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The impacts of energy prices on industrial foreign investment location: evidence from global firm level data

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Abstract

This paper analyzes the role of energy prices in firms' investment location decisions in the manufacturing sector. Building on the application of discrete choice theory to the firm location problem, we specify a conditional logit model linking bilateral foreign direct investment (FDI) activity to relative energy prices. We then empirically test this link using a global dataset of M&A deals in the manufacturing sector covering 41 countries between 1995 and 2014, using econometric techniques adapted from the estimation of gravity models. The results suggest that upon deciding to invest, firms are attracted to regions that have lower energy prices. However, counterfactual simulations reveal that unilateral implementation of a \$50/tCO$_2$ carbon tax by various coalitions of countries is expected to have limited negative impact on the attractiveness of economies to foreign industrial investments. Hence, our results support the pollution haven effect, but find the magnitude is limited and could be addressed with targeted measures in the most energy intensive sectors.

Keywords: FDI; Mergers and Acquisitions; energy prices; firm location; competitiveness impacts; carbon leakage.
JEL: F21, F64, H23, Q52

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1. Introduction

Policies to incentivize emissions reductions and foster low carbon technologies are proliferating around the world. Over fifty carbon pricing policies are now adopted in over forty countries covering a fifth of global emissions (World Bank and Ecofys, 2018). The emerging carbon price level set by these initiatives, however, lie far below the target range of \$40-\$80/tCO_2 recommended by the Stern-Stiglitz Commission (Stern and Stiglitz, 2017). A major factor that curbs ambition in efforts to tackle climate change, is the perceived threat of carbon leakage and competitive disadvantages associated with stringent regulation. These reflect concerns that faced with higher carbon prices, firms will respond by relocating or 'leaking' production abroad, then export their products back without paying the carbon price. These fears are magnified by trends in globalization which has enhanced economic integration such that firms, in an attempt to remain competitive, are pursuing different forms of cross-border investments, including foreign direct investments, mergers, joint ventures and offshoring.

This paper contributes to the long-standing literature on the link between environmental regulation and foreign direct investment,[1] making a number of advances both theoretically and empirically. Previous papers look at aggregate FDI flows, typically exploit the variation in environmental regulation within a country, and assess if jurisdictions with lax policy can attract more inbound FDI flows (List et al., 2004; Millimet and Roy, 2015), or discourage outbound FDI flows (Cole and Elliott, 2005; Hanna, 2010). Some recent contributions use firm level data in a specific country, and measures of environmental policy stringency across potential host countries, to assess if the latter can explain the destination choice for outbound FDI flows (Wagner and Timmins, 2009; Raspiller and Riedinger, 2008; Manderson and Kneller, 2012; Ben Kheder and Zugravu, 2012). Several studies test the pollution haven effect[2] by estimating the effect of energy prices on firm or industry location decisions (Kahn and Mansur, 2013; Panhans et al., 2016). While these empirical studies of the pollution haven effect have been illuminating, the results yield mixed conclusions. Generally, the

[1]See Cole et al. (2017) for a recent review of the FDI and environment literature.

[2]The Pollution Haven Effect (PHE) posits that differences in regulatory stringency can, at the margin, have an effect on trade flows and investment decisions (Taylor, 2004). This can be distinguished from the Pollution Haven Hypothesis (PHH) which predicts that trade liberalization leads to a migration of polluting industry to countries with less stringent regulation (McGuire, 1982)

literature has failed to uncover robust evidence to solve the controversy over the link between environmental policy and industrial flight. However, several shortcomings in existing studies suggest that the question has not been fully answered.

The lack of robust evidence may be attributable to a number of issues. First, the theory linking environmental policy to industry location focuses on *comparative* cost advantage. In other words, cross-border investment decisions under the pollution haven scenario is driven by regulatory costs in *both* countries, hence it is the *relative* costs that matter. Identifying this requires a bilateral setting with paired data. Second, studies on the inbound FDI cover a limited geographical scope. The lack of variation in other determinants of production location is problematic for identification. Third, many previous studies relied on aggregated FDI data that capture total FDI flows, which does not allow controlling for multitude of confounding factors such as sector and country level trends. Fourth, studies on outbound FDI require measures of environmental regulation stringency in host countries or states but good measures are difficult to come by.

This paper aims to overcome these limitations and test whether a causal relationship exists between carbon pricing policies and FDI. Our study marks a notable departure from the previous literature in several ways. The empirical framework adopted draws on the recent literature on the determinants of cross-border investments, which use bilateral flows and a base model consisting of gravity-type covariates, borrowing from the empirical bilateral trade literature (Anderson, 2011; Head and Mayer, 2014; Anderson and Yotov, 2012).[3] We adopt this framework, and to our knowledge this is the first study to explicitly test the effect of *relative* energy prices on FDI.

For identification, this paper exploits exogenous variations in industrial energy prices. We argue that this is a superior measure of environmental policy, compared to those employed in previous studies. These included broad measures such as a participation in a treaty, or composite indicators which are subject to measurement error, due to the multidimensional nature of environmental policy

[3] A rich theoretical and empirical literature has considered the general determinants of FDI and cross border M&A activity. Studies highlight the importance of traditional gravity factors – geographical and cultural proximity, market size (Blonigen and Piger, 2014). Other determinants explored include stock market valuations and exchange rates (Erel et al., 2012), and tariff-jumping and trade costs (Brainard, 1997). The impact of relative input costs such as cross-border variations in energy prices or environmental policy stringency has received less attention.

(Brunel and Levinson, 2016)[4]. Industrial energy prices that include taxes is highly correlated with measures of environmental policy stringency measures (Sato et al., 2015). It offers a way to measure the difference in carbon prices faced by firms, since carbon pricing measures raise the price of fossil fuels which in turn increases energy prices (Aldy and Pizer, 2015), overcoming the fact that carbon prices are relatively new and prices have been extremely low in general such that there is limited variation to exploit. Based on elasticities estimates of relative energy price impacts, we will conduct counterfactual analysis to simulate the impacts of relative carbon prices.

We analyse whether bigger differences industrial energy prices increases bilateral mergers and acquisitions (M&A) transactions. The FDI literature has found that determinants vary depending on the mode of FDI – greenfield investments vs M&A (e.g. Nocke and Yeaple (2007)). We focus on M&A and exclude greenfield investments. This is partly due to data availability even though, in the countries we consider in our dataset, M&A account for half of cross border investments[5] (UNCTAD, 2018). To motivate our empirical analysis, we also build on theoretical models of FDI flows, namely Head and Ries's (2008) dartboard model of M&A. This model is founded in the application of discrete choice theory to the firm location problem. We use this framework to derive a model linking location choice in bilateral acquisitions to relative energy prices.

To implement this empirical strategy, we take advantage of a global database on firm level M&A from Thomson Reuters. This provides a exhaustive[6] listing of M&A deal counts in the manufacturing sectors, covering transactions made both cross-border and domestic, made by both publicly listed companies and non listed companies. This dataset provides detailed information on the transactions made, specifying the type of investment (e.g. asset acquisition), along with detailed sectoral and locational information on the acquiring and target firms. We use this dataset to construct a panel of global, bilateral cross-border investments aggregated at the sector level (distinguishing 22 industrial subsectors). The relative energy price variable is constructed from the database provided

[4]Regulations target different pollutants arising from different media such as air, water and land, different polluters such as industry and households, and can take many forms such as pollution reduction targets and technology standards.

[5]In OECD member countries and the BRICS, M&A transactions accounted for 50% of cross-border investment flows by value over the period 2003-2014 (UNCTAD, 2018).

[6]While the original Thomson-Reuters dataset is exhaustive, the availability of our other covariates – sectoral energy prices in particular – restricts our application to a subset.

by Sato et al. (2019). Our final dataset includes around 70,000 transactions – of which 22,000 are cross-border – by firms in 41 countries between 1995 and 2014. In contrast to the previous literature, this approach allows for the estimation of regulatory effects that are purged of bias associated with country-pair and industry specific trends. This is particularly important because, during this period, there were many factors (e.g., supply chain integration, trade agreements, technology changes) that may have had differential impacts on sector-level FDI. The global coverage of the dataset is also unique and ensures the results can be interpreted in a global context. However, the large panel size renders usual maximum likelihood estimation intractable. To overcome this, we implement a custom estimator based on an iterated reweighted least square (IRLS) implementation of the PPML estimator, extending Guimaraes (2016)'s two high dimensional fixed effects PPML estimator to a large number of fixed effects.

We report three main findings. First, we find evidence that relative industrial energy prices have an impact on the cross-border investment activity of industrial firms. Specifically, firms tend to engage in more cross-border investments when their domestic energy prices increase in relative terms against foreign prices. Our preferred specification suggests that an increase of 10% in the relative industrial energy price differential between two countries is expected to increase by 3.2% the number of acquisitions of firms or assets located in the lower energy price country by firms based in the more expensive country. Second, we find that this effect is significantly larger for emerging economies: the impact of relative energy costs is four times larger for transactions targeting non-OECD countries than for those targeting OECD countries. Third, we find that the effect of relative energy prices on cross-border investment decisions is heterogeneous across sectors, and grows with sectoral energy intensity.

We subject our results to a number of checks. We first ensure that our findings are robust to the inclusion of sectoral measures of labor and capital costs. We then make use of recent econometric advances in the estimation of structural gravity models in the trade literature (Anderson and Yotov, 2012; Fally, 2015) to confirm our findings using a second identification strategy described in section 6.2. We also confirm the robustness of our results to the dynamics of firms' investment decisions and the use of an alternate energy price index.

We finally complement these findings with counterfactual simulations of the implementation of a \$50/tCO_2 carbon tax by three different coalitions of countries, and estimate the resulting impact on inbound investment activity. We find that unilateral implementation by developed economies would have a limited negative impact on their attractiveness to foreign industrial investments.

This paper is structured as follows. Section 2 develops a simple theoretical framework to guide our empirical analysis and section 3 presents the econometric strategy. In section 4 we describe the sources and structure of the constructed data set, and give descriptive statistics. In section 5, we turn to estimating the impact of energy prices on M&A deals and present the results of our estimations before performing an assortment of robustness checks in section 6. We finally present extensions on sectoral heterogeneity and our counterfactual simulations in section 7 before concluding.

2. Theoretical framework

In this section, we develop a simple model of cross-border M&A inspired by Head and Ries's (2008) dartboard model, in turn inspired by the application of McFadden's (1974) discrete choice theory to the firm location problem. We also draw from applications of this model by Hijzen et al. (2008) and Coeurdacier et al. (2009), who study the impact of trade costs and the European integration on FDI respectively.

In the following, we propose a model for the choice of investment location conditional on the decision to invest. We consider the firm's investment decision as a two step process: first, the firm decides whether to invest in another firm, and second it chooses its target. We are only concerned with the second step of this decision process, which determines the location of the investment.

Let g be a firm operating in sector $k \in \mathcal{S}$ and country $i \in \mathcal{C}$, with $\mathcal{S}$ the set of all sectors and $\mathcal{C}$ the set of all countries. Consider now a second firm h, $h \neq g$, operating in sector l and country j – $(j, l) \in \mathcal{C} \times \mathcal{S}$. The special cases of domestic $(i = j)$ and horizontal $(k = l)$ investments are encompassed in this framework. We are interested in deriving the probability that g acquires h conditional on g having decided to invest in another firm.

Let π_h be the profit that firm g can expect if it acquires h. We consider a reduced-form profit function π_h , log-linear in the characteristics of h. In the following, we shall only consider the variation in these characteristics observed at the country and sector level. Therefore, for a given characteristic X_c, we assume that for any firm h operating in country j and sector l, $X_{c,h} = X_{c,jl}$. Examples of $X_{c,jl}$ include covariates such as sectoral energy prices. We have, with ε_h a stochastic component:

$$\pi_h \equiv \sum_c \beta_c \ \log X_{c,h} + \varepsilon_h = \sum_c \beta_c \ \log X_{c,jl} + \varepsilon_h \tag{1}$$

Under the assumption that the perturbation term ε_h is distributed as a Type I extreme value (McFadden, 1974), we have from discrete choice theory the following familiar multinomial logit expression for the probability $P_{g,h}$ that g acquires h:

$$P_{g,h} = \frac{\exp(\pi_h)}{\sum\limits_{h'} \exp(\pi_{h'})} \tag{2}$$

We now write n_{jl} the number of firms that operate in country j and sector l. Aggregating at the target sectoral and country levels, we get the probability that g acquires a firm in country j and sector l:

$$P_{g,jl} = \frac{n_{jl}\exp(\pi_{jl})}{\sum\limits_{j' \in \mathcal{C}, l' \in \mathcal{S}} n_{j'l'}\exp(\pi_{j'l'})} \tag{3}$$

Summing over all firms in acquiring country i and sector k, we can express the number of deals m_{ijkl} observed between country-sector pairs (i, k) and (j, l):

$$m_{ijkl} = \frac{n_{ik} n_{jl}\exp(\pi_{jl})}{\sum\limits_{j' \in \mathcal{C}, l' \in \mathcal{S}} n_{j'l'}\exp(\pi_{j'l'})} \tag{4}$$

Since $i \in \mathcal{C}$ and $k \in \mathcal{S}$, we finally get:

$$m_{ijkl} = \frac{n_{ik} n_{jl}\exp(\pi_{jl} - \pi_{ik})}{\Omega_{ijkl}} \tag{5}$$

with $\Omega_{ijkl} \equiv \sum\limits_{j' \in \mathcal{C}, l' \in \mathcal{S}} n_{j'l'}\exp(\pi_{j'l'} - \pi_{ik})$.

