginal effects cannot be computed as with linear models (Ai and Norton, 2003). I follow Beazer and Blake (2018) and compute the difference in predicted probability when *OECD Signatory* changes from 0 to 1 holding everything else at its mean, *i.e.* when an average firm passes from not being subject to being subject to anti-bribery laws, conditional on observed values of *Host PACI*. I compute 95% confidence intervals of this estimated difference simulating 1000 draws from its sampling distribution (King et al., 2000). The approach is appropriate in the case of a non-linear multiplicative model (Zelner, 2009).

[Figure 2 about here.]

Figure 2 shows the results obtained when considering the estimates of model 1 in table 1 and the data support for the mediator variable, to ensure they 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[23]. When *OECD signatory* changes from 0 to 1, the predicted probability that a firm will incorporate a subsidiary changes conditionally on the level of corruption of the host economy, in a non-monotonic way.

The effect can be roughly divided in panels (a), (b) and (c). In panel (a) the change in predicted probability is close to zero for very clean host economies (*e.g.*: Canada, Denmark, Sweden). Then it increases as the host country becomes more corrupt, indicating that firms from countries with anti-bribery policies have a larger probability of investing here. At its maximum, firms from signatories have a 40% higher probability of investing

[23]I report analogous plots for all specifications of table 1 in appendix.

in host countries in this interval (Singapore, Taiwan) than their competitors. In panel (b), as the host country becomes more corrupt, this quantity remains positive but declines in size. This indicates that OECD anti-bribery policies still benefit firms in economies like Brazil, China, Italy, Malaysia, Mexico, and the United Arab Emirates, but to a lesser extent. For extreme levels of corruption, as in panel (c), firms from signatory countries are worse off. They have a lower probability of investing here than their counterparts, a quantity that reaches a minimum of -50% for host countries at the right-end of the corruption scale like Egypt, India, Kazakhstan, Nigeria, or Russia.

In appendix I propose extensive tests to show that results are robust to: the use of traditional, perception-based indexes of corruption. To the use of the original PACI measure in Escresa and Picci (2017). To the exclusion of outlier countries. To the exclusion of countries that ratified the Convention within the 2006-2011 time-frame. To the use of dyadic country-level data, modelled with three different specifications: difference-in-differences (DiD), random effects models, and Heckman selection models.

I further investigate my argument moving to a sector-specific analysis, which also works as a placebo test. The mechanism I advance should be observable only in industries where bribes are typically paid. In sectors where bribery is no typical custom, instead, anti-bribery policies should not enter firms' decision-making. I exploit information in the database from Escresa and Picci (2017) to perform this step. I first obtain a list of industries where at least one case of cross-border bribery was prosecuted before 2005. I argue that these industries represent sectors where bribes are more often paid[24]. 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")[25], and one including the rest of the sectors in the sample ("placebo").

Figure 3 reports the point estimates and confidence intervals obtained within these subsamples. Full disclosure of the results is reported in appendix. For each subsample I replicate the model including no controls (only random effects) and all controls. Estimates

[24]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.

[25]The list of industries in this set and their industrial classification is reported in appendix.

of the parameters associated with the interaction terms are consistent with the ones presented in table 1 for the "test" subsample. They are even more significant, as standard errors shrink. This indicates that they are estimated with even more precision. They are never distinguishable from zero, instead, in the "placebo" subsample. This indicates that the conditional effect of the OECD Convention can be observed only within industries where corruption is a customary practice.

[Figure 3 about here.]

To conclude my firm-level quantitative analysis, I present the marginal effect of *OECD Signatory* on *Subsidiary* incorporation in the "test" subsample, following the same procedure discussed for figure 2. Results relative to both model specifications of figure 3 are presented in figure 4. Broadly speaking, the interpretation is the same as for figure 2. Yet, when all controls are included (figure 4b), the marginal effect of *OECD Signatory* for very low levels of *Host PACI* is negative and distinguishable from 0. This is a result that runs counter the argument proposed in the previous section. Data support in this range of host countries is limited, so the result must be taken cautiously. Yet I speculate this finding might be explained by the fact that host countries with low levels of corruption are also those which are more likely to cooperate with home countries in anti-bribery investigation, an aspect that my argument does not consider. All else equal this might deter firms from signatories from investing here.

[Figure 4 about here.]

3.2 Country-level data

The previous exercise shows firms under the OECD Convention make different investment choices than their competitors. They have a higher probability of investing in mid-corrupt countries, but are way less likely to invest in highly corrupt ones. Next question comes naturally: do these choices impact foreign investments of signatory countries, when aggregated?

