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# The Determinants of Cross-Border Corruption

Laarni Escresa and Lucio Picci \*

19 November 2019

An edited version of this paper is forthcoming in *Public Choice*

## Abstract

We reconsider the question of what determines corruption at the cross-national level, using new data – observations of occurrences of cross-national corruption - and methods. We find that economic development and a small population is associated with lower levels of corruption, as are freedom of the press, political rights, the presence of established democratic institutions, the salience of women’s role in society, and low exports of natural resources such as oil. The particular structure of the data also allows for the first time to consider the “relational aspects” of corrupt relationships, which come to the fore when parties to the corrupt transaction, the briber and the bribee, reside in different countries. Overall, we find limited evidence that the relational factors that we consider affect corruption, beyond the effects that they often have on bilateral trade.

**Keywords:** Corruption, Measures of corruption, Judicial statistics, Culture

**J.E.L. Codes:** H11, H50, D73, C18, C43, F53, F55

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## 1. Introduction

The determinants of corruption have been extensively researched using cross-national data, but there are reasons that justify addressing this question again. First, the available measures of corruption at the cross-national level have been widely criticized (Charron 2016, Donchev and Ujhelyi 2014, Klitgaard 2016, Knack 2007, Kurtz and Schrank 2007). Second, data on corruption cases at the cross-national level have recently been made available that can serve as an alternative measure (Escresa and Picci, 2017). As an added advantage, they allow to consider the relational aspects of corruption, a topic which has attracted little attention so far. Third, considering occurrences of cross-border corruption is interesting in its own right, given the relevance of the phenomenon.

Researches on cross-national corruption most often use perception-based measures of corruption, such as Transparency International's Corruption Perception Index (TI-CPI; Lambsdorff 1999; Saisana and Saltelli 2012; Transparency International 2012) or the World Bank's Corruption Control Indicator (WB-CCI; Kaufmann et al. 2009).<sup>1</sup> As both indexes result from the aggregation of differently defined indexes, what they exactly measure is not clear. More importantly, perception-based measures may be weakly correlated with actual experiences of corruption (Razafindrakoto and Rouband, 2010, Seligson, 2006, Olken 2009), and psychological mechanisms (Sherif and Cantril 1945) may make perceptions selective so as to merely confirm already existing expectations about country traits. There is a danger of 'echo chamber' effects, which is made more acute by the vast media coverage that measures such as the *TI-CPI* have received (Golden and Picci 2005). For instance, Picci (2018) illustrates that availability heuristics appeared to have influenced Transparency International's Bribe Payer's Index, contributing to a narrative of corruption seen as an element of "national culpability".<sup>2</sup>

We consider instead observed cases of cross-national corruption, defined as the bribery by a firm headquartered in a particular country of a public official in a foreign country. The United States (US), with the Foreign Corrupt Practices Act (FCPA) of 1977, was the first country to criminalize such behaviour. On 15 February 1999 the OECD Anti-Bribery

<sup>1</sup> See, for reviews, Lambsdorff (2006), Treisman (2007, 2015), and Klitgaard (2016).

<sup>2</sup> Victimization statistics, obtained from surveys aimed at eliciting information on bribes being paid, have been used more rarely in these studies. On their shortcomings, see Treisman (2007) and Kraay and Murrell (2016).

Convention came into force, requiring signatory countries to adopt similar legislations<sup>3</sup>. As a result, many cases of alleged cross-border corruption have been investigated around the world.

The validity of our choice of data and methods rests crucially on the “equal treatment assumption”, according to which the probability that a case emerges (in a given jurisdiction), once bribery occurs, does not depend on the identity or characteristics of the country where it took place. For example, if two firms, X and Y, both based in the USA, bribed one a public official in Nigeria, and the other, in Finland, we assume that the probability that these cases enter our analysis is roughly the same. We argue that such an assumption is plausible with an appropriate choice of the cases to be considered, and also following a series of robustness tests. It must be emphasized that our results obviously do not rest on the assumption that the strength of enforcement of the OECD Convention is even roughly the same around the world – which, as our data also shows, is certainly not the case.

We first provide a selective reading of the literature on the causes of corruption, and we then illustrate our data, methods, and results. In the concluding section, we interpret them also under the light of the debate on the “narratives of corruption”, with particular reference to the role played by the available measures of the phenomenon.

## **2. The determinants of corruption: a selective survey**

Probably the most solidly established result in the available literature indicates the negative impact on levels of corruption of economic development, as represented by GDP per capita. The likely presence of endogeneity, together with the difficulty of finding suitable instrumental variables, should be considered when interpreting this and other results (Serra 2006). The availability of abundant natural resources has been associated, albeit not conclusively, with higher levels of corruption (Ades and Di Tella 1999, Serra 2006, Treisman 2007).

<sup>3</sup> Brewster (2017) provides a convincing explanation on the main drivers of FCPA enforcement. For the determinants of the overall enforcement of the OECD Anti-Bribery Convention by signatories other than the US, see Kaczmarek and Newman (2011) and Choi and Davis (2014), on which we shall return. To date, 44 countries have signed the Convention, 8 of which are non-OECD countries

A factor that has not received much attention in the literature, but that instead we consider explicitly, is the size of polities. Mungiu-Pippidi (2015: 85) notes that the “size (of population) is not significantly associated with corruption today when all the states of the world are considered, although [...] limited population might have played a historical role in enabling collective action”. She also points out that most states that have become less corrupt in recent history have small populations. Knack and Azfar (2003) argue that a positive relationship between country size and corruption is present, but it likely arises from the presence of a selection bias. Size might affect perceptions of corruption because (everything else being equal) big polities tend to generate more cases of corruption, a possibility coherent with the finding in Escresa and Picci (2017) that more populous countries tend to be significantly less corrupt according to their measure of corruption based on actual reported corruption cases, the Public Administration Corruption Index (PACI), when compared to both the TI-CPI and the WB-CCI (see their Table 6).

Levels of corruption are influenced by the extent to which various institutions are able to constrain public officials from rent-seeking and opportunistic behavior. In particular, the public availability of information on corrupt exchanges affects the degree to which parties can be held accountable. Evidence shows that countries with greater freedom of the press have lower levels of corruption (Brunetti and Weder 2003, Chowdhury 2004, Treisman 2007). Curtailment of press freedom might take several forms (Freille et al. 2007, Kalenborn and Lessman 2013), such as the level of government spending on newspaper advertisements (Di Tella and Franceschelli 2011).

The effects of formal institutions on corruption have been extensively studied, and the role of democratic institutions has been found to be nuanced. Some find a nonlinear relationship between degrees of democracy and corruption (Montinola and Jackman 2002; Sung 2004), where the relevant conditioning factor is the initial level of democratization, or the level of economic development as in Charron and Lapuente (2010). Treisman (2007) and Keefer (2007) report that a long history of democracy leads to less corruption.

Other mechanisms by which public officials can be restrained are through the degree of electoral competition, and the extent that different branches of government can effectively exercise checks and balances. Persson et al. (2003) find that, in general, lower barriers to entry, as implied by larger electoral districts and by open electoral lists, are associated with less corruption. A presidential form of government might expand the scope for rent-seeking and

corruption, as in Kunicova and Rose-Ackerman (2005), while Gerring and Thacker (2004) find evidence that parliamentary systems, which arguably imply a higher degree of control on the executive branch of government, are less corrupt. Chang and Golden (2010) find that personalistic regimes lead to more corruption as compared to military and single party authoritarian regimes, possibly because the shorter time horizon of personalistic rulers incentivizes the creation of extractive institutions. On the other hand, studies that examine the relationship between political or fiscal decentralization and corruption have yielded mixed results, both theoretically and empirically (Fisman and Gatti 2002, Treisman 2007, Fan et al. 2008, Fredriksson and Vollebergh 2009, Goel and Nelson 2011)

Cross-country studies point to a negative relationship between the salience of women's role in society and corruption. Among the explanations offered are the higher standards of ethical and pro-social behavior displayed by women (Dollar et al. 2001), gender-biased socialization mechanisms of the "old boys' club" sort which exclude women from corruption networks (Swamy et al 2001), or the parallel development of institutions that both paved the way for more gender equality, and less corruption (Sung 2003).

