COMMENTS HIGHLY APPRECIATED

AN EMPIRICAL TEST OF THE DUTCH DISEASE HYPOTHESIS USING A GRAVITY MODEL OF TRADE

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Abstract

Although the core model of the Dutch Disease makes unambiguous predictions regarding the negative effect of a resource boom on a country's manufacturing exports, the empirical literature that has followed has not clearly identified this effect. I attribute this to the failure of the existing literature to combine enough data to produce a sufficiently powerful and exogenous test. I will use the World Trade Database to systematically test this hypothesis in a gravity model of trade. World energy prices are used to bypass issues of endogeneity regarding primary exports. A one percent increase in world energy price is estimated to decrease a net energy exporter's real manufacturing exports by almost half a percent. Similarly, after instrumentation, a one percent increase in an energy exporting country's net energy exports is estimated decrease the country's real manufacturing exports by 8 percent. The corresponding confidence intervals are tight and these results are shown to be quite robust.

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> "[…] in the words of Lord Kahn [1905-1989], 'when the flow of North Sea oil and gas begins to diminish, about the turn of the [21st] century, our island will become desolate.' Any disease which threatens that kind of apocalypse deserves close attention."
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> "The Dutch Disease," *The Economist*, November 26, 1977: pp-82-83.

1 Introduction

It is widely assumed in the literature that natural resource booms tend to harm the countries in which they occur. Most famously, Sachs and Warner (1995) show that economies with a high ratio of natural resource exports to GDP in 1971 tended to have low growth rates during the subsequent period 1971-89. This negative relationship holds true after controlling for other usual determinants of economic growth, such as initial per capita income, trade policy, government efficiency, and investment rates.

Sachs and Warner (1995) conclude that "one of the surprising features of modern economic growth is that economies abundant in natural resources have tended to grow slower than economies without substantial natural resources." Such a statement deserves careful scrutiny if only because of its implications for both development policy in the third world, and for macroeconomic policy in industrialized countries. At the same time, there is a growing debate among academics, development and environment related lobbyists and policy makers regarding whether or not resource abundant countries should be encouraged to exploit their resource bases.

A growing literature is dealing with an increasing number of aspects of the "resource curse". There are two main areas of active research[1]. The first can be termed the "political economy of mineral rent generation and distribution." The second covers the "general equilibrium effects of a minerals boom", including the spending effects of the mineral rents. This paper focuses on this second literature and what is probably the best-known and the most classical formulation of the resource curse hypothesis, namely the Dutch Disease — hereafter DD — hypothesis .

In a nutshell, the DD primarily refers to a situation in which a booming export sector increases the relative price of non-tradable goods and services, thus hurting the rest of the tradable goods sector. Its name arose from the effects attributed to the discoveries of North Sea gas on the Dutch manufacturing sector. Corden and Neary (1982) present what has come to be known as the "core model" of DD

[1] The terminology hereafter is adopted from Daniel (1986).

economics. In this paper, I test systematically one of its main unambiguous testable hypotheses, one that has generated the most concern among economists, the hypothesis that resource booms lead to reductions in manufacturing exports, controlling for all other determinants of trade.

There is a large literature on the empirics of the Dutch Disease. This literature is mostly a collection of country — sometimes comparative — case-studies for the OECD and developing countries. Spatafora and Warner (1999, 2001) is the only exception. Their dataset is composed of 18 oil-exporting developing countries covering a period running from the mid sixties to the eighties. They find that Dutch Disease effects are "strikingly absent" from their data. However, there exists, to the best of my best knowledge, no generalized cross-country econometric test of the consequences of resource booms on real manufacturing trade.

The general conclusion of this literature is that there are symptoms of the Dutch Disease in most instances of a commodity boom but that it is very difficult to disentangle DD effects from the domestic and international macroeconomic conditions prevailing at the time of the shock. Price led energy booms tend to be accompanied by (world-wide) recessions. Hamilton (1983) is known to have most convincingly isolated this pattern for the United States and provides evidence that we can still expect this pattern to be at work (Hamilton, 1996). Rotemberg and Woodford (1996) propose an explanation for the surprisingly strong elasticity of output to oil prices — a 10% oil price innovation is associated, five or six quarters later, with a 2.5% decline in output — based on imperfect competition in product markets.

I use a gravity model of manufacturing trade to control for the macro-economic circumstances faced at home and by trade partners, as well as other important geographic determinants of trade. The choice for this particular model is due to its excellent empirical track record and its theoretical consistency with a variety of different views of trade. This last characteristic will exempt me from having to adhere to any particular such view of trade. In other words, I use the gravity model of trade as a reduced form of a potentially more complex structural model that I leave unspecified.

Acknowledging the potential endogeneity of commodity exports to manufacturing trade, the world price of energy — in real terms — is used to identify price led oil booms. Indeed, the world energy price can be safely assumed to be exogenous to any single country's manufacturing exports. Yet these world prices are highly correlated with domestic energy prices, and thus changes in value added in the energy sector. They are therefore an ideal instrument variable for energy booms.

Corden (1984) points out that a resource boom can take place in three different ways: first, there can be exogenous technological progress in the booming resource sector; second, the country can see a windfall discovery of some natural resources; third, there can be an exogenous rise in the world price of a natural resource exported by a country. In this paper, I focus on this third kind of natural resource boom. Similarly, there are potentially other forms of booms associated with types of commodities other than energy. This, of course, implies that there are potential cases of Dutch Disease that this paper will overlook. Since the purpose is to show the existence of at least some form of Dutch Disease, I simply leave the identification of other forms of Dutch Disease as interesting paths for further research. Future research will have to determine whether other types of booms and booms associated with other kinds of commodities have similar effects to those identified here, if any.

With this identifying assumption in hand, two types of elasticities are estimated. First, the real energy price elasticity of manufacturing exports in net energy exporting countries is estimated to be, on average, close to *minus* ½. In other words, a 1% increase in the price of energy will, everything else held constant, decrease a net energy exporter's real manufacturing exports by half a percent. Second, the net energy export elasticity of manufacturing exports, when instrumented by world oil prices, is *minus* 0.08. In other words, a 1% increase in net energy exports will, everything else held constant, decrease a net energy exporter's real manufacturing exports by one eighth of a percent. These elasticity estimates are very significant, and the corresponding confidence intervals are tight.

I conduct extensive sensitivity analysis and these "benchmark" results turn out to be quite robust. They are robust to the introduction of country specific fixed effects on top of country pair fixed effects. This eliminates the concern for the corresponding type of omitted variable bias. Censored tobit regressions reveal that results are not dependent on the presence or treatment of small export observations. Results are also robust to changes in the currency unit of measurement. Outlier analysis reveals that results are not significantly dependent on any country or country pair. Further, the main conclusions of this paper are not dependent on the inclusion of any geographical group of countries, or set of countries characterized by a specific type of *effective* exchange rate regime.

In contrast with the existing literature, I find strong evidence of the DD in the world trade data. Price-led energy booms, systematically tend to hurt energy exporters' manufacturing exports. This paper dispels doubts about the empirical relevance of the DD. It is intuitive, after all, that when juxtaposing the marginally convincing evidence found in numerous countries, one be able to either reject or accept the DD hypothesis[2].

One should be careful not to over-interpret the results reported in this paper, however. In most cases, booms result in increased GDP levels, and hence increased welfare, for energy producers. This is not the question in this paper. Further, one only needs to worry about the DD insofar as there is indeed something desirable about having a large proportion of manufacturing exports. There is, indeed, evidence that productivity growth can sometimes be very strong in resource extraction industries, both in industrialized and developing countries. In addition, the role that manufacturing plays vis-à-vis the primary sector can vary from country to country, for example, because of factor intensity reversal.

On the other hand, there is a widespread assumption in the structural tradition of the development literature that industrialization should be *per se* a development goal. More recently, Matsuyama (1992), Sachs and Warner (1995, 1999), and their followers, have explicitly modeled economic growth as a function of the relative size of the manufacturing sector. It is not the purpose of this paper to settle this issue. Rather, the statistic and economic significance of the result found here leads me to argue in favor of careful future empirical testing of the effect on productivity of sectoral changes in output and exports resulting from energy and other resource booms.

This paper is organized as follows. Section 2 reviews the theory and empirics of the Dutch Disease. Section 3 describes the methodology and data. Section 4 presents the main results. Section 5 proceeds with sensibility analysis followed by Section 6 that reports on outlier analysis. I conclude in Section 7.

[2] If authors in the DD empirical literature systematically reported comparable estimates of the effect of resource shocks on manufacturing exports, it would be interesting to see if the same conclusion is reached through meta-analysis. Such is far from the case, unfortunately.

2 Literature Review

2.1 The Dutch Disease theory

The DD refers to a situation in which a boom in an export sector leads to a shift of production factors towards the booming sector and an increase in the prices of non-tradable goods and services, thus hurting the rest of the tradable goods sector. Its name arose from the effects presumably caused by the discoveries of North Sea gas on the Dutch manufacturing sector. Corden (1984) notes that the term appears to have been coined in *The Economist* of November 26, 1977.

Dorrance and Leeson (1997) trace the idea itself back to Meade. Meade spent six months in Australia in 1956. While there, he observed the effect of growth in Australia's resource exports, and identified what came to be called the DD (Corden, 1996). The first paper approaching this question is actually by Meade and Russell (1957). Corden (1984) and Corden and Neary (1982) are the cornerstones of a vast DD literature that developed around how a natural resource boom can trigger a process of "de-industrialization"[3].

Corden and Neary (1982) present what they call and what has come to be known as the "core model" of Dutch Disease economics[4]. They assume a small open economy that produces three goods: two are traded at exogenously given international prices; the third is a non-traded good whose price is determined by domestic supply and demand. The traded goods sector includes a booming good, and a non-booming one. The non-traded good is typically thought to be produced by the service sector (but it can be extended to the construction sector, etc). The main model assumes capital to be sector specific whereas labor is assumed to be mobile. A resource boom affects the rest of the economy in two main ways: the "resource movement effect" and the "spending effect".

On the supply-side, an exogenous increase in the value of the booming sector's output raises the marginal product of labor in that sector. A shift of labor to the booming sector from all other sectors will ensue and a contraction of the tradable sector will result from its reduced use of production factors. This is the *resource movement effect*. This factor movement also leads to an increase in the price of non-traded

[3] See Corden (1984) for a review of the early DD literature written in the 1970's.
[4] The presentation made in this section of the "core model" of the DD is inspired from two very clear presentations by Adolfo Meisel Roca (1999) and Karlygash Kuralbayeva, Ali M. Kutan and Michael L. Wyzan (2001).

goods since, *ex ante*, it results in excess demand for non-tradables. Since the price of tradables is exogenously determined in world markets, the rise in the prices of non-tradables is equivalent to an appreciation of the real exchange rate.

On the demand side, the boom, leads to increased income at home and therefore, to increased demand for *all* goods. Since the price of tradables is set on world markets, this extra spending raises the relative price of non-tradables, resulting in a further appreciation of the real exchange rate. In response, labor shifts from the tradables sector to the non-tradables sector resulting in a contraction of the non-booming tradables sector. This is the *spending effect*.

With one specific non-mobile factor (capital) and one mobile factor (labor) in all sectors, both the resource movement effect and the spending effect imply a shift of labor away from the manufacturing sector, resulting in an unambiguous decline in manufacturing output[5]. The booming sector's output increases since the value of output initially increases, and it absorbs *ex post* production factors coming from other sectors. There is ambiguity regarding the change in non-tradable output. The spending effect implies an expansion of this sector, yet the resource movement effect implies a contraction.

The strength of the spending effect depends on the propensity to consume services. Typically, when a resource boom occurs, increased government spending on construction and public services is likely to be the main channel for use of mineral rents. Mineral states have been documented to lavishly spend their mineral revenues on numerous development projects and programs (see for example William Ascher, 1999). Hence, this marginal propensity to consume non-tradables will be high. The strength of the resource movement effect depends on the factor intensity of each sector. If the booming resource sector is the capital intensive sector — as is often the case in LDCs as well as in more industrialized countries, albeit to a lesser extent — the spending effect will dominate the resource movement effect.

Corden and Neary (1982) call "direct deindustrialization" the movement of labor from the manufacturing sector to the booming sector. The flow of labor out of the non-tradable sector, together with the increased demand for goods from that sector due to the spending effect, causes a further shift of labor

[5] Corden and Neary (1982) show the implications of two other sets of assumptions about factor mobility between sectors. One can assume that capital is mobile between the manufacturing and services sectors, or that capital is mobile among all three sectors. In these cases, resource allocation cannot be determined without a prior knowledge of parameter values. In the rest of this paper, given the lack of unambiguous predictions from the models with mobile capital, the DD model will refer to the basic model with one specific non-mobile factor (capital) and one mobile factor (labor).

from the manufacturing sector to the non-tradable sector. They refer to this second shift as "indirect de-industrialization". For reasons related to those mentioned above, indirect industrialization is expected to be more important than direct deindustrialization.

When the net effect of the spending and the resource movement effects are considered together, we obtain the following 4 main results:

(R1) The real exchange rate unambiguously appreciates[6];

(R2) There is a likely though theoretically ambiguous increase in non-traded output;

(R3) Production in the manufacturing sector unambiguously falls;

(R4) There is a fall in manufacturing exports.

There are thus three unambiguous testable hypotheses. I do not test for R1. Instead I refer the reader to a recent contribution by Chen and Rogoff (2002). They show R1 holds for the few mineral-rich countries they select for their data availability (even though that's not enough to solve the PPP puzzle).

In theory, both R2 and R3 are testable hypotheses but sectoral production data is available in reliably comparable form for only a few countries (mostly the OECD). Regarding R2, there is little doubt throughout the DD literature that service output rises in response to resource booms (see Spatafora and Warner, 1999 and 2001 for the most systematic results). In any case, testing an ambiguous implication of a model is unavoidably less attractive because it does not allow inferences regarding the general validity of the theoretical model itself. In other words, finding supportive as well as dismissive evidence regarding R2 would both be compatible with the DD model.

In this paper, I will only test explicitly for R4, principally because of the richness of trade data compared to the scarcity and unreliability of manufacturing production data comparable across countries. Under the hypotheses of the DD model, R3 implies R4, but it possible to imagine R4 without R3. In other words, domestic production of manufactured goods *could* increase as a result of a resource boom while manufacturing exports would decrease: the hypothesis tested in this paper, R4, is necessary but not sufficient for R3.