I propose a second statistical exercise to answer to this question. I draw on country-level information about FDI outflows from the United Nations Conference on Trade and Development (UNCTAD) database. I consider information relative to 192 countries between 1980 and 2019. For each home country I measure *FDI* outflows in three ways: as percentage of national GDP, as a per capita measure, and as a percentage of total world FDI. The top panel of figure 5 shows time variation of *FDI* outflows as GDP percentage for the U.S., over the period considered. The bottom panel shows U.S. GDP over time, to ensure changes in the top panel are not due to drastic variations in the denominator. The dashed vertical line indicates the timing the OECD Convention entered into force in the country[26]. The drop in FDI following its entry into force suggests concerns about the anti-business nature of anti-bribery policies might be motivated. Yet, can this drop be *causally* attributed to the entry into force of the Convention?

[Figure 5 about here.]

I perform a DiD analysis to answer the question. The goal is to compare FDI outflows of countries that ratified the OECD Convention with those of countries that did not, and to assess whether differences can be attributed to the timing and entry into force of the agreement. The Convention entered into force at different times for signatory countries. This prevents from running a canonical 2×2 DiD model with two groups ("treated" and "untreated") and two time periods ("pre" and "post" treatment). A solution to the problem is represented by the two-way fixed effects model in equation 4. Goodman-Bacon (2018) shows parameter β_1 equals a weighted sum of the coefficients from all possible 2×2 comparisons between groups (comprising the untreated group and groups treated at different times).

$$FDI_{it} = \beta_1 \, OECD \, Convention_{it} + \mathbf{X}'_{\mathbf{it}}\boldsymbol{\gamma} + \delta_i + \phi_t + \alpha_i \, t + u_{it} \tag{4}$$

The dependent variable *FDI* in model 4 is one of the three possible measures of foreign

[26]The U.S. had already an anti-bribery regulation in force before this date, yet the FCPA was revised to adapt it to the recently-signed OECD Convention and law experts generally believe U.S. prosecutors ramped up its enforcement after adoption of the Convention (Brewster, 2017).

investment outflows. Index i represents the country of origin of the investment and t the year. The variable *OECD Convention* is a binary indicator that takes value 1 after the Convention has entered into force in a given country[27], 0 otherwise. It takes value 0 on all yearly observations for countries that never ratified the agreement. Coefficient β_1 represents the effect of the adoption of the OECD Convention on *FDI*, which is of interest here. All model specifications include country-fixed effect (δ_i) and year-fixed effect (ϕ_t). In some specifications I also include a country-specific time trend ($\alpha_i\, t$).

In some specifications I also include additional control variables, represented in equation 4 by the matrix $\mathbf{X_{it}}$. Some specifications report *FDI* as GDP percentage, others as a per capita measure. Matrix $\mathbf{X_{it}}$ therefore includes a yearly logged GDP measure for each country, and population size, to make sure variations in the dependent variable do not depend on changes in its denominator. Also, FDI outflows and adoption of the OECD Convention can both be explained by wealth of a country. In addition to the logged GDP I therefore control for GDP per capita and GDP growth (%) of each country. I also control for its exports of goods and services, to proxy for openness. Descriptive statistics are reported in appendix.

3.2.1 Results

I estimate model 4 using ordinary least squares (OLS). Standard errors are clustered at the country-level. Results are reported in table 2. For all three variants of the *FDI* measure I proceed as follows. First, I estimate model 4 including only fixed effects. Then, I include country-specific time-trends. Then, I include all controls, which shrinks sample size due to missingness in covariates for some country-year. This makes for three possible model specifications for each measure of the *FDI* dependent variable.

[Table 2 about here.]

The coefficient associated with *OECD Convention* never meets statistical significance, in any of the specifications, for any of the FDI measures. This suggests that drops in

[27] A table in appendix summarizes which countries belong to each group. See footnote 19 for information on ratification status.

FDI like the one in figure 5 cannot be causally attributed to anti-bribery laws under the Convention. In appendix, I show that these null-results hold when considering outward FDI stocks, instead of flows, and when excluding the United States from the analysis, given that the country already had an anti-bribery regulation in place. I also run a series of placebo tests to ensure *OECD Convention* does not capture simultaneous international economic events and I find no evidence of that.

The validity of OLS estimates in table 2 depends on the absence of confounders that are not controlled for by the design. Country-level and year-level fixed effects control for heterogeneity at these levels and mitigates concerns of endogeneity. The inclusion of country-specific time trends ensures much of country-specific heterogeneity is also controlled for. Yet, the so-called "parallel trend assumption" must still be met in order to provide a causal interpretation of the (null-) results from a DiD model. The assumption states that, in the absence of treatment, the treated group would have followed the same trend of the control group. If the assumption holds, then the control group can be used to build a counterfactual for treated units.