The extent to which cultural factors might influence levels of corruption is the object of increasing attention. However, unpacking culture is complicated, and developing quantitative measures to express its different dimensions problematic. Paldam (2001) finds that religion affects levels of corruption, and in particular that reform Christianity is beneficial in this respect compared to other pre-reform strands of Christianity. Serra (2006) also confirms that countries that have a larger proportion of Protestants tend to be less corrupt, as does Treisman (2007), who however finds that including these factors in the analysis does not change significantly results on other variables of interest.

There is evidence that the behavior of agents in a foreign country is influenced by the habits and customs of their country of origin. For instance, tax evasion by foreign-owned firms in the US (DeBacker, et al. 2015), and even parking violations (Fisman and Miguel 2007) are found to be correlated with corruption levels in the agents' countries of origin. On the other hand, Picci (2018), using a dataset very similar to the one of this paper, does not find that firms headquartered in more corrupt countries have a higher propensity to bribe abroad, once certain control variables aimed at capturing the opportunity to corrupt are taken into consideration.

Our research also relates with a few works that used gravity models to study the relationship between corruption and patterns of trade. Dunlevy (2006) explores how immigrant

networks facilitate trade with their country of origin, possibly also because of the advantage that they might have in navigating corrupt bureaucracies. Immigrants networks are found to be more useful if the language in the home country is different, and institutions are not similar. Dutt and Traca (2010) also use a gravity model, to explore whether bribery of customs officials hinders bilateral trade by acting like a tax, or enhances it through the avoidance of tariff barriers. They conclude that in a majority of cases corruption serves as an obstacle to trade, but that in countries with high tariff barriers the marginal observed effect is in fact positive. Using bilateral investment data, Habib and Zurawicki (2002) find that levels of FDI are negatively correlated with levels of corruption in the host country, and positively correlated with the absolute difference between levels of corruption between the home and the host country. These studies however only partially relate to the present one, which to the best of our knowledge does not have antecedents in explicitly considering, in a cross-country setting, relational variables as determinants of corruption.

### **3. The data on corruption cases**

We use an updated version of the dataset illustrated in Escresa and Picci (2017), covering the years from 2000 to 2014. It documents reported cases of cross-border corruption involving firms in a “headquarters country” (which we also indicate with the shorthand HQ), and public officials in a “foreign country” (FO). Since a single legal case or enforcement action lodged against one firm may involve more than one corrupt transaction, we treat each event as a separate case. Cases are coded according to the observed outcome: “positive” if the accused party was either found to be guilty or, while not admitting guilt, accepted to pay a fine (as in a non-prosecution or deferred prosecution arrangement in the US); “not positive” if the case was eventually dropped or ended up in an acquittal; or “ongoing” if no available evidence was found to determine it as “positive” or “not positive” (see the Data Appendix for more details on the dataset).

In the fifteen-year period, we observe a total of 1095 cases, irrespective of their outcome. We recorded information on where enforcement of the case occurred first: either in the headquarters country (627 cases), in a third country jurisdiction (271 cases, with the US acting as the third country jurisdiction in 172 of them), or in the foreign country (127 cases). For the purpose of our analysis we use two subsets of these cases: the wider one is comprised of the 898 cases that were first enforced either in the headquarters country, or in a third country



jurisdiction. The smaller subset only includes the 271 cases that were first enforced in a third country jurisdiction.

**[Table 1 about here]**

Table 1 describes the wider of the two subsets, showing the number of cases by headquarters country (top part) and by the foreign public official's country (bottom part). Of a total of 898 cases, 503 are classified as positive, 288 as ongoing, and the rest as not positive. Firms are headquartered in 42, mostly industrialized, countries. First in the list is the US, reflecting its economic relevance, its early adoption of the FCPA, and the proactive stance taken by the Department of Justice and the Securities and Exchange Commission. United Kingdom, Germany and France follow in the list. The set of countries in which public officials are at the receiving end of alleged bribes is much wider, with at least one case recorded in a total of 134 countries. China leads the list, with 106 cases, followed by Nigeria, Russia and India.

The top panel of the table also shows the list of headquarters countries that are responsible for about 97.5% of the total number of cases, with the numbers in parenthesis indicating cumulative percentages. Just two countries – the US and the UK – make up for half of the total number of cases. Overall, the dataset includes all countries for which at least one case has been observed in the period under consideration, for a total of 5596 pairwise observations.

**[Table 2 about here]**

Table 2 permits to better appreciate the rareness of corrupt events in the dataset. Approximately 92% of pairwise observations are zeros – or 96%, when considering cases that were first enforced in third country jurisdictions. Those that are not, most often indicate that only a single case has been observed for a given pair of countries, with the sporadic occurrence of higher frequencies. In the next section, we explain why it is of fundamental importance in our analysis to consider these two subsets and exclude those 127 cases that were first enforced in the foreign country.

#### **4. Modeling bilateral corruption transactions**

Our estimation strategy observes determinants of corruption of public officials in the foreign country using “observation points” elsewhere (the headquarters countries and/or third country

jurisdictions). In order to do this, we only include cases which were first enforced either in the headquarters country, or in third country jurisdictions. *We crucially exclude those cases that were enforced first in the foreign country where the actual bribing allegedly took place.* By excluding them, we control for the varying propensity of foreign countries to act upon occurrences of corruption involving their own public officials. In different words, it is not the number of cases first enforced in a given jurisdiction, but their *geographic distribution* that is considered to be informative of levels of corruption *outside of that jurisdiction*.

Such a consideration, however, hinges on the “equal treatment assumption”: that a given jurisdiction (which is not the foreign country) acts (as a first enforcer) upon a given corrupt transaction involving firms from country  $i$  and public officials in country  $j$ , with a probability which does not depend on the identity of the foreign country  $j$ .<sup>4</sup>

Since the US is responsible for much of the total information available (about 42% of the non zero observations - see Table 1) a simplistic model to research the determinants of corruption would focus only on the cases first enforced in the US and involving generic countries  $j$ :

*Simplistic model:*

$$\text{cases}_{USj} = \alpha_1 \dots + \dots \beta_1 \cdot X_j^1 \dots + \dots \beta_k \cdot X_j^k \dots + \dots + \delta_1 \cdot Z_{USj}^1 \dots + \delta_q \cdot Z_{USj}^q + \dots \epsilon_{jt}$$

where  $X_j^1 \dots X_j^k$  represent  $k$  characteristics of the foreign country  $j$ , whereas  $Z_{USj}^1 \dots Z_{USj}^q$  are variables expressing  $q$  relational concepts, such as distances and trade flows between the US and country  $j$ .

The obvious shortcoming of such approach would be that, in only considering the US as the “point of view” of the data, it would discard all cases – around 58% of the total - involving firms not headquartered in the US. To overcome such limitation cases involving all  $i$  headquarters countries ( $\text{cases}_{ij}$ ) might be pooled together. The *pooled model*, which is the one

<sup>4</sup> Please note: we refer to the probability that a corrupt transaction emerges *once it has occurred*. On the other hand, the *total number of corrupt transactions* observed in a given foreign country obviously depend on that country’s characteristics. The “equal treatment assumption” corresponds to Assumption 1 in Escresa and Picci 2017, Appendix A, where its role is considered in guaranteeing the validity of their measure of corruption.

that we adopt, also includes  $\text{dummies\_HQ}_i$ , country dummies that control for varying levels of judicial activism.

*Pooled model:*

$$\text{cases}_{ij} = \alpha_1 \dots + \dots \beta_1 \cdot X_j^1 \dots + \dots \beta_k \cdot X_j^k \dots + \dots + \delta_1 \cdot Z_{ij}^1 \dots + \delta_q \cdot Z_{ij}^q + \sum_i \rho_i \cdot \text{dummies\_HQ}_i + \epsilon_{ij}$$

In order to take into account the rareness of corrupt events between two pairs of countries, resulting in many zero observations (see Table 2), we adopt the Poisson estimator, with errors clustered by country pairs. The Poisson estimator has been shown to be appropriate in such cases, notwithstanding the high frequency of zeros (see Silva and Tenreyro, 2011), with the advantage of providing results that are invariant to the scale of the dependent variable (unlike, for example, the negative binomial model).<sup>5</sup> One further advantage of its use with the present data is that the estimated coefficients can be interpreted as elasticities of the impact of the regressors on levels of corruption. On the other hand, values of perception-based indices do not correspond to known levels of corruption, so that when using them, the estimated coefficients are not easily interpreted (see Escresa and Picci, 2017).