For this to happen, domestic demand for manufactured goods would have to grow more than exports would have to shrink *as a result of a resource boom*. Exports will turn out to be too strongly

[6] This can take the form of a nominal appreciation or a change in the domestic : foreign aggregate price ratio.

affected by resource booms for this to be plausible. Had manufacturing exports only been marginally affected by resource booms, the dichotomy between manufacturing production and exports would have been more important to highlight.

In any case, many authors argue that international trade leads to firm-level learning about foreign technology and markets; and so, independent of production, manufacturing exports slumps are often perceived as a concern of their own. MacGarvie (2002) provides evidence of such learning effects using patent data citations. Frankel and Romer (1999) instrument trade across countries with geographic variables, and observe that trade has a positive effect on income. They conclude that their effect is economically significant and robust, albeit marginally significant from an econometric point of view.

Finally, it should be noted that the DD effects discussed here are, of course, working on top of the rest of the shocks affecting the economy. Specifically, declines in manufacturing exports in response to a resource boom have to be thought of against the background of their counterfactual. The importance of a "ceteris paribus" analysis will be illustrated when graphical evidence is examined, and will be further justified by an examination of the empirical DD literature.

2.2 Existing Empirical literature

What about the empirical relevance of the DD hypothesis? There are two branches to this literature. One covers resource-booms in OECD countries. Corden (1984) surveys the early empirical literature on industrialized countries. A general book referring to the "oil or industry" issue with respect to Canada, Mexico, the Netherlands, Norway, and Britain was edited by Barker and Brailovsky (1981). The other branch of the empirical DD literature studies resources booms in developing countries. This second branch succeeded the other although there is some overlap. McMahon (1997) reviews the results of eight different studies focusing on developing countries.

In general, the development side of this literature tends to put more emphasis on rent-seeking behavior and poor governance whereas the original literature focuses more on prices and structural issues. This can be explained in part by the differing concerns each group of authors have. In OECD countries, the concern is about 'de-industrialization' and the "ballooning role of the state"; many of these papers were also written in an era when economists were responding to the worry that the United States was turning into “a nation of hamburger flippers”, a process sometimes referred to as “servicesation”. Regarding developing countries, in the *relative* absence of industry, development economists are more concerned with ‘de-agriculturization’ and detrimental effects on burgeoning social and political infrastructures of newly decolonized nations in particular. I review each literature briefly in turn.

The Netherlands

In the late 1950s a very large deposit of natural gas was discovered in the north of The Netherlands. Development began in 1963 and by 1977 a country which had traditionally been an energy importer became an energy exporter. For Ellman (1981), cheap domestic energy seemed wholly favorable to the economy, at least into the late 1960s and early 1970s. But during the 1970s, the guilder appreciated relative to most currencies. The textiles and clothing industries almost vanished. There was a decline in metal manufacturing, mechanical engineering, vehicles, ships, and even construction and building materials. The service sector expanded noticeably, and seemed to be “taking over.”

Corden (1984) argues that “the true DD in the Netherlands was not the adverse effects on manufacturing of real appreciation but rather the use of booming sector revenues for social service levels which are not sustainable, but which have been politically difficult to reduce.” Barker (1981) and Kremers (1985) conclude that it is difficult to “accuse” the gas discoveries of causing the severe decline in several Dutch manufacturing industries between 1973 and 1977. Most of Western Europe was having a similar experience, most specifically Germany, the main trading partner of The Netherlands. These countries were also witnessing substantial growth in unemployment.

Figures I and II help compare the experience of The Netherlands with that of its trading partners. Generally, periods of increasing real energy prices correspond to periods of shrinking manufacturing exports as a share of GDP. Conversely, real energy price crunches correspond to periods of increases of the share of manufacturing trade intensity. However, the pattern observed in The Netherlands looks very similar to that of France and Germany which are not net energy exporters.

Figure II plots manufacturing exports for the same four countries in real 1995 dollars, *i.e.* the real exchange rate index is applied to these series in order to capture the volume of manufacturing exports in the local currency equivalent. Here again the Dutch experience is very similar to that of its main EC trading partners. In fact, French manufacturing exports take the worst dip although France is not an energy exporter.

The United Kingdom

In the United Kingdom, Ross (1986) reminds us that commercial production of oil started in 1975 when the (first) recession had already began. The real exchange rate did appreciate by 51%-55% between 1977 and 1980. Manufacturing output dipped by 4% between 1973 and 1979 and by 14% between 1979 and 1982.

However, it is difficult to hold oil responsible for the pound's real appreciation. Simultaneously, monetary policy was markedly tightened with, consequently, high nominal interest rates prevailing between 1979 and 1981. Further, the status of the pound as a 'petrocurrency' concurrently to high oil prices turned it into a secure haven, especially given the government's tough deflationary stance.

Buiter and Miller (1981, 1983) document that, against a background of declining output, labor productivity started to grow rapidly from 1980, especially in the manufacturing sector. For Sachs and Brandon (1983) a lot of these productivity gains (as well as of reductions in inflation) simply followed from the Thatcher government's tolerance towards unemployment. Accordingly, Jeffrey Sachs thinks that the UK government was simply playing with its "sacrifice ratio", even though he acknowledges rational expectations issues.

Forysth (1985) concludes that there is evidence of DD effects, but that it is impossible to measure the precise impact of the booms on structural change. In particular, North Sea oil was imposed on a poor macroeconomic background, and Forsyth thinks that, to some extent, the United Kingdom appeared to be exacerbating the structural effects by spending its oil revenues too fast.

Looking back to Figures I and II we can compare the experience of The United Kingdom during the early 1980s with that of other main European countries. While the share of manufacturing exports in GDP fell in the United Kingdom, this experience also bears a lot of resemblance to that of its main EC trading partners. However, manufacturing exports intensity seemed to have dipped further than in the other three countries, possibly reflecting the highest relative importance of mineral rents in the UK.

In Figure II, manufacturing exports for the same four countries in real 1995 dollars also dips more markedly than in The Netherlands, Germany and France. This provides for a check on Figure I where it might be argued that the share of manufacturing exports in GDP declined because rents boosted GDP with possibly no effects on exports *per se*. Figure II suggests that such is not the case. Yet, France, not a net energy exporter, also experienced a sharp dip in the early 1980s.

Developing countries

Regarding LDCs, there have also been numerous country case studies. A few authors have case-studied countries in parallel. McMahon (1997) reviews the results of eight different studies focused on developing countries. The main conclusion of his paper is that the DD does not spell doom. Instead doom is generated by economic policies that are inappropriate to begin with, or by inadequate policy responses to a resource boom.

Many authors simply find little evidence of a DD in many of their case studies (Gelb 1988, Cuddington 1989, Davis 1983) except in the sense that resource booms allow governments to go on with counter-productive policies for longer than otherwise (Auty 1993 and 1994, Collier and Gunning 1996). Most governments were able to tax away the largest part of the resource rents[7]. The experience of these countries was, according to these authors, otherwise very analogous to that of other countries that had import substitution strategies in the 1970s.

Gelb (1988) and Spatafora and Warner (1999, 2001) analyze the performance of oil boom countries. Gelb (1988) case-studies these countries in parallel. Spatafora and Warner (1999, 2001) is the work that is closest to what is undertaken here. Their dataset in made of 18 oil countries observed between 1965 and 1989. These authors all find that favorable terms-of-trade shocks boost non-tradable output but that DD effects are remarkably lacking. This paper will reach opposite conclusions essentially because my dataset, by including resource importers as well, controls much better for the counterfactual case of absence of boom; in other words I propose a much more powerful test. I will return to this later.

In Kazakhstan (Kuralbayeva, Kutan, and Wyzan 2001), Kuwait (Looney 1991), Nigeria, Mexico, Venezuela (Roemer 1985) and Saudi Arabia (Looney 1989) exchange rate appreciation followed oil booms. Authors generally argue that this appreciation possibly caused contraction of industrial output as compared to the no-boom counter-factual. However, in Kuwait, Nigeria, Indonesia and Mexico, the growth rate of the manufacturing sector was actually greater than or equal to that of non-tradables. In Venezuela, all

[7] In another working paper entitled "Natural Resource Abundance and Human Capital Accumulation", I expend on this phenomenon in greater detail. This paper is available from the author upon request.

sectors grew slowly but with a non-tradable sector only growing at a yearly 5.5%, one cannot characterize these symptoms as typical of the DD, argues Roemer (1985).

Daniel (1986) contends that the outcomes of both booms and slumps are dependent on the government's policies, and hence upon the political process. Jazayeri (1986) concludes that in Iran and Nigeria, the manufacturing sector's growth rate is not fully consistent with the DD model, unless import substitution policies are taken into account. As Roemer (1985) points out, trade protection can turn manufacturing activities into non-tradables.

On this account, Indonesia has been the "model pupil" (of the I.M.F.). Warr (1985) observes that in spite of distinctive effects on the domestic prices, it is not clear whether the economy's structure has been affected at all. Roemer (1994) explains that during the oil boom, the Indonesian government dodged the worst impacts of Dutch disease thanks to careful exchange rate management. Indonesia devalued its exchange rate periodically during its petroleum boom. In 1986, a crawling peg was officially adopted and the rupiah kept its real value long after.

The effect of booms in other primary commodities has also been investigated. Most studies are inconclusive while Columbia seems to be the exception (Cuddington, 1989, Davis, 1983). Kamas (1986) and Roca (1999) examine the effects of large expansions in coffee (and illegal drug exports) revenues. As a result, the relative price of nontraded output soared and there was a real appreciation of the Columbian exchange rate. The nontraded sector's growth rate accelerated, while traded output slowed down. In the realm of metals, Spilimbergo (1999) makes no mention of DD effects and concludes that copper has actually helped the Chilean economy on various macroeconomic accounts.

Economic history

Finally, the DD hypothesis has also been considered by economic historians. Forsyth and Nicholas (1983) consider the inflow of American treasures into the 16th century Spain. They interpret the consequences on the Spanish industry in DD terms. Cairnes (1859) considers the gold discoveries in Australia in the 1850s.

He identified effects on some Australian industries that we would regarded as DD effects nowadays[8]. This episode has also been studied more recently by Maddock and McLean (1984).

Summary

The general impression that emerges from the empirical DD literature is that there is some evidence, although by no means strong, that some countries, specifically oil producers, have shown symptoms of the DD. Most authors struggled to disentangle the pure DD effects on the manufacturing sector from the effects of trade crunches that follow recessions usually accompanying energy price spikes. In LDCs, authors struggle with disentangling manufacturing trade patterns due to DD effects from the general failure of the development of a competitive manufacturing sector, most often policy induced.

A major problem of current contributions to the literature is that by analyzing each country as an independent case study, or at best a subset of mineral countries, one is implicitly discarding very useful information, *i*.e. the controls offered by relatively resource-scare countries. Further, there is the obvious need to control for the economic conditions prevailing in trade partner countries. Also, resource booms tend to cause recessions in resource dependent countries, thus hurting manufacturing exports in these countries through what the gravity literature would call the "own-GDP" effect on trade. This effect will make the identification of DD effects in resource abundant countries harder by assimilating their experiences with those of resource-abundant countries, hence the need to account for domestic economic conditions prevailing in all resource-dependent countries as well.

The purpose of this paper is precisely to propose a generalized test for the DD. In a nutshell, I want to test for the DD hypothesis as a joint hypothesis for all countries in response to these methodological issues. Econometrically speaking, a bilateral trade flow setup naturally suggests itself. An additional advantage of working with manufacturing export data rather than production data becomes clear at this point. Many developing countries have long pursued import-substitution industrial policies, allowing inefficient industries to prosper, or at least survive, in spite of their poor productivity performance by global standards. Manufacturing exports, on the other hand, are presumably *less* amenable to protection.

[8] There is the question of the original filiation of the DD concept. It is well possible that Meade was exposed to Cairnes' work while on his leave in Australia. This is a pure conjecture on behalf of the author of this paper, however.

An increase in manufacturing exports is more likely to be indicative of productivity improvement than a mere increase in domestic production.

My empirical question is as follows: controlling for the other empirically relevant determinants of trade, do energy exporters tend to export less manufacturing in times of booms (and vice versa)? To answer this question, I need to choose an empirical model that explains trade flows. I have chosen the gravity model of trade because of its celebrated empirical success. I would not want to spuriously identify or fail to identify a DD effect for failing to model trade in an otherwise convincing manner. The data used in this paper, the gravity model of trade, and the details behind my empirical approach are presented in the following section.

3 Methodology and Data

I estimate the effect of energy price-led resource-booms on manufacturing trade exploiting. I use trade data that covers most countries for most of the last three decades of the 20th century. During the time span covered in my sample, 1970-1997, there has been plenty of variability in the real price of energy. I exploit this price variability, in times of boom as well as slump; I allow this variability to affect manufacturing exports of net energy exporters as well as of net energy importers.

Corden (1984) points out that a resource boom can be take place in three different ways: first, there can be exogenous technological progress in the booming resource sector; second, the country can see the windfall discovery of some natural resources; third, there can be an exogenous rise in the world price of a natural resource exported by a country. This paper focuses on this third kind of price-led natural resource boom because of the relative ease with which it can be exogenously identified. There are thus cases of DD that this paper will overlook. However, the purpose is to show the existence of at least some form of DD, in contrast to the inconclusiveness of the existing empirical literature. I leave the identification of other forms of DD as interesting paths for further research.

The main strategy of this paper is to regress trade in manufactured goods at the country level on net energy exports and to instrument net energy exports with world energy prices. Of course, many other things affect trade. While these other factor are not of direct interest here, I need to model their effects so as to be able to see if there is any remaining role for resource booms and slumps. Fortunately, the gravity model of international trade is a simple yet credible setup for this purpose. The gravity model of international trade will allow me to implement my variables without having to worry about a general lack of explanatory power on behalf of the rest of the model.

The rest of this section is organized as follows. First, the methodology behind the gravity model of trade is presented (section 3.1). Next, I discuss the construction of the variables that will allow testing for the DD (section 3.2). Finally, I acquaint the reader with the rich panel dataset used in this paper (section 3.3).

3.1 Gravity Methodology[9]

The origin of the gravity model of trade is traced back to Tinbergen (1962) and Pöyhönen (1963) by Rose (2000). The gravity analogy comes from the fact that trade between two countries is modeled as a function of their GDP, that is their economic *mass*, and as a measure the distance between these countries. Leamer and Levinsohn (1995) survey recent empirical contributions to this literature. Results in this literature are systematically consistent, statistically significant and economically meaningful.