The assumption is not testable. At best, the researcher can get an idea of whether the assumption is reasonable by looking at pre-treatment trends. Figure 6 compares yearly trends for mean FDI flows across signatories and non-signatories. The three panels respectively show signatories where the Convention entered into force in 1999, those where it entered into force in 2000, and those where it entered into force in 2001[28]. In each panel the dashed vertical lines represents the year of entry into force for that treated group. Pre-treatment trends appear rather similar across signatories and non-signatories in all three panels. In fact, post-treatment trends too tend to be very similar to each other, a further descriptive evidence inducing skepticism on the effect of the OECD Convention on country-level investment flows. Overall, there is convincing evidence that concerns about the OECD Convention affecting country-level investments are not motivated.

[Figure 6 about here.]

[28]These groups represent the most numerous ones. See footnote 19.

4 Discussion and conclusion

This section concludes the study discussing its limitations and implications. The main limit of the empirical analysis is represented by its possibility of inferring causality. Borrowing from the language of randomized controlled trials, in neither of the two empirical exercises the "treatment" is assigned at random. Firms are not randomly assigned to the group subject to anti-bribery laws in the first exercise, neither are countries' investments in the second exercise. This introduces potential sources of endogeneity in the analysis.

In fact, investing firms do not self-select into a treatment or control group either: the Convention was adopted by their home countries. As such, one could argue, concerns on endogeneity are misplaced: "treatment" occurs at the level of the home country i but the effect is studied at the level of the firm f. The argument at best mitigates, but does not rule out concerns of endogeneity, which can still arise if companies from signatories are fundamentally different from those of non-signatories, with respect to both outcome and treatment variables.

Given the absence of randomization this plausible concern is only ruled out insofar as the factors causing heterogeneity have been accounted for in the models. From this point of view, the inclusion of several random effects in the multilevel models and of fixed effects and unit-specific time-trends in the DiD gives confidence that much of it is controlled. Yet, the lack of knowledge of the treatment assignment procedure, due to the absence of randomization, fundamentally condemns this study to *assume* treatment is as if random, conditionally on included controls and random/fixed effects. If the assumption is violated, causality cannot be inferred and estimates must be taken as descriptive. Future studies could therefore implement experimental or quasi-experimental strategies to ensure randomization of treatment assignment and to assess whether a solid causality can be inferred to confirm or contradict these findings.

A second limitation of the study concerns the mechanism 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. The mechanism proposed explains these hypotheses based on the room that

firms subject to anti-bribery rules have to leverage on regulations and refuse requests of bribes. This operating space, in turn, would depend on the power enjoyed by public officials, thus on the level of corruption of the host country (Ades and Di Tella, 1999): It shrinks where corrupt public officials enjoy a disproportionately large power.

This mechanism cannot be tested by the present analysis. Sector-specific evidence presented in figure 3 suggests that the effect in place involves only industries where bribes are a custom, and not the rest. This is consistent with the mechanism provided, which should not hold in industries where bribes are no usual custom. Future qualitative studies could complement the present analysis. They could investigate negotiations of firms in typically corrupt industries with foreign public officials for the award of contracts. This decision-making process could be studied to assess if the explanation provided here is appropriate, and to what extent competing mechanisms can be advanced, instead. Until then, the quest remains open on which mechanism ultimately explains the findings presented here.

Finally, the study explicitly does not consider strategies to invest in a foreign market other than corporate ownership. Licensing and joint ventures, yet, are potential ways for firms to invest in a foreign economy. They can expose firms from signatories of the Convention to a lesser risk of interaction with corrupt public officials, and might therefore be a preferred strategy (Arbatskaya and Mialon, 2018; Chapman et al., 2019). A future study could therefore investigate the effect of the Convention on these alternative investment strategies.

Net of these limitations this study makes valuable contributions. First, findings describe interesting quantitative patterns in firms' behaviors, holding constant a large series of potential confounders and sources of heterogeneity. This insight is valuable in itself, given that behaviors of firms in relation to anti-bribery policies are generally unclear. It shows that cross-border policies adopted by home states under the umbrella of an international institution like the OECD Convention affect decisions of firms about whether to invest in specific host countries. This effect is non-linear in the level of corruption of the host country, which mitigates concerns about their anti-business nature. Although

firms under anti-bribery policies are worse off in extremely corrupt economies, they are better off in a range of mildly corrupt countries. It corroborates this policy evaluation by showing that aggregate flows have not been impacted by the adoption of these policies.