This expression is functionally similar to the gravity equation commonly used in the trade literature (Head and Mayer, 2014). The number of deals[7] between two country sector pairs is proportional to the economic size of the two sectors considered – measured here by the number of firms operating in each sector. Further, Ω_{ijkl} can be construed as an indicator of the financial attractiveness of a sector in a given country – and therefore the difficulty to acquire one of its targets: the more profitable targets in a given country-sector pair are, the larger Ω_{ijkl} becomes, and the smaller the probability for potential acquirers to out compete the rest of the world and achieve a deal. Ω_{ijkl} is therefore a remoteness index comparable to that found in trade theory (Anderson, 2011). It plays in effect the role of a multi-lateral resistance (MLR) term in equation (5).[8]

Importantly, injecting equation (1) into (5), we get:

$$m_{ijkl} = \frac{n_{ik} n_{jl} \prod_c \left(\frac{X_{c,jl}}{X_{c,ik}} \right)^{\beta_c}}{\Omega_{ijkl}} \tag{6}$$

In the case of sectoral energy prices, (6) implies that the number of deals is directly related to the ratio of energy prices between the target and host countries, thus to the sectoral energy price of the target country *relative* to that of the host country. A decrease (resp. increase) in this ratio is thus expected to cause an increase (resp. decrease) in the number of deals observed between the country pair considered. This result is intuitive: when energy prices in country j become cheaper relative to those of country i, firms in country i are expected to be incentivized to invest in country j.

3. Empirical strategy

Consider the following Poisson specification for the determination of bilateral transactions (6):

$$m_{ijklt} = \exp \left[\log n_{ikt} + \log n_{jlt} + \sum_c \beta_c \left(\log X_{c,jlt} - \log X_{c,ikt} \right) - \log \Omega_{ijklt} \right] + \varepsilon_{ijklt} \tag{7}$$

The main challenge to estimate equation (7) is to adequately control for the multi-lateral re-

[7]Note that this model use the number of transactions to proxy for M&A activity, yet an improved measure is the deal values. Unfortunately, data availability constraints prevent using M&A deal values as the outcome variable. Nonetheless we rise to the challenge in Appendix C.

[8]The empirical trade literature has shown that it is necessary to account not only for bilateral trade resistance (the barriers to trade between a pair of countries) but also multilateral trade resistance (the barriers to trade that a country faces with all its trading partners).

sistance term Ω_{ijkl}. As highlighted in the previous section, equation (6) yields a gravity model including an MLR term, similar in its functional form to the structural gravity models commonly used in the analysis of international trade flows (Anderson and van Wincoop, 2003). This allows us to benefit from the recent advances achieved in the econometric estimation of this class of models.

In particular, Anderson and Yotov (2012) and Fally (2015) have shown that multilateral resistance terms can be accounted for by an appropriately designed set of fixed effects. In our context, where the number of deals is observed at the country-sector level repeatedly over time, it is possible to specify a fixed effects structure consistent with structural gravity following Piermartini and Yotov (2016):

$$m_{ijklt} = \exp\left[\log n_{ikt} + \log n_{jlt} + \sum_c \beta_c \left(\log X_{c,jlt} - \log X_{c,ikt}\right) + \alpha_{ij} + \eta_{ikt} + \nu_{jlt}\right] + \varepsilon_{ijklt} \qquad (8)$$

In equation (8), α_{ij} capture time-invariant country-pair effects, while η_{ikt} and ν_{jlt} are country-sector-year fixed effects. However, under this specification, our coefficients of interest, the β_c, are not identifiable. Indeed the locational characteristics of the acquiring and target country-sector pairs are collinear with η_{ikt} and ν_{jlt} respectively[9].

To overcome this difficulty, we relax the fixed effect structure to account for most of the confounding factors that may influence firms' choice of investment location while maintaining the identifiability of the β_c. This is detailed below in section 3.1.

However, to ensure that our findings are not spurious, we complement this specification with a robustness check implementing a second identification strategy. In Section 6.2, we follow the approach suggested by Anderson and Yotov (2012) and Fally (2015), and regress our dependent variables on the fixed effects structure consistent with structural gravity laid out in equation (8). We then regress the country-sector-year fixed effects (η_{ikt} and ν_{jlt}) recovered from this regression on the locational characteristics X_c. This specification is described in section 6.2.

[9]This stems from the fact that our main regressors of interest, the logarithms of the ratios of locational characteristics in the acquiring and target country-sectors, are not truly dyadic variables. Instead, these ratios result from a linear combination – a difference – of two monadic variables: the log of the characteristics X_c, observed for the acquiring and target firms.

3.1. Main specification

In our main specification, we include country-pair, country-year and sectoral fixed effects. Country-pair fixed effects account for the time invariant characteristics commonly considered in gravity models, including but not limited to: distance, commonality of language or system of law, colonial history. Since these factors do not form the focus of this study, identifying their individual impact on investment activity is not relevant in our context.

Sectoral effects allow us to capture systematic differences in cross-border investment activity between sectors. Such variation can be explained differences in market structure, technology or specificities of the product manufactured. Country-time form the largest group of fixed effects included. They account for the country-specific macroeconomic environment and any independent variable which vary at the country-time granularity. This includes a number of factors identified in the M&A literature to be correlated with the number of deals between two given countries, irrespective of their market sizes (Di Giovanni, 2005), such as exchange rates or stocks valuation.

Importantly, country-time fixed effects control for production factor costs at the aggregate level in the countries on both sides of the transaction: namely country-wide mean labor, capital and energy costs. They also control for country-level policies that may influence investment decisions in the manufacturing sector, such as cross-sectoral environmental policy. Further, country-time fixed effects also encompass time fixed effects, which control for the highly cyclical nature of global merger and acquisition flows (Erel et al., 2012).

This rich set of fixed effects allow us to control for confounding factors that may influence firms' choice of investment location other than our regressor of interest, relative energy costs, as is common in the gravity literature (Head and Mayer, 2014; Arvis and Shepherd, 2013). Finally, we also control for the existence of a free-trade agreement between a given country pair. We note that in this specification, identification rests on within-country cross-sectoral energy price differences.

Estimating equation (9) requires an estimate of the number of potential acquiring and target companies, n_{ik} and n_{jl}, in the countries and sectors considered. While some databases, notably UNIDO's INDSTAT, do provide this data for a number of countries, data availability remains poor

[10]. To improve our geographical and temporal coverage, we follow Hijzen et al. (2008) and measure the economic size of the acquiring and target sectors using their respective sectoral GDP in each country.

In the reduced form profit function, we include our main regressor of interest, the ratio of energy prices in the country-sector of the acquiring and target companies. The industry-wide cost of the other main production factors, labor and capital, is accounted for by the country-time fixed effects. Yet cross-sectoral differences in the cost of these two factors could also have an impact on firms' investment decisions (Wheeler and Mody, 1992). We assess the robustness of our results to this possibility in section 4.3.

Our final model specification is therefore:

$$\begin{aligned} m_{ijkl,t} = \exp\Big[&\beta_1 \log GDP_{ik,t} + \beta_2 \log GDP_{jl,t} + \beta_e \log e_{ijkl,t} + \\ &\beta_5\, fta_{ij,t} + \alpha_{0,ij} + \alpha_{1,k} + \alpha_{2,l} + \alpha_{3,it} + \alpha_{4,jt}\Big] + \varepsilon_{ijkl,t} \end{aligned} \tag{9}$$

where for each country-sector pair ik (acquirer) or jl (target), $GDP_{ik,t}$ and $GDP_{jl,t}$ are the sectoral GDP, $fta_{ij,t}$ is a dummy indicating the presence of a free-trade agreement concerning the exchange of goods between countries i and j. Our main parameter of interest is β_e, which captures the impact of relative energy prices on investment activity between two country-sector pairs.

$e_{ijkl,t}$ measures the ratio of energy prices between the acquiring and target country-sector pairs. In our dataset, we also consider transactions in which a firm invests in a sector distinct from its own main activity. However, when deciding the location of an investment in a given target sector l, the investing firm is going to compare energy costs in this sector l across locations – including its own domestic country. Between two given country-sector pairs, the relevant energy price ratio should therefore be calculated between the energy cost *in sector* l in the target country and that of the acquirer, regardless of the acquirer's main sector of activity. For a transaction between country-sector pairs ik and jl, we therefore consider the following log-ratio:

[10]Using INDSTAT2's estimate of the number of companies by sector in lieu of sectoral GDP would decrease the number of transactions included in our sample by around 25%.

$$e_{ijkl,t} = \log\left(\frac{E_{jl,t}}{E_{il,t}}\right) \tag{10}$$

where E is our measure of sectoral energy costs in each country, as defined in section 4.

3.2. Estimator choice and computational feasibility

To keep the estimation computationally manageable, we aggregate the original sectoral breakdown, available in our dataset at the 4-digit SIC level, up to the 2-digit ISIC (revision 3.1) level, distinguishing 22 sectors [11] (see list of included ISIC sectors summarized in Table B.2). Despite this aggregation, our sample with 41 countries over a 20 year period yields more than 16 million potential observations[12]. Data availability reduces this sample size to between 6 and 8 million observations depending on the covariates included in the specifications estimated.

As is often the case in balanced bilateral trade datasets, most observations in the sample are zeros. Failure to properly take these zero values into account would lead to biased estimates, which rules out estimations by OLS on the log of our dependent variable. In their seminal contribution, Silva and Tenreyro (2006) show that the best estimator in this context is Poisson Pseudo-Maximum Likelihood (PPML) with heteroscedasticity-consistent standard errors, which can handle the potential overdispersion and consistently outperforms potential alternatives such as zero-inflated Poisson or negative binomial. The panel nature of our dataset requires applying clustering to the standard errors. We opt for the most conservative design by clustering at the country-sector pair level, which is the unit of observation in our panel.

However, the size of the dataset makes a straight maximum likelihood estimation intractable. Instead, we implement a custom estimator based on an iterated reweighted least square (IRLS) implementation of the PPML estimator, extending Guimaraes (2016) two high dimensional fixed effects PPML estimator to a large number of fixed effects. We further implement one-way and multi-way clustering, building on Zylkin (2018). This estimator, which we describe in Appendix D, makes the estimation of our model feasible in a reasonable amount of time on modern high

[11] Our dataset is restricted to the manufacturing sectors both on the acquirer and target sides. In particular, acuqisitions by non-manufacturing firms are not included.

[12] 41 origin countries $\times$ 41 target countries $\times$ 22 origin sectors $\times$ 22 destination sectors $\times$ 20 years = 16,272,080.

performance hardware[13].

4. Data

This paper combines a variety of data sources into a unique, global data set suitable for assessing the link between energy price and M&A deals. Our ultimate sample includes a total of 69,979 deals that occurred between 1995 to 2014 across 41 countries and in 22 manufacturing sectors (see Figure 1). Following the model presented in section 2, the sample includes both domestic and cross-border deals, the latter accounting for 22,241 deals. This section describes the sources and structure of the data.

4.1. The Mergers and Acquisitions dataset

M&A outcomes are obtained from the proprietary Thomson-Reuters Mergers and Acquisitions database. This data is believed to be highly accurate, and the coverage of realized transactions is close to complete[14]. It contains information on deals including transaction date, a set of variables describing both acquiring and target companies such as country of origin and main 4-digit SIC sector activity, as well as the nature of the deal. It does not include deals that were announced but fell through.

We organize this dataset in several ways. The database categorizes deal types based on the share of ownership. To create a measure of M&A transaction numbers, we restrict our sample to deals where the acquirer fulfills any of the following criteria:

- full merger with the target company
- increase of its interest from below to above 50%
- acquisition of the remaining interest it does not already own [15]

To complement this M&A category, we also consider a subset of deals labeled "Acquisition of Assets", whereby only a subset of a target company's assets (pertaining to one of its division, branch

[13]All estimation results provided in this article were obtained on the London School of Economics' Fabian high-performance computing (HPC) cluster. Using our IRLS implementation of the PPML estimator described in Appendix D, our main specification takes approximately one hour to estimate on a 24-core CPU equipped with 15 Go of RAM.

[14]In particular, it is used by the United Nation Conference on Trade and Development to compile its annual World Investment Report (UNCTAD, 2018).

[15]These correspond respectively to the categories labeled "Merger", "Acquisition of Majority Interest" and "Acquisition of Remaining Interest" in the original Thomson-Reuters taxonomy.

or even a single plant) is acquired. This category is of relevance to our inquiry, as it represents a more specific case of cross-border investment location choices compared to measures of general M&A. We present results estimated on this specific subset.

The year variable is taken from the deal announcement date, rather than completion date. The announcement date corresponds to the first public statement by any of the involved parties regarding the merger, acquisition or acquisition of assets considered. We deem this closer to the relevant time period in which the acquirer obtains information on production factor costs. The mean time to completion is under a month, and for the majority of the transactions observed, both dates are identical.

Given the focus of our study, we restrict our sample to deals observed in the manufacturing sector. As mentioned, for computational feasibility, deals are aggregated to the 2-digit (ISIC Rev 3.1) sector level[16]. One exception is the "Basic metals" sector (sector 27). This 2-digit sector combines *Iron and steel* (2710) and *Non-ferrous metals* (2720) which are highly heterogeneous in terms of energy mix, and therefore energy prices. Hence we retain this separation in our analysis[17]. Table 1 provides an overview of our sectoral coverage and illustrates that about two-thirds of M&A deals observed in the manufacturing sector occur between companies located within the same country.

4.2. Energy prices

To assess the level of energy prices faced by firms, we exploit variations in the country and the sector of the acquirer and the target. The energy price data by country-sector-year is therefore key for our identification strategy. We obtain internationally comparable industrial energy price data from Sato et al. (2019) in which a Fixed Energy Price Index (FEPI) is constructed for each country-sector pair by weighting fuel prices for four carriers (oil, natural gas, coal and electricity) by the

[16] We do not consider ISIC sectors 36, *Furniture; manufacturing n.e.c.* due to the large heterogeneity of firms included in that category, which makes it impractical to attribute a single corresponding energy price; and 37, *Recycling*, due to an absence of transaction observed in our dataset.