This paper builds upon the empirical credibility of the gravity model of trade. This model is very straightforward and is aimed at modeling empirically the size of international trade between countries. It is amenable to extensions and it has been used to analyze a growing range of issues: the emergence of a yen bloc (Frankel and Wei, 1993), the effect of borders on trade (McCallum, 1995), the causal link between trade and growth (Frankel and Romer, 1999), departures from the law of one price (e.g., Engel and Rogers, 1996), and the effect of currency union membership trade (Rose, 2000, Glick and Rose, 2002).

Some authors have proceeded to provide theoretical justification to the gravity model of trade. There are in fact several ways to justify this approach that range from increasing returns goods differentiation across countries, monopolistic competition, reciprocal dumping, to cross-country differences in factor endowments or technology. I refer the reader to Evenett and Keller (2002) and Feenstra, Markusen and Rose (2001) for recent contributions and references to this question and to Anderson (1979) and Bergstrand (1985, 1989) for older contributions and references.

[9] This section's presentation of the gravity model of trade is strongly inspired by Rose (2000) and Glick and Rose (2002).

The objective of this paper is not to horse-race these different theoretical foundations against each other. The fact that results are not attached to a specific set of assumptions regarding international trade increases the results' generality, as Rose (2000) argues. Of course, this also implies that this paper sheds no light on the relative merits of these different sets of assumptions.

In this paper, I focus my attention on manufacturing exports. This is not unusual at all in the literature. Bergstrand (1989), for example, estimates a similar model down to single digit SITC industry groups. Feenstra, Markusen and Rose (2001) showed that the gravity model can also be derived from models of trade in differentiated products (see also Helpman, 1984 and Bergstrand, 1985). When they calibrate their model, they find that differentiated goods give a more solid theoretical validation of the gravity model than do commodities. Since I model manufacturing exports, my results are on the safe side. Finally, I augment the model with a number of extra controls suggested by Rose (2000) and Glick and Rose (2002):

$$\begin{aligned}\ln\left(X_{ijt}\right) &= b_0 + b_1\ln\left(Y_{it}\right) + b_2\ln\left(Y_{jt}\right) + b_3\ln\left(D_{ij}\right) + b_4 Lang_{ij} + b_5 Cont_{ij} + b_6 FTA_{ij} \\ &+ b_7 Landl_{ij} + b_8 Islandf_{ij} + b_9 ComCol_{ij} + b_{10} CurCol_{ij} + b_{11} ComNat_{ij} + \tilde{\mathbf{ß}}\mathbf{DD}_{ijt} + e_{ijt}\end{aligned} \quad (1)$$

where i and j indicate countries, t indicates time, and the variables are defined as follows:

- X_{ijt} is the real value of manufacturing exports from i (referred later on as the "origin" country) to j (the "destination" country) at time t,
- Y_{it} is real GDP of country i in period t,
- D is the distance between i and j,
- $Lang$ is a dummy which is equal to 1 if i and j have a common language,
- $Cont$ is is a dummy which is equal to 1 if i and j share a land border,
- FTA is a dummy which is equal to 1 if i and j belong to the same regional trade agreement,
- $Landl$ is the number of landlocked countries in the country-pair (0, 1, or 2).
- $Island$ is the number of island nations in the pair (0, 1, or 2),
- $ComCol$ is a dummy which is equal to 1 if i and j were ever colonies after 1945 with the same colonizer,
- $CurCol$ is a dummy which is equal to 1 if i and j are colonies at time t,
- $ComNat$ is a dummy which is equal to 1 if i and j remained part of the same nation during the sample (e.g., France and Guadeloupe, or the UK and Bermuda),
- $\tilde{\mathbf{ß}}\mathbf{DD}_{ijt}$ is a vector of resource boom indicators and its corresponding vector of coefficients, to be defined below.
- e_{ijt} stands for the other influences on manufacturing exports, assumed to be well behaved.

3.2 Dutch Disease terms

The coefficient vector of interest is $\tilde{\mathbf{ß}}$, the vector of estimated effects of a price-led resource boom on manufacturing trade. Three different approaches will be taken to define $\mathbf{DD}_{ijt}$:

1. The first approach consists in using the world price of energy. I discuss below why I can safely assume the world price of energy to be reasonably exogenous to any single country's manufacturing exports. The exogeneity of the dependent variable is the main advantage of this approach. The drawback is that the interpretation of the resulting coefficient is more hazardous. This approach will yield an *average* energy price elasticity of manufacturing exports. Obviously, this elasticity will be an average because we cannot expect a relatively large energy exporter to be affected to the same degree as a relatively modest energy exporter by the same change in world price of energy.
2. Second, I will use total net energy exports from each country on both sides of the manufacturing export flows. The principal advantage of this first approach is the ease of interpretation of results. It will yield the net energy export elasticity of manufacturing exports. The drawback is that I cannot rule out reverse causality running from manufacturing exports (and hence, changes in manufacturing productivity) towards net energy exports.
3. The third and last approach aims at combining the advantages of the two proceeding approaches. It consists in using net energy exports instrumented by the world price of energy. This should yield an elasticity whose interpretation is both meaningful and devoid of concerns for reverse causality.

Of course, net energy exporters and importers cannot be expected to be affected the same way by, say, a rise in the real price of energy. In fact, they are expected to be affected in opposite ways. The effect of an energy crisis on a net energy exporter is sometimes referred to as *reverse* DD effects. Because of the potential mismatch between theoretical models of the DD and the effects to be expected on net energy importers, this paper will not estimate reverse DD effects and will hence strictly focus on net energy exports and exporters.

World price of energy

The real energy price index is indexed in 1995 dollars. I take this US dollar world price index as exogenous to any single country. A few exceptional countries such as Saudi Arabia for oil and Chile for copper may cast some doubt on this assumption. It turns out no single observation nor pair of observations exercises a significant effect on the coefficients of interest according to DFBeta statitistics.

It could be argued that the real price of oil for example is not exogenous to a *group* of countries like OPEC. The basic problem with the cartel story is exposed most forcefully by Cremer and Salehi-Isfahani (1989). Basically, OPEC did not to have the characteristics of an effective cartel. It was too culturally and politically fractious. In fact, OPEC did not begin to set output quotas until 1982, at a time when oil prices had already increased substantially. In fact, in Section 6.2, I show that the exclusion of the Middle-East and Northern African countries does not exercise a significant effect on the coefficient of interest according to DFBeta statitistics. In fact, including these countries in the sample will tend to *weaken* the Dutch Disease coefficients of interest here.

There remains the question of how to explain the 12-years of rising oil prices. In the late 70s, according to Krugman (2000), economists began to put forward that a large share of this rise was due to perverse supply responses. Cremer and Salehi-Isfahani (1989) emphasize that oil differs from ordinary commodities in three important ways: oil is exhaustible, its production is controlled by governments, and oil is a major item on the revenue side of oil exporters' government budgets.

To circumvent this potential source of endogeneity further, I use a world *energy* price index rather than specific commodity prices. In any case, it should be noted that our concern should be endogeneity to manufacturing exports, *i.e.* our dependent variable. It is unrealistically far fetched to assume material endogeneity of real world energy prices to the manufacturing export performance of any specific country or cartel of countries. Historical hindsight reveals that manufacturing export concerns have simply not been driving cartel price coordination.

Energy price-led resource boom variables are constructed in this following way:

EPE_{it}, "energy price exposure",

$$= 1(i \text{ is net energy exporter}) \text{ x } \log(\textit{real price of energy}) \quad (2)$$

In this first approach, $$\mathbf{DD}_{ijt} \equiv EPE_{it} - EPE_{jt} \quad (2')$$

$$\Rightarrow \tilde{\text{ß}}\mathbf{DD}_{ijt} = b_{12}EPE_{it} + b_{13}EPE_{jt} \tag{2''}$$

So that (1) becomes:

$$\begin{aligned} \ln\left(X_{ijt}\right) &= b_0 + b_1\ln\left(Y_{it}\right) + b_2\ln\left(Y_{jt}\right) + b_3\ln\left(D_{ij}\right) + b_4 Lang_{ij} + b_5Cont_{ij} + b_6FTA_{ij} \\ &+ b_7Landl_{ij} + b_8Islandf_{ij} + b_9ComCol_{ij} + b_{10}CurCol_{ij} + b_{11}ComNat_{ij} \\ &+ b_{12}EPE_{it} + b_{13}EPE_{jt} + e_{ijt} \end{aligned} \tag{1'}$$

The Appendix Table documents the resulting classification for net energy exporters. A potential complication is that a country's "specialization" as a net energy exporter can be endogenous to manufacturing exports. It reasonably safe to assume that the overall status of a country as a net exporter or importer of energy or metals is, *ceteris paribus*, exogenous to its manufacturing exports, and reflects to a large degree its geological endowment. But it is perfectly imaginable that a year-on-year drop in manufacturing exports could drive a country to become a net exporter of energy. While this potential limitation is to be acknowledged, concerns are limited by the fact that instrumentation of net energy exports (as in the third approach below) *strengthens* rather than weakens the conclusions of this paper. In other words, endogeneity seems to be working against the conclusions of this paper.

<u>Net energy exports</u>

The second approach to the measurement of DD effects consists in using as dependent variables total net energy exportsat both ends of the manufacturing exports flow:

$TNEX_{it}$, "(total) net energy exports",

$$\equiv \sum_j EX_{ijt} - EX_{jit} = \sum_j NEX_{ijt} \tag{3}$$

In this second approach, $$\mathbf{DD}_{ijt} \equiv TNEX_{it} - TNEX_{jt} \tag{3'}$$

$$\Rightarrow \tilde{\text{ß}}\mathbf{DD}_{ijt} = b_{12}TNEX_{it} + b_{13}TNEX_{jt} \tag{3''}$$

So that (1) becomes:

$$\begin{aligned} \ln\left(X_{ijt}\right) &= b_0 + b_1\ln\left(Y_{it}\right) + b_2\ln\left(Y_{jt}\right) + b_3\ln\left(D_{ij}\right) + b_4 Lang_{ij} + b_5Cont_{ij} + b_6FTA_{ij} \\ &+ b_7Landl_{ij} + b_8Islandf_{ij} + b_9ComCol_{ij} + b_{10}CurCol_{ij} + b_{11}ComNat_{ij} \\ &+ b_{12}TNEX_{it} + b_{13}TNEX_{jt} + e_{ijt} \end{aligned} \tag{1''}$$

It is important to note that $TNEX_{it}$ is truncated at zero since this paper does not try to identify reverse DD effects as explained above.

Instrumented net energy exports

The last approach defines $\mathbf{DD}_{ijt}$ as in (3') and then consists in instrumenting (total) net energy exports with the world price of energy. Here too, note that $TNEX_{it}$ is truncated at zero since this paper does not try to identify reverse DD effects as explained above; similarly, the instrument variable, the real price of energy is replaced with zero whenever $TNEX_{it} \leq 0$. This implies that there are actually two instrument variables, each $\geq 0 \Leftrightarrow TNEX_{it} \left(\text{resp. } TNEX_{jt}\right) \geq 0$

This third approach combines the advantages of the two preceding approaches. Its interpretation will be a straightforward total net energy export elasticity of manufacturing exports. And yet, instrumentation by world energy prices will limit worries regarding reverse causality running from changes in manufacturing exports to changes in energy exports.

Estimation procedure

I followed the norm in the literature by first using ordinary least squares, with robust and clustered standard errors (since pairs of countries are likely to be highly dependent across years), with or without year dummy variables. These results are reported in Appendix Table 2. Relying on clustered OLS results would *strengthen* the conclusions of this paper. Nonetheless, my preferred estimation strategy lays in panel data techniques, given the panel structure of the data used here and for technical reasons that will be expended on in Sections 4 and 5.1.

I use both fixed and random effects estimators extensively below. I rely on the robust fixed effects "within" estimator, which essentially adds a set of country-pair specific intercepts to the equation, and thus exploits only the time series dimension of the data set around country-pair averages. I will also report results with and without extra time-dummies aimed at controlling for time trends in the explained and explanatory variables. In section 5, I will present results from sensitivity and outlier analysis as well as for panel Tobit models for censored data. It turns out that my main results are very robust.

3.3 The Data Set

The core of the data used in this paper comes from the World Trade Data sets described in Feenstra, Lipsey and Bowen (1997). The World Trade Database (WTDB), put together by Statistics Canada, contains bilateral trade flows for all countries over 1970-1992, recently updated up to 1997, classified according to the Standard International Trade Classification. 98% of all trade is considered to be covered by this database.

Manufacturing exports are extracted by keeping exports falling under BEA 34-Industry Code 15-29, and 31 to 34. The technical reason behind this choice of industrial categories is that I want to keep BEA industry categories that are also classified as manufactured goods in the ISI classification. The economic rationale behind this choice is that I want a non-arbitrary rule to exclude industries that lay too closely to the borderline between the primary and secondary sectors.

Energy exports are extracted by keeping exports falling under all SITC Code 3XXX, *i.e.* all categories corresponding to energy production. For purposes of classification, country i will simply be classified as a net energy exporter in period t if $TNEX_{it}$, as defined in (3), *i.e.* total net exports of country i in period t is strictly greater than zero. The series for the world price of energy come from the International Financial Statistics (International Monetary Fund 2002). These are $US nominal indexes with 1995 as base year. The real equivalent of these series is used after deflating the nominal series by the US CPI (most commodities are priced in dollars). Hence, technically, energy price exposure as defined in (2) is computed as $EPE_{it} = 1(TNEX_{it} > 0) \text{ x } [\log(\textit{world price of energy}_t) - \log(CPUS_t)]$

The binary variables for common language, sharing a land border, belonging to the same regional trade agreement, colonies, having ever been colonies after 1945 with the same colonizer, remaining part of the same nation during the sample, the number of landlocked countries in the country-pair, the number of island nations in the pair, and distance between counties are all taken from Glick and Rose (2002). Real GDP and population data are taken from the World Bank (2002).

4 Estimates

My preferred specification is that of country-pair fixed effects. Glick and Rose (2002) also recommend this approach. I have also computed OLS results, although they are not the focus of this paper. Appendix Table 3 reports these results: the conclusions drawn in this paper would be strengthened by using least squares estimation. The reasons for the preference given to panel estimation techniques are manifold:

First, fixed effects (and to a lesser extent random effects) allow me to set aside concerns about omitted country pair characteristics. Time invariant relative factor abundance characteristics of country pairs are implicitly controlled for by the fixed effect specification. Hence, there is no need to worry about, for example, the relative energy price exposure or net energy export variables simply picking up the effect of some country-pair specific omitted variable that would be highly associated with one of the components of this variable. I return to this issue in the Section 5.1 where I report the sensitivity of the results to the introduction of country dummies for the origin or the destination countries. Second, fixed effects are an efficient way to set aside concerns about trade diversion issues.