Beyond the study of anti-bribery policies, contributions in the article shed a light on the complex relationship between institutions and foreign investment. Previous studies have tended to concentrate on the effects of institutions of the host (Alesina and Dollar, 2000; Jensen, 2003; Li and Resnick, 2003) and home country (Beazer and Blake, 2018; Habib and Zurawicki, 2002) on FDI. I show that policies enforced by home countries across borders interact with characteristics of host countries to affect perceptions of political risk for private investors. This is also relevant insight for the study of regulatory international networks and of their effects on transnational private actors (Farrell and Newman, 2016). Methodologically, moreover, the article provides yet more evidence urging scholars of FDI to study firm-level decisions rather than aggregate flows, which can obscure heterogeneous effects on individual companies.

Finally, policy implications can be derived. Estimates show that firms from signatories of the OECD Convention benefit from anti-bribery policies in a range of corrupt host countries like China, Mexico and Brazil. Yet, they also have a 50% lower probability of investing in very corrupt economies, including Angola, India, Nigeria, and Russia. The fact that their investments are jeopardized precisely in these economies is a reason of concern. If, all else equal, firms subject to anti-bribery laws are pulled away from these economies, investments will be undertaken more by firms which do not need to observe the same standards. With mocking irony, this undermines the desired effect of western anti-bribery policies precisely in corrupt host countries where they would be most needed. This pessimistic consideration aligns with that provided recently by Brazys and Kotsadam (2020). It adds to previous findings on the perverse effects of extraterritorial anti-bribery policies (Chapman et al., 2019; Jensen and Malesky, 2018). It also urges to a reconsideration of the extent to which these policies are the appropriate tool in the hands of regulators to favor development of bribe-importing countries.

A Appendix

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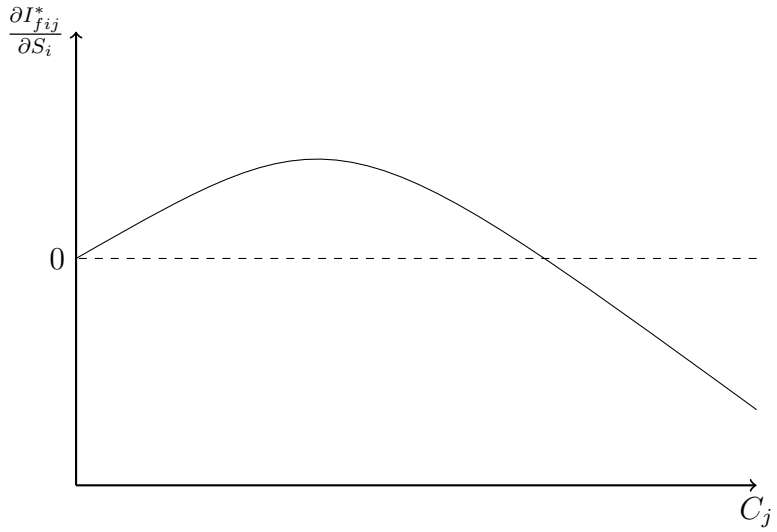


Figure 1: Non-monotonic marginal effect of the OECD Convention on investments, conditional on host country corruption