<sup>5</sup> Use of the Poisson estimator in datasets with a structure that is similar to ours is common in the international trade literature (following Silva and Tenreyro, 2006). The known presence of convergence problems (see <http://personal.lse.ac.uk/tenreyro/lgw.html>) led us to use the Windmejer and Silva (1997) version of the estimator – as implemented in the PPML routine in *Stata*. The occurrence of zeros in the dependent variable might have suggested the use of a zero-inflated formulation of the Poisson model. However, the determination of the presence of corrupt exchanges between two countries, *vis a vis* their intensity, do not seem to be two logically distinct problems, as is somehow implied when using such empirical model. Results might suffer from forms of endogeneity, which is notoriously difficult to treat because of the dubious validity – or strength – of a rather long list of candidate instruments that have been proposed. See Treisman (2007) for IV results using perception-based (and also, experience-based) measures, and for some comments on the broader issue of finding suitable instruments.

## **The equal treatment assumption**

The soundness of our inferential analysis crucially depends on the plausibility of the “equal treatment assumption.” Excluding from the analysis those cases that were first enforced in the foreign country should control for the different levels of judicial activism there. Additional reasons however allow to argue in favour of the plausibility of the equal treatment assumption.

First, note that the assumption is not directly testable, simply because the true number of corrupt transactions is not observable.<sup>6</sup> Escresa and Picci (2017) provide evidence that the differences between their index of corruption, which is based on a dataset very similar to the present one (and which also excludes those cases that have been enforced first in the foreign country) and the prevailing perception-based measures of corruption, are not driven systematically by the characteristics of the foreign country. This might be interpreted as an indicator of equal treatment, conditional on those perception-based measures not suffering from the same bias.

Cases first enforced in the headquarters country may likely emerge (or not) depending on its characteristics – its judiciary, the availability in the headquarters country of relevant information on firms, among others. On occasions, information generated in the foreign country, may not result in a local inquiry, but might spur legal actions in a different country, which would then act as first enforcer. However, while an accurate analysis in this respect of all the cases considered for the purpose of the present study would represent a daunting task, in the painstaking work that led to the building of our dataset, we did not encounter any such case. It might also be argued that the degree of collaboration between the foreign country’s judiciary and that of the headquarters country might have an impact in determining the *outcome* of a case.

<sup>6</sup> The determinants of the enforcement of the Convention (or of the FCPA) are not directly relevant and beyond the scope of our study. See for instance, Kaczmarek and Newman (2011) on how FCPA prosecution of non-US corporations might have pushed foreign countries to better comply with the Convention, and Brewster (2017) on how US compliance with the FCPA increased following the adoption of the Convention. Choi and Davis (2014), on the other hand, present an analysis which is *conditional* on FCPA enforcement, focusing on the level of sanctions.

However, they are arguably less important when the focus is on its mere *beginning or discovery*. For this reason also we present results based on all cases, irrespective of their outcome.<sup>7</sup>

On this count, we acknowledge that many cases, particularly in the US, are self-reported by firms. The equal treatment assumption would be violated if firms exhibited a higher propensity to self-report when acting in foreign markets where they perceive a higher risk of being caught, which could depend on the degree of freedom of the press and on civil liberties in those foreign countries. However, if this were relevant, we'd expect to find that these variables *positively* influence (detected) levels of corruption while, as we will illustrate, the opposite is true. We also carry out the analysis excluding all cases first enforced in the US, where self-reporting is arguably more important, to find results similar to our preferred ones.

The exclusion of all cases first enforced in the US – both regarding firms headquartered there, and elsewhere – also addresses two further possible departure from the equal treatment assumption. First, we consider the possibility of so-called industry “sweeps”: the targeting of an entire industry by prosecutors, suspecting the presence of an industry-wide pattern of wrongdoing. Inasmuch as such industries interact with foreign countries unequally, such action would again imply a departure from the equal treatment assumption. Also, we acknowledge the possibility of “country sweeps”, if prosecutors target firms because they are doing business with a particular country, possibly because they believe that it is affected by a pattern of wrongdoing. Arguably, only the US has a number of cases big enough for such broad strategies to be of possible relevance. Again, as mentioned above, we also carry out our analysis excluding all cases first enforced in the US, to find results similar to our preferred ones.<sup>8</sup>

We also acknowledge the possibility that the decision to act as a third country enforcer might be negatively related to the foreign country's level of judicial activism. A given jurisdiction might be compelled to initiate an enforcement action involving firms headquartered

<sup>7</sup> Several factors influencing the way that cases are judged, together with the criterion of presumption of innocence, would likely lead to many false negatives, thus providing a further justification for considering cases regardless of their outcome (that is, including acquittals).

<sup>8</sup> We cannot rule out the possibility that countries co-ordinate to carry out such industry-, or country-, “sweeps”, but we are not aware of any evidence pointing to the presence such complex form of international coordinated action. We are grateful to Matthew Stephenson for pointing out this and other possible departures from the equal treatment assumption.

in another country if it has the impression that it would go unchecked otherwise. Excluding all cases first enforced in the US, which is responsible for most of the third country enforcement, should address this possibility.

The claim that relational characteristics (between the headquarters and the foreign country) may invalidate the equal treatment assumption would hold even less water, if we limit our attention to cases first enforced in third country jurisdictions (as we also do). For example, if the headquarters country and the foreign country have a long habit of reciprocal interactions – possibly because one was a colony of the other (a case which however we will explicitly control for) - the probability that an occurrence of corruption is detected in the latter might be higher than otherwise. But the same relational characteristic arguably would not affect the probability of detection of a case *in a third-country jurisdiction*.

It should also be noted that the mode of discovery of cases was not just a result of deliberate anti-corruption efforts by law enforcement agencies in the firm headquarter countries. Some of them emerged in the conduct of investigation from other potential offenses such as corporate fraud, while others are discovered following the action of whistleblowers. Also, many of the judicial cases that we consider generate multiple observations, because a given firm allegedly paid bribes in more countries. The heterogeneity of the modes of discovery, and the frequent presence of multiple observations within a single overall corruption case, addresses concerns that cases arise due to the selective enforcement of governments, either as part of a broader international policy, or as driven by perceptions of corruption.

Last, and notwithstanding all the previous arguments, an *a priori* knowledge of the likely mechanisms that in principle could invalidate the equal treatment assumption might indicate the direction of the resulting bias. For example, it might be argued that more freedom of the press in the foreign country could increase (but not decrease) the probability that a case surfaces in the media of the foreign country, and that it then makes its way to the home country's judiciary, which would act as first enforcer. For this reason, we would have good reasons to be suspicious if the results indicated that more freedom of the press is associated with more corruption. But if the opposite result emerges, as it does, then at most we could suspect that the true effect is even greater than the estimated one.

### **Recursive coefficient estimates**

We consider cases involving firms headquartered in the 25 countries listed in the top part of Table 1, accounting together for 97.55% of all cases observed, and we discard the cases (2.45%

of them) originating from firms headquartered in the remaining 17 countries – each of which contributed to fewer than 4 cases during the fifteen year period under consideration. We leave out those small countries, and the related few cases, because when they are also considered, their very pronounced infrequency in some occasions precludes the Poisson estimator to converge (also see note 5). This must have a very modest effect on our results. To prove this, and also to show the overall soundness and appropriateness of the pooling of the “simplistic model” discussed earlier, we estimate recursive coefficients using a simple baseline model, where the number of cases is explained by means of two regressors only (and the constant): bilateral logged exports originating in the headquarters country ( $\text{Ln}(\text{exports})$ ), which are meant to represent the amount of bilateral transactions between couples of countries that are vulnerable to corruption, and (logged) per capita income in the foreign country in 1999, ( $\text{Ln}(\text{gdp cap}) \text{ FO}$ ), that is, the year before the beginning of the time period covered by the data on occurrences of corruption. We focus on the estimated coefficient of the latter, representing the effect of income on corruption, with the purpose of observing how it changes when we estimate the model many times, progressively adding more “observation points”, *i.e.*, headquarters countries.