[17] Energy consumption for iron and steel production is dominated by coal use, while non-ferrous metals, which comprise mostly aluminum smelting in most countries, requires principally electricity. These two energy carriers diverge in price significantly, and should not be conflated in our dataset. For the sake of completeness, we shall add that these two sectors are complemented respectively by 2731, Casting of iron and steel, and 2732, Casting of non-ferrous metals.

Figure 1: Number of transactions in the manufacturing sector by acquiring and target country (1995-2014)

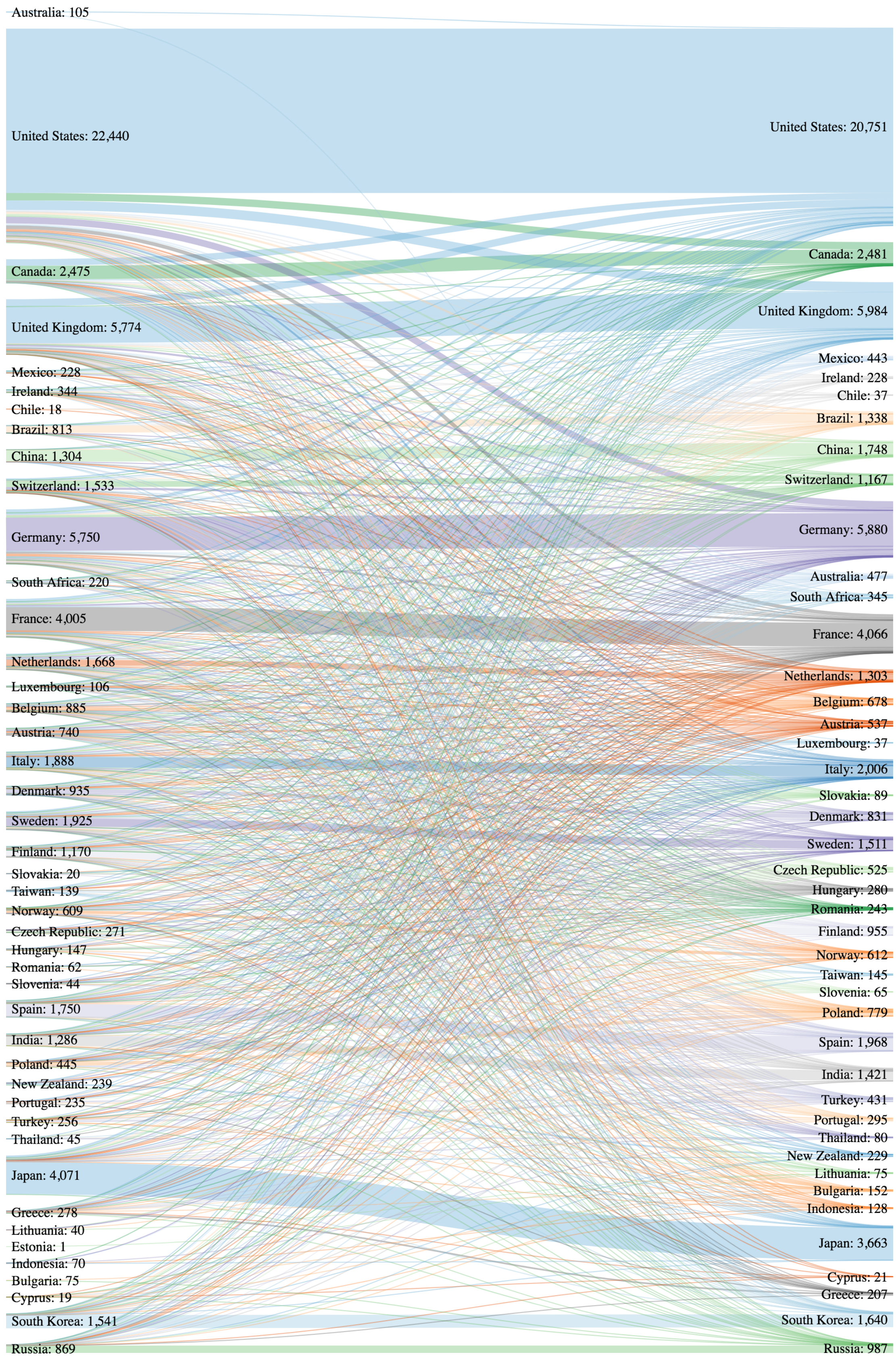


Figure 2: Map of total transactions by acquiring firm location (1995-2014)

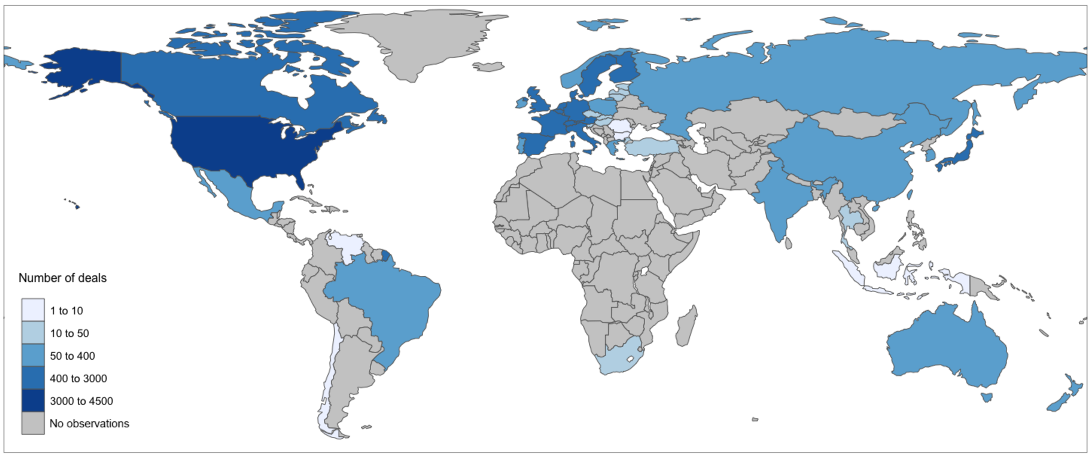


Figure 3: Map of total transactions by target firm location (1995-2014)

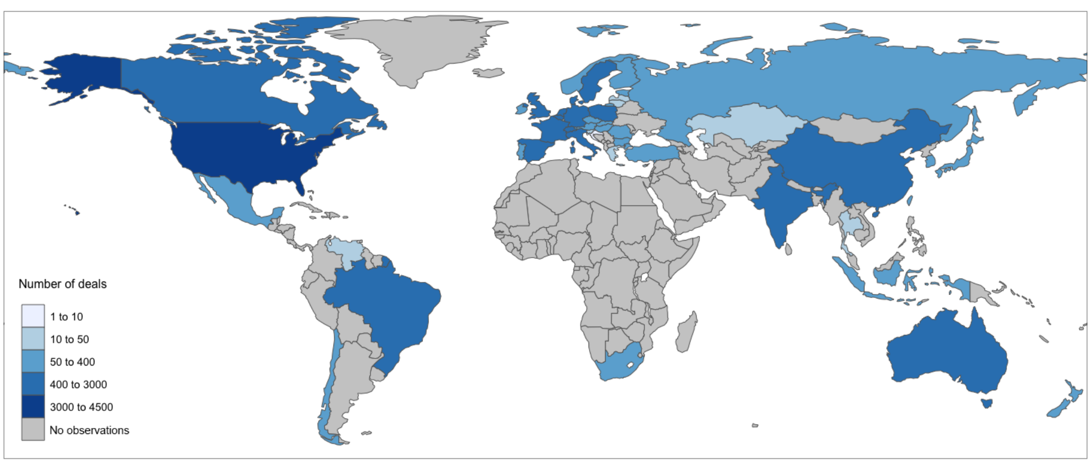


Table 1: Number of transactions by manufacturing subsectors (1995-2014)

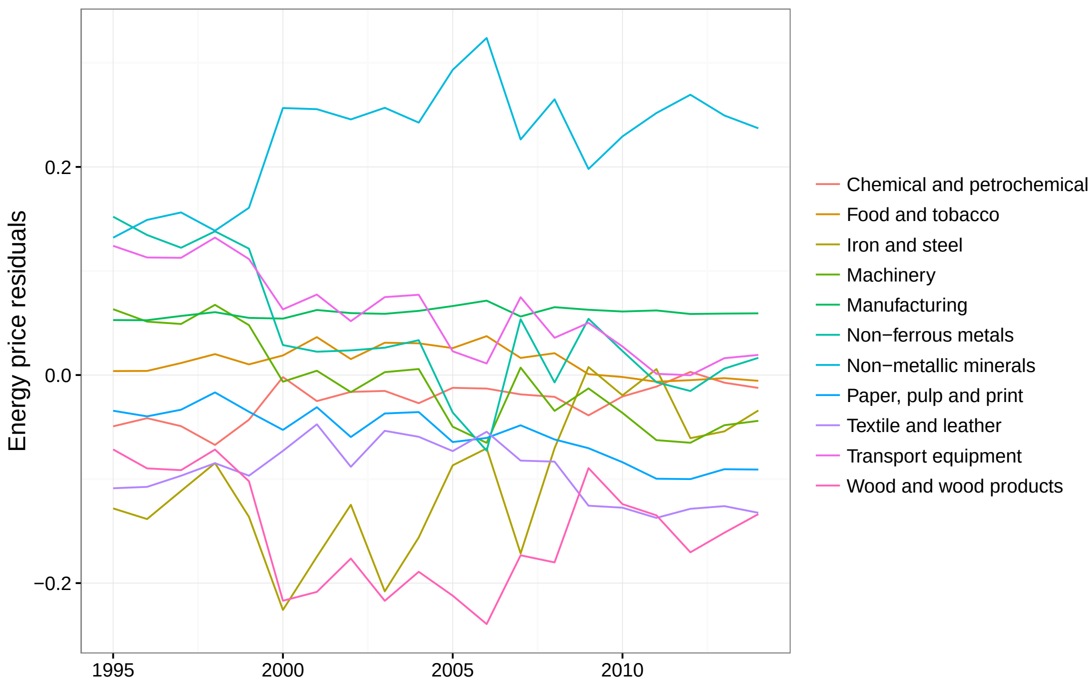
Manufacturing subsector	Within-country	Cross-border
Chemicals and chemical products	6,839	3,649
Food and beverages	5,657	2,224
Printing and publishing	4,673	998
Machinery and equipment n.e.c.	4,507	2,834
Medical, precision and optical instruments	2,652	1,265
Fabricated metal products	2,456	1,253
Rubber and plastics products	2,221	1,221
Coke,refined petroleum products,nuclear fuel	2,201	1,073
Basic metals	2,050	896
Non-metallic mineral products	1,980	1,082
Electrical machinery and apparatus	1,808	1,021
Radio,television and communication equipment	1,772	710
Motor vehicles, trailers, semi-trailers	1,620	1,000
Textiles	1,443	699
Paper and paper products	1,258	617
Furniture; manufacturing n.e.c.	1,063	424
Other transport equipment	942	358
Wearing apparel, fur	773	193
Wood products (excl. furniture)	750	236
Office, accounting and computing machinery	814	333
Leather, leather products and footwear	206	90
Tobacco products	53	65

consumption of each fuel type in that country-sector. The FEPI is available for 12 industrial sectors in 32 OECD and 16 non-OECD countries between 1995 and 2014. The fixed-weight price index is constructed for a given country i, sector k and year t, combination according to the following equation:

$$FEPI_{ikt} = \sum_j \frac{F^j_{ik}}{\sum_j F^j_{ik}} \cdot \log(P^j_{it}) = \sum_j w^j_{ik} \cdot \log(P^j_{it}) \tag{11}$$

where F^j_{ik} are the input quantity of fuel type j in tons of oil equivalent (TOE) for sector k in country i and P^j_{it} denotes the real TOE price of fuel type j for total manufacturing in country i at time t in constant 2010 USD. The prices P^j_{it} are transformed into logs before applying the weights so that the log of the individual prices enter linearly in the equation[18,19].

Figure 4: FEPI cross-sectoral variation (France, 1995-2014)



The weights, w_{ik}^{j}, applied to fuel prices are fixed over time. The use of fixed weights in this index is particularly useful, as it alleviates the common endogeneity concern. That is, energy prices can vary with the amount of energy consumed, hence the choice of fuel types is an endogenous firm decision (Lovo et al., 2014). For example, technological change, fuel substitution or industry-specific shocks on output demand could potentially affect the distribution of fuel consumption within sectors and, ultimately, the sector-level energy prices (Linn, 2008). Using fixed weights in the FEPI allows it to capture only energy price changes that come from variations in fuel prices, while ignoring changes in the mix of fuel inputs. In our main results, we employ average weights corresponding to the mean energy mix over the entire 1995-2014 period[20].

Figure 4 plots the residuals of our energy price index by sector regressed on time fixed effects for

[18]Note that taking the exponential of the FEPI yields the weighted geometric mean of the different fuel prices, so equation (11) is the log of the weighted geometric mean.

[19]The same methodology is employed in the construction of the country level index.

[20]Section 6.4 tests the robustness of the results to alternative definitions of fuel weights.

France during our sample time period. It illustrates how the FEPI captures within-sector variation in energy price over time, which drives our identification. We observe that energy prices have been relatively volatile in energy intensive sectors such as Iron and Steel and Non-metallic minerals, while prices have been more stable in sectors primarily based on electricity such as Machinery.