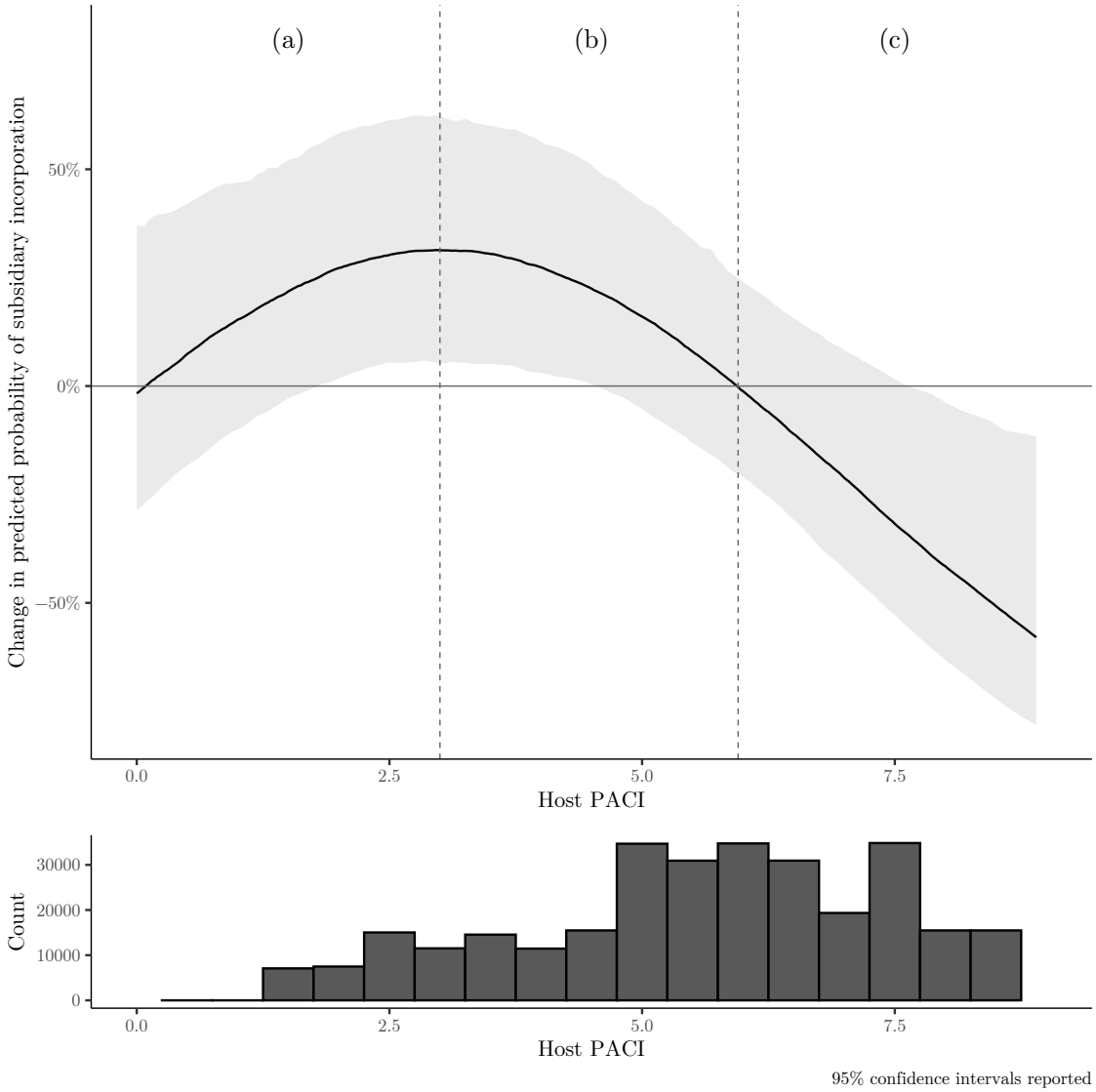


Figure 2: The non-linear effect of *OECD Signatory* on *Subsidiary*, conditional on *Host PACI*