*Base pooled model:*

$$\text{cases}_{ij} = \alpha_1 + \beta_1 \cdot \text{Ln}(\text{exports})_j + \beta_2 \cdot \text{Ln}(\text{gdp cap}) \text{ FO}_j + \sum_i \rho_i \cdot \text{dummies\_HQ}_i + \epsilon_{ij}$$

We start by estimating this model with only the US as the headquarters country, which alone contributes to 41.6% of the total number of cases (Table 1; note that in this case, the base pooled model coincides with the “simplistic model” above). Then we include the second largest contributor, the UK, (that is, we estimate the pooled model while considering only two headquarters country), and then Germany, and so on, including one country at a time, to eventually include all 42 countries, that is, all observations of occurrences of corruption (first enforced either in the headquarters country, or in third country jurisdictions) that we might use.

We focus on  $\beta_2$  as our coefficient of interest, representing the impact of logged per capita GDP on levels of the observed occurrences of corruption. In the end, we had 42 estimates of the coefficient of interest, shown in Figure 1 together with 95% confidence intervals. From left to right, each one is computed by gradually including more headquarters countries as “observation points”. The estimated  $\beta_2$ s are always negative and significant, and they change only modestly as more headquarters countries - and information on cases - are included. In particular, we

observe that the estimated coefficient of interest does not change in any appreciable way as we add the last countries, whose firms overall contribute only few of the non-zero observations of corruption.

**[Figure 1 about here]**

The stability of the results, as we move from the single observation point of the simplistic model - the left-most value of the estimated coefficient in Figure 1 – also suggests that the headquarters country dummy variables of the pooled model adequately control for the varying levels of those countries’ judicial activism in prosecuting cases of cross-border corruption.

We carried out the same exercise only looking at cases that have been first enforced in third country jurisdictions. Most of these cases (172 out of a total of 271) were judged in the US, and involved firms headquartered elsewhere. Swiss firms represent 15.9% of the cases in this category, followed by France, the UK, and the US. Recursive coefficients for this exercise are rather stable, as Figure A1 (in the Appendix) indicates.

## **5. Estimation results**

We now turn our attention to the estimates of the pooled model, which are shown in Table 4 and 5. Table 3 shows, for the reference year 2005, pairwise correlations between the continuous variables that are used as explanatory factors (for details, see the Data Appendix).<sup>9</sup> Our choice of variables is necessarily selective, considering the numerous possible determinants identified in the literature. To the extent that it is possible, we follow the choices of Treisman (2007).<sup>10</sup> We

<sup>9</sup> For several of them we observe a high degree of association which might lead to multicollinearity. In interpreting the signs of the correlation between variables, attention should be paid to how they are defined (see the Data Appendix). For example, for *Democracy*, higher values correspond to “more”, whereas for *Freedom of the press* the opposite holds.

<sup>10</sup> Treisman (2007) also considers a series of control variables which represent historical characteristics of countries, such as their legal origin or colonial past. He finds that they do not influence the qualitative results on the other variable of interests. Also, the abundance of fixed effects in our model creates problems in identifying too many time-invariant variables – an issue



read results by also considering the case when the dependent variable only includes cases first enforced only in third country jurisdictions, as shown in Tables A4 and A5 in the Appendix, and we refer to them only when those results diverge in meaningful ways from those of Table 4 and 5.

Column 1 of Table 4 shows results for the base model, the same whose recursive estimates are shown in Figure 1. The estimated effect of logged per capita GDP (-0.615) corresponds, in Figure 1, to the circle on the right-hand vertical line. Logged per capita GDP is significant in most specifications. The log of bilateral exports from the headquarters to the foreign country are always highly significant, with elasticities that in most estimates are below one half, which is sensibly less of what is usually found when estimating gravity models of trade (Disdier and Head 2008). Logged population has a positive effect, which is statistically significant in most specifications, consistent with some of the considerations in Mungiu-Pippidi (2015: 85).<sup>11</sup> The estimated effect is sizeable, particularly in the results of Table 5, where the estimated elasticity is as high as 34%.

Escresa and Picci (2017) find that populous countries appear to be less corrupt according to the PACI when compared with the leading perception-based measures of corruption. Such finding on the one hand is coherent with a situation where the perceptions of corruption are *positively* related with population size, and on the other, offers indirect support to the authenticity of the positive country size effects on levels of corruption that we report. The positive effect of logged population is also detected in most (but not all) cases when we limit our attention to cases first enforced in third country jurisdictions only – see Table A4 and A5.

Political rights, freedom of the press, newspaper circulation, and the age of democracy (“Democratic since 1950”) lead to less corruption. The individual coefficients are statistically significant in most specifications, notwithstanding the degree of collinearity among the variables that emerges from Table 3. Overall, the beneficial effects of these proxies for democracy and openness, which are coherent with much of the extant literature (see, among others, Treisman 2007), are one of the clear-cut results emerging from our research.

which is familiar in the international trade literature.

<sup>11</sup> Our results do not suffer from the sample bias suggested in Knack and Azfar (2003), since the availability of the dependent variable is not conditional on levels of corruption.

We find that presidential democracy tends to lead to more corruption, as in Kunicova and Rose-Ackerman (2005). In the results of Table 4 (but not of Table A4) we find that open-list electoral systems are associated with more corruption, which is the opposite of what emerges in Persson et al. (2003). We do not find evidence pointing to any effect of district magnitude on less corruption, unlike Chang and Golden (2006), nor of a pure plurality systems.

Please note that when considering the previous four variables, the analysis is conducted on a significantly smaller subset of countries. The same applies to the next characteristic of governance that we consider, namely, a measure of decentralization. In the results of Table 4 (but not of Table A4, which only considers cases first enforced in third country jurisdictions) we detect a significant positive effect of decentralization. Note however that the inclusion of this variable results also in the loss of significance of the estimated coefficient of logged population. Size of polity is rather highly correlated with our measure of decentralization (the correlation coefficient is slightly above 0.5), so prudence is needed when interpreting those two estimated coefficients individually.

We also include some variables to capture characteristics of economic governance. We do not find any significant effect of economic openness, as captured by the share of imports on total product, nor of the variable “Years opened to trade” and of “time to open a firm”. On the other hand, we find that countries exporting more oil tend to be associated with more corruption, as in Treisman (2007).

Last, countries where the share of women among members of parliament is higher tend to be associated with less corruption, confirming results that have been reported in the literature.

In Table 5 we also consider estimates of models that include relational variables. We omit variables expressing characteristics of democracies, first because we desire to focus on the wider possible set of observations, and also because we found that for the most they were not significant. Geographic distance is not significant in the results of Table 5, but appears to have a negative effect in some of the specifications shown in Table A5. Geographic contiguity between countries is never found to be significant. In interpreting these results we should keep in mind that bilateral exports, which are strongly influenced by distance, appear among the regressors. A null effect of the distance variable would lead us to conclude that there is no evidence that cross-border corruption cases decay with distance faster, or more slowly, than bilateral trade. A *negative* effect – which we detect in some specifications of Table A5 – would imply on the other hand that corruption transaction suffer from geographic distance more than bilateral trade - or, differently worded, that the “transportation cost” of bribes is higher than that of traded goods.

The presence of a former colonial link is found to have a positive and significant effect on corruption in most specifications. This result should be interpreted under the light of what we know about the influence of past colonial links on bilateral trade flows. Head et al. (2010) estimate gravity models to find that past colonial links positively affect bilateral trade, but that such an effect has weakened over time. So, the positive effect of the colonial link on cross-border corruption cases that we detect indicates that former colonial links have a disproportionate effect on those case, that is, even after controlling for bilateral trade. However, once we only consider cases first enforced in third country jurisdictions, while the effect of colonial links on corruption is always estimated to be positive, it is never statistically significant.

We consider some more variables that are meant to capture cultural proximity of the headquarters and the foreign country. We do not consider the presence of a common language, since we surmise that in the types of corrupt transaction that we observe, potential bribers are (self-)selected so as to be able to communicate in the country where they operate. We however also adopt a widely used measure of overall cultural proximity which is *language proximity*, as in Fearon (2003). We also consider two different measures of proximity of religious attitudes. *Religious proximity* is the probability of meeting a person of the same religion, computed on the whole population *Religious attitude proximity* is the probability that a religious person encounters another religious person, regardless of their particular faith. We find some evidence that the religious proximity variables might have a positive effects on the number of observed cases of corruption, but overall our results indicate little statistical significance of the estimated coefficients on these “cultural” variables.

### **Robustness of results**

We have seen that restricting our attention to just those cases that were first enforced only in third country jurisdictions (Tables A4 and A5) provides results that are very similar to those that also include cases first enforced in the headquarters country (Tables 4 and 5), even if the two datasets differ significantly (898 vs. 271 observations – see Table 1).