4.3. Other covariates

We complement energy prices with three additional sets of covariates. First, we approximate the size of the potential pool of acquirer or target companies in a given country-sector pair using sectoral industrial activity as measured by sectoral GDP. We obtain this data from the INDSTAT2 database provided by UNIDO, which reports the value added by ISIC Rev. 3.1 industrial sector at the 2-digit level. As explained in section 4.1, we further disaggregate the value added of *Basic metals* between *Iron and steel* and *Non-ferrous metals* using data from INDSTAT4[21], which provides sectoral information on industrial subsectors at the 4-digit level of the ISIC 3.1 classification.

Table 2: Summary statistics

Transaction side	Variable	Mean	Std. dev.	25^{th} perc.	Median	75^{th} perc.	Obs.
Bilateral	Transactions	0.01	0.31	0.00	0.00	0.00	10,610,945
	Energy price ratio	1.16	0.77	0.69	1.00	1.39	8,876,420
Acquirer	log GDP	21.84	2.55	20.45	21.77	23.07	10,610,945
	Annual wage ($2010)	31,052	27,341	15,335	27,486	43,028	7,436,709
	Labor cost-share	0.45	0.58	0.35	0.47	0.56	”
	Capital cost-share	0.19	0.96	0.09	0.14	0.20	”
Target	log GDP	21.62	2.55	20.15	21.63	22.98	10,610,945
	Annual wage ($2010)	27,138	33,559	8,786	22,556	39,077	7,459,882
	Labor cost-share	0.45	0.54	0.34	0.46	0.56	”
	Capital cost-share	0.27	1.25	0.09	0.15	0.23	”

Second, in order to control for sectoral variations in the cost of other production factors such as labor and capital, we also collect in each sector the following variables: total wages and salaries, employment and gross fixed capital formation. These variables are all collected in US dollars at current market exchange rates. The robustness of our results to these covariates is discussed in detail in section 4.3. Third, we also control for the existence of a free-trade agreement between each country pair, obtained from the CEPII gravity dataset (CEPII, 2018).

[21]To maximize coverage, we use the average of the ratio between *Iron and steel* and *Non-ferrous metals* taken over the period 1995-2014 for years in which this ratio is not observed.

Table 2 presents summary statistics for the dependent and independent variables used in the estimations.

5. Results

5.1. Main results

Table 3: Main results

	All transactions		Acquisition of assets	
	(1) Baseline	(2) Cross-border	(3) Baseline	(4) Cross-border
log $e_{ijkl,t}$	-0.321***	-0.295***	-0.316***	-0.289***
	(0.0949)	(0.0947)	(0.107)	(0.108)
log $GDP_{ik,t}$	0.692***	0.686***	0.715***	0.696***
	(0.0519)	(0.0230)	(0.0594)	(0.0249)
log $GDP_{jl,t}$	0.668***	0.641***	0.690***	0.655***
	(0.0524)	(0.0215)	(0.0603)	(0.0243)
FTA	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes
Pseudo-LL	-227,099	-105,132	-162,877	-74,810
Deviance	196,604	139,291	151,232	103,132
Observations	8,250,030	7,922,200	7,513,020	7,186,030
Transactions	67,903	21,291	46,780	14,685

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our main results are shown in Table 3 and cover transactions over the period 1995 to 2014 and correspond to specification (9). We find evidence that relative industrial energy prices have an impact on the choice of investment location for industrial firms. Column (1) shows the point estimate for β_e is -0.32, implying that a 10% increase in the relative industrial energy price differential between two countries is expected to increase the number of acquisitions by 3.2%. Specifically, firms tend to engage in more cross-border investments when their domestic energy prices increase in relative terms against foreign prices.

Our theoretical model as specified in equation (6) encompasses all deals including domestic deals, yet we also estimate our main specification on the subset of our dataset restricted to cross-border transactions only[22]. Results are reported in column (2) of Table 3. We confirm our main findings, with a point estimate of -0.30, slightly below the estimate obtained on the entire sample.

The logarithm of GDP in both the acquirer and target countries have a positive effect on investment activity. As expected from our gravity model, the number of transactions between a pair of countries is proportional to the size of their economies. The coefficients on the acquirer and target country's GDP are of comparable magnitude, supporting the symmetry of their respective contributions.

5.2. Heterogeneity across developed and emerging economies

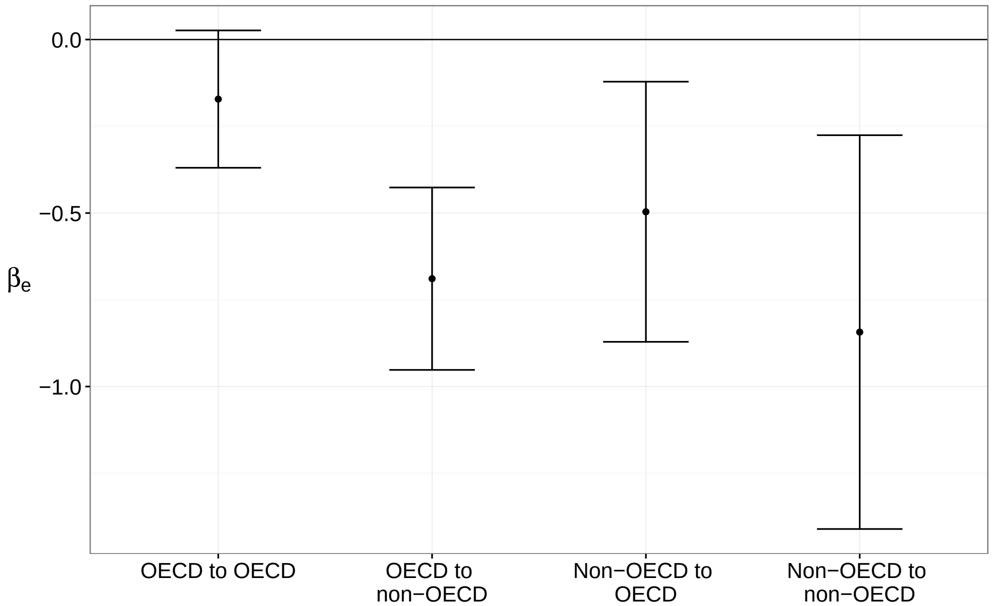
The literature on the impact of relative production factor costs on FDI has focused on investment flows originating in developed countries and targeting emerging economies (Arauzo-Carod et al., 2010). The effect of relative energy prices we found above could stem mostly from that subset of transactions. To test this hypothesis, we estimate how the impact of energy prices on investment location decisions varies depending on whether the acquiring and target firms are based in an OECD or non-OECD country. In practice, we interact our coefficient of interest β_e with four indicator variables corresponding to the four possible combinations of origin and target location, inside and outside OECD member countries. Results are synthesized in Figure 5, and provided in full in Table A.1.

We find that the impact of relative energy costs is twice as large (-0.69) as the sample mean when a transaction occurs from an OECD-based firm to a non-OECD target. Interestingly, the effect on transactions among OECD countries is weakly significant ($p = 0.15$), since the point estimate is four times smaller (-0.17).

The estimates for the last two groups, which include transactions originating from non-OECD countries, are much less robust since the corresponding number of observed transactions is very

[22] We report both the number of observations and the number of transactions observed. The former includes all combinations of country-sector-year in which we observe our covariates but as mentioned, no transactions occurred for most of these combinations. The latter reports the total number of transactions actually observed in the sample. Hence, restricting the sample to cross-border transactions does not impact the sample size significantly, but it does reduce the number of transactions observed by nearly 70%. This is coherent with the share of cross-border transactions reported in section 4.1.

Figure 5: Impact of relative energy prices as a function of OECD membership



Note: 90% confidence intervals.

small (585 and 80 cross-border transactions respectively). Still, they also point towards a greater sensitivity to energy costs in emerging economies.

Overall, our main results show that differences in energy price do affect investment decisions, supporting the pollution haven effect. However, before settling on an interpretation of these estimates, we must subject them to further scrutiny and investigate challenges to the internal and external validity of our results.

6. Robustness

6.1. Robustness to other production factor costs

Are our results driven by omitted variable bias? Previous analyses of the determinants of firms' cross-border investments decisions have found that labor and capital costs are two important covariates (Erel et al., 2012). These are accounted for *at the aggregate country-wide level* in our main specification by the inclusion of country-time fixed effects on both the acquirer and target sides. However, it is possible that like energy costs, variations in labour and capital costs are sectorally differentiated. We thus examine the robustness of our findings to the inclusion of sectoral measures of labor and capital costs as possible confounding factors.

We first consider labor. In keeping with our theoretical model, it may seem intuitive to compute a ratio of sectoral unit labor costs between each country-sector pair, which can be defined as follows (Ceglowski and Golub, 2012):

$$RULC_{ijkl} = \frac{w_{il}}{w_{jl}} \frac{e_{ijl}^{PPP}}{e_{ij}} \tag{12}$$

$$\text{with} \quad w_{il} = \frac{a_{il} W_{il}}{p_{il}}, \quad a_{il} = \frac{L_{il}}{GDP_{il}}, \quad e_{ijl}^{PPP} = \frac{p_{il}}{p_{jl}}$$

where W_{il} is the average annual wage in country i and sector l expressed in national currency, p_{il} the sectoral price index, L_{il} the sectoral labor employment, and a_{il} the sectoral unit labor requirement (the inverse of productivity). e_{ij} is the market exchange rate between countries i and j. e_{ijl}^{PPP} is the *sectoral* purchasing power parity exchange rate for sector l between countries i and j.

Equation (12) implies that relative unit labor costs between two country-sector pairs depend on relative sectoral labor productivity, relative sectoral real wages, and the ratio between the sectoral PPP exchange rate and the aggregate market exchange rate. Yet bringing this equation to the data raises a number of issues. First, its evaluation requires the availability of PPP exchange rates at the sector level. Failing to capture differences in sectoral price levels across countries would bias the RULC indicator. However, to our knowledge, these are not available at the level of coverage required by our dataset[23]. Second, this indicator ignores the large heterogeneity of skills in the workforce and cannot capture the differences in labor qualification across countries and sectors, which could also be a source of bias when calculating a ratio of unit labor costs (Noorbakhsh et al., 2001).

These difficulties highlight the well-known issues in estimating relative level of labor costs across countries, particularly at the sectoral level (Arauzo-Carod et al., 2010). Instead, we resort to alternative approaches commonly used in the comparative advantage and firm location literature to account for sectoral differences in labor costs. Following Guimaraes et al. (2004) and Brülhart et al.

[23]The closest equivalent we found is the Groningen Growth and Development Center's Productivity Level Database, which provides a benchmark estimate of sectoral PPP for the year 2005 in a number of ISIC sectors at the 2-digit level, for a subset of the countries included in our dataset.

(2012), we first include the ratio of sectoral real annual wages to account for the gross difference in labor costs:

$$w_{ijkl} = \frac{W_{il}/p_i^m}{W_{jl}/p_j^m} \tag{13}$$

We compute total annual wages at the sector level using total sectoral labor compensation in USD at market exchange rate and sectoral employment provided by UNIDO's INDSTAT2 database (see section 4.3). Wages in equation (13) labor compensation are then deflated using manufacturing GDP deflators (p_i^m and p_j^m) computed from the World Bank's World Development Indicator database[24].

We then account for differences in labor productivity between sectors by complementing the wage ratio with additional controls. Namely, we include the sectoral cost-shares of labor in value added, on both sides of the transaction (Head and Ries, 1996; Chen and Moore, 2010). These cost shares are computed by taking the ratio of total sectoral labor compensation and sectoral value added.

We adopt a similar strategy to control for sectoral differences in capital costs, by including the cost share of capital in value added. Capital cost shares are calculated by taking the ratio of sectoral gross fixed capital formation and sectoral value added, also available in INDSTAT2.

Results of these robustness checks are presented in Table 4. In columns (1) and (2), we include labor cost shares and wage ratio separately, before including both in column (3). We include both labor and capital cost shares in column (4), and include the whole set of controls in column (5).

Overall, our estimate of β_e is robust to the inclusion of sectoral variations in the cost of other production factors. We also find that on average, the transactions we observe tend to favor labor intensive sectors, but avoid capital intensive ones. This is reminiscent of the footloose hypothesis, whereby the most capital intensive industries are also the least likely to be mobile (Ederington et al., 2005). The smaller point estimate – between -0.24 and -0.27 – than in our baseline results is explained by the restrictions placed by labor and capital cost data availability on our sample. More specifically, INDSTAT2's coverage is better in OECD countries, which increase the relative share of

[24]Except for China, where real manufacturing GDP is not available for the period we consider. We instead use the aggregate GDP deflator.

Table 4: Inclusion of other production factor costs

	All transactions				
	(1) Labor cost-share	(2) Wage ratio	(3) Labor c.s. & wages	(4) Labor c.s. & capital c.s.	(5) All
log $e_{ijkl,t}$	-0.272**	-0.242**	-0.263**	-0.269**	-0.261**
	(0.109)	(0.110)	(0.108)	(0.109)	(0.109)
log $w_{ijkl,t}$		-0.283***	-0.256***		-0.246***
		(0.0800)	(0.0810)		(0.0806)
log $GDP_{ik,t}$	0.722***	0.722***	0.722***	0.723***	0.723***
	(0.0632)	(0.0634)	(0.0632)	(0.0633)	(0.0633)
log $GDP_{jl,t}$	0.633***	0.668***	0.634***	0.634***	0.635***
	(0.0653)	(0.0666)	(0.0652)	(0.0653)	(0.0652)
Labor cost-share (acq.)	-0.0333		-0.0145	-0.0706	-0.0495
	(0.113)		(0.114)	(0.124)	(0.124)
Labor cost-share (tar.)	0.512***		0.494***	0.571***	0.550***
	(0.124)		(0.124)	(0.129)	(0.128)
Capital cost-share (acq.)				0.0360	0.0339
				(0.0728)	(0.0731)
Capital cost-share (tar.)				-0.100*	-0.0969*
				(0.0541)	(0.0537)
FTA	Yes	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes	Yes
Pseudo-LL	-147,186	-147,531	-147,167	-147,115	-147,097
Deviance	118,424	118,597	118,403	118,356	118,335
Observations	4,271,897	4,274,970	4,271,897	4,260,055	4,260,055
Transactions	46,737	46,737	46,737	46,737	46,737

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

within-OECD transactions in our sample. Given that transactions between firms based in developed economies are less sensitive to relative energy prices, as evidenced in section 5.2, this biases our estimate of β_e downwards. We confirm this explanation by estimating our baseline regression on a sample identical to that used in column (5), and find a point estimate of $\beta_e = -0.25$ (see Table in the Appendix).