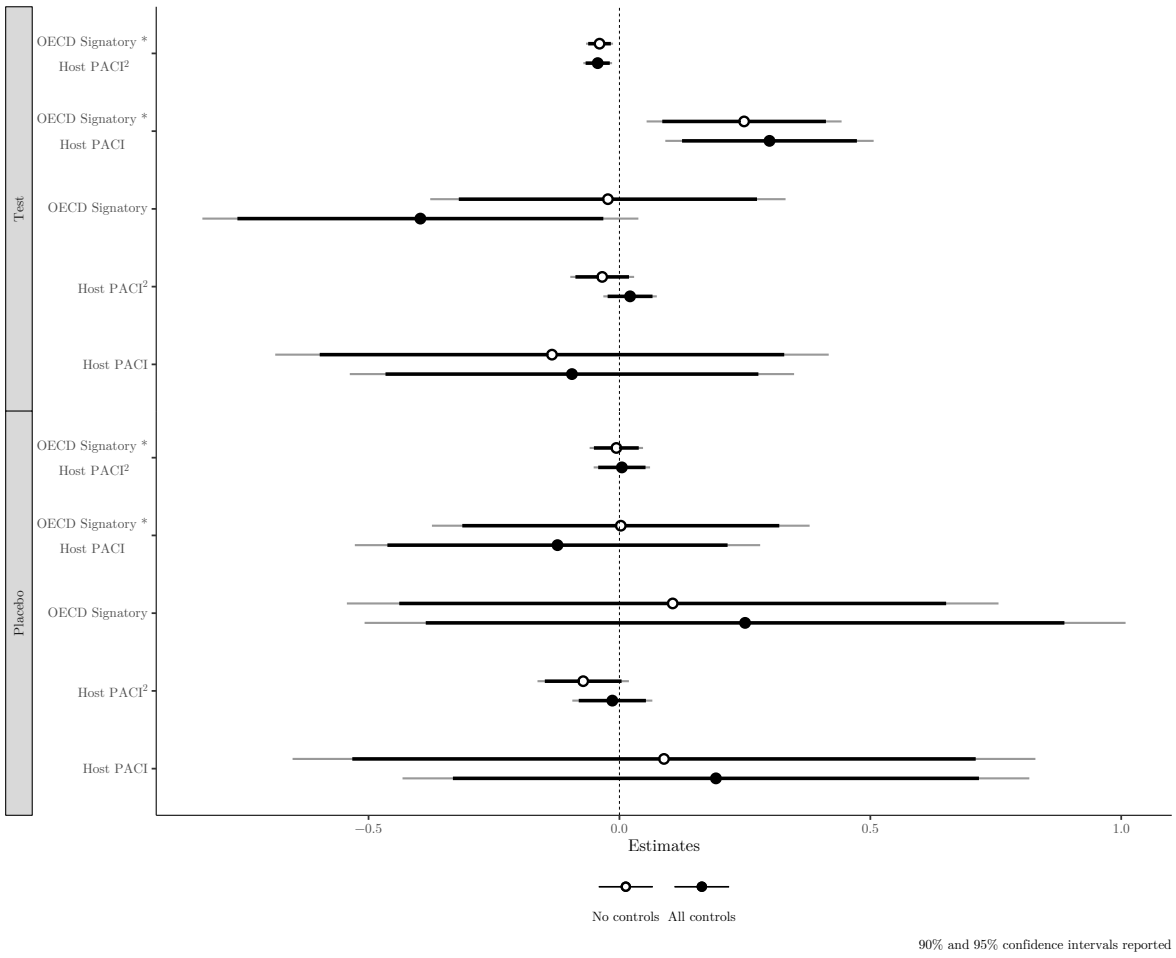
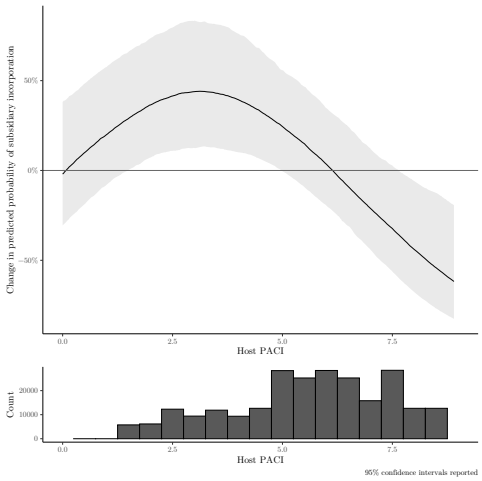
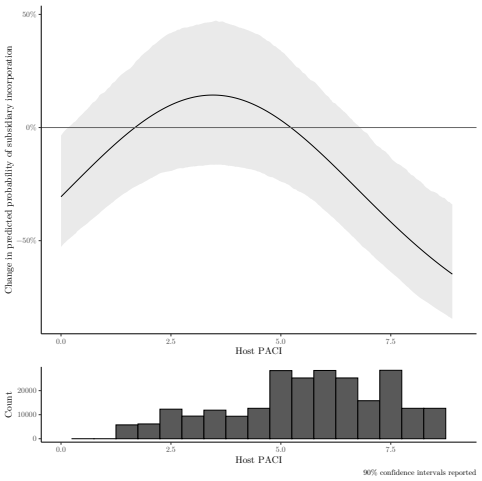


Figure 3: Coefficient plot. Market-specific results. Industries with ("test") and without ("placebo") at least one case of foreign bribery before 2006



(a) Model with random intercepts, no controls



(b) Model with random intercepts, all controls

Figure 4: The effect of *OECD Signatory* on *Subsidiary*, conditional on *Host PACI*, in industries with at least one case of foreign bribery before 2006.