Third, a fixed effect specification allows me to make inferences about the effect of resource *booms* on changes in manufacturing exports, rather than cross-country inferences regarding the effect of resource *abundance* on manufacturing export specialization. The standard interpretation of fixed effects estimates in the gravity literature is that they measure the effect of a change in an explanatory variable, here energy price exposure or total net energy exports, on the steady-state level of the dependent variable, here manufacturing exports.

Finally, I use time-dummies to control for trends in the dependent and independent variables. Whenever informative, I will report results with and without these time-dummies. It turns out that the null hypothesis of joint insignificance of time-dummies can always be rejected with very high confidence. Their inclusion, however, does not alter the results in any meaningful way. In the rest of this paper, I will report both country-pair fixed and, when informative, random effects. Random effect estimates allows for a check on the robustness of coefficients on country pair characteristics — such as common language — to the introduction of $\mathbf{DD}_{ijt}$. The coefficients for these country pair characteristics turn out to be robust to the introduction of $\mathbf{DD}_{ijt}$, which is reassuring.

Estimates will be presented as follows. I will first report standard gravity model estimates, using this paper's dataset, prior to the introduction of my new DD variables. The idea is to have a baseline against which to compare the following estimates, and to make sure the baseline regressions are comparable to those in the existing gravity literature. Next, I estimate (1') and (1''), *i.e.* my amended version of the gravity model incorporating DD effects. Results for the estimation of (1'') will be reported with — and, wherever relevant , without — instrumenting total net energy exports. These regressions will be referred to as the "benchmark" regressions to which sensitivity and outlier analysis will be applied later.

4.1 "Baseline" regressions

Table 1 displays estimates of the gravity model using my dataset prior to the introduction of any DD variables. The gravity model results are standard as well as intuitive. The model fits the data well, explaining from half to two-thirds of the variation in manufacturing trade flows. The gravity coefficients are economically and statistically significant with sensible interpretations. For instance, economically larger and richer countries export more manufactured goods; the further apart, the fewer manufactured goods two countries export to each other. A common language, land border and membership in a regional trade agreement encourage manufacturing exports, as does a common colonial history. Even the same nation coefficient is intuitively signed (contrary to Glick and Rose, 2002).

Note that the coefficient on own-GDP is greater than unity as well as greater than the coefficient on partner-GDP as in Feenstra, Markusen and Rose's (2001) simulations for differentiated goods. These authors find that this coefficient consistently rises as they move from homogenous to differentiated goods. This is consistent with a stronger home market effect for differentiated goods: manufacturing can move from one country to another more easily than production of resource-based homogenous goods (commodities) can. Time dummies cannot be rejected and so will be kept.

The Breusch-Pagan test for random effects strongly rejects the null hypothesis of no group (*i.e.* country-pair) random effects. This warrants the panel estimation method adopted throughout this paper. Further, the Hausman test for fixed *vs.* random effects strongly rejects the null hypothesis of zero correlation between the random effects and the repressors. This warrants the preference given to the fixed effects method in this paper.

4.2 "Benchmark" Results

Starting with Table 2, I introduce resource $\mathbf{DD}_{ijt}$ in each of its three forms. In Table 2A, I report first the estimates resulting from the introduction of "energy price exposure" variables. Next, Table 2B reports estimates resulting from the use of total net energy exports. Finally, in Table 2C are reported results corresponding to net energy exports instrumented by the world energy price.

Energy price exposure

In Table 2A estimates corresponding to (1') are presented. Here, $\mathbf{DD}_{ijt}$ consists of EPE_{it} and EPE_{jt}, energy price exposure for the origin and destination of the manufacturing export flow, respectively. Columns (2A.1) and (2A.3) report random effect estimates, with and without time-dummies, respectively. Columns (2A.2) and (2A.4) report fixed effect estimates, with and without time-dummies, respectively.

The Breusch-Pagan test for random effects again strongly rejects the null hypothesis of no group (*i.e.* country-pair) random effects and the Hausman test for fixed *vs*. random effects again strongly rejects the null hypothesis of zero correlation between the random effects and the regressors. This is the reason why the fixed effects approach is preferred throughout this paper over both random effects and even more so, over least squares (see Appendix Table 2). Also, the hypothesis that all time-dummies are jointly insignificant is rejected with a *p*-value below 1%.

Looking at all columns, (2A.1)- (2A.4), it appears that energy price booms in the origin country hurt manufacturing exports while booms in the destination country — by inducing real depreciation — actually boost manufacturing exports (in the origin country). Perhaps surprisingly, without time dummies, in Columns (2A.2) and (2A.4), energy booms at the destination are estimated to have a larger impact than booms in the origin country (in absolute value.) Except in (2A.4), random effects without time dummies, the hypothesis that the coefficient on the origin variable is equal to *minus* the coefficient on the destination variable cannot be confidently rejected. Taken together, this is indirect and preliminary evidence for the hypothesis that the resource movement effect is dwarfed by spending effects.

Quantitatively, the energy price elasticity of manufacturing exports is estimated to be quite high. The preferred specification (2A.2), fixed effects with time dummies, yields a 95% confidence interval no wider than [-.58,-.36] for energy price shocks to the origin, and no wider than [.27, .48] for energy price shocks to the destination. These intervals are tight and provide unambiguous evidence for energy price-led DD effects. These results are economically significant. All specifications yield a point estimate statistically different from zero below a 1% level of significance.

Net energy exports

In Table 2B estimates corresponding to (1") are presented. Here, $\mathbf{DD}_{ijt}$ consists of $TNEX_{it}$ and $TNEX_{jt}$, (total) net energy exports for the origin and destination of the manufacturing export flow, respectively. Columns (2B.1) and (2B.3) report random effect estimates, with or without time-dummies, respectively. Columns (2B.2) and (2B.4) report fixed effect estimates, with or without time-dummies, respectively.

Here too, the Breusch-Pagan test for random effects again strongly rejects the null hypothesis of no group (*i.e.* country-pair) random effects and the Hausman test for fixed *vs.* random effects again strongly rejects the null hypothesis of zero correlation between the random effects and the regressors. Also, the hypothesis that all time-dummies are jointly insignificant is rejected with a *p*-value below 1%.

Looking at all columns, (2B.1)-(2B.4), net energy exports in the origin country intuitively hurt manufacturing exports while energy booms in the destination country actually boost *the origin's* manufacturing exports. In all four specifications, surges in energy exports at the destination are estimated to have a larger impact than booms in the origin country (in absolute value.) Moreover, the hypothesis that the coefficient on the origin variable is equal to *minus* the coefficient on the destination variable can be confidently rejected. This is relatively strong evidence for the hypothesis that the resource movement effect is dwarfed by spending effects. This is consistent with the consensus found in the DD literature.

Quantitatively, the energy export elasticity of manufacturing exports is estimated to be relatively small. The preferred specification (2B.2), fixed effects with time dummies, yield a 95% confidence interval no wider than [-.013,-.006] for energy price shocks to the origin, and no wider than [.016, .023] for energy price shocks to the destination. These intervals are tight and supportive of the existence of unambiguous – albeit small – DD effects. All specifications yield a point estimate statistically different from zero below a 1% level of significance.

Instrumented net energy exports

Finally, in Table 2C estimates corresponding to (1'') are also presented. Here, $\mathbf{DD}_{ijt}$ consists of $TNEX_{it}$ and $TNEX_{jt}$, (total) net energy exports for the origin and destination, now instrumented with the world price of energy. This approach combines the advantages of the two preceding approaches. Its interpretation will be a straightforward net energy export elasticity of manufacturing exports. And yet, instrumentation by world energy prices will limit worries regarding reverse causality running from changes in manufacturing exports to changes in energy exports. Columns (2C.1) and (2C.3) report random effect estimates, with or without time-dummies, respectively. Columns (2C.2) and (2C.4) report fixed effect estimates, with or without time-dummies, respectively.

Once again, the Breusch-Pagan test for random effects strongly rejects the null hypothesis of no group (*i.e.* country-pair) random effects and the Hausman (I) test for fixed *vs.* random effects again strongly rejects the null hypothesis of zero correlation between the random effects and the regressors. Again, the hypothesis that all time-dummies are jointly insignificant is rejected with a *p*-value below 1%. Another Hausman test (II) is run to see if instrumented results systematically differ from the non-instrumented results reported in Table 2B. The results do differ systematically and significantly in all cases (2C.1-4), indicating that instrumented results are to be preferred.

After instrumentation, net energy export increases in the origin country still intuitively hurt manufacturing exports while booms in the destination country actually boost manufacturing exports. But in this case, in all four specifications, surges in energy exports at the *origin* are estimated to have at least as large an effect as booms in the *destination* country (in absolute value.) With time-dummies around, the hypothesis that the coefficient on the origin variable is equal to *minus* the coefficient on the destination variable cannot be confidently rejected. However, without time-dummies, this hypothesis is confidently rejected whereas the point estimate for the effect on the origin is clearly larger in absolute value than the point estimate on the destination.

Quantitatively, the energy export elasticity of manufacturing exports turns out to be economically significant. The preferred specification (2C.2), fixed effects with time dummies, yield a 95% confidence interval no wider than [-.10,-.06] for energy price shocks to the origin, and no wider than [.06, .11] for energy price shocks to the destination. These intervals are tight and supportive of the existence of unambiguous DD effects All specifications yield a point estimate statistically different from zero below a 1% level of significance. These point estimates and their corresponding confidence interval are an order of magnitude higher than when net energy exports are not instrumented.

Summary

In all three cases, using energy price exposure, or net energy exports, instrumented or not, significant evidence is found for DD effects. The energy price as well as net energy exports both have a significant and negative effect on manufacturing exports. The energy price elasticity of manufacturing exports in generally well above .4 (in absolute value). The energy export elasticity of manufacturing exports is around 1% (resp. 2%) at the origin (resp. destination) of the manufacturing export flow. Once instrumented with energy prices, the absolute value of is elasticity generally reaches 8% in the preferred specification at both ends of the manufacturing export flow.

These are economically significant coefficient. The corresponding confidence intervals are tight, implying that we can be confident that the coefficients in question do have the sign that the point estimate suggest in all likelihood. In all cases, (1-4), in all three tables, (2A)-(2.C), tests indicates that fixed effects with time-dummies is to be preferred on statistical ground to the other 3 reported specifications, as well as to OLS (see Appendix Table 2).

Further, Hausman tests reveal that “instrumented” coefficient estimates differ systematically and significantly from regular panel results. Hence reverse causality running from manufacturing to energy exports seems to be a serious concern. This may explain why “instrumented” coefficient estimates for energy exports are an order of magnitude larger than standard panel coefficients. A plausible cause for this is that changes in omitted variables — say, aggregate productivity shocks — will affect both manufacturing and energy exports in the same direction, which leads to expect positive correlation between these two — in fact *all* — types of exports. Absent control for these omitted variables influencing both the dependent and the independent variables, the error term and the dependent variable of interest will not be uncorrelated. However, instrumentation by the world energy price allows extracting from net energy exports changes the component that solely has to do with energy price led booms. As argued above, manufacturing exports in any single country are expected to be exogenous to the world price of energy.

5 Sensitivity analysis

In this section, I conduct sensitivity analysis of the benchmark results reported in the previous section. In turn, I will deal with the following issues: are excluded *country*-specific characteristics driving the results?

Is trade diversion what explains the identification of the main result of this paper? What is the impact of small observations for manufacturing trade on the results? How does exchange rate regime affect the results? And does measuring exports and other economic variables in different currencies affect the results?

5.1 Country specific characteristics and trade diversion

Countries are likely to specialize in manufacturing exports, to differing degrees, because of various fundamental country characteristics that the gravity model of trade may fail to capture. This worry arises because country-*pair* fixed effects may not capture well enough the effect of *country* specific omitted variables. One might worry that institutional factors, among other things, are driving the degree of specialization in manufacturing exports as well as in resource exports.

To check for the presence of such bias in the benchmark results, I introduce origin and destination dummies into the specification, on top of country-pair fixed effects. It is not possible to structure the panel error term specification around countries rather than country pairs because panel estimation procedures typically do not allow repeated time values within the panel.

To avoid the problem of exhaustion of degrees of freedom (and inversion of near-singular matrices), I will only introduce origin and destination country-specific effects in turn. Also, I need to return to the random country-pair effects to further free some degrees of freedom. The corresponding results are reported in Table 3A.

Origin and destination dummies are always jointly significant at a *p*-value lower than 1%. Results are somewhat affected by the introduction of country dummies. The right points of comparison are columns (2A.1) in Table 2A, (2B.1) in Table 2B and (2C.1) in Table 2C.

Let's first compare columns (3A.1) and (3A.4) with column (2A.1). The own-GDP coefficient increases with the introduction of origin country dummies and decreases with the introduction of destination country dummies. The partner-GDP coefficient decreases with the introduction of origin country dummies and increases with the introduction of destination country dummies. Comparing (3A.2) and (3A.3) with (2B.2), and (3A.5) and (3A.6) with (2C.2) leads to the same observations. It is beyond the scope of this paper to draw inferences concerning the gravity model of trade from these observations.

In the case of the introduction of origin country dummies, the absolute value of the coefficient on energy price exposure for the origin is weakened by 2 percent from 0.49 up to 0.47. The coefficient on net energy exports at the origin is unchanged with and without instrumentation. As for the destination coefficient it is actually strengthened by one percent in the case of energy price exposure, unchanged in the case of net energy exports and weakened by one percent in the case of instrumented net energy exports.

In the case of the introduction of destination country dummies, the absolute value of energy price exposure for the destination is weakened this time by 8 percent from 0.46 to 0.38. The absolute value of the coefficient on net energy exports is weakened by one percent without instrumentation and by 2 percent from when instrumented by world energy price. The origin coefficient is barely affected.

I conclude that, the introduction of origin and destination country dummy variables from the benchmark specifications, while statistically warranted, is not worth the corresponding loss of degrees of freedom (that cost the ability to use the recommended country-pair fixed effect setup), all the more given their modest quantitative impact on the coefficients of interest.

There is an interesting byproduct to these results. The question is: Are energy exporter simply diverting their manufacturing exports towards countries with respect to which they are not subject to the same type of shocks (specifically, other energy exporters). This issue is not specific to resource price shocks and has come to haunt many results derived with the use of gravity models, in particular those regarding the so-called *border effect*.

Anderson and Van Wincoop (2001) most forcefully make this point. They point out that "the theory, first developed by Anderson (1979), behind the gravity model of trade tells that after controlling for size, trade between two regions is decreasing in their bilateral barriers *relative to* the average barrier of the two regions to trade with all their partners."