6.2. Robustness to controlling for multi-lateral resistance terms

As noted, an important challenge in these estimations is to adequately control for the multilateral resistance term. To test the robustness of our results to this, we implement a second identification approach compatible with the fixed effect structure theoretically consistent with structural gravity, as described in section 3. This identification strategy further offers the possibility to explore the heterogeneity of relative energy price sensitivity across sectors, notably as a function of their energy intensity as is demonstrated in section 7.1.

Identification strategy

Our model, as formulated in equation (6), is not identifiable by controlling for the multilateral resistance terms through a fixed effect structure consistent with structural gravity. However, an alternative estimation strategy can still allow us to indirectly recover the effect of relative energy prices on investment location. In a first step, we project our dependent variable – investment activity as measured by the number of deals observed – on the fixed effect structure described in equation (8). Thus, we first estimate (14) using PPML:

$$m_{ijklt} = \exp\left[\alpha_{ij} + \eta_{ikt} + \nu_{jlt}\right] + \varepsilon_{ijklt} \tag{14}$$

We then recover the estimated η_{ikt} and ν_{jlt}, and use them as dependent variables in second-step linear regressions that include the locational characteristics X_c of the acquirer and the target. We estimate both equations jointly as Seemingly Unrelated Regressions (SUR):

$$\begin{cases} \eta_{ikt} & = \gamma_1^{acq} \log GDP_{ik,t} + \gamma_e^{acq} \log E_{il,t} + \epsilon_{ikt}^{acq} \\ \nu_{jlt} & = \gamma_1^{tar} \log GDP_{jl,t} + \gamma_e^{tar} \log E_{jl,t} + \epsilon_{jlt}^{tar} \end{cases} \tag{15}$$

While this procedure does not estimate the impact of relative production factors costs directly, it offers a number of interesting possibilities. First, $\hat{\gamma}_e^{tar} - \hat{\gamma}_e^{acq}$ still provides an estimate of the effect of the difference in energy costs between target and acquirer. The use of SUR allows us to derive confidence intervals for our point estimates using the delta method[25] (Hoef, 2012). Based

[25]The size of our dataset makes it impractical to implement the bootstrap.

on our theoretical model, we therefore expect $\hat{\gamma}_e^{tar} < \hat{\gamma}_e^{acq}$ if acquirers are attracted to invest in country-sectors where production costs are lower.

Second, our main specification's reliance on cross-sectoral energy costs variance to drive identification rules out estimating β_e by sector, even though exploring sectoral heterogeneity would be of interest in our inquiry. This second procedure suffers no such limitation. Since the fixed effects structure in equation (14) is orthogonal by construction, it is possible to estimate (15) separately for distinct groups of sectors. This allows us to examine the heterogeneity of the energy price effect, which we perform in section 7.1.

Estimation results

We now implement the procedure laid out in equations (15) and present the results. Table 5 reports the regression of sectoral GDP and sectoral energy price indices on country-sector-year fixed effects. These fixed effects are estimated in a first stage PPML estimation, both on the acquirer and target side, as laid out in subsection 6.2. We then stack them and regress them in a single SUR estimation as discussed above on the locational characteristics of the acquirer and target firms.

Table 5: Regression on acquirer and target country-sector-year fixed effects

	(1) Baseline	(2) Cross-border
$\log FEPI_{ik,t}$	0.110***	0.118***
	(0.008)	(0.009)
$\log FEPI_{jl,t}$	-0.138***	-0.155***
	(0.008)	(0.008)
$\log GDP_{ik,t}$	0.689***	0.677***
	(0.002)	(0.002)
$\log GDP_{jl,t}$	0.776***	0.769***
	(0.002)	(0.002)
β_e	-0.249***	-0.273***
	(0.009)	(0.010)
R^2	0.08	0.08
Observations	17,980,815	17,325,155
Transactions	69,912	69,912

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Our results are consistent with our theoretical predictions and with our previous results. We find that the projection of investment activity on country-sector-year fixed effects is negatively correlated with the energy price at the target destination. This strengthens our main finding, by supporting that firms do take into account production factor costs when deciding on the location of their investments, and that they are attracted to low energy costs destinations.

The point estimate for the impact of relative energy prices, which we note $\beta_e \equiv \hat{\gamma}_e^{tar} - \hat{\gamma}_e^{acq}$, is remarkably similar to the one we obtain in our main identification strategy in column (1) of Table 3. It is however substantially smaller when controlling for labor costs, while remaining highly significant. This may again be biased by measurement error in our estimate of sectoral unit labor costs. This discrepancy is reduced when restricting the sample to cross-border deals only.

6.3. Time lags

A further area of concern for potential endogeneity is our use of current-period energy prices in both acquirer and target countries. Indeed, cross-border investments may result in an increase of activity in countries on the receiving end of foreign investments while potentially reducing it on the acquiring side. This in turn would impact energy demand, and therefore energy prices, on both sides of any given deal – yielding a source of endogeneity between cross-border activity and energy prices. We control for this by using the one-year lag of energy prices in specification (9):

$$
\begin{aligned}
m_{ijkl,t} = \exp\Big[&\beta_1 \log GDP_{ik,t} + \beta_2 \log GDP_{jl,t} + \beta_e \log e_{ijkl,t-1} + \\
&\beta_5\, fta_{ij,t} + \alpha_{0,ij} + \alpha_{1,k} + \alpha_{2,l} + \alpha_{3,it} + \alpha_{4,jt}\Big] + \varepsilon_{ijkl,t}
\end{aligned} \tag{16}
$$

Besides, we have considered so far that firms react to changes in energy prices very rapidly, within a one-year window. Yet, analysis of economic actors' response to exogenous price or policy signals in the trade literature has revealed that they may in fact adjust their reaction over a multiple year period (Head and Mayer, 2014). Industrial firms' decision process in choosing the location of their investments may follow the same temporality. To test against this possibility, we follow Hijzen et al. (2008) and aggregate our dataset over two, three and four-year intervals by taking the mean of the dependent variable and of each regressor over the interval considered:

[26] While the number of observations is reduced by the aggregation procedure, the sample covered is identical in columns (2)-(4).

Table 6: Robustness to time lag[26]

	(1) 1-year lag	(2) 2-year averages	(3) 3-year averages	(4) 4-year averages
log $e_{ijkl,t-1}$	-0.307***			
	(0.0942)			
log $e_{ijkl,t}$		-0.338***	-0.307***	-0.324***
		(0.0944)	(0.0945)	(0.0967)
log $GDP_{ik,t}$	0.688***	0.699***	0.678***	0.689***
	(0.0517)	(0.0514)	(0.0496)	(0.0501)
log $GDP_{jl,t}$	0.666***	0.675***	0.658***	0.672***
	(0.0523)	(0.0518)	(0.0503)	(0.0507)
FTA	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes
Pseudo-LL	-223,279	-111,840	-70,077	-52,826
Deviance	191,885	144,473	113,661	96,011
Observations	7,773,922	4,118,580	2,605,890	1,976,266
Transactions	66,611	67,903	67,903	67,903

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

$$\overline{x}_t^{\tau} = \sum_{t'=t}^{t+\tau-1} \frac{x_{t'}}{\tau}, \text{ with } \tau \in \{2,3,4\}, x \in \{m_{ijkl}, GDP_{ik}, GDP_{jl}, e_{ijkl}\} \tag{17}$$

The results of these robustness checks are presented in Table 6. We find that our estimate of β_e is remarkably similar across specifications, varying between -0.35 and -0.31. This is statistically indistinguishable from our main estimate of -0.32. Firms' response to relative energy prices thus appear consistent in both the short and long-run, which strengthens the validity of our static model.

6.4. Alternative energy price index

Our findings in section 5 hinge on the energy price index used to measure sectoral energy costs in each country represented in our dataset. As discussed in section 4.2 we use fixed weights in our weighted energy price index to mitigate the risk of endogeneity.

In this section, we test another alternative energy price index introduced in Sato et al. (2019): the variable-weight energy price level (VEPL). This latter measure of sectoral energy prices departs from the FEPI by weighting the price of each energy carrier using the actual energy mix observed in each year – hence yielding a *variable*-weight indicator. Further, energy prices are observed at current market exchange rates while the FEPI used in our main dataset is constructed from energy prices measured in constant 2010 USD.

Table 7: Main results using the VEPL energy price index

	All transactions		Acquisition of assets	
	(1) Baseline	(2) Cross-border	(3) Baseline	(4) Cross-border
$\log e_{ijkl,t}^{VEPL}$	-0.214**	-0.178*	-0.218*	-0.177
	(0.107)	(0.106)	(0.122)	(0.119)
$\log GDP_{ik,t}$	0.685***	0.685***	0.709***	0.692***
	(0.0559)	(0.0258)	(0.0639)	(0.0279)
$\log GDP_{jl,t}$	0.663***	0.641***	0.687***	0.655***
	(0.0563)	(0.0238)	(0.0647)	(0.0268)
FTA	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes
Pseudo-LL	-207,117	-91,772	-149,583	-65,693
Deviance	172,371	120,027	134,048	89,658
Observations	6,291,857	6,014,498	5,799,762	5,523,243
Transactions	63,681	19,039	44,131	13,204

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As described in section 4.2, the VEPL suffers by construction from the endogeneity and it captures both changes to prices and fuel mix. As we know that sectors switch fuels in response to prices, this suggests that using VEPL instead of FEPI would underestimate the effect of relative energy prices. Indeed as shown in Table 7, the estimated β_e appears a third smaller, at -0.19 in the main specification. The point estimates of β_e are also less well identified which may be due to the reduced data availability of the VEPL indicator, which requires an observation of the sectoral energy mixes for every single year. This is evidenced by the size of the total sample, smaller by a quarter when using VEPL rather than our main FEPI index. Even then, the magnitude and sign of the β_e estimated using VEPL remain consistent with our main results.

6.5. *Further robustness tests*

While the geographical coverage of our dataset accounts for most of the world's economy, some countries are more represented than others – both as a consequence of their economic size and the degree of their integration in the global economy. The top five countries on the receiving end of transactions include the United States (30% of all transactions observed), the United Kingdom (9%), Germany (8%), France (6%) and Japan (5%). The ranking and the proportions are similar on the acquiring side.

To ensure that our results are not driven by any single one of these nations, we remove each country separately from the dataset (both on the acquiring and target sides) and re-estimate our baseline model. Results are presented in Table A.3. They support our main findings, with a very consistent estimate of β_e comprised between -0.30 and -0.35.

7. Extensions

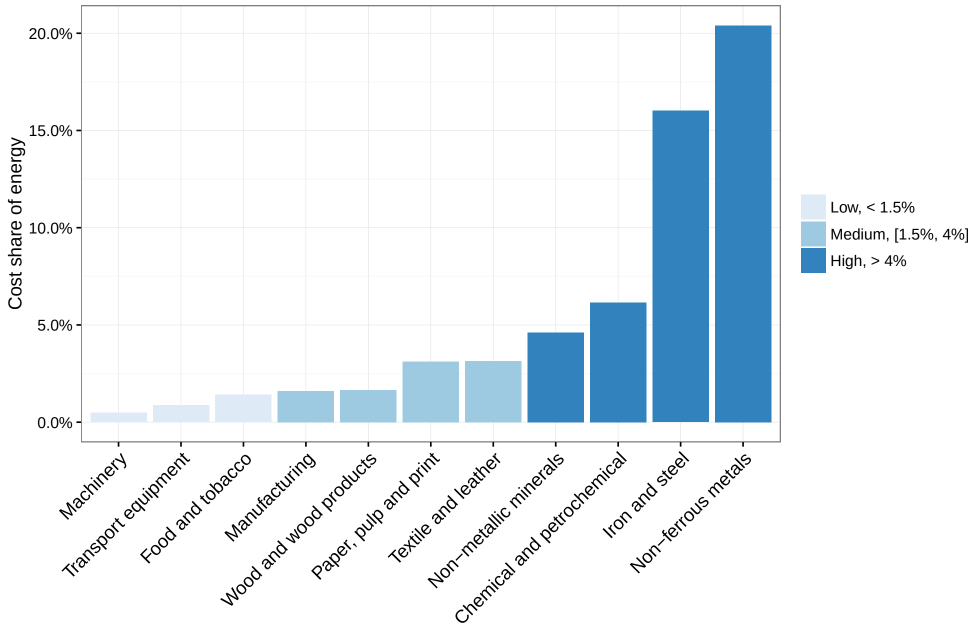
7.1. *Heterogeneity across sectors*

Theory predicts that the effect of energy price on foreign investment decisions is more pronounced in energy intensive sectors where energy costs represent a higher share of overall production costs, and this is broadly supported by related empirical papers (e.g. Panhans et al. (2016), Aldy and Pizer (2015) and Sato and Dechezleprêtre (2015)). Here we evaluate whether energy-intensive sectors are more sensitive to relative energy prices when making investment location decisions, using our second estimation strategy, equation (14).