The presence of any departure from the “equal treatment assumption”, on which our results hinge, would possibly affect different countries (taken as “observation points”) differently. Stability of results as we consider different sets of headquarters country as observation points provides indirect evidence supporting the equal treatment assumption. We already commented upon the stability of the recursive coefficients of the base model of Figure 1, and A-1.

As a last exercise, we consider the possibility that the US as an enforcer of the OECD convention is an outlier of sort, considering its early adoption of the FCPA. We estimate all models of Table 4 and 5 also excluding all cases first enforced in the US – both involving firms headquartered there, and when acting as a third country jurisdiction. With few exceptions, results (Table A4-b and A5-b) change modestly.

## **6. Discussion and conclusions**

In this paper we have presented new evidence on the determinants of corruption and offered two main contributions. First, we proposed a new route to estimate the determinants of corruption at the cross-national level, measured as occurrences of cross-border bribery. By adopting an appropriate estimation strategy, we obtained results that do not suffer from the well-known shortcomings of other measures of corruption at the cross-national level. Moreover, for the first time in a cross-country context, we were able to explore the extent to which relational factors between pairs of countries may facilitate or hinder corrupt transactions.

We find that per capita GDP has a negative impact on corruption. Older democracies tend to be less corrupt; freedom of the press, the salience of women’s role in society, and the overall extent of political rights are associated with less corruption, while the opposite holds for presidential systems. Of the variables meant to capture characteristics of the economic system of countries, exports of oil favor corruption, a result can be interpreted as supportive of the so-called “natural resource curse”. These results are not unlike those that have been found in the extant literature.

The concept of corruption that we employ is precisely defined, and it is certainly narrower than the vague concept underlying perception-based indicators. A focus on cross-border corruption, like ours, is justified by the relevance of the phenomenon, which often involves important contracts of high value made by prominent multinational corporations. However, when applied to corruption at large, our results pose obvious issues of external validity. Public officials may respond differently when dealing with representatives of foreign – vs. national - firms. Also, in deciding whether to offer a bribe, representatives of firms abroad might react to the characteristics of the local context differently from local actors. There is however evidence indicating that firms doing business abroad tend to mimic their local counterparts. For example, Hellman, et al (2002), using data from the Business Environment and Enterprise Performance Survey (conducted by the World Bank and the EBRD; see <http://data.worldbank.org/data-catalog/BEEPS>), show that foreign firms are as likely as domestic

firms to pay kickbacks, and results in the same spirit might also be found in Gueorguiev and Malesky (2012) and in Soreide (2006).

We believe however that comparisons of our results with the extant ones should consider the broader debate on corruption and its determinants. Considering the intrinsic difficulties in measuring corruption, it is puzzling that perception-based measures have been used so widely and nonchalantly, unfortunately also when definitely they should not - as when they are meant to measure changes *in time* (vs. in space) of corruption (see the arguments in Escresa and Picci, 2016). Experimentation with different measures represents an important research agenda, aimed at a better understanding both of their properties, and of the phenomenon of corruption in general.

For the first time we presented in a cross-country context an analysis of the effects of relational factors on corruption. We interpreted results while considering that the same variables might influence bilateral trade flows, which we also include as an explanatory variable. We found scant evidence that the different concepts of country distance that we considered influence corruption “flows” differently from how they might affect bilateral trade, with the exceptions of the two variables representing religious proximity, and of past colonial ties, whose significance however might have more than one explanation. In terms of their determinants, corrupt cross-border transactions don’t appear to be very different from trade transactions *tout-court*.

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## DATA APPENDIX

**Corruption data.** Version of the dataset used: 3 May 2019. Collection of reported cases of cross-border corruption first used in Escresa and Picci (2017) .<sup>12</sup> Sources: Trace International Compendium (<http://www.traceinternational.org/compendium>), several US DOJ and SEC documents, OECD (various years), and other databases and publications, such as Shearman and Sterling 2013, Transparency International 2009 and 2013, Cheung et al. 2012, and Choi and Davis 2014. We cross-checked information also using other news sources, among them the Wall Street Journal Risk and Compliance Journal (<http://www.wsj.com/news/risk-compliance-journal>), and also corruption blogs such as the “FCPA Blog” <http://www.fcpablog.com/>. Cases reported in multiple sources were laboriously consolidated to avoid double counting. The reference period for each case is the year when the bribe was allegedly paid, but in some instances this date had to be presumed from the available data. The term *public official* is used in a broad sense, encompassing both bureaucrats and politicians. Cases where corruption occurs in more than one country are recorded as separate. On the other hand, if more than one bribe is allegedly paid by a firm in a single country within the same occurrence of corruption, only one case is recorded. Cases where the briber is a person (not acting on behalf of a firm) are excluded, as are all the cases pertaining to the Iraq’s “Oil for Food” affair, because of their peculiar characteristics. In the occurrences where more than one jurisdiction took action on a given case, an accurate reading of the available evidence allowed to single out the jurisdiction where the case was first enforced, that is, where it first emerged.

**Colonial link.** Indicates whether two given countries have ever been a colony of the other in modern times. Source: Head, et al (2010).

**Contiguous.** A dummy variable indicating the presence of a common border between pairs of countries. Source: Mayer and Zignano (2011).

**Democratic since 1950.** Dummy variable that indicates whether a country has been an electoral democracy since 1950 based on the classification by Beck, et al (2001). Source: Treisman (2007).

<sup>12</sup> Giana Mildred, Santos Lim and Lorenzo Crippa contributed to different updates of the dataset.

**Distance.** The distance between the capital cities of any two given countries. Source: Mayer and Zignago (2011).

**District magnitude.** Measure of the magnitude of an electoral district using the average number of representatives elected from each electoral district. Source: Beck, et al (2001) as cited in Treisman (2007).

**Exports.** Exports between any two given countries. Source: United Nations COMTRADE bilateral import/export data, as organized by the Center for International Data (Available at [http://cid.econ.ucdavis.edu/Html/WTF\\_bilateral.html](http://cid.econ.ucdavis.edu/Html/WTF_bilateral.html), last accessed on 22 May 2019).

**FH press freedom.** Measure of press freedom based on an evaluation of the legal environment, political and economic factors that contribute towards media independence and access to news and information. Source: Freedom House

**Fiscal decentralization.** Indicators of fiscal decentralization as defined in Fisman and Gatti (2002) which is the share of subnational government spending from total spending of all levels of government. Source: Government Finance Statistics, International Monetary Fund as cited in Treisman (2007).

**Fuel exports.** Share of fuel in exports for a given country. Source: Treisman (2007).

**GDP per capita.** Year 1999. Measured in current international dollars, PPPSource: The World Bank.

**Imports % GDP.** Share of imports out of GDP. Source: Treisman (2007).

**Language proximity.** Data from the Ethnologue Project (<http://www.ethnologue.com/>), as collected and organized by James Fearon (Fearon, 2003). The similarity between two languages is based on the distance between “tree branches” (“for example [...] Byelorussian, Russian and Ukrainian share their first three classifications as Indo-European, Slavic, East Branch languages”; Fearon, 2003). Unlike in Fearon’s work, who obtains his measure by dividing the number of branches that are in common by the maximum number of branches that any language has (which is equal to 15), we divide it by the maximum number of branches within each couple of language, so as to take into account that the granularity of the branch definition may be not the same across languages.”) See also Picci 2010, from which the previous description is taken.

**Newspaper circulation.** The number of newspapers in circulation conditional on the level of democratic liberties for a given country. Source: Adsera, et al (2003) as cited in Treisman (2007)

**Open list system.** Indicates whether a country has an open or a closed list system. Source: Beck, et al (2001) as cited in Treisman (2007).

**Political rights.** Extent of political rights that exist for a given country or territory. Source: Freedom House as cited in Treisman (2007).

**Population.** Population in a given country or territory. Source: IMF-World Economic Outlook October 2018.

**Presidential dem.** Treisman's (2007) measure of presidentialism following Beck's (2001) classification and where countries with FH scores below 5.5 are assigned a value of 0. Source: Treisman (2007).