Feenstra (2002) identifies two possible remedies to this type of problem. One is the use of country-pair fixed effects, as is done throughout this paper. The other is the introduction of explicit multilateral resistance terms. He concludes that both approaches yield consistent estimates of the border effect at stake in this literature.

The advantage of the multilateral resistance term is that it generates more consistent estimates whereas the advantage of the fixed effect approach is its obvious simplicity. Overall, Feenstra (2002) concludes that "since the fixed effects method produces consistent estimates of the average border effect across countries, and is easy to implement, so it might be considered to be the preferred empirical method."

In any case, the fact that the conclusions derived from benchmark results withstand the introduction of origin or destination dummies is reassuring with respect to trade diversion issues. Indeed, if these issues plagued the benchmark results, say by resulting in an omitted multilateral DD term, one would expect the introduction of country specific dummies to have a marked effect on the energy price or net energy exports coefficients.

In other words if there was systematic trade diversion in the case of energy exporters, then controlling for country characteristics, and hence energy export specialization, should have had a major impact of **DD** coefficients. Reassuringly, there is no such major effect. This is not to say that no diversion is taking place but, rather, that it does not affect the main conclusions of this paper.

5.2 Censoring Data

The next issue I deal with is that of small observations. Observations of very small manufacturing exports between countries are not conceptually very different from absence of manufacturing trade between these same countries. This raises the issue of the potential importance of treating these observations differently, *i.e.* as censored observations, than the larger manufacturing exports observed elsewhere. To tackle this issue, tobit censored regressions naturally suggest themselves. Here, manufacturing export observations below 1 million 1995 dollars have been censored as non-observations. Table 3B reports panel tobit results with or without time dummies, for energy price exposure and net energy exports.

The right points of comparison are columns (2A.1) & (2A.3) in Table 2A, and (2B.1) & (2B.3) in Table 2B. It appears from the comparison of (2A.1) with (3B.1), (2A.3) with (3B.2), (2B.1) with (3B.3), and (2B.3) with (3B.4) that censoring small observations has little impact on results. DD coefficient estimates strengthen in the case of energy price exposure in (3B.1) and (3B.2) and are unchanged in the case of total net energy exports in (3B.3) and (3B.4). This is remarkable given that there are as many as 25,464 censored observations.

5.3 Currency Definition

Table 3C tabulates key results when instead of working in real (1995) US dollars, all economic variables are evaluated instead in the origin country's currency or the destination country's currency. Benchmark results in US dollars are reproduced for comparison purposes. The transformation of the dollar figure is done using the real exchange rate index with 1995=100 as base year. Foreign currency variables are evaluated in 1995 US dollar equivalents. In any case, country pair dummies would absorb the effect of differing currency units, so there is no concern about changing the country pair base unit of measurement.

Measuring economic variables in the origin's currency improves the linear fit and measuring economic variables in the destination's currency further improves the linear fit. All coefficients remain highly significant despite small changes in the magnitude of the coefficients for energy price exposure (3C.1-2) and instrumented net energy exports (3C.3-4). The Hausman test indicates that instrumented results are to be preferred to non-instrumented results regardless of the currency unit of measurement.

In short, changing currency units for economic variables does not affect key results. The US dollar benchmark results are kept as a benchmark because it is the standard approach in the gravity literature, and there are obvious benefits to working in a third country's currency as it avoids some of the interpretation problems associated with potentially volatile exchange rates.

5.4 Instrumenting aggregate income

Table 3D tabulates key results when domestic and trade partner's aggregate income are instrumented to deal with the possibility that reverse causality running from manufacturing to aggregate income is biasing the coefficients of interest. Columns (3D.1), (3D.2), and (3D.3) report coefficients for fixed effects estimates with time dummies for energy exposure, net energy exports and instrumented net energy exports respectively. Columns (3D.1), (3D.2), and (3D.3) should be respectively compared to columns (2A.2), (2B.2) and (2C.2).

The new first stage variables used to instrument aggregate income and the two countries' population size. The first thing to note is that a Hausman test confidently rejects the null hypothesis of no systematic variation between the instrumented results and benchmark results. This result lends support to the idea that manufacturing exports play a systematic role for economic development.

The coefficients on domestic and partner income are weakened in all three cases. This is consistent with a positive effect of manufacturing exports on domestic income, and hence this lends some support to authors who are worried about DD effects on productivity and growth. I leave this as an interesting question for future research. As for the coefficients on DD variables, they are either unchanged or strengthened. There is thus no concern with this type of endogeneity as far as the conclusions of this paper are concerned.

6 Outlier analysis

This section takes on a couple of issues similar to that of the previous section. The question asked here is slightly different though: Is the inclusion of a specific group of observations driving the results in any meaningful way? This section will consider two important types of country groupings and their influence on the benchmark estimates. First, the effect of the inclusion of some exchange rate regimes will be investigated. Second, the effect of the inclusion of geographical areas will be considered.

6.1 Exchange Rate Regime

Let's first pay attention to one important aspect of the policy context in which energy booms take place. Real exchange rate appreciation is thought to be the principal mechanism of operation of the DD, so it makes sense to take a look at exchange rate regimes as a way to see how the results are affected by policy. Broda (2001) revisits the question of the relative merits of exchange rate arrangements for cushioning real shocks. He concludes that floating exchange rates work best for shielding against this type of shocks. It should be noted that this cushioning works precisely by affecting the terms of trade and hence trade itself.

It is often pointed out in the DD literature that regarding oil booms, one should expect resource movement effects to be shadowed by spending effects because of the "enclave economy" characteristics and high capital intensity of oil exploitation activities. The earlier finding that DD effects are of similar magnitude at the origin and destination is consistent with the consensus in the DD literature. It follows that we expect the role of the real exchange rate to be important.

Chen and Rogoff (2002) revisit the PPP puzzle in three OECD economies (Australia, Canada, and New Zealand) where primary commodities represent a significant portion of their exports. For Australia and New Zealand especially, they find that the price of their commodity exports — which they also take to be generally exogenous to these small economies — has a strong and stable influence on their real exchange rate, although this is not enough to explain the PPP puzzle. Figure III shows graphical evidence of this using my dataset.

Therefore, in Table 4A, I exclude from the sample origin and destination countries which have exchange rate regimes that I classify either as "effective floats" or "effective fixers". Data on exchange rate regimes is taken from Ghosh, Gulde, Ostry, and Wolf[10] (1996). They categorize regimes according to both the publicly stated commitment of the central bank (their *de jure* classification) and the observed behavior of the exchange rate (their *de facto* classification).

Neither approach is fully adequate. A country that declares to have a pegged exchange rate might in fact proceed to frequent changes in parity. Conversely, a country might experience very small exchange rate movements, even though the central bank has no formal requirement to uphold parity. I mimic Broda's (2001) approach and mix both criteria: I classify as either "effective floaters" or "effective fixers" countries that have not only a publicly stated commitment to such regime but also that have been observed not to manage their currencies, or to abstain from frequent adjustments, respectively. All other cases are lumped into a large 'intermediate' category.

The exclusion of floating or fixing origin as well as destination countries only has a marginal effect on the coefficient for ***DD*** variables. Most of the corresponding DFBetas do not get near or above 2 in absolute value despite the fact that we are discarding a non-negligible number of observations in each case. The only two exceptions are the cases of excluding origin counties with an effective floating or fixed exchange rate regime. Yet, excluding either of these exchange rate regimes would actually *strengthen* the conclusion reached in this paper.

This is why none of these groups of countries is hereafter excluded from the sample. I do not attempt to derive any policy implications from these results. The only significant DFBetas are contradictory in their interpretation. In any case, the ability to maintain a certain type of exchange rate regime may well depend itself on the absence of DD shocks. Besides, the country's choice of its exchange

[10] I am thankful to Professor Holger Wolf from Center for German and European Studies, School of Foreign Service at Georgetown University for sharing his data.

rate regime is possibly endogenous to the nature of the shocks facing the country, making causal inferences very dangerous.

6.2 Geographical Areas

The second issue this section tackles with is that of the impact on estimates of the exclusion of entire geographical entities. I divide the world as the World Bank (2002) does: I classify all countries into eight non-overlapping and exhaustive geographical areas: East Asia and Pacific, Eastern Europe & Central Asia, Middle East & Northern Africa, South Asia, Western Europe, Northern America, Sub-Saharan Africa, Latin America & the Caribbean. Table 4B reports the estimates and DFBetas statistics corresponding to the *exclusion* of the corresponding countries fro m the sample. These statistics have been computed for the energy price exposure and net energy exports.

There are few cases where the exclusion of a whole geographical impacts significantly the coefficient on energy price exposure and instrumented net energy exports. The exclusion of East Asia and Pacific weakens the coefficient on energy price exposure and (instrumented) net energy exports for the origin. The exclusion of Latin America and the Caribbean also weakens the coefficient on energy price exposure and instrumented net energy exports for the origin.

As for the exclusion of any other group, the result is either an insignificant DFBeta or a negative DBeta, in which case the exclusion of the corresponding group of countries would actually strengthen the conclusions of this paper. Interestingly the “Middle East & Northern Africa” area does not seem to have much of an effect on the results.

For the sake of the generality of the test proposed here, I keep these two groups in the dataset used to derive benchmark results. However, I want to highlight these observations as an interesting path for further research. In other words, why have “East Asia & Pacific” as well as “Latin America & the Caribbean” been apparently so sensitive to Dutch Disease shocks? And why has Sub-Saharan Africa been conversely relatively insensitive to them? The answer may well simply be that to be subject to the DD, a country must obviously have some non-negligible amount of manufacturing activity to loose.

7 Conclusion

Acknowledging potential endogeneity issues of commodity exports to manufacturing trade, world real energy prices are used to identify price led resource booms. Indeed, world commodity prices can be safely assumed to be exogenous to any single country's manufacturing exports.

With this identifying assumption in hand, the world energy price elasticity of manufacturing exports in net energy exporters is estimated to be *minus* .49 for shock taking place at the origin of the manufacturing flow, and .46 for shocks taking place at the destination of this flow. In other words, a one percent increase in the price of energy will, everything else held constant, and on average, decrease a net energy exporter's real manufacturing exports by about half a percent. Correspondingly, a one percent increase in world energy price affecting the destination will boost the origin's manufacturing exports by .38% in the preferred specification.

Of course, the effect of an energy boom will depend on the relative importance of this shock to the economy in question. To facilitate interpretation, a second type of elasticity is estimated: the total net energy export elasticity of manufacturing exports. Once instrumented by world energy prices, this elasticity is estimated to be minus 0.08 both for shock taking place at the origin of the manufacturing flow and .for shocks taking place at the destination of the flow of manufacturing goods. In other words, a one percent increase in the total net energy exports of a country will, everything else held constant, hurt this country's real manufacturing exports by .08 percent. Similarly, a one percent increase in the destination country's net energy exports will boost the origin's manufacturing exports also by .08% in the preferred specification.

These elasticity estimates are highly significant and the corresponding confidence intervals are tight. Results are robust to the introduction of country specific fixed effects, alleviating concerns about trade diversion issues. Results are not dependent on the presence or treatment of small export observations, nor to changes in the currency unit of measurement.

Reassuringly, results are not significantly affected by any country unit; and further, the main conclusion of this paper is not dependent on any specific group of countries. Yet, the sensitivity of East Asia and the Pacific as well as of Latin America and the Caribbean, and the insensitivity of sub-Saharan Africa to the DD of is highlighted as an interesting path for further research.

The main result found in this paper is somewhat surprising in the current state of the DD literature. I find strong evidence of the DD in the World Trade Data. Energy-price led booms have systematically tended to hurt energy exporters' manufacturing exports. In contrast with the existing literature, this paper puts aside doubts about the empirical relevance of the DD, particularly regarding energy exporters. It is intuitive, however, that by juxtaposing the marginally convincing evidence found in numerous countries, one should be able to either reject or accept the DD hypothesis.

It is important not to over-interpret the results of this paper. Booms are known to result in increased GDP levels, and hence aggregate welfare, for energy producing nations. This paper is not disputing this fact. Further, one only needs to worry about the DD to the extent that there is in fact something desirable about having a large proportion of manufacturing exports. Indeed, productivity growth can sometimes be very strong in resource extraction industries. Similarly, the role played by the manufacturing sector compared to the primary sector can vary from country to country, among other reasons because of the possibility of factor intensity reversal.

In contrast, in the structural tradition of the development literature, it is typically assumed that industrialization is key to the goal of economic development. More recently, authors like Matsuyama (1992), as well as Sachs and Warner (1995, 1999) explicitly model economic growth as a function of the relative size of the manufacturing sector. It is not the purpose of this paper to settle this issue. Rather, the statistic and economic significance as well as the robustness of results found here lead me to argue in favor of careful future empirical testing of the effect of sectoral changes in output and exports, specifically those resulting from energy booms, on productivity and growth.