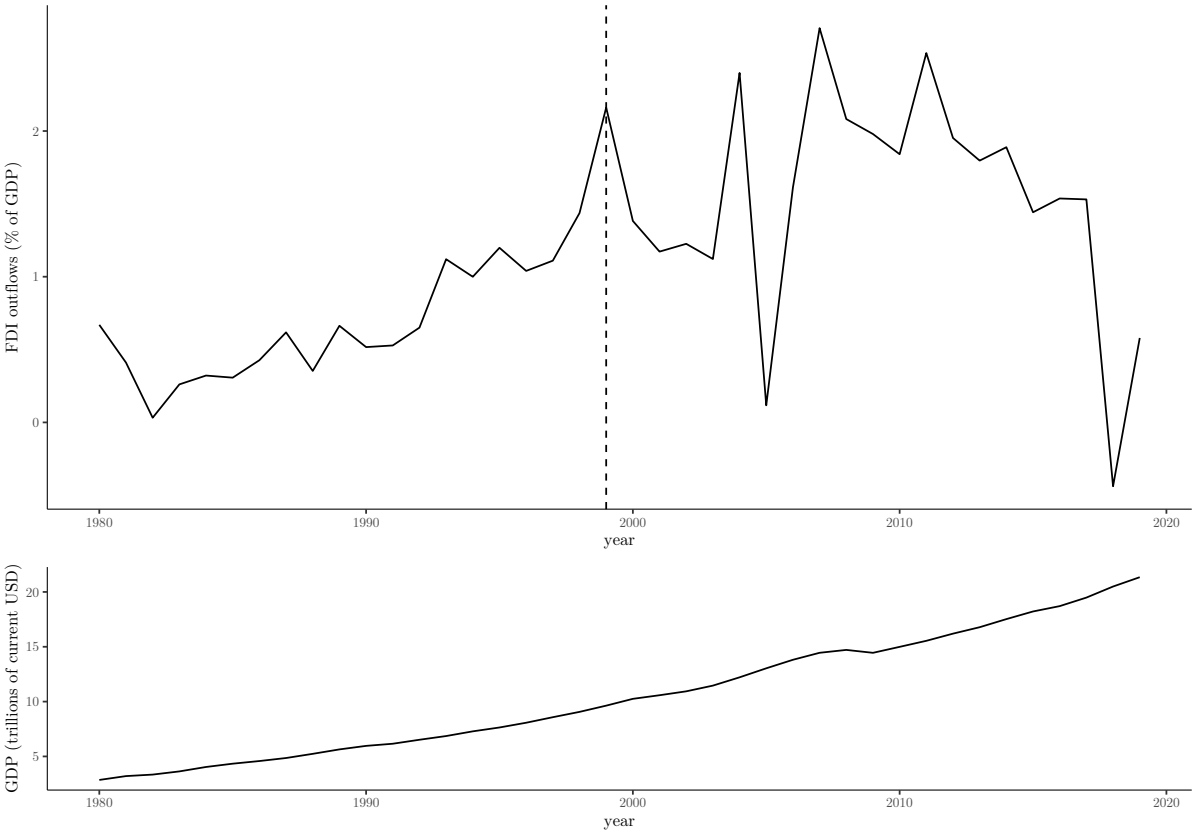
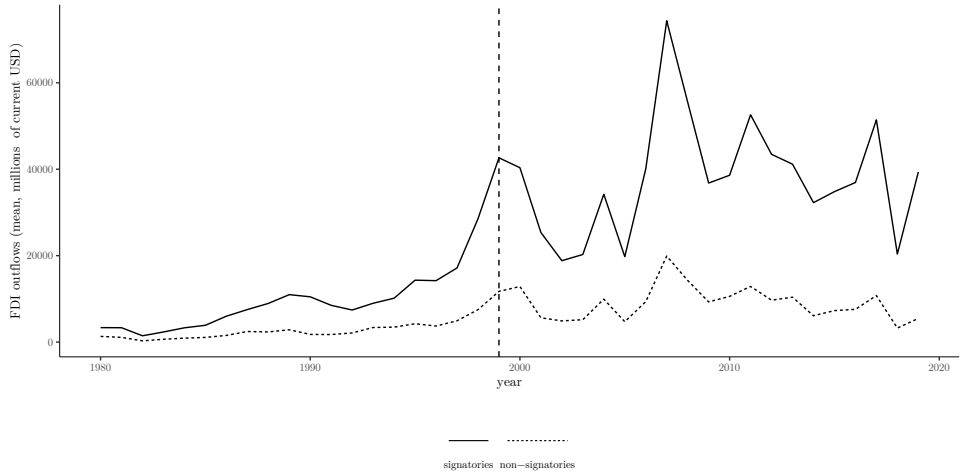
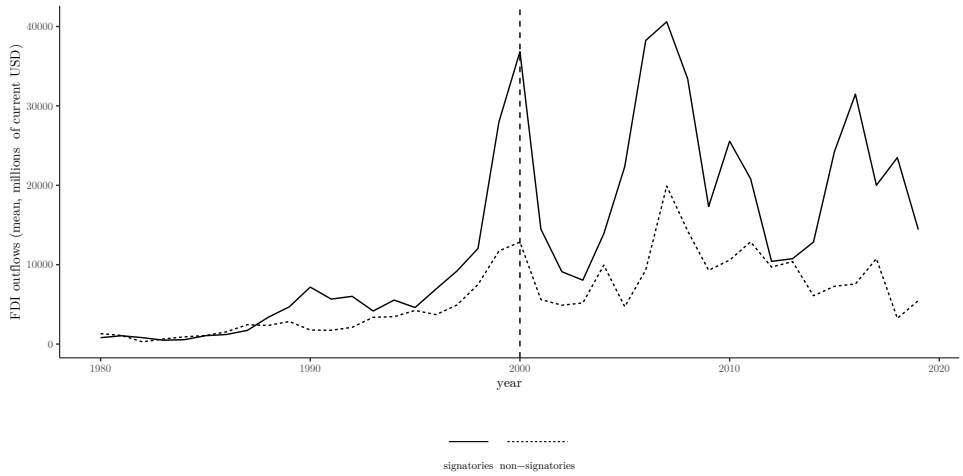