Specifically we delineate groups of sectors defined by their energy intensity and estimate equation (15) for each group. This aggregation of sectors helps to preserve a sufficient number of observed transactions in each group. Given our emphasis on energy prices, we estimate energy intensity through the share of energy costs in the total real output of each sector as measured by value added. Energy use data is obtained from the IEA, which is then combined with our energy price index and UNIDO's sectoral value added to yield our energy intensity indicator. The mean energy intensity of each sector over our entire sample is presented in Figure 6.

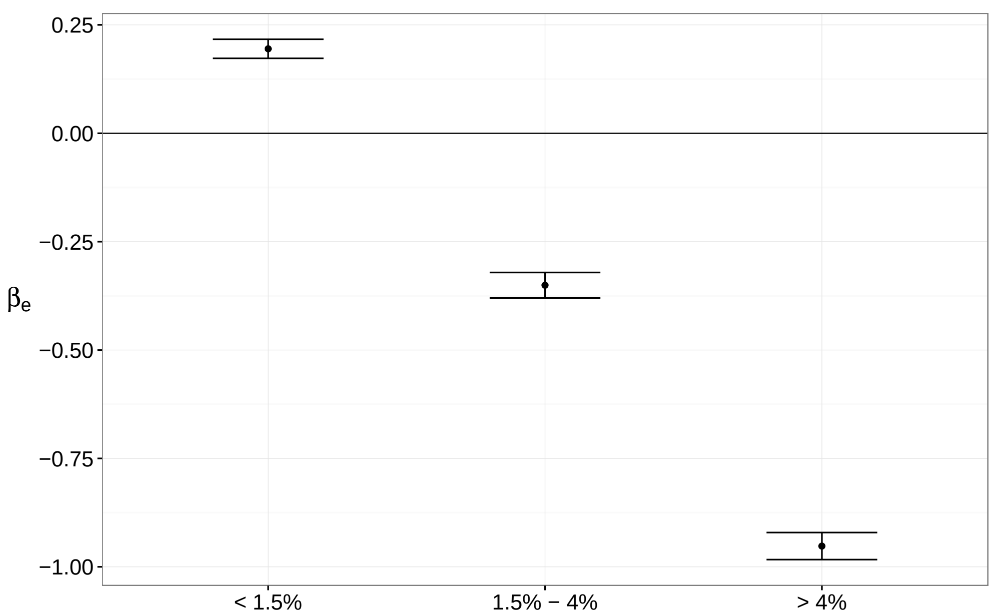
We distinguish three groups: low energy intensity, corresponding to an energy cost share of less than 1.5%; medium intensity, between 1.5% and 4%; and high intensity, above 4%. The cutoffs have

Figure 6: Mean cost-share of energy by sector (All countries, 1995-2014)



been chosen to balance the three groups, regarding both the number of sectors and the number of transactions observed in each group. After estimating equation (15) in each intensity group, we compute an estimate of $\beta_e = \hat{\gamma}^{tar} - \hat{\gamma}^{acq}$. The results are presented in Figure 7. In complement, full results are provided in Table A.4.

Figure 7: Impact of relative energy prices as a function of sectoral energy intensity



We find significant heterogeneity in the impact of relative energy prices on investment activity, with estimates for β_e ranging from -0.95 to 0.19. It shows sectoral sensitivity to relative energy prices grows with energy intensity, in line with intuition and previous findings in the literature (e.g. Panhans et al. (2016)). Interestingly, we find a positive effect on non-energy intensive sectors albeit small in magnitude. Following similar empirical findings (e.g. Ben Kheder and Zugravu (2012)) a number of theoretical papers [27] offer possible rationale. For example, cross border investments in non-energy intensive sectors may be more aligned with local-market oriented FDI rather than export-oriented FDI, the former being less sensitive to environmental regulation (Tang, 2015). Additionally, if acquiring firms have more energy efficient technologies, they have a cost advantage to domestic firms in host country (Dijkstra et al., 2011).

7.2. Policy simulation

Our findings broadly support the pollution haven effect. However, are the effects economically important? To provide a better understanding of the magnitude of potential effects, we perform a number of counterfactual simulations using the model of M&A transactions introduced in section 2 and the parameters estimates obtained in section 5. More specifically, we simulate the implementation of a carbon tax of \$50/tCO_2 in three different policy scenarios:

- The European Union implements the carbon tax unilaterally
- EU and OECD member countries, except the United States, implement the carbon tax
- All countries in our sample implement the carbon tax

This choice of scenarios illustrates various degrees of increasing international collaboration, from complete isolation of the EU in the first variant to a globally coordinated carbon tax implementation in the third scenario. We consider the United States separately in the second scenario to reflect the recent decision of the US government to pull out of the Paris Agreement[28]. Note that in all variants, we consider the gross impact in the absence of any compensatory policies such as free allocation in emissions trading or border carbon adjustment (Morris, 2018). These measures would limit the impacts we describe here.

[27] See Cole et al. (2017) for a recent summary.
[28] 'Donald Trump confirms US will quit Paris climate agreement', The Guardian, June 1st, 2017

To simulate the impact of a carbon tax, we first consider a cross-section as the basis for our conterfactual experiment. We choose the year 2010, which offers the widest coverage among recent years in our dataset. We then use the carbon content of each fossil energy carrier – coal, oil and natural gas – to calculate the equivalent increase in price that results from the carbon tax. This step modifies relative energy prices in all pairs of country-sectors in which a country that implemented the tax appears.

We then estimate the impact on investment activity by applying our model of investment location, equation (6), to the new relative energy prices calculated above and the parameters we estimated in section 5. In order to provide a more faithful assessment of cross-border impacts, we use the geographically heterogeneous estimates of β_e obtained in section 5.2.

By denoting with a star the counterfactual number of transactions, relative energy prices and multi-lateral resistance terms impacted by carbon taxation, the relative impact on investment activity can be estimated as follows:

$$\frac{m^*_{ijkl}}{m_{ijkl}} = \left(\frac{e^*_{ijkl}}{e_{ijkl}}\right)^{\beta_{e,ij}} \frac{\Omega_{ijkl}}{\Omega^*_{ijkl}} \tag{18}$$

where $\beta_{e,ij}$ takes one of the four values estimated in section 5.2 based on countries i and j's OECD membership status.

Note that in equation (18), we take into account the fact that changing relative energy prices in a subset of countries modifies the multi-lateral resistance terms Ω_{ijkl} for the entire dataset. The implementation of a carbon tax in country j thus affects investments received from another country i through the direct modification of relative energy costs between the two countries, but also through the change in the attractiveness of j against all other countries, as measured by Ω_{ijkl}.

This is in contrast with a simpler approach, which would only consider the direct impacts resulting from the change in bilateral relative energy costs – term $\left(\frac{e^*_{ijkl}}{e_{ijkl}}\right)^{\beta_{e,ij}}$ in equation (18). By analogy with the structural gravity literature, this simpler method would yield *partial equilibrium* effects, while the approach adopted in equation (18) is equivalent to what Yotov et al. (2013) label *conditional equilibrium* effects.

Yet, it should be emphasized that neither approaches yield general equilibrium effects. In particular, we cannot consider in our framework the impact of the carbon tax on sectoral and aggregate economic activity, nor on firm entry and exit. Taking into account the consequences of reduced foreign investments on domestic activity would reduce the relative attractiveness of countries that implement a carbon tax even more, further increasing the negative impact of the tax on investment inflows. The following results thus only set lower bounds on the true magnitude of the effects. The detailed analysis of these general equilibrium aspects is left to future inquiries.

Table 8: Impacts of the implementation of a $50/t$CO_2$ carbon tax on the number of domestic firms acquired

	EU-only	EU and OECD without the US	All
United States	1.0%	2.2%	-3.0%
European Union	-3.7%	-2.6%	0.9%
Japan	1.0%	-2.4%	1.1%
BRICS	1.0%	2.2%	-29.3%
Other OECD	1.0%	-3.9%	-0.5%
Other non-OECD	1.0%	-0.5%	-8.2%

We perform the simulation by first computing the increase in fossil energy prices resulting from the carbon tax in countries that implement it. We then compute an updated set of Ω^*_{ijkl} using our carbon tax augmented energy prices[29]. As is the case in the estimation of the model, the computation of Ω^*_{ijkl} makes use of sectoral GDP *in lieu of* the number of firms. We finally apply equation (18) to obtain the impact of the carbon tax on the number of cross-border transactions.

We consider the impact of the three scenarios outlined above on the number of acquisitions – which corresponds to FDI *inflows* – relative to the baseline. Hence, a reported -5% in a given country implies that 5% fewer industrial firms would be acquired in that country as a result of the carbon tax.

Results are presented by regional aggregates in Table 8, and at the country level in Figures 8 through 10. Aggregate effects at the country or regional levels are obtained as the mean of sectoral

[29]The calculation of Ω_{ijkl} requires information on both the acquiring and target sides. The reference cross-section includes more than 700,000 observations. Computing the multi-lateral resistance terms thus involves calculations on a 700,000 $\times$ 700,000 matrix, which is impractical on commodity hardware. The algorithm was therefore implemented on high-performance Nvidia Tesla V100 GPU using the Google Compute Engine. This custom implementation brought down the runtime to compute a single set of Ω_{ijkl} from 19 hours to a more manageable 30 min, thereby making the present simulations feasible.

Figure 8: Implementation of a $50/tCO$_2$ carbon tax by the EU only

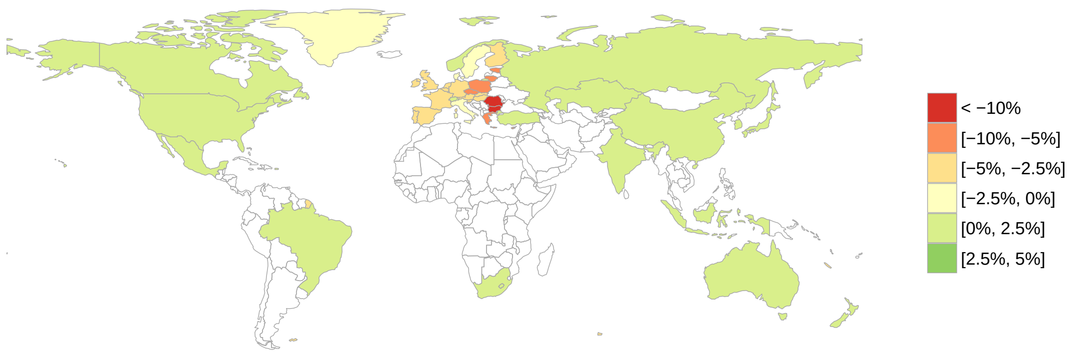


Figure 9: Implementation of a $50/tCO$_2$ carbon tax by EU and OECD countries except the U.S.

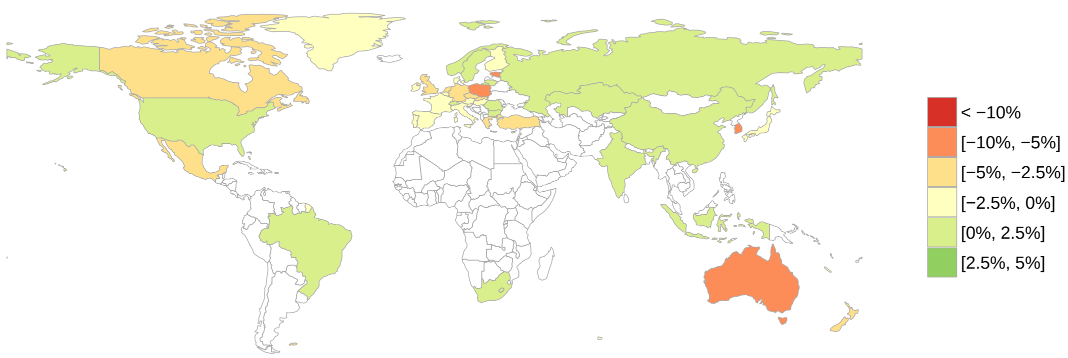
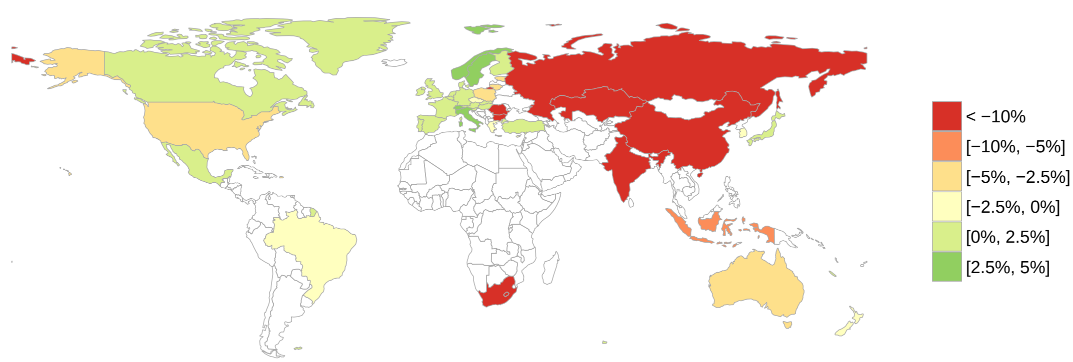


Figure 10: Implementation of a $50/tCO$_2$ carbon tax by all countries



Note: Impact on the number of firms acquired in relative terms against a 2010 baseline. For example, an impact of -5% in a given country implies that 5% fewer industrial firms would be acquired in that country as a result of the carbon tax.

impacts weighted by sectoral GDP. In complement, detailed estimates at the country level are provided in Table A.3.