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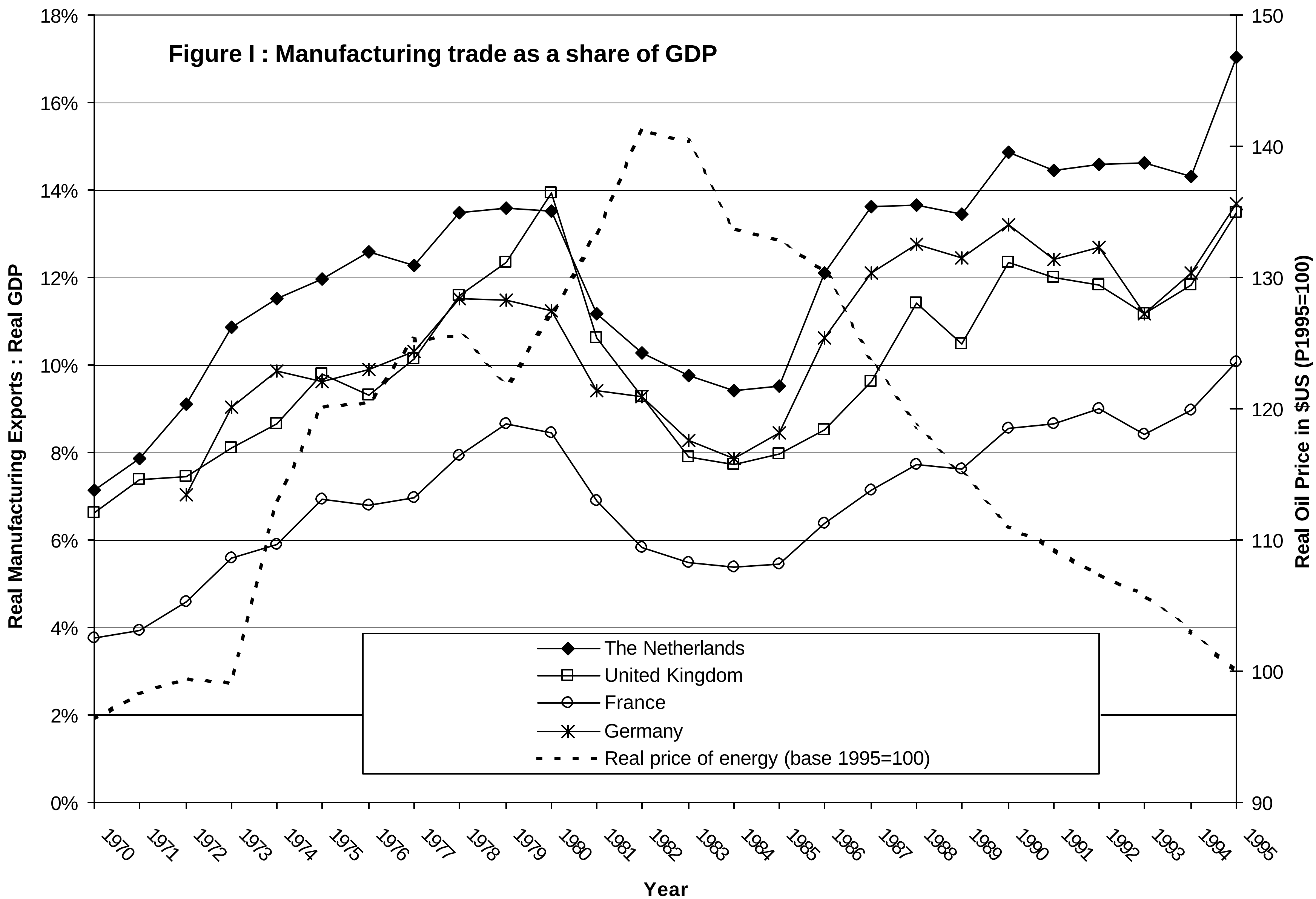

Figure I : Manufacturing trade as a share of GDP
Real Manufacturing Exports : Real GDP
Real Oil Price in $US (P1995=100)
Year
18%
16%
14%
12%
10%
8%
6%
4%
2%
0%
150
140
130
120
110
100
90
1970
1971
1972
1973
1974
1975
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993
1994
1995
The Netherlands
United Kingdom
France
Germany
Real price of energy (base 1995=100)

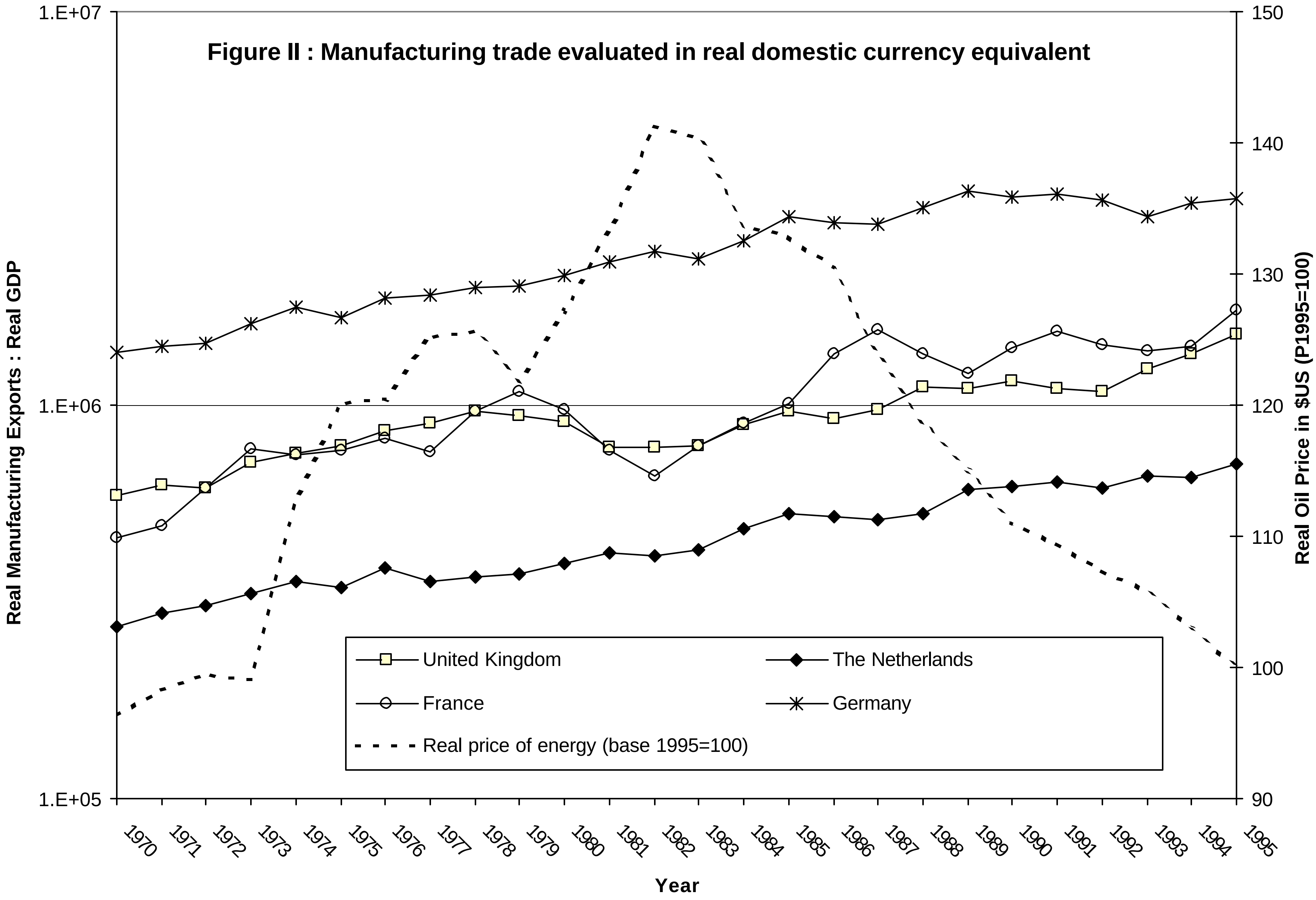
Figure II : Manufacturing trade evaluated in real domestic currency equivalent
Real Manufacturing Exports : Real GDP
Real Oil Price in $US (P1995=100)
1.E+07
1.E+06
1.E+05
150
140
130
120
110
100
90
1970
1971
1972
1973
1974
1975
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993
1994
1995
Year
United Kingdom
The Netherlands
France
Germany
Real price of energy (base 1995=100)

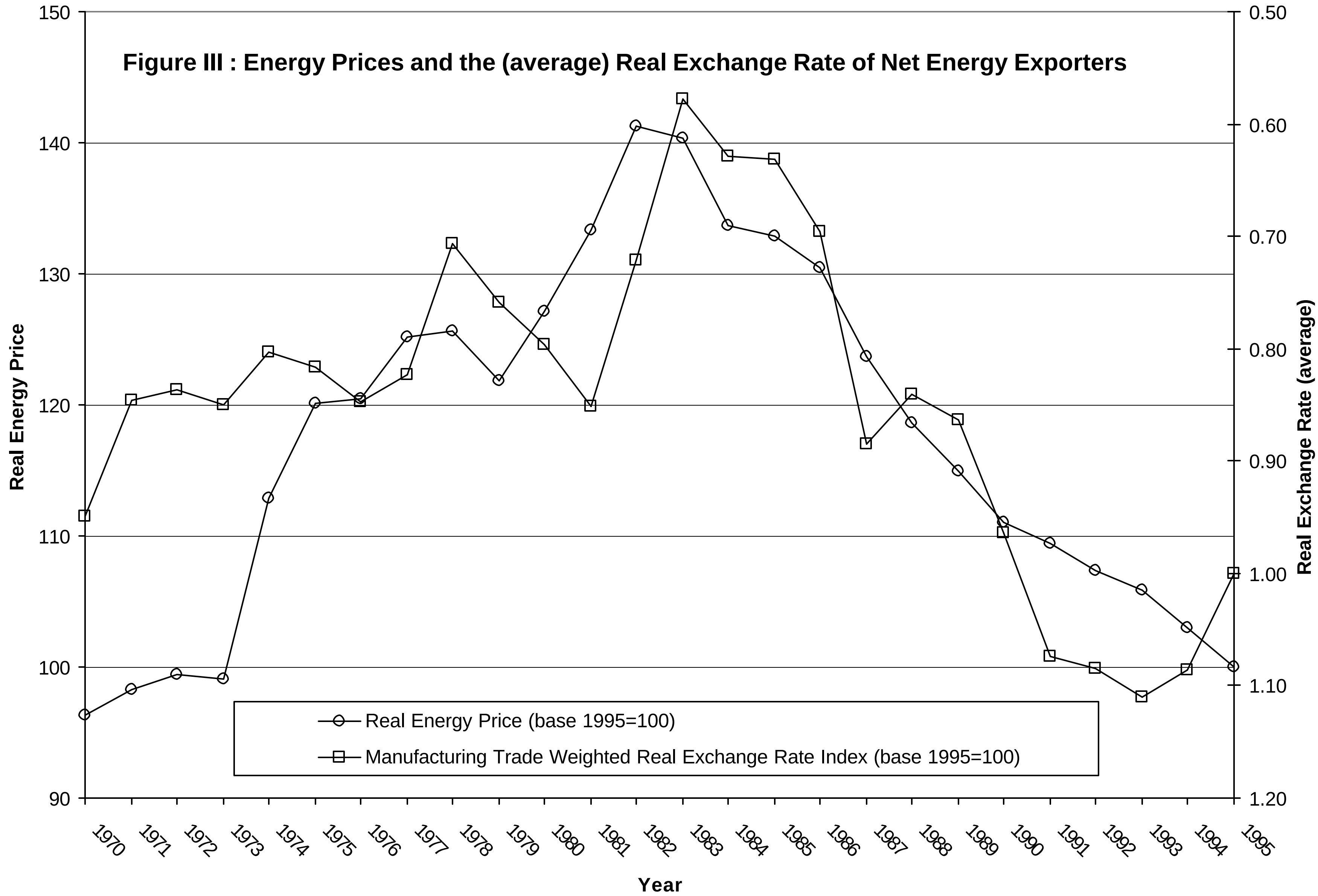
Figure III : Energy Prices and the (average) Real Exchange Rate of Net Energy Exporters
Real Energy Price
Real Exchange Rate (average)
Year
150
140
130
120
110
100
90
0.50
0.60
0.70
0.80
0.90
1.00
1.10
1.20
1970
1971
1972
1973
1974
1975
1976
1977
1978
1979
1980
1981
1982
1983
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993
1994
1995
Real Energy Price (base 1995=100)
Manufacturing Trade Weighted Real Exchange Rate Index (base 1995=100)

Table 1: Gravity model regressions

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation	**Random Effects**	**Fixed Effects**	**Random Effects**	**Fixed Effects**
	(1.1)	**(1.2)**	**(1.3)**	**(1.4)**
Log real GDP of origin	1.32 (0.01)***	1.90 (0.02)***	1.35 (0.01)***	1.44 (0.02)***
Log real GDP of destination	0.80 (0.01)***	1.40 (0.02)***	0.83 (0.01)***	0.99 (0.02)***
Log of distance	-1.23 (0.03)***		-1.25 (0.03)***	
1 (Common Language)	0.64 (0.06)***		0.65 (0.06)***	
1 (Common Border)	0.37 (0.13)***		0.35 (0.13)***	
1 (Regional Trade Agreement)	0.82 (0.11)***		0.73 (0.11)***	
1 (Common colonizer)	1.07 (0.07)***		1.16 (0.07)***	
1 (Colonial relationship)	1.46 (0.16)***		1.41 (0.16)***	
1 (Same nation in the sample)	2.26 (0.43)***		2.34 (0.43)***	
# of land-locked countries in pair	-0.15 (0.04)***		-0.12 (0.04)***	
Constant	-39.40 (0.37)***	-77.53 (0.81)***	-40.50 (0.32)***	-56.57 (0.34)***
Time dummies	p(F)=.00	p(F)=.00	Excluded	Excluded
Observations	135,947	137,650	135,947	137,650
Country pairs	9,651	10,137	9,651	10,137
R-squared	0.64	0.51	0.64	0.52
Breusch and Pagan	p(c^2)=.00		p(c^2)=.00	
Hausman	p(c^2)=.00		p(c^2)=.00	

Standard errors in parentheses
** significant at 10%; ** significant at 5%; *** significant at 1%*
Estimated coefficients and standard errors are not reported for time dummies, instead the p-value of a joint significance test is reported.
Random and fixed effects grouped on country-pairs.

Table 2A: Benchmark estimates:
Energy price exposure

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation	**Random Effects**	**Fixed Effects**	**Random Effects**	**Fixed Effects**
	(2A.1)	**(2A.2)**	**(2A.3)**	**(2A.4)**
Log real GDP of origin	1.31 (0.01)***	1.91 (0.02)***	1.34 (0.01)***	1.46 (0.02)***
Log real GDP of destination	0.80 (0.01)***	1.39 (0.02)***	0.83 (0.01)***	0.98 (0.02)***
Log of distance	-1.23 (0.03)***		-1.25 (0.03)***	
1 (Common Language)	0.64 (0.06)***		0.65 (0.06)***	
1 (Common Border)	0.37 (0.12)***		0.35 (0.13)***	
1 (Regional Trade Agreement)	0.82 (0.11)***		0.72 (0.11)***	
1 (Common colonizer)	1.07 (0.07)***		1.16 (0.07)***	
1 (Colonial relationship)	1.46 (0.16)***		1.41 (0.16)***	
1 (Same nation in the sample)	2.26 (0.43)***		2.34 (0.43)***	
# of land-locked countries in pair	-0.15 (0.04)***		-0.13 (0.04)***	
Energy price exposure (origin)	-0.49 (0.06)***	-0.47 (0.06)***	-0.45 (0.05)***	-0.34 (0.05)***
Energy price expos. (destination)	0.46 (0.05)***	0.38 (0.05)***	0.51 (0.05)***	0.53 (0.05)***
H0: orgin = - dest.	p(F)=.71	p(F)=.24	p(F)=.32	p(F)=.00
Constant	-39.35 (0.36)***	-77.48 (0.81)***	-40.54 (0.32)***	-56.77 (0.34)***
Time dummies	p(F)=.00	p(F)=.00	Excluded	Excluded
Observations	135,947	137,650	135,947	137,650
Country pairs	9,651	10,137	9,651	10,137
R-squared	0.64	0.52	0.64	0.52
Breusch and Pagan	p(c^2)=.00		p(c^2)=.00	
Hausman	p(c^2)=.00		p(c^2)=.00	

Standard errors in parentheses
*significant at 10%; ** significant at 5%; *** significant at 1%*
For time dummies, the p-value of a joint significance test is reported.
Random and fixed effects grouped on country-pairs.

