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Foreign Demand and Greenhouse Gas Emissions: Empirical Evidence with Implications for Leakage

Geoffrey Barrows* and Hélène Ollivier†

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Abstract

With asymmetric climate policies, regulation in one country can be undercut by emissions growth in another. Previous research finds evidence that regulation erodes the competitiveness of domestic firms and leads to higher imports, but increased imports need not imply increased emissions if domestic sales are jointly determined with export sales or if emission intensity of manufacturing adjusts endogenously to foreign demand. In this paper, we estimate for the first time how production and emissions of manufacturing firms in one country respond to foreign demand shocks in trading partner markets. Using a panel of large Indian manufacturers and an instrumental variable strategy, we find that foreign demand growth leads to higher exports, domestic sales, production, and CO_2 emissions, and slightly lower emission intensity. The results imply that a representative exporter facing the average observed foreign demand growth over the period 1995-2011 would have increased CO_2 emissions by 1.39% annually as a result of foreign demand growth, which translates into 6.69% total increase in CO_2 emissions from Indian manufacturing over the period. Breaking down emission intensity reduction into component channels, we find some evidence of product-mix effects, but fail to reject the null of no change in technology. Back of the envelope calculations indicate that environmental regulation that doubles energy prices world-wide (except in India) would only increase CO_2 emissions from India by 1.5%. Thus, while leakage fears are legitimate, the magnitude appears fairly small in the context of India.

Keywords: leakage; trade and environment; product mix; technological change

JEL codes: F14; F18; Q56

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1 Introduction

Global climate change represents a serious threat to human welfare, yet governments have been remarkably slow in their efforts to curb Greenhouse Gas (GHG) emissions.[1] A central reason for the lack of GHG regulation is that emission reductions are a public good that benefit everyone, while the costs of regulation are born mostly by firms and residents in countries that impose the regulation. Lacking a comprehensive global commitment to regulate GHGs, individual countries fear that unilateral efforts will erode competitiveness relative to other nations and thus shift polluting activities to unregulated places. If this "leakage" effect – or Pollution Haven Effect (PHE), as it is known in the academic literature – is strong, then not only do firms in the regulated country lose competitiveness, but the regulations may fail even to achieve their primary goal of lowering emissions.

While the argument that stricter environmental regulation in one country causes emissions elsewhere to increase seems compelling intuitively, we have little ex-post empirical evidence to support it.[2] To date, the empirical literature has focused mainly on the relationship between environmental regulation and imports (Ederington & Minier, 2003; Ederington et al., 2005; Levinson & Taylor, 2008; Branger et al., 2016; Aichele & Felbermayr, 2015) or FDI/plant entry (Eskeland & Harrison, 2003; Kellenberg, 2009; Hanna, 2010). But mere changes in imports or plant location may not be sufficient to measure leakage because (1) domestic and foreign sales may be jointly determined, which means that production need not scale 1-for-1 with exports (Berman et al., 2015); and (2) increased foreign demand may incentivize firms to invest in new technologies (Cui et al., 2015; Cherniwchan, 2017; Gutiérrez & Teshima, 2018) and/or adjust their product mix (Mayer et al., 2014, 2016; Barrows & Ollivier, 2018), which can both generate changes in emission intensity. Thus, even if there is increased exports in one country as a result of regulation in another, the overall impact on GHG emissions remains an open empirical question.

In this paper, we take as given that environmental regulation erodes the competitive-

[1] Regional regulatory markets have been established in many places in the world, including the EU, California, the US East coast, South Korea, Australia, New Zealand, British Columbia, and Tokyo, among others; but even for these programs, the cost of carbon remains very low, thereby limiting the extent of GHG emission reductions.

[2] There is a large literature that estimates leakage ex-ante using computable general equilibrium models (see Carbone & Rivers (2017) for a review). Also, several recent papers have explored the potential for leakage from sub-national policies using simulation-based modeling. For example, Fowlie (2009), Bushnell & Chen (2012), Bushnell et al. (2014), and Caron et al. (2015) explore leakage possibilities across states due to California's recently enacted carbon cap-and-trade system, with a focus on the electricity sector. Fell & Maniloff (2018) study the ex-post impacts of a CO_2 cap-and-trade program in select Northeastern US states on emissions in other unregulated states in the electricity sector.

ness of domestic firms – as previous research strongly indicates (Dechezleprêtre & Sato, 2017; Aichele & Felbermayr, 2015; Levinson & Taylor, 2008) – and investigate for the first time in the literature what this change in competitiveness means for CO_2 emissions from manufacturing in another country. To address this question, we estimate elasticities of exports, output, emissions, and emission intensity at the firm and firm-product level in a developing country – India – to weighted average foreign import demand shocks in trading partner destinations. Environmental regulation is one of many determinants of import demand, but conditional on the identifying variation being orthogonal to unobserved determinants of production and emissions in India, the product of the elasticity of emissions with respect to foreign demand and the elasticity of foreign demand with respect to environmental regulation delivers an estimate of leakage.