As expected, we find that the first scenario decreases the attractiveness of EU's industrial firms, leading to a 3.7% reduction in the expected number of firms acquired. Symmetrically, other regions experience a 1% increase in the number of their expected inbound transactions. The effect is homogeneous in all regions outside of the EU as a result of the conditional equilibrium approach adopted. The positive effect on each country's relative attractiveness is averaged into an aggregate impact by the adjustments in the multi-lateral resistance terms. The effect is heterogeneous across the EU, due to variation in energy mix: the impact ranges from -0.8% in Sweden to -16.1% in Bulgaria. In this latter case, the effect is magnified by the larger sensitivity to energy prices we apply to non-OECD member countries.

In the second scenario, coordination of the EU with all other developed countries – except the US – reduces the negative impacts sustained in Europe to -2.4%. However, we find in the third scenario that implementing a uniform \$50/tCO_2 globally would very negatively impact China, India, Russia, and South Africa, with impacts comprised between -20% and -42%. This is expected from the carbon intensity of their economy – in particular, the share of fossil fuels in their electricity mix was comprised between 70% and 95% in 2010 – and the higher contribution of energy-intensive activities to their industrial sector. The effect is again compounded by the greater sensitivity of non-OECD countries to relative energy prices.

By contrast with the first scenario, a global carbon tax would actually make European countries more attractive to foreign investors, with countries such as Norway or Sweden experiencing increases in the number of acquisitions of 4.2% and 3.9% respectively.

8. Conclusion

Concerns about the potential competitive disadvantages associated with high carbon prices contribute to the failure to implement necessary levels of carbon pricing (Stern and Stiglitz, 2017). These fears are magnified by trends in globalization, which has enhanced economic integration and increased cross-border investment activity.

In this paper, we address this issue using an identification strategy driven by exogenous variations in industrial energy prices at the sector level. Building on the application of discrete choice theory to the firm location problem, we specify a conditional logit model linking bilateral investment activity to relative energy prices. We estimate this model by combining a rich firm level dataset of 70,000 M&A transactions covering 41 countries between 1995 and 2014 with a dataset on industry level energy prices. This represents the first analysis in a global context to focus on the role of *relative* energy prices as possible determinants of cross border investments.

As predicted by our theoretical model, we find evidence that relative industrial energy prices have an impact on industrial foreign investment location, which broadly supports the pollution haven effect. Specifically, firms tend to engage in more cross-border investments when their domestic energy prices increase in relative terms against foreign prices. Our preferred specification suggests that an increase of 10% in the relative industrial energy price differential between two countries is expected to increase by 3.2% the number of acquisitions of firms or assets located in the lower energy price country by firms based in the more expensive country.

We also find evidence of heterogeneity in the effect of relative energy prices across countries and sectors. We find that the impact of relative energy costs on investment location is four times larger for transactions targeting non-OECD countries than for those targeting OECD countries. In addition, we find that the effect is heterogeneous across sectors, and grows with sectoral energy intensity.

To assess the magnitude of the impact of relative energy price differentials on inbound industrial investment flows, we conduct counterfactual simulations of the implementation of a \$50/tCO_2 carbon price by three different coalitions of countries. We find that unilateral implementation by developed economies would have a limited negative impact on their attractiveness to foreign industrial investments.

These results support the argument for better targeting leakage prevention measures to complement carbon pricing for energy-intensive industrial activities. Our results indicate that the expected adverse impacts on leakage and competitiveness are likely to be limited in sectoral scope and magnitude. Currently, excess allocation of free allowances is prevalent in carbon emissions trading systems across the world (e.g. California, European Union, New Zealand, South Korea and the

Chinese pilot schemes). In Europe, compensation is also granted to electricity-intensive sectors for the indirect emissions costs in electricity prices. These measures should instead focus on the most energy-intensive sectors.

Our analysis can be extended in several directions. The dataset could be augmented with more comprehensive data on the value of the transactions observed, to improve the quantification of the effect. Alternatively, an analysis focused on the subset of the transactions involving listed companies, for which relevant covariates at the firm level are publicly available, could be conducted. Additionally, the model developed in this paper provides a basis for the calibration of border carbon adjustments that would cancel out the negative impacts of unilateral carbon pricing on the attractiveness to foreign industrial investments. It could be further extended to a full structural gravity model, which would allow the estimation of general equilibrium effect of relative energy prices on industrial investment location. This and other extensions are left for future research.

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Appendix A Complementary results

Table A.1: Impact of relative energy prices as a function of OECD membership

	All transactions			
	(1) OECD to OECD	(2) OECD to non-OECD	(3) Non-OECD to OECD	(4) Non-OECD to non-OECD
log $e_{ijkl,t}$	-0.172	-0.689***	-0.496**	-0.843**
	(0.120)	(0.160)	(0.228)	(0.345)
log $GDP_{ik,t}$	0.692***			
	(0.0519)			
log $GDP_{jl,t}$	0.670***			
	(0.0526)			
FTA	Yes			
Country-pair FE	Yes			
Country-time FE	Yes			
Sectoral FE	Yes			
Pseudo-LL	-227,076			
Deviance	196,570			
Observations	5,992,902	1,545,829	1,021,245	316,444
Transactions	60,626	2,331	585	4,361
Cross-border transactions	18,295	2,331	585	80

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: Baseline regression estimated on the subsample where labor and capital costs are observed

	All transactions
	Baseline
log $e_{ijkl,t}$	-0.251**
	(0.110)
log $GDP_{ik,t}$	0.722***
	(0.0634)
log $GDP_{jl,t}$	0.666***
	(0.0667)
FTA	Yes
Country-pair FE	Yes
Country-time FE	Yes
Sectoral FE	Yes
Pseudo-LL	-147,555
Deviance	118,622
Observations	4,274,970
Transactions	46,737

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: Robustness to the removal of the most represented countries in the dataset

	All transactions				
	(1) Without the US	(2) Without the UK	(3) Without Germany	(4) Without France	(5) Without Japan
log $e_{ijkl,t}$	-0.352***	-0.346***	-0.305***	-0.299***	-0.328***
	(0.0791)	(0.104)	(0.101)	(0.100)	(0.103)
log $GDP_{ik,t}$	0.723***	0.680***	0.690***	0.691***	0.705***
	(0.0381)	(0.0564)	(0.0570)	(0.0550)	(0.0541)
log $GDP_{jl,t}$	0.689***	0.654***	0.667***	0.669***	0.677***
	(0.0390)	(0.0567)	(0.0577)	(0.0556)	(0.0545)
FTA	Yes	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes	Yes
Pseudo-LL	-158,339	-199,200	-196,681	-204,864	-213,622
Deviance	157,649	170,795	166,623	173,192	185,778
Observations	7,697,720	7,633,199	7,606,422	7,665,916	7,795,597
Transactions	41,943	59,929	59,778	62,342	63,547

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: Impact of relative energy prices as a function of sectoral energy intensity

	Sectoral energy intensity		
	(1) Low	(2) Medium	(3) High
log $FEPI_{ik,t}$	-0.429***	0.740***	0.237***
	(0.012)	(0.016)	(0.017)
log $FEPI_{jl,t}$	-0.234***	0.390***	-0.715***
	(0.010)	(0.016)	(0.016)
log $GDP_{ik,t}$	0.654***	0.732***	0.685***
	(0.003)	(0.004)	(0.005)
log $GDP_{jl,t}$	0.665***	0.799***	0.937***
	(0.003)	(0.005)	(0.004)
β_e	0.195***	-0.350***	-0.952***
	(0.013)	(0.018)	(0.019)
N	8,054,834	5,897,748	4,028,233
R^2	0.10	0.08	0.09

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: Counterfactual simulation results by country

	EU-only	EU and OECD without the US	All
Australia	1.0%	-6.2%	-2.9%
Austria	-3.5%	-2.3%	1.2%
Belgium	-3.0%	-1.8%	1.7%
Brazil	1.0%	2.2%	-1.6%
Bulgaria	-16.2%	-15.2%	-12.1%
Canada	1.0%	-3.4%	0.0%
China	1.0%	2.2%	-36.6%
Cyprus	-6.5%	-5.4%	-2.0%
Czechia	-5.9%	-4.8%	-1.4%
Denmark	-2.4%	-1.3%	2.3%
Estonia	-7.5%	-6.4%	-3.1%
Finland	-3.1%	-1.9%	1.6%
France	-2.5%	-1.4%	2.1%
Germany	-4.0%	-2.9%	0.6%
Greece	-5.3%	-4.2%	-0.8%
Hungary	-2.5%	-1.4%	2.2%
India	1.0%	2.2%	-15.9%
Indonesia	1.0%	2.2%	-8.0%
Ireland	-3.1%	-2.0%	1.5%
Italy	-2.1%	-1.0%	2.6%
Japan	1.0%	-2.4%	1.1%
Kazakhstan	1.0%	2.2%	-43.1%
Lithuania	-7.9%	-6.8%	-3.4%
Luxembourg	-3.1%	-2.0%	1.5%
Mexico	1.0%	-2.7%	0.8%
Netherlands	-3.8%	-2.7%	0.8%
New Zealand	1.0%	-4.3%	-0.9%
Norway	1.0%	0.6%	4.2%
Poland	-8.8%	-7.7%	-4.4%
Portugal	-2.9%	-1.7%	1.8%
Romania	-16.0%	-15.0%	-11.9%
Russia	1.0%	2.2%	-20.8%
Slovakia	-3.7%	-2.6%	0.9%
Slovenia	-2.8%	-1.6%	1.9%
South Africa	1.0%	2.2%	-41.8%
South Korea	1.0%	-5.4%	-2.0%
Spain	-3.6%	-2.5%	1.0%
Sweden	-0.8%	0.3%	3.9%
Switzerland	1.0%	0.2%	3.8%
Taiwan	1.0%	2.2%	-17.8%
Turkey	1.0%	-3.4%	0.1%
United Kingdom	-4.6%	-3.4%	0.0%
United States	1.0%	2.2%	-3.0%

Appendix B Sectoral classifications

Table B.1: IEA sectors definitions

IEA	ISIC rev. 4
Iron and steel	241, 2431
Chemical and petrochemical	20, 21
Non-ferrous metals	242, 2432
Non-metallic minerals	23
Transport equipment	29, 30
Machinery	25, 26, 27, 28
Mining and quarrying	07, 08, 099
Food, beverages and tobacco	10, 11, 12
Paper, pulp and printing	17, 18
Wood and wood products	16
Construction	41, 42, 43
Textile and leather	13, 14, 15
Industry	22, 31, 32

Table B.2: Correspondence between ISIC 3.1 and IEA sectors

ISIC 3.1 Code	ISIC 3.1 Name	IEA Sector
15	Food and beverages	Food and tobacco
16	Tobacco products	Food and tobacco
17	Textiles	Textile and leather
18	Wearing apparel, fur	Textile and leather
19	Leather, leather products and footwear	Textile and leather
20	Wood products (excl. furniture)	Wood and wood products
21	Paper and paper products	Paper, pulp and print
22	Printing and publishing	Paper, pulp and print
23	Coke,refined petroleum products,nuclear fuel	Chemical and petrochemical
24	Chemicals and chemical products	Chemical and petrochemical
25	Rubber and plastics products	Manufacturing
26	Non-metallic mineral products	Non-metallic minerals
2710	Iron and steel	Iron and steel
2720	Non-ferrous metals	Non-ferrous metals
28	Fabricated metal products	Machinery
29	Machinery and equipment n.e.c.	Machinery
30	Office, accounting and computing machinery	Machinery
31	Electrical machinery and apparatus	Machinery
32	Radio,television and communication equipment	Machinery
33	Medical, precision and optical instruments	Machinery
34	Motor vehicles, trailers, semi-trailers	Transport equipment
35	Other transport equipment	Transport equipment

Appendix C Transactions value

In section 2, we model the *number* of transactions expected between two country-sector pairs as a function of relative energy prices. Yet a natural extension to our analysis would be to move beyond the number of transactions and gather data on the value of the mergers and acquisitions. Regressing this as the dependent variable would enable testing if energy prices also affect cross-border investment volumes in terms of transaction value.

Unfortunately, the deal value is reported for less than 40% of the transactions observed in our dataset. Besides, the level of coverage is highly heterogeneous as a function of the ownership structure of the acquiring and target firms (see Table C.1). Firms acquiring privately held companies are under no obligation to reveal the value of their acquisitions. As a consequence, we observe transaction value between two private companies, or between a public acquirer and a privately held target in only 22% and 49% of cases respectively – while these two categories account for a combined 88% of our entire dataset. Our database only provides transaction value consistently for deals occurring between publicly listed companies, yet they account for less than 10% of the transactions we observe.

Previous applications of the Thomson-Reuters M&A dataset to other research questions have also recognized this issue by favoring a count model approach in most cases (Hijzen et al., 2008; Feito-Ruiz and Menéndez-Requejo, 2011; Dowling and Aribi, 2013).

Table C.1: Transaction values coverage

Firm ownership		Share of	Share of transaction values
Acquirer	Target	all transactions	observed within category
Private	Private	47%	22%
Public	Private	41%	49%
Private	Public	3%	64%
Public	Public	9%	84%

Despite these severe limitations, we apply our model to the transaction value data available in our dataset as an exploratory analysis. Results are reported in Table C.2. We find the expected sign for β_e. The magnitude of the estimate is larger on all transactions (-0.44 *vs* -0.32), but smaller on acquisition of assets (-0.18 *vs* -0.32). However, in all cases the point estimates of β_e fail to reach statistical significance. More extensive data on transaction values will be needed to confirm these preliminary findings.