Table 2B: Benchmark estimates (continued)
Net energy exports

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation	**Random Effects**	**Fixed Effects**	**Random Effects**	**Fixed Effects**
	(2B.1)	**(2B.2)**	**(2B.3)**	**(2B.4)**
Log real GDP of origin	1.31 (0.01)***	1.90 (0.02)***	1.34 (0.01)***	1.44 (0.02)***
Log real GDP of destination	0.79 (0.01)***	1.39 (0.02)***	0.82 (0.01)***	0.98 (0.02)***
Log of distance	-1.22 (0.03)***		-1.24 (0.03)***	
1 (Common Language)	0.63 (0.06)***		0.64 (0.06)***	
1 (Common Border)	0.36 (0.12)***		0.35 (0.12)***	
1 (Regional Trade Agreement)	0.84 (0.11)***		0.74 (0.11)***	
1 (Common colonizer)	1.06 (0.07)***		1.14 (0.07)***	
1 (Colonial relationship)	1.46 (0.16)***		1.41 (0.16)***	
1 (Same nation in the sample)	2.23 (0.42)***		2.31 (0.42)***	
# of land-locked countries in pair	-0.15 (0.04)***		-0.12 (0.04)***	
Net energy exports (origin)	-0.01 (0.00)***	-0.01 (0.00)***	-0.01 (0.00)***	-0.01 (0.00)***
Net energy exports (destination)	0.03 (0.00)***	0.02 (0.00)***	0.03 (0.00)***	0.02 (0.00)***
H0: orgin = - dest.	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.02
Constant	-39.05 (0.36)***	-77.39 (0.81)***	-40.18 (0.31)***	-56.50 (0.34)***
Time dummies	p(F)=.00	p(F)=.00	Excluded	Excluded
Observations	135,947	137,650	135,947	137,650
Country pairs	9,651	10,137	9,651	10,137
R-squared	0.64	0.52	0.64	0.52
Breusch and Pagan	p(c^2)=.00		p(c^2)=.00	
Hausman	p(c^2)=.00		p(c^2)=.00	

*Standard errors in parentheses - significant at 10%; ** at 5%; *** at 1%*
Estimated coefficients and standard errors are not reported for time dummies, instead the p-value of a joint significance test is reported.
Random and fixed effects grouped on country-pairs.

Table 2C: Benchmark estimates (continued)
Net energy exports (instrumented)

Dependent Variable: Real manufacturing trade between country of origin and country of destination
Endogenous variable: Net energy exports
Instrument variable: Energy price exposure (origin and destination)

Method of estimation	Random Effects	Fixed Effects	Random Effects	Fixed Effects
	(2C.1)	(2C.2)	(2C.3)	(2C.4)
Log real GDP of origin	1.30 (0.01)***	1.91 (0.02)***	1.32 (0.01)***	1.44 (0.02)***
Log real GDP of destination	0.79 (0.01)***	1.38 (0.03)***	0.81 (0.01)***	0.93 (0.02)***
Log of distance	-1.23 (0.03)***		-1.23 (0.03)***	
1 (Common Language)	0.64 (0.06)***		0.64 (0.05)***	
1 (Common Border)	0.36 (0.12)***		0.34 (0.12)***	
1 (Regional Trade Agreement)	0.83 (0.11)***		0.77 (0.11)***	
1 (Common colonizer)	1.05 (0.07)***		1.12 (0.07)***	
1 (Colonial relationship)	1.50 (0.16)***		1.44 (0.15)***	
1 (Same nation in the sample)	2.24 (0.42)***		2.27 (0.41)***	
# of land-locked countries in pair	-0.17 (0.04)***		-0.13 (0.04)***	
Net energy exports (origin)	-0.08 (0.01)***	-0.08 (0.01)***	-0.07 (0.01)***	-0.05 (0.01)***
Net energy exports (destination)	0.10 (0.01)***	0.08 (0.01)***	0.11 (0.01)***	0.14 (0.01)***
H0: orgin = - dest.	p(F)=.36	p(F)=.98	p(F)=.03	p(F)=.00
Time dummies	p(F)=.00	p(F)=.00	Excluded	Excluded
Observations	135,947	137,650	135,947	137,650
Country pairs	9,651	10,137	9,651	10,137
R-squared	0.65	0.52	0.65	0.53
Breusch and Pagan	p(c^2)=.00		p(c^2)=.00	
Hausman I	p(c^2)=.00		p(c^2)=.00	
Hausman II	p(c^2)=.00	p(c^2)=.00	p(c^2)=.00	p(c^2)=.00

Standard errors in parentheses
** significant at 10%; ** significant at 5%; *** significant at 1%.*
For time dummies and the constant,
the p-value of a joint significance test is reported.
Random and fixed effects grouped on country-pairs.

Table 3A: Sensitivity Analysis
Origin and Destination country dummy variables

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation Random Effects	**Energy P exposure**	**Net energy exports**	**Net energy exports (IV)**	**Energy P exposure**	**Net energy exports**	**Net energy exports (IV)**
	Origin Fixed Effects			**Destination Fixed Effects**		
	(3A.1)	**(3A.2)**	**(3A.3)**	**(3A.4)**	**(3A.5)**	**(3A.6)**
Log real GDP of origin	1.91 (0.02)***	1.90 (0.02)***	1.91 (0.02)***	1.30 (0.01)***	1.30 (0.01)***	1.90 (0.02)***
Log real GDP of destination	0.78 (0.01)***	0.77 (0.01)***	0.77 (0.01)***	1.38 (0.02)***	1.39 (0.02)***	0.77 (0.01)***
Energy price exposure (origin)	-0.46 (0.06)***	--	--	-0.48 (0.06)***	--	--
Energy price expos. (destination)	0.47 (0.05)***	--	--	0.38 (0.05)***	--	--
Net energy exports (origin)	--	-0.01 (0.00)***	-0.08 (0.01)***	--	-0.01 (0.00)***	-0.08 (0.01)***
Net energy exports (destination)	--	0.03 (0.00)***	0.09 (0.01)***	--	0.02 (0.00)***	0.08 (0.01)***
			[Unreported country-pair specific variables]			
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00
Origin dummies	**p(F)=.00**	**p(F)=.00**	**p(F)=.00**	Excluded	Excluded	Excluded
Destination dummies	Excluded	Excluded	Excluded	**p(F)=.00**	**p(F)=.00**	**p(F)=.00**
R-squared	0.74	0.74	0.74	0.67	0.67	0.67

*Standard errors in parentheses - significant at 10%; ** significant at 5%; *** significant at 1%*
Estimated coefficients and standard errors are not reported for time dummies, instead a the p-value of a joint significance test is reported. Random effects grouped on country pairs.

Table 3B: Sensitivity Analysis
Censoring small export observations

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation:	**Panel Tobit**	**Panel Tobit**	**Panel Tobit**	**Panel Tobit**
	(3B.1)	**(3B.2)**	**(3B.3)**	**(3B.4)**
Log real GDP of origin	1.35 (0.01)***	1.35 (0.01)***	1.30 (0.00)***	1.32 (0.00)***
Log real GDP of destination	0.85 (0.01)***	0.81 (0.00)***	0.82 (0.01)***	0.75 (0.01)***
Log of distance	-1.39 (0.01)***	-1.39 (0.01)***	-1.16 (0.01)***	-1.09 (0.01)***
1(Common Language)	0.49 (0.03)***	0.32 (0.03)***	0.63 (0.03)***	0.67 (0.03)***
1(Common Border)	0.24 (0.04)***	0.21 (0.05)***	0.78 (0.04)***	0.90 (0.04)***
1(Regional Trade Aggreement)	0.44 (0.04)***	0.41 (0.05)***	0.89 (0.04)***	0.92 (0.04)***
1(Common conlonizer)	0.97 (0.04)***	0.92 (0.04)***	0.97 (0.05)***	1.05 (0.04)***
1(Colonial relashionship)	1.81 (0.00)	1.43 (0.04)***	1.66 (0.05)***	2.08 (0.00)
1(Same nation in the sample)	3.46 (0.08)***	1.19 (0.00)	3.45 (0.08)***	0.93 (0.00)
# of land-locked countries in pair	-0.21 (0.03)***	-0.20 (0.02)***	-0.22 (0.02)***	-0.11 (0.02)***
Energy price exposure (origin)	-0.46 (0.05)***	-0.50 (0.05)***	--	--
Energy price expos. (destination)	0.56 (0.05)***	0.54 (0.04)***	--	--
Net energy exports (origin)	--	--	-0.01 (0.00)***	-0.01 (0.00)***
Net energy exports (destination)	--	--	0.03 (0.00)***	0.03 (0.00)***
Time dummies	p(F)=.00	Excluded	p(F)=.00	Excluded
Uncensored obs.	110,483	110,483	110,483	110,483
Censored obs.	25,464	25,464	25,464	25,464

Standard errors in parentheses
****significant at 10%; ** significant at 5%; *** significant at 1%*
Time-dummies are included in all regressions but Estimated coefficients and standard errors are not reported for time dummies,
All observations below 1 million 1995 dollars *have been censored*

Table 3C: Sensitivity Analysis; Currency definition

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation: **Country pair fixed-effects**	***Benchmark $US***	**Origin's Currency**	**Destination's Currency**	***Benchmark $US***	**Origin's Currency**	**Destination's Currency**
	(2A.2)	**(3C.1)**	**(3C.2)**	***(2C.2)***	**(3C.3)**	**(3E.4)**
Log real GDP of origin	1.91 (0.02)***	0.98 (0.02)***	0.83 (0.02)***	1.91 (0.02)***	1.00 (0.02)***	0.84 (0.02)***
Log real GDP of destination	1.39 (0.02)***	0.57 (0.02)***	0.31 (0.02)***	1.38 (0.03)***	0.56 (0.02)***	0.27 (0.02)***
Energy price exposure (origin)	-0.47 (0.06)***	-0.22 (0.06)***	-0.43 (0.06)***	--	--	--
Energy price expos. (destination)	0.38 (0.05)***	0.43 (0.05)***	0.57 (0.05)***	--	--	--
Net energy exports (origin) (instrumented)	--	--	--	-0.08 (0.01)***	-0.06 (0.01)***	-0.09 (0.01)***
Net energy exports (destination) (instrumented)	--	--	--	0.08 (0.01)***	0.12 (0.02)***	0.18 (0.02)***
Constant	-77.48 (0.81)***	-37.06 (0.37)***	-26.02 (0.34)***	-77.46 (0.85)***	-37.18 (0.39)***	-25.43 (0.36)***
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00
Observations	137,650	134,771	134,864	137,650	134,771	134,864
Country pairs	10,137	10,012	10,013	10,137	10,012	10,013
R-squared	0.52	0.61	0.63	0.52	0.62	0.64
Hausman	--	--	--	p(F)=.00	p(F)=.01	p(F)=.00

*Standard errors in parentheses- * significant at 10%; ** significant at 5%; *** significant at 1%*
For time dummies, the p-value of a joint significance test is reported.
Currency conversion done using a real exchange rate index with base 1995=100

Table 3D: Sensitivity Analysis
Net Energy Exports – IV regressions

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Endogenous variables: **Log real GDP of origin and destination (and net energy exports in 3D.3)**

Instrument variables: **Population of origin and destination (and energy price exposure in 3D.3)**

Method of estimation:	**(3D.1)**	**(3D.2)**	**(3D.3)+**
IV Fixed Effects			
Log real GDP of origin	1.88 (0.13)***	1.86 (0.13)***	1.77 (0.13)***
Log real GDP of destination	1.10 (0.14)***	1.13 (0.14)***	1.23 (0.14)***
Energy price exposure (origin)	-0.47 (0.06)***	--	--
Energy price expos. (destination)	0.41 (0.06)***	--	--
Net energy exports (origin)	--	-0.01 (0.00)***	-0.08 (0.01)***
Net energy exports (destination)	--	0.02 (0.00)***	0.09 (0.01)***
Constant	-69.99 (4.38)***	-70.08 (4.39)***	-70.44 (4.49)***
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00
Observations	137,650	137,650	137,650
Country pairs	10,137	10,137	10,137
R-squared	0.52	0.53	0.53
Breusch and Pagan	p(c^2)=.00	p(c^2)=.00	p(c^2)=.00
Hausman	p(c^2)=.00	p(c^2)=.00	p(c^2)=.00

Standard errors in parentheses
** significant at 10%; ** significant at 5%; *** significant at 1%*
Estimated coefficients and standard errors are not reported for time dummies, instead the p-value of a joint significance test is reported.
Fixed effects grouped on country-pairs.