Figure 5: Trends of U.S. FDI outflows (as GDP %) over time, and U.S. GDP



(a) Signatory countries with entry into force of the Convention in 1999



(b) Signatory countries with entry into force of the Convention in 2000

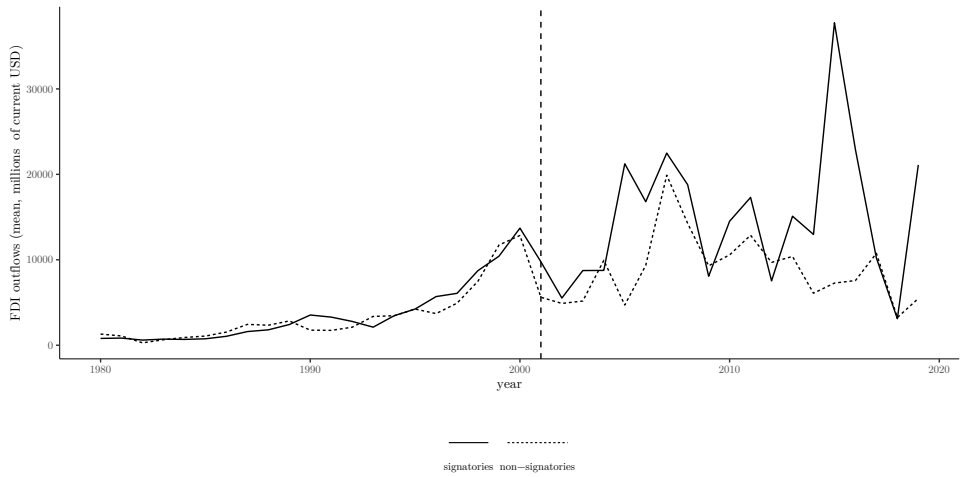


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Figure 6: Comparison of FDI outflows trends between signatories to the Convention in 1999 (a), 2000 (b), 2001 (c) and those that never signed it

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

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

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

Table 2: Country-level data. The effect of the OECD Convention on FDI outward flows. Difference-in-differences models

	Dependent variable:								
	FDI (GDP %)			FDI per capita			FDI (world %)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
OECD Convention	−30.56	−6.21	0.78	−14.35	−2.86	0.42	−0.18	0.14	0.24
	(27.75)	(6.89)	(0.96)	(12.64)	(3.25)	(0.28)	(0.34)	(0.31)	(0.30)
GDP (log)			−0.25			−0.60			0.31**
			(0.72)			(0.37)			(0.13)
GDP per capita			0.06			0.13*			−0.01
			(0.09)			(0.07)			(0.02)
GDP growth (%)			−0.01			−0.00			0.00
			(0.01)			(0.00)			(0.00)
Exports (goods and services)			0.00			0.00			−0.00
			(0.00)			(0.00)			(0.00)
Population			0.03			0.01			−0.05
			(0.08)			(0.03)			(0.10)
Country Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year Fixed Effect	✓	✓	✓	✓	✓	✓	✓	✓	✓
Country time trend		✓	✓		✓	✓		✓	✓
R^2	0.56	0.76	0.39	0.56	0.82	0.47	0.76	0.83	0.82
Adjusted R^2	0.54	0.73	0.34	0.54	0.80	0.42	0.75	0.81	0.80
Observations	5387	5387	4370	5387	5387	4370	5387	5387	4370
N. of countries	192	192	161	192	192	161	192	192	161
Note:									*p<0.1; **p<0.05; ***p<0.01