Table C.2: Main results using the values of transactions

	All transactions		Acquisition of assets	
	(1) Baseline	(2) Cross-border	(3) Baseline	(4) Cross-border
log $e_{ijkl,t}$	-0.438	-0.321	-0.181	-0.128
	(0.405)	(0.382)	(0.410)	(0.396)
log $GDP_{ik,t}$	0.261*	0.329**	0.585***	0.641***
	(0.136)	(0.167)	(0.121)	(0.109)
log $GDP_{jl,t}$	0.521***	0.664***	0.694***	0.755***
	(0.153)	(0.152)	(0.120)	(0.114)
FTA	Yes	Yes	Yes	Yes
Country-pair FE	Yes	Yes	Yes	Yes
Country-time FE	Yes	Yes	Yes	Yes
Sectoral FE	Yes	Yes	Yes	Yes
Pseudo-LL	-18,288,076	-8,665,322	-5,731,126	-3,107,450
Deviance	47,268	38,852	37,090	27,787
Observations	5,246,776	5,048,648	4,755,583	4,557,754
Transactions	37,287	7,349	37,287	7,349

All results estimated with a Poisson Pseudo-Maximum Likelihood estimator. Standard errors in parentheses. All standard errors clustered by acquiring-target country-sector pairs.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix D PPML estimation through IRLS

D.1 IRLS estimation

In the following, we outline the structure of the PPML estimator utilized to perform the estimations presented in this article. The estimator is an implementation of the classic Silva and Tenreyro (2006) PPML estimator using an Iterated Reweighted Least Squares strategy, building on the work of Guimaraes (2016) and Zylkin (2018).

The PPML estimator is designed to estimate models of the following form:

$$\mu_i = \exp(\beta x_i)\varepsilon_i \tag{D.1}$$

where μ_i is the dependent variable, x_i the covariates of interest, β the parameters we seek to estimate and ε an error term. This functional form can also be interpreted as a Generalized Linear Model with link log.

This class of model is usually estimated through maximum likelihood, which entails a high dimensional non-linear optimization problem. In particular, in our present application, the x_i will include the full set of fixed effects, bringing the number of covariates above 3,500. Combined with the large size of our dataset – which includes more than 6 million observations – this led in practice to unmanageable estimation run times.

However the GLM class of models can also be estimated through a procedure called Iterated Reweighted Least Squares (Rodríguez, 2018). This latter approach substitutes the non-linear maximization problem solved by the maximum likelihood estimator with a converging sequence of simple OLS linear regressions.

In the following, we adopt the notations of Rodríguez (2018) to implement IRLS in practice. Given a trial estimate of the parameters $\hat{\beta}$, we calculate the estimated linear predictor $\hat{\eta}_i = \hat{\beta}x_i$ and use that to obtain the fitted values $\hat{\mu}_i = \exp\left(\hat{\beta}x_i\right)$. Using these quantities, we calculate the working dependent variable:

$$z_i = \hat{\eta}_i + \frac{y_i - \hat{\mu}_i}{\hat{\mu}_i} \tag{D.2}$$

Next we calculate the iterative weights, which in the case of a Poisson distribution simplify to:

$$w_i = \hat{\mu}_i \tag{D.3}$$

The estimate of parameter β is then improved by regressing the working dependent variable z_i on regressors x_i using weights w_i[30]:

$$\hat{\beta} = \left(\boldsymbol{X}'\boldsymbol{W}\boldsymbol{X}\right)^{-1}\boldsymbol{X}'\boldsymbol{W}\boldsymbol{z} \tag{D.4}$$

The substitution of non-linear maximization for a sequence of OLS regressions is of great practical importance to our purposes, as it allows to partial out our fixed effects from both the working dependent variable z_i and our regressors x_i. The Frisch–Waugh–Lovell theorem then ensures that the variance-covariance (VCV) matrix of the β we obtain will be proportional to the VCV matrix obtained if the fixed effects are explicitly included (Woodward et al., 2006).

To partial out our fixed effects structure, we use Correia (2016)'s `reghdfe` Stata program which partials out high-dimensional fixed effects from linear models using recent advances in graph theory.

This approach makes applying PPML to our specific high-dimensional fixed effects structure and large dataset feasible.

[30]See Rodríguez (2018) for a full derivation

D.2 Code listing

```
*! version 1.0
// Estimates Poisson regression with n fixed effects
// Author: Aurélien Saussay
// Based on poi2hdfe by Paulo Guimarraes
// and ppml_panel_sg by Thomas Zylkin
// Using reghdfe by Sergio Correia
// Date: Nov 4, 2018

program poinhdfe, eclass
version 12

	if replay() {
		if ("‘e(cmd)’" != "poinhdfe") error 301
		Display
	}
	else {
		Estimate ‘0’
	}

end


//------------
// Estimation
//------------
program define Estimate, eclass

	syntax varlist [if] [in], Absvars(varlist) [CLUSTer(varlist) tol(real 1.0e-8)
	    SAVEFE ACCEL(string) VERBOSE]

	// Collect regressors
	tokenize ‘varlist’
	local y ‘1’
	macro shift
	local xvars ‘*’

	timer clear
	timer on 1

	// Clustering
	local cluster_count : word count ‘cluster’

	// Verbosity of the output
	if "‘verbose’" == "verbose" {
		local is_quiet = ""
	}
	else {
		local is_quiet = "quiet: "
	}

	// Sample to be used
	// loc varlist ‘depvar’ ‘indepvars’ ‘cluster’
	tempvar touse
```

```
marksample touse, strok // based on varlist + cluster + if + in + weight

// Id of the observations that will be put back in
tempvar mata_id
gen `mata_id' = _n
putmata `mata_id' if `touse', replace

// Temporary variables
tempvar off dev olddev mu eta W z temp res yhat ll
tempname b vcov fit

// Initialisation
gen double `off' = 0
gen double `dev' = 0
gen double `olddev' = 0
qui sum `y'
local meany = r(mean)
gen double `mu' = (`y' + `meany')/2
gen double `eta' = ln(`mu')
gen double `W' = 1
gen double `z' = 0
gen double `temp' = 0

local dif = 1
local old_dif = 0
local counter = 0

// Main estimation loop
local irls_vars "`z' `xvars'"
// Create HDFE object
mata: HDFE = fixed_effects("`absvars'", "`touse'", "pweight", "`W'", 0, 0)

// Use acceleration?
if ("`accel'" == "lsmr") {
	mata: HDFE.estimate_cond()
	mata: HDFE.acceleration = "lsmr"
}
else if ("`accel'" != "") {
	mata: HDFE.acceleration = "`accel'"
}

`is_quiet' while (abs(`dif') > `tol') & (abs(`dif' - `old_dif') > `tol') {
	quiet replace `W' = `mu'
	quiet replace `z' = `eta' + (`y' - `mu') / `mu'

	capture drop `res'
	capture drop _RES_*

	// Remove fixed effects from the regressors
	// Update weights
	putmata `W' if `touse', replace
	mata: HDFE.update_sorted_weights(`W')
	// Partial out
	mata: hdfe_variables = HDFE.partial_out("`irls_vars'", 1) // 1=Save TSS of
```

```
            first var if HDFE.tss is missing
        // New generated vars - with the fixed effects partialled out
        loc partial_varlist ""
        foreach v in `irls_vars' {
          loc partial_varlist "`partial_varlist' _RES__`v'"
        }
        getmata (`partial_varlist') = hdfe_variables, id(`mata_id') replace

        gen(_RES_) keepsingletons

        // Regress the dependent variable on the regressors demeaned of the fixed
            effects
        _regress _RES_* `if' [pw = `W'], nocons
        _predict double `res' `if', res

        replace `eta' = `z' - `res'
        replace `mu' = exp(`eta')
        replace `olddev' = `dev'
        replace `dev' = 2 * `mu'
        replace `dev' = 2 * (ln(`y' / `mu') - (`y' - `mu')) if `y' > 0
        replace `temp' = sum(reldif(`dev', `olddev'))

        local old_dif = `dif'
        local dif = `temp'[_N] / _N
        local counter = `counter' + 1
        noisily di "`counter' iteration (difference in deviance is `dif')"
}

// Recover estimated b and vcov matrices
local N = e(N)
matrix `b' = e(b)
matrix `vcov' = e(V)

// Deviance - measurement of fit
replace `dev' = sum(`dev')
local deviance = `dev'[_N]

di
di "Convergence achieved after `counter' iterations"
di
di "Computing standard errors"
di

// After convergence, first compute standard errors

// Clustered standard errors
quiet {
        // Single cluster
        if (`cluster_count' == 1) {
                _regress _RES_* `if' [pw = `W'], nocons cluster(`cluster')
                matrix `vcov' = e(V)
        }

        // Two clusters
```

```
else if (`cluster_count' == 2) {
	tempname c1 c2 c12 v_1 v_2 v_12
	tokenize `cluster'

	egen `c1' = group(`1') `if'
	egen `c2' = group(`2') `if'
	egen `c12' = group(`1' `2') `if'

	foreach cvar in 1 2 12 {
		_regress _RES_* `if' [pw = `W'], nocons cluster(`c`cvar'')
		matrix `v_`cvar'' = e(V)
	}

	matrix `vcov' = `v_1' + `v_2' - `v_12'
}

// Three clusters
else if (`cluster_count' == 3) {
	tempname c1 c2 c3 c12 c13 c23 c123 v_1 v_2 v_3 v_12 v_13 v_23 v_123
	tokenize `cluster'

	egen `c1' = group(`1') `if'
	egen `c2' = group(`2') `if'
	egen `c3' = group(`3') `if'
	egen `c12' = group(`1' `2') `if'
	egen `c13' = group(`1' `3') `if'
	egen `c23' = group(`2' `3') `if'
	egen `c123' = group(`1' `2' `3') `if'

	foreach cvar in 1 2 3 12 13 23 123 {
		_regress _RES_* `if' [pw = `W'], nocons cluster(`c`cvar'')
		matrix `v_`cvar'' = e(V)
	}

	matrix `vcov' = `v_1' + `v_2' + `v_3' - `v_12' - `v_13' - `v_23' + `
	    v_123'
}

// Four clusters
else if (`cluster_count' == 4) {
	tempname c1 c2 c3 c4 c12 c13 c14 c23 c24 c34 c123 c124 c134 c234
	    c1234
	tempname v_1 v_2 v_3 v_4 v_12 v_13 v_14 v_23 v_24 v_34 v_123 v_124
	    v_134 v_234 v_1234
	tokenize `cluster'
	di "Tokenizing results"
	di "`1' `2' `3' `4'"

	egen `c1' = group(`1') `if'
	egen `c2' = group(`2') `if'
	egen `c3' = group(`3') `if'
	egen `c4' = group(`4') `if'
	egen `c12' = group(`1' `2') `if'
	egen `c13' = group(`1' `3') `if'
```

```
                egen `c14' = group(`1' `4') `if'
                egen `c23' = group(`2' `3') `if'
                egen `c24' = group(`2' `4') `if'
                egen `c34' = group(`3' `4') `if'
                egen `c123' = group(`1' `2' `3') `if'
                egen `c124' = group(`1' `2' `4') `if'
                egen `c134' = group(`1' `3' `4') `if'
                egen `c234' = group(`2' `3' `4') `if'
                egen `c1234' = group(`1' `2' `3' `4') `if'

                foreach cvar in 1 2 3 4 12 13 14 23 24 34 123 124 134 234 1234 {
                        _regress _RES_* `if' [pw = `W'], nocons cluster(`c`cvar'')
                        matrix `v_`cvar'' = e(V)
                }

                matrix `vcov' = `v_1' + `v_2' + `v_3' + `v_4' - `v_12' - `v_13' - `
                    v_14' - `v_23' - `v_24' - `v_34' + `v_123' + `v_124' + `v_134' +
                     `v_234' - `v_1234'
                matrix list `vcov'
        }
}

// Ensure the variance-covariance matrix is semidefinite positive
// Replace negative eigenvalues by zeroes
// (Cameron, Gelbach and Miller, JBES 2011; Zylkin 2017)
tempname U lambda lambdaMat
matrix symeigen `U' `lambda' = `vcov'

local needAdjustment = 0

foreach j of numlist 1/`=colsof(`lambda')' {
        if `lambda'[1, `j'] < 0 {
                matrix `lambda'[1, `j'] = 0
                local needAdjustment = 1
        }
}

local needAdjustment = 0
if (`needAdjustment') {
        matrix `lambdaMat' = diag(`lambda')
        matrix `vcov' = `U' * `lambdaMat' * (`U')'
}

// Names for b and vcov matrices
matrix rownames `b' = `y'
matrix colnames `b' = `xvars'
matrix rownames `vcov' = `xvars'
matrix colnames `vcov' = `xvars'

// Compute R^2
quiet {
        // Fitted y
        gen `yhat' = exp(`eta')
        cor `yhat' `y' `if'
```

```
		local r2 = r(rho)^2
	}

	// Compute log pseudo-likelihood
	// Adapted from PPML
	quiet {
		gen double `ll' = e(N) * (-exp(`eta') + `y' * `eta' - lngamma(`y' + 1)) `if
		    '
		sum `ll' `if', meanonly
		local ll = r(mean)
	}

	// Return everything in e()
	ereturn post `b' `vcov', depname(`y') obs(`N')
	ereturn scalar dev = `deviance'
	ereturn local cmdline "poinhdfe `0'"
	ereturn local cmd "poinhdfe"
	ereturn local crittype "log pseudolikelihood"

	ereturn scalar r2 = `r2'
	ereturn scalar ll = `ll'

	// Save fixed effects in __hdfe`i'__
	if ("`savefe'" != "") {
		reghdfe `z' `xvars' `if' [pw = `W'], absorb(`absvars', savefe)
		    keepsingletons
	}

	Display

	timer off 1
	timer list 1
end


//-----------------------
// Display results table
//-----------------------
program define Display
	// Display results
	_coef_table_header, title( ******* Poisson Regression with n High-Dimensional
	    Fixed Effects ********** )
	_coef_table, level(95)
end
```