Table 4A: Outlier Analysis: Exchange Rate Regime

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Excluded **Exchange Rate Regime**	**Floating Origin**	**Fixing Origin**	**Floating Destin.**	**Fixing Destin.**	**Floating Origin**	**Fixing Origin**	**Floating Destin.**	**Fixing Destin.**
	(4A.1)	**(4A.2)**	**(4A.3)**	**(4A.4)**	**(4A.5)**	**(4A.6)**	**(4A.7)**	**(4A.8)**
Log real GDP of origin	1.95 (0.03)***	2.03 (0.03)***	1.96 (0.03)***	1.87 (0.03)***	1.97 (0.03)***	2.01 (0.03)***	1.97 (0.03)***	1.87 (0.03)***
Log real GDP of destination	1.35 (0.03)***	1.38 (0.02)***	1.38 (0.02)***	1.40 (0.03)***	1.34 (0.03)***	1.39 (0.03)***	1.37 (0.03)***	1.41 (0.03)***
Energy price exposure (origin):1	-0.52 (0.06)***	-0.68 (0.06)***	-0.50 (0.06)***	-0.42 (0.06)***	--	--	--	--
Energy price expos. (destin.):2	0.36 (0.06)***	0.42 (0.06)***	0.41 (0.06)***	0.31 (0.06)***	--	--	--	--
Net energy exports (origin):1	--	--	--	--	-0.11 (0.01)***	-0.12 (0.01)***	-0.09 (0.01)***	-0.08 (0.01)***
Net energy exports (destination):2	--	--	--	--	0.08 (0.01)***	0.09 (0.01)***	0.10 (0.01)***	0.07 (0.01)***
DFBeta 1	*-.92*	*-3.81*	*-.67*	*.88*	<u>*-2.61*</u>	<u>*-3.61*</u>	*-1.23*	*.83*
DFBeta 2	*-.29*	*.93*	*.64*	*-1.24*	*-.01*	*.72*	*1.19*	*-1.15*
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00
Observations	115,002	118,710	116,499	114,707	115,002	118,710	116,499	114,707
Country pairs	9,913	9,681	9,917	9,675	9,913	9,681	9,917	9,675
R-squared	0.48	0.54	0.50	0.52	0.49	0.54	0.51	0.53

Method of estimation: Country pair fixed-effects *grouped on country-pairs.*

*Standard errors in parentheses - * significant at 10%; ** significant at 5%; *** significant at 1%*
Estimated coefficients and standard errors are not reported for time dummies, instead the p-value of a joint significance test is reported.
DFBeta is for energy price exposure or net energy exports; underlined DFBetas are significant

Table 4B: Outlier analysis: Excluding Geographical Regions

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Excluded geographical region	**East Asia & Pacific (4B.1)**	**Eastern Europe & Central Asia (4B.2)**	**Middle East & Northern Africa (4B.3)**	**South Asia (4B.4)**	**Western Europe (4B.5)**	**Northern America (4B.6)**	**Sub-Saharan Africa (4B.7)**	**Latin America & the Caribbean (4B.8)**
Energy price exposure (origin)	-0.16 (0.06)***	-0.52 (0.06)***	-0.41 (0.06)***	-0.50 (0.06)***	-0.47 (0.07)***	-0.57 (0.06)***	-0.65 (0.06)***	-0.22 (0.06)***
DFBeta	*5.41*	*-.95*	*1.02*	*-.58*	*.012*	*-1.74*	*-3.32*	*4.37*
Energy price expos. (destin.)	0.38 (0.06)***	0.36 (0.05)***	0.40 (0.05)***	0.34 (0.05)***	0.35 (0.07)***	0.36 (0.05)***	0.43 (0.05)***	0.42 (0.05)***
DFBeta	*.07*	*-.40*	*.40*	*-.65*	*-.56*	*-.39*	*1.01*	*.79*
Net energy exports (origin)	-0.03 (0.01)***	-0.09 (0.01)***	-0.08 (0.01)***	-0.09 (0.01)***	-0.14 (0.02)***	-0.09 (0.01)***	-0.09 (0.01)***	-0.05 (0.01)***
DFBeta	*5.31*	*-.80*	*.42*	*-.30*	*-5.88*	*-1.09*	*-1.18*	*3.35*
Net energy exports (destination)	0.09 (0.01)***	0.08 (0.01)***	0.09 (0.01)***	0.07 (0.01)***	0.10 (0.02)***	0.08 (0.01)***	0.09 (0.01)***	0.10 (0.01)***
DFBeta	*.49*	*-.56*	*.17*	*-.98*	*1.07*	*-.18*	*.54*	*1.28*
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00	p(F)=.00
Observations	116,608	135,905	123,066	130,768	121,671	136,132	126,942	131,670
Country pairs	8,931	10,019	9,032	9,677	10,018	10,130	9,939	10,013

Method of estimation: Country pair fixed-effects *grouped on country-pairs.*

*Standard errors in parentheses - * significant at 10%; ** significant at 5%; *** significant at 1%*
Estimated coefficients and standard errors are not reported for time dummies,
instead the p-value of a joint significance test is reported.
DFBetas are for energy price exposure or net energy exports; underlined DFBetas are significant

Appendix Table 1: Net energy exporting and importing country classification

Country Name	70	71	72	73	74	75	76	77	78	79	80	81	82	83	84	85	86	87	88	89	90	91	92	93	94	95	96	97
Afghanistan	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	-1	-1	.	.	.	-1	.	.	1	-1
Albania	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	-1	-1	-1	-1
Algeria	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Angola	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Argentina	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1	-1	-1	-1	1	1	1	1	1	1	1	1
Australia	1	1	1	1	1	1	1	-1	1	1	-1	1	1	1	-1	1	1	1	1	1	1	-1	1	1	-1	-1	1	1
Austria	-1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Bahamas, The	-1	1	1	1	-1	1	1	-1	1	1	-1	1	1	1	1	1	1	1	1	-1	-1	-1	1	-1	-1	-1	-1	-1
Bahrain	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1	.	1	1	1	1	1	1	1	-1	-1	1
Bangladesh	.	.	.	-1	.	-1	-1	-1	-1	.	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Barbados	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1
Belgium	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Belize	.	.	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	.	-1	-1	-1	-1	-1	-1
Benin	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	.	1	1	1	1	.
Bhutan	.	.	.	.	.	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	1	1	1	1	1	-1	-1	.
Bolivia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1
Brazil	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Brunei	.	.	.	.	.	.	.	.	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Bulgaria	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Burkina Faso	.	.	.	.	.	.	.	.	-1	-1	-1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	-1
Burundi	.	.	.	.	.	.	.	.	-1	-1	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	.	.	1	.
Cambodia	-1	-1	-1	-1	-1	-1	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	.	-1	.	-1	-1	-1	.	-1
Cameroon	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Canada	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Central African Republ	.	.	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	1	-1	.	1	.	-1	-1	-1	-1	.
Chad	.	.	.	.	.	.	.	.	.	.	.	.	-1	.	-1	.	.	.	.	-1	.	.	.	.	.	-1	.	.
Chile	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
China	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1
Colombia	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Comoros	.	.	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.
Congo, Rep.	.	.	.	.	.	.	.	.	1	1	1	1	1	1	1	1	.	1	1	1	1	1	1	1	1	1	1	1
Costa Rica	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Cote d'Ivoire	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	-1	-1	-1	1	1	1	1	-1	-1	-1	1	-1	1

Cyprus	.	.	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Czech Republic	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	-1	-1	1	1	1	1	-1	-1	-1
Denmark	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1
Djibouti	.	.	.	.	.	.	.	.	.	-1	.	.	-1	-1	.	-1	-1	.	.	.	.	.	-1	-1	-1	-1	-1	.
Dominican Republic	-1	.	.	.	.	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1
Ecuador	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1	1	1	1	1
El Salvador	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Equatorial Guinea	.	.	-1	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	1	.	1	1	1
Ethiopia	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	.	-1	-1	-1	-1	-1
Fiji	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1	-1	1	-1	.	-1
Finland	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
France	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Gabon	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	.	1	1
Gambia, The	.	.	.	.	.	-1	.	-1	-1	-1	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	-1	-1	.	.
Germany	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Ghana	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Greece	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Grenada	.	.	.	.	.	-1	.	.	.	-1	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	.
Guatemala	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Guinea	.	-1	.	.	.	.	-1	-1	.	.	.	-1	.	.	.	-1	.	-1	.	.	.	.	.	-1	-1	-1	-1	1
Guinea-Bissau	.	-1	-1	.	-1	-1	.	-1	-1	-1	.	-1	.	.	-1	-1	-1	.	-1	.	-1	-1	-1	.	.	-1	-1	.
Guyana	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	.	-1	.	.	.	.	.	-1	.	-1	.	.	-1	.
Haiti	.	.	.	.	.	.	.	.	.	.	.	.	-1	-1	.	-1	.	.	.	-1	.	-1	.	.	.	-1	-1	.
Honduras	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Hungary	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1	-1	-1	-1	1	-1	-1	-1
Iceland	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	-1	.	.	-1	-1	-1	-1	-1	-1	-1
India	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1
Indonesia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Iraq	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Ireland	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Israel	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Italy	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Jamaica	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Japan	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Jordan	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Kenya	-1	-1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	1	-1	-1	-1	-1

Kiribati	.	.	-1	.	.	-1	-1	.	.	.	-1	-1	.	.	.	.	.	.	.	.	.	.	.	1	1	.	.	.
Kuwait	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	1	1	1	1
Lao PDR	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	.	-1	-1	.	-1	-1	.	-1	1	-1
Lebanon	-1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Liberia	.	.	-1	-1	-1	-1	.	.	.	.	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Libya	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Madagascar	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	.	-1	-1	.	-1	-1	-1	-1	-1
Malawi	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	.	.	.	.	.	-1	-1	-1	-1
Malaysia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Maldives	.	.	.	.	.	.	.	.	.	.	-1	-1	.	.	.	.	.	.	.	.	-1	.	.	-1	.	.	.	.
Mali	-1	-1	-1	1	1	1	1	-1	-1	.	.	-1	.	.	.	.	.	.	-1	-1	.	.	.	.	.	-1	-1	-1
Malta	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1
Mauritania	.	.	-1	-1	.	-1	-1	.	-1	.	-1	1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1
Mauritius	.	.	-1	-1	.	-1	-1	-1	-1	-1	-1	.	.	-1	.	.	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	.	-1
Mexico	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Mongolia	.	.	.	.	.	.	.	.	1	1	.	.	.	.	.	.	.	.	-1	-1	.	.	.	-1	.	1	-1	.
Morocco	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Mozambique	-1	1	1	-1	-1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1
Myanmar	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Nepal	.	-1	.	.	.	.	.	.	.	.	.	.	-1	.	.	.	-1	-1	.	.	.	.	.	.	-1	.	-1	.
Netherlands	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	-1	1	1	-1	1	1	1	1	1	-1	-1
Netherlands Antilles	-1	1	1	1	1	1	-1	-1	-1	1	-1	1	1	1	1	1	1	1	1	1	-1	1	1	-1	1	1	-1	-1
New Zealand	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	1	1	-1	-1	.
Nicaragua	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Niger	.	.	.	.	.	.	.	.	.	-1	-1	-1	-1	-1	-1	-1	-1	1	.	1	-1	-1	.	1	.	1	1	1
Nigeria	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Norway	-1	-1	-1	-1	-1	1	1	-1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1	1	1	1	1	1
Oman	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	-1	1	1	1	1
Pakistan	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Panama	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Papua New Guinea	1	-1	-1	1	-1	-1	-1	1	1	-1	-1	1	-1	-1	-1	1	-1	-1	1	-1	-1	1	1	1	1	1	1	1
Paraguay	.	.	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	.	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Peru	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Philippines	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Poland	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1
Portugal	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1

Qatar	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Romania	1	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1	1	-1	-1	1	-1	-1	1	-1	-1	-1
Rwanda	.	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	-1	.	.	.	.	.	.	.	.	.
Saudi Arabia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Senegal	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	.
Seychelles	.	.	.	.	.	.	-1	-1	.	.	.	.	.	.	-1	.	.	-1	-1	-1	.	.	.	-1	-1	-1	1	-1
Sierra Leone	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	.	.	-1	-1	.	.	-1	-1
Singapore	1	1	1	1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	-1	1	1	-1	-1	-1
Somalia	.	.	-1	-1	.	.	.	-1	-1	-1	1	-1	-1	.	.	-1	-1	.	-1	-1	-1	.	.	-1	.	.	.	.
South Africa	1	1	-1	1	.	.	.	.	.	.	.	.	.	.	.	1	1	1	1	1	1	1	1	.	.	.	1	.
Spain	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Sri Lanka	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Sudan	-1	1	1	1	1	-1	-1	1	-1	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	.	-1	.	.	.	-1	.	.
Suriname	.	-1	.	.	-1	-1	-1	-1	.	-1	.	.	.	.	-1	.	.	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Sweden	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Switzerland	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Syrian Arab Republic	1	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1
Thailand	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Togo	-1	.	-1	-1	-1	-1	.	-1	-1	-1	-1	-1	-1	-1	.	-1	-1	-1	.	.	-1	-1	-1	-1	.	-1	-1	-1
Trinidad and Tobago	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Tunisia	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	-1	-1	-1	-1
Turkey	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Uganda	.	.	.	.	.	.	-1	-1	-1	-1	.	-1	.	.	-1	.	-1	.	.	.	.	.	-1	.	.	-1	-1	.
United Arab Emirates	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1
United Kingdom	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1	1	1	1
United States	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Uruguay	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1
Venezuela	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	-1	1	1	1	1	1
Vietnam	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	-1	-1	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1
Yemen, Rep.	.	.	.	.	.	.	.	.	.	.	-1	-1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Zambia	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	-1	1	1	1	.	.	.	-1	-1	.	-1	-1	-1	-1	-1
Zimbabwe	1	1	1	1	1	1	1	1	-1	.	.	.	.	-1	-1	-1	-1	1	1	1	-1	-1	.	-1	-1	-1	1	-1

Appendix Table 2: Clustered OLS results

Dependent Variable: **Real manufacturing trade between country of origin and country of destination**

Method of estimation Clustered OLS	Energy P exposure	Net energy exports	Net energy exports (IV)
	(A2.1)	(A2.2)	(A2.3)
Log real GDP of origin	1.25 (0.01)***	1.23 (0.01)***	1.26 (0.01)***
Log real GDP of destination	0.73 (0.01)***	0.71 (0.01)***	0.74 (0.01)***
Log of distance	-1.14 (0.03)***	-1.12 (0.03)***	-1.17 (0.03)***
1 (Common Language)	0.63 (0.06)***	0.59 (0.06)***	0.65 (0.06)***
1 (Common Border)	0.25 (0.13)*	0.21 (0.13)	0.24 (0.13)*
1 (Regional Trade Agreement)	0.84 (0.09)***	0.89 (0.09)***	0.81 (0.10)***
1 (Common colonizer)	0.97 (0.09)***	0.96 (0.09)***	0.97 (0.10)***
1 (Colonial relationship)	1.41 (0.15)***	1.39 (0.15)***	1.50 (0.15)***
1 (Same nation in the sample)	1.84 (0.56)***	1.77 (0.57)***	1.89 (0.54)***
# of land-locked countries in pair	-0.22 (0.04)***	-0.19 (0.04)***	-0.27 (0.04)***
Energy price exposure (origin)	-1.77 (0.19)***	--	--
Energy price expos. (destination)	0.76 (0.17)***	--	--
Net energy exports (origin)	--	-0.04 (0.01)***	-0.11 (0.01)***
Net energy exports (destination)	--	0.11 (0.00)***	0.05 (0.01)***
Constant	-36.52 (0.43)***	-35.74 (0.43)***	-36.90 (0.46)***
Time dummies	p(F)=.00	p(F)=.00	p(F)=.00
Observations	135,947	135,947	135,947
# of clusters	9,651	9,651	9,651
R-squared	0.64	0.65	0.64

*S.e. in parentheses * sign. at 10%; ** sign. at 5%; *** sign. at 1%*
The p-value of a joint significance test is reported for time dummies.
Observations are clustered on country pairs.