**Pure plurality sys.** Indicates whether electoral rules in a given country is based on plurality where the most number of votes win (vs majority rules). Source: Beck, et al (2001) as cited in Treisman (2007)

**Religious attitude proximity.** Probability that a religious person in country  $i$  encounters another religious person in country  $j$ , regardless of their religious membership and affiliation: product of shares of religious persons with respect to the whole population. Source: Maoz and Henderson (2013)

**Religious proximity.** Probability of a person in country  $i$  meeting another person in country  $j$  who belong to the same religion: products of shares of persons with the same religion with respect to the whole population. Source: Maoz and Henderson (2013)

**SD inflation.** Measure of variability of inflation based on the annual variance of monthly inflation. Source: Braun and di Tella (2004), as cited in Treisman (2007)

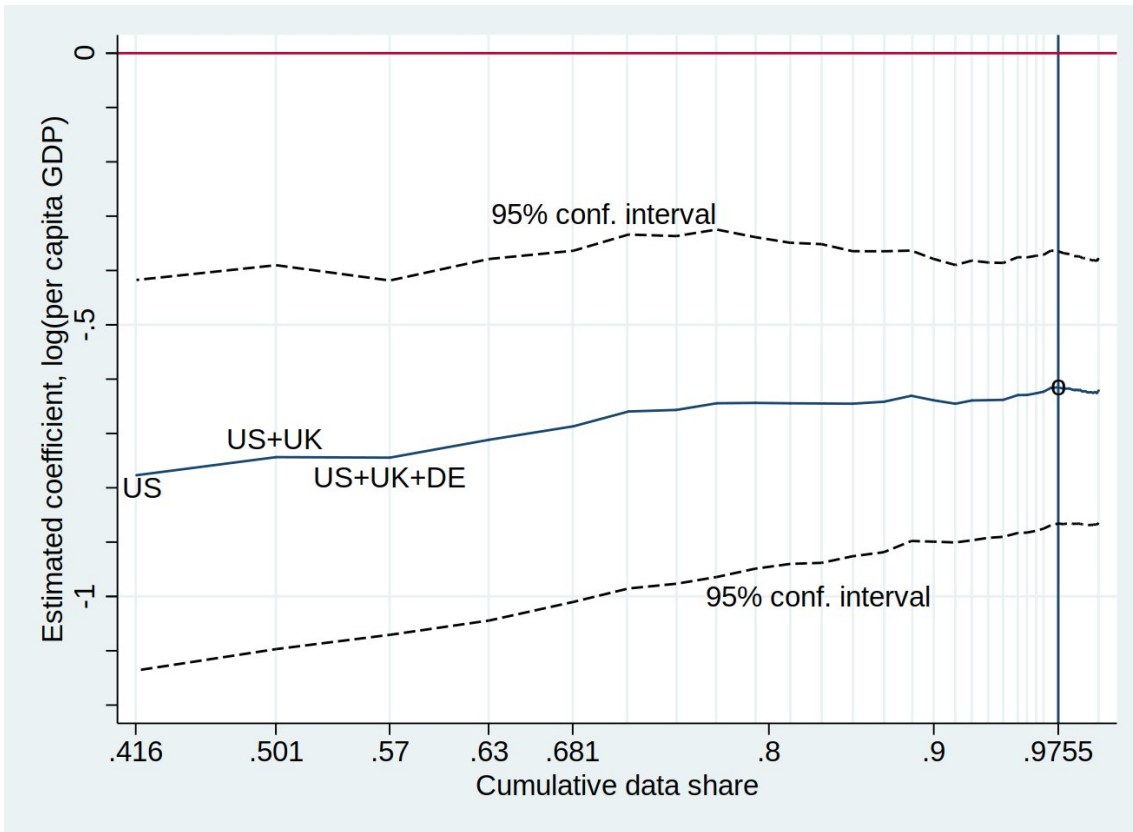
**Time required to open a firm.** Time required to complete the regulatory process of a starting a firm. Source: Djankov, et al (2002) as cited in Treisman (2007).

**Women in government.** Share of women in parliament (lower legislature). Source: Inter Parliamentary Union, as cited in Treisman (2007)

**Year opened to trade.** A variable that indicates the year in which a country opened itself to trade based on Sachs, et al (1995) classification. Source: Treisman (2007)

## Figures

**Figure 1. Estimated impact of GDP per capita, base model. Recursive coefficients.** Cases enforced first in the headquarters country and in third country jurisdictions.



Note: Point estimates (continuous line) of the coefficient on the per capita income in the base model, together with 95% confidence interval, as additional countries are added. The left-most estimate only includes the US (representing 41.6% of cases), then the US and UK together (representing 50.1% of observations – see Table 1), etc. The thick vertical line represents data coverage (97.55% of total number of observed occurrences) used for main results of paper.

**Tables. Table 1. Summary of cross-border corruption cases**

	Total Cases ( cumulative % coverage)	Positive Cases	Ongoing Cases
Number of cases:	898	503	288
<b>By headquarters country:</b>			
United States	374 (41.6)	254	82
United Kingdom	76 (50.1)	36	30
Germany	62 (57.0)	29	33
France	54 (63.0)	31	21
Switzerland	46 (68.1)	38	6
Italy	30 (71.5)	6	18
Canada	26 (74.4)	6	11
Spain	22 (76.8)	1	7
Australia	21 (79.2)	9	11
Brazil	19 (81.3)	9	9
Netherlands	17 (83.2)	10	5
Japan	17 (85.1)	12	4
Sweden	17 (87.0)	3	12
Korea	15 (88.6)	15	0
Portugal	12 (90.0)	0	6
China	12 (91.3)	7	4
Argentina, Norway	9 each (93.3)	6	8
Austria, Denmark	8 each (95.1)	7	9
Finland, Israel	5 each (96.2)	10	0
Bermuda, Chile, Hungary	4 each (97.5)	4	3
<b>others</b>	22 (100%)	9	9
<b>By foreign country</b>			
China	106	66	28
Nigeria	41	30	5
Russia	38	22	11
India	33	19	10
Libya	26	6	17
Indonesia	25	19	5
Brazil	24	12	8
Mexico	22	16	5
Kazakhstan	22	8	9
Angola	20	8	9
Argentina	17	10	7
Thailand	16	14	2
Egypt	16	12	4
Venezuela	16	8	5
Thailand	16	14	2
<b>others</b>	476	253	163

Note. Cases are those first enforced in the headquarters country or in any third-country jurisdiction. The “headquarters country” is where the firm which allegedly corrupted public officials abroad is headquartered. The “Foreign country” is the country where the act of (alleged) corruption took place. Positive cases refer to cases that were found guilty (see Escresa and Picci 2017 for details). Ongoing cases are those for which we have no information of their conclusion.



**Table 2. Distribution of cases**

All years	Number of cases observed, with relative frequencies										
	0	1	2	3	4	5	6	7	8	9	≥10
Cases, regardless of outcome, first enforced not in the FO country (total: 898)	5,149 92.01%	295 5.27%	85 1.52%	26 0.46%	14 0.25%	9 0.16%	1 0.02%	6 0.11%	0	2 0.04%	9 0.16%
Cases, regardless of outcome, first enforced in 3 <sup>rd</sup> country jurisdictions (total: 271)	5376 96.07%	182 3.25%	30 0.54%	3 0.05%	5 0.09%	0	0	0	0	0	0

Note: The total number of observations is 5596, which equals 42 (the number of headquarters country) times 134 (the number of foreign countries), minus those cases in which the identity of the headquarters and of the foreign country coincides (32 cases). Relative frequencies, in percentages, are not reported when they are equal to zero.

**Table 3. Correlation between variables, year 2005**

	Ln(Pop)	ln(GDP p.c. 1999)	Political rights (lower=freeer)	FH press freedom	Newsp circ. 1996	Fiscal decentraliz	Fuel exports	Imports % GDP	Yr opened to trade	Time to open firm
ln(GDP p.c. 1999)	-0.1942** 121									
Political rights (lower=more)	0.0201 125	-0.3961*** 120								
FH press freedom (higher=freeer)	-0.0725 125	0.3937*** 120	-0.9425*** 128							
Newsp circ. 1996	-0.0623 107	0.6868*** 106	-0.5149*** 108	0.5045*** 108						
Fiscal decentraliz	0.5452*** 47	0.0664 47	0.0875 47	-0.1076 47	0.2507 42					
Fuel exports	-0.1151 102	0.1446 100	0.4964*** 102	-0.4470*** 102	-0.0837 92	-0.1176 46				
Imports % GDP	-0.5070*** 114	0.1989** 112	-0.1653* 113	0.1577* 113	0.0739 102	-0.4451*** 46	-0.1800** 98			
Yrs opened to trade	-0.0213 104	-0.4880*** 102	0.4216*** 105	-0.4489*** 105	-0.4589*** 95	0.1071 45	0.2142** 89	-0.2952*** 98		
Time to open firm	0.1462 74	-0.4051*** 73	0.2575** 73	-0.3156*** 73	-0.4148*** 68	-0.2146 39	0.1372 72	-0.1247 72	0.4365*** 70	
Women in parliament	0.0400 98	0.1823* 96	-0.4185*** 99	0.4353*** 99	0.2517** 88	0.1225 41	-0.1888* 85	0.1623 91	-0.1966* 86	-0.3323*** 60

Notes. Estimated pairwise correlation (above) and number of observations (below).\*\*\*, \*\*, \*: p-value <0.01, 0.05, <0.1. For a description of the variables, see Appendix A.

**Table 4. Pooled model. Dependent variable: all cases enforced first not in the Foreign Country.**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
ln(exports)	0.506*** (0.0736)	0.322*** (0.0615)	0.446*** (0.0558)	0.376*** (0.0672)	0.544*** (0.0692)	0.534*** (0.0645)
ln(population)		0.248*** (0.0571)	0.142** (0.0663)	0.190** (0.0757)	0.124 (0.0965)	0.184* (0.102)
ln(GDP p.c. '99)	-0.615*** (0.128)	-0.331*** (0.113)	-0.0466 (0.0914)	-0.0358 (0.179)	-0.765*** (0.194)	-0.547*** (0.158)
Political rights (lower = freer)			-0.101 (0.0674)	-0.160 (0.110)	-0.248* (0.135)	-0.410*** (0.116)
Democratic since 1950			-0.845*** (0.231)	-0.444* (0.247)	-0.753*** (0.205)	-0.804*** (0.222)
FH press freedom			-0.0202*** (0.00651)	-0.0338*** (0.00802)	-0.0289** (0.0123)	-0.0276*** (0.00975)
Newsp circ. 1996			-0.00345*** (0.000638)	-0.00106 (0.000743)	-0.00148 (0.00113)	-0.00266*** (0.000884)
Presidential dem				0.177** (0.0839)		
Pure plurality syst				0.0831 (0.328)		
Open-list system				0.602*** (0.191)		
District magnitude				0.000539 (0.00187)		
Fiscal decentraliz					0.0127* (0.00666)	
Fuel exports						0.00718*** (0.00252)
Imports % GDP						0.00132 (0.00639)
Yr opened to trade						0.0104 (0.00683)
Time to open firm						-0.311 (0.198)
Women in govt %						-0.0155* (0.00854)
Observations	2,985	2,985	2,616	1,325	993	1,281
R-squared	0.567	0.641	0.508	0.602	0.689	0.718

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present.

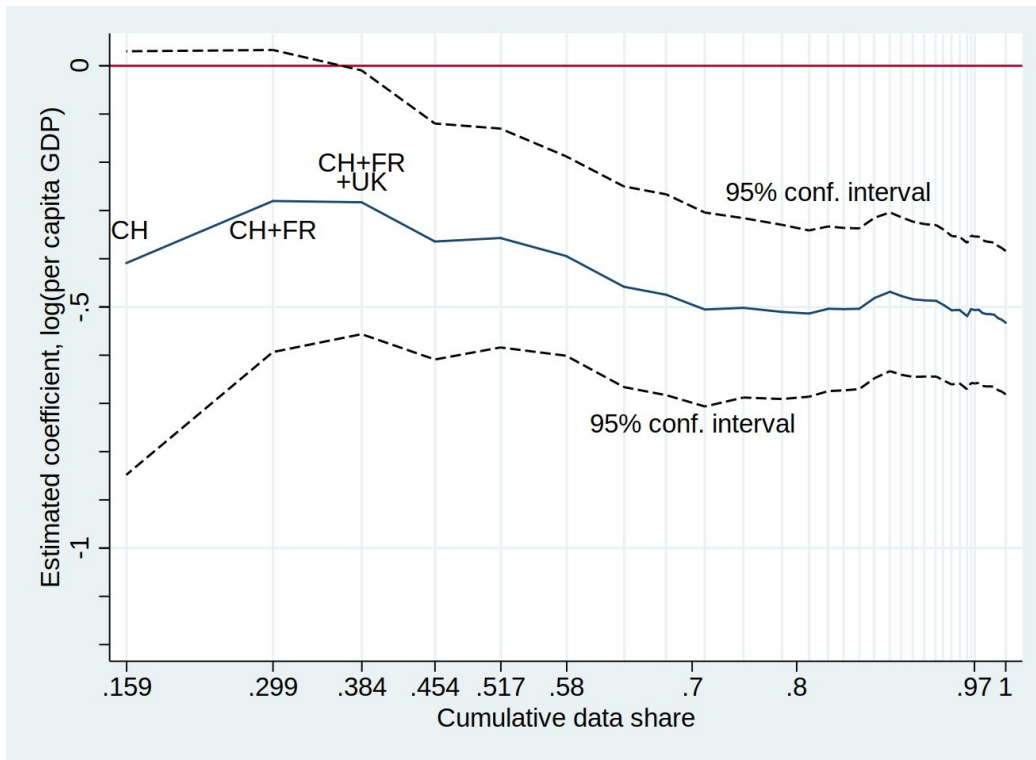
**Table 5. Pooled model. Dependent variable: all cases enforced first not in the Foreign Country. With relational variables.**

VARIABLES	(1)	(2)	(3)	(4)	(5)
ln(exports)	0.310*** (0.0849)	0.263*** (0.0676)	0.373*** (0.0657)	0.312*** (0.0634)	0.404*** (0.106)
ln(population)	0.258*** (0.0618)	0.340*** (0.0708)	0.201*** (0.0737)	0.268*** (0.0745)	0.346*** (0.134)
ln(GDP p.c. 1999)	-0.298** (0.131)	-0.232** (0.118)	0.0357 (0.0969)	0.103 (0.103)	-0.297 (0.198)
Political rights (lower = freer)			-0.105 (0.0660)	-0.119* (0.0667)	-0.331*** (0.119)
Democratic since 1950			-0.906*** (0.232)	-0.935*** (0.234)	-1.056*** (0.253)
FH press freedom			-0.0203*** (0.00637)	-0.0216*** (0.00611)	-0.0265*** (0.00966)
Newsp circ. 1996			-0.00325*** (0.000634)	-0.00275*** (0.000656)	-0.00204** (0.000959)
ln(distance)	0.0188 (0.0968)	-0.166 (0.106)	-0.0561 (0.102)	-0.137 (0.106)	0.0495 (0.128)
Contiguous	0.0349 (0.238)	0.0973 (0.232)	0.399 (0.257)	0.418 (0.260)	0.608** (0.308)
Colonial link	0.559* (0.336)	0.549* (0.330)	0.623** (0.253)	0.570** (0.276)	0.826*** (0.317)
Language proximity		-0.488 (0.330)		-0.210 (0.301)	-0.137 (0.487)
Religion proximity		-0.234 (0.224)		0.389* (0.232)	0.832** (0.367)
Religious attitude proximity		0.385 (0.979)		0.658 (0.657)	0.375 (1.269)
Fuel exports					0.00687*** (0.00255)
Imports % GDP					0.00907 (0.00637)
Yr opened to trade					0.00662 (0.00685)
Time to open firm					-0.375* (0.208)
Women in govt %					-0.0169** (0.00782)
Observations	2,985	2,646	2,616	2,431	1,231
R2	0.612	0.665	0.511	0.510	0.713

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present.

## Appendix

Figure A-1. Estimated impact of GDP per capita, recursive coefficients, base model. Cases enforced first only in third country jurisdictions.



Note: Point estimates (continuous line) of the coefficient on the per capita income in the base model, together with 95% confidence interval, as additional countries are added. The left-most estimate only includes Switzerland (representing 15.9% of cases), then the Switzerland and France together (representing 29.9% of observations, etc. Thick vertical line represents data coverage (97% of total number of observed occurrences) used for the results of Tables A4 and A5.

**Table A4. Pooled model. Dependent variable: all cases enforced first in 3<sup>rd</sup> country jurisdictions**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
ln(exports)	0.415*** (0.0399)	0.284*** (0.0645)	0.435*** (0.0742)	0.299*** (0.104)	0.409*** (0.134)	0.586*** (0.0910)
ln(population)		0.191*** (0.0715)	0.129 (0.0926)	0.116 (0.134)	0.117 (0.146)	0.110 (0.160)
ln(GDP p.c. '99)	-0.533*** (0.0758)	-0.338*** (0.108)	-0.0278 (0.114)	0.146 (0.247)	-0.573** (0.259)	-0.433** (0.205)
Political rights (lower = freer)			-0.172* (0.0900)	0.0513 (0.177)	-0.346** (0.175)	-0.321* (0.178)
Democratic since 1950			-1.407*** (0.368)	-0.661* (0.395)	-0.898** (0.395)	-1.099** (0.485)
FH press freedom			-0.0233*** (0.00874)	-0.0172 (0.0115)	-0.0418*** (0.0159)	-0.0268* (0.0145)
Newsp circ. 1996			-0.00451*** (0.00109)	-0.00251** (0.00110)	-0.00277 (0.00171)	-0.00348** (0.00142)
Presidential dem				0.351** (0.148)		
Pure plurality syst				-1.054 (0.655)		
Open-list system				0.266 (0.298)		
District magnitude				-0.00251 (0.00267)		
Fiscal decentraliz					0.00436 (0.0102)	
Fuel exports						0.01000** (0.00422)
Imports % GDP						0.000181 (0.00918)
Yr opened to trade						0.0195* (0.0118)
Time to open firm						-0.0632 (0.235)
Women in govt %						-0.00965 (0.00887)
Observations	4,139	4,139	3,108	1,316	783	1,326
R-squared	0.133	0.148	0.143	0.152	0.209	0.238

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present. Results obtained using all data available.

**Table A5. Pooled model. Dependent variable: all cases enforced first in 3<sup>rd</sup> country jurisdictions. With relational variables.**

VARIABLES	(1)	(2)	(3)	(4)	(5)
ln(exports)	0.232*** (0.0787)	0.298*** (0.0798)	0.307*** (0.0933)	0.376*** (0.0934)	0.324* (0.166)
ln(population)	0.255*** (0.0868)	0.226** (0.0880)	0.262** (0.111)	0.203* (0.113)	0.426* (0.221)
ln(GDP p.c. 1999)	-0.288** (0.117)	-0.349*** (0.124)	0.0897 (0.129)	-0.0286 (0.134)	-0.354 (0.258)
Political rights (lower = freer)			-0.190** (0.0894)	-0.177* (0.0960)	-0.303 (0.184)
Democratic since 1950			-1.460*** (0.386)	-1.343*** (0.397)	-1.089** (0.525)
FH press freedom			-0.0242*** (0.00857)	-0.0242*** (0.00863)	-0.0264* (0.0145)
Newsp circ. 1996			-0.00449*** (0.00115)	-0.00360*** (0.00115)	-0.00301* (0.00158)
ln(distance)	-0.171 (0.118)	-0.195 (0.126)	-0.318** (0.137)	-0.271* (0.147)	-0.211 (0.231)
Contiguous	-0.298 (0.384)	-0.281 (0.386)	-0.0916 (0.369)	-0.184 (0.389)	0.615 (0.493)
Colonial link	0.306 (0.296)	0.114 (0.325)	0.293 (0.318)	0.0981 (0.339)	0.111 (0.437)
Language proximity		0.154 (0.359)		0.253 (0.393)	1.049* (0.592)
Religion proximity		-0.403 (0.299)		0.153 (0.354)	-0.352 (0.640)
Religious attitude proximity		2.114** (0.964)		2.281* (1.318)	0.247 (2.938)
Fuel exports					0.00837* (0.00442)
Imports % GDP					0.00999 (0.0104)
Yr opened to trade					0.0204 (0.0130)
Time to open firm					-0.188 (0.243)
Women in govt %					-0.0164* (0.00876)
Observations	4,139	3,303	3,108	2,735	1,230
R2	0.146	0.137	0.142	0.146	0.249

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present. Results obtained using all data available.

**Table A4-b. Pooled model. Dependent variable: all cases enforced non in the foreign country, excluding all cases first enforced in the US**

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
ln(exports)	0.392*** (0.0407)	0.389*** (0.0689)	0.558*** (0.0737)	0.505*** (0.0809)	0.612*** (0.103)	0.624*** (0.0890)
ln(population)		0.00379 (0.0797)	-0.0506 (0.106)	-0.106 (0.113)	0.00322 (0.148)	-0.0409 (0.163)
ln(GDP p.c. '99)	-0.457*** (0.0832)	-0.453*** (0.136)	-0.286** (0.131)	-0.297 (0.225)	-0.882*** (0.270)	-0.800*** (0.249)
Political rights (lower = freer)			0.0486 (0.109)	0.0723 (0.181)	0.0405 (0.201)	-0.355 (0.222)
Democratic since 1950			-1.009** (0.435)	-0.930** (0.422)	-0.899** (0.378)	-1.603*** (0.390)
FH press freedom			-0.00868 (0.0106)	-0.0239* (0.0142)	-0.00448 (0.0183)	-0.0272 (0.0178)
Newsp circ. 1996			-0.00260*** (0.000889)	-3.76e-05 (0.00104)	-0.00115 (0.00134)	-0.00231* (0.00125)
Presidential dem				0.230* (0.130)		
Pure plurality syst				0.134 (0.480)		
Open-list system				0.877*** (0.269)		
District magnitude				-0.000516 (0.00243)		
Fiscal decentraliz					0.0147 (0.0126)	
Fuel exports						0.00469 (0.00456)
Imports % GDP						-0.00745 (0.00877)
Yr opened to trade						0.00480 (0.0109)
Time to open firm						-0.687** (0.292)
Women in govt %						-0.000582 (0.00917)
Observations	2,766	2,766	2,422	1,220	911	1,078
R-squared	0.114	0.114	0.149	0.228	0.184	0.161

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present.

**Table A5-b. Pooled model. Dependent variable: all cases enforced non in the foreign country, excluding all cases first enforced in the US. With relational variables.**

VARIABLES	(1)	(2)	(3)	(4)	(5)
ln(exports)	0.319*** (0.0857)	0.263*** (0.0846)	0.461*** (0.0909)	0.363*** (0.0958)	0.602*** (0.162)
ln(population)	0.0732 (0.0906)	0.155* (0.0941)	0.0407 (0.107)	0.141 (0.116)	0.0602 (0.192)
ln(GDP p.c. 1999)	-0.325** (0.133)	-0.212 (0.143)	-0.171 (0.132)	-0.0173 (0.149)	-0.614** (0.313)
Political rights (lower = freer)			0.0362 (0.104)	0.0405 (0.104)	-0.231 (0.207)
Democratic since 1950			-1.017** (0.451)	-1.093** (0.449)	-1.725*** (0.377)
FH press freedom			-0.00782 (0.0101)	-0.00976 (0.00964)	-0.0264 (0.0173)
Newsp circ. 1996			-0.00228*** (0.000866)	-0.00203** (0.000898)	-0.00163 (0.00119)
ln(distance)	-0.0809 (0.135)	-0.186 (0.149)	-0.0544 (0.154)	-0.146 (0.161)	0.218 (0.219)
Contiguous	-0.0962 (0.341)	0.00850 (0.362)	0.158 (0.342)	0.218 (0.345)	0.553 (0.460)
Colonial link	1.308*** (0.278)	1.280*** (0.298)	1.102*** (0.277)	1.162*** (0.289)	1.151*** (0.422)
Language proximity		-0.300 (0.404)		-0.278 (0.420)	-0.223 (0.702)
Religion proximity		0.0184 (0.305)		0.742** (0.328)	1.586** (0.627)
Religious attitude proximity		1.298 (0.984)		-0.184 (0.914)	-1.057 (1.910)
Fuel exports					0.00546 (0.00422)
Imports % GDP					0.00560 (0.00891)
Yr opened to trade					0.00106 (0.0113)
Time to open firm					-0.696** (0.311)
Women in govt %					-0.00729 (0.00869)
Observations	2,766	2,536	2,422	2,330	1,078
R2	0.129	0.126	0.170	0.159	0.189

Note: A Poisson estimator is used for all models, with residuals clustered for country pairs. Country fixed effects are present.