India offers two key advantages as an empirical context. First, Indian firms report detailed information on output and energy inputs, from which we can compute CO_2 emissions per physical quantities of output at the firm and product-line level. We compute CO_2 emissions by multiplying self-reported energy usage statistics from the firms by CO_2 content emission factors of different energy types, following a strategy from previous research (Martin, 2012; Marin & Vona, 2017; Forslid et al., 2018; Barrows & Ollivier, 2018). Second, India is a major emitter of CO_2 emissions and the fastest growing emitter since 1980 of any large country. Hence, understanding the determinants of emissions growth in India in particular are important for forecasting global CO_2 emissions and designing effective policy.

We relate changes in exports, production, and CO_2 emissions in a sample of roughly 3000 large Indian manufacturing firms over the period 1996–2011 to changes in weighted average foreign demand shocks of the products they produce. The strategy follows recent work that takes changes in imports from countries other than the country studied as proxies for destination-product specific taste or income shocks (Hummels et al., 2014; Mayer et al., 2014, 2016). The identification assumption is that year-to-year import demand growth in India's destination markets is orthogonal to unobserved determinants of firm-level manufacturing outcomes in India. To probe this assumption, we present placebo tests on outcomes of non-exporting firms and test for differential trends in outcomes between 1990–1995 for firms that will see larger vs smaller foreign import demand growth over the period 1996–2011.

Instrumenting year-to-year fluctuations in foreign demand between 1996 and 2011 with base-year-weighted average foreign demand, we find in our sample that 10% higher firm-level weighted-average foreign demand leads to 5.72% higher export sales and 1.27% higher domestic sales for exporters. Both results are statistically significant at the 1% level. We also estimate that 10% higher firm-level weighted-average foreign demand leads to 2.14% higher production (in physical quantities), and 1.64% higher CO_2 emissions, for exporting firms, statistically significant at the 1% level. In all cases, we fail to reject no corresponding impact on non-exporting firms, which suggests that product-specific technological trends do not drive the results. Further placebo checks confirm that outcome trends between 1990–1995 do not correlate with future demand shocks, which supports the parallel trend assumption. The rise in domestic sales in response to higher foreign demand suggests complementarities in production between foreign and domestic sales, as in Berman et al. (2015), and the fact that production increases more than emissions means that average emission intensity endogenously responds to foreign demand. Both points imply that studying foreign import flows alone is insufficient to address the leakage question.

To put the magnitudes in context, our results imply (using partial equilibrium computations) that total CO_2 emissions from manufacturing increased by 6.69% over the period 1995–2011 as a result of observed foreign demand growth, which accounts for 4.58% of the total observed growth in CO_2 emissions from manufacturing in India. By contrast, ignoring the endogenous fall in emission intensity leads one to overestimate this increase by 30%, and ignoring both emission intensity effects and complementarities between domestic and foreign sales leads one to underestimate total CO_2 impacts by 58%. Multiplying the CO_2 emissions trade elasticity by an elasticity of foreign import demand to energy prices implies that doubling energy prices everywhere in the world except India would increase CO_2 emissions from Indian manufacturing by only 1.5%, or about 6.8 Megatons annually.

We next test for individual channels of emission intensity adjustment. We separate changes in firm emission intensity into an across-product component and a within-product component over time. The across-product component results from systematic shifts in output shares across products with heterogeneous emission intensities of production. Mayer et al. (2014, 2016) show how competition can systematically alter the product mix of firms, leading to changes in firm-average productivity. Barrows & Ollivier (2018) extend Mayer et al. (2014, 2016) to multiple inputs (including energy) and likewise show how competition can alter product mix and firm-average emission intensity. The within-product component reflects technological efficiency gains. Models from Bustos (2011) and Cui et al. (2015) show how better market access can induce firms to invest in variable-cost-savings technology, which may lower product-level emission intensity over time. Changes in managerial practices, fuel source, or quality of inputs could also contribute to within-product effects (Cherniwchan, 2017; Gutiérrez & Teshima, 2018), though we refer to the sum of all these effects broadly as "technology."

To estimate the different channels, we employ two strategies. First, we estimate impacts at the firm level separately for single-product and multi-product firms. By conditioning on single-product firms, we rule out product-mix effects by construction. Hence, any remaining emission intensity changes can be attributed to technological change. In this sample, we fail to reject the null of no impact. By contrast, we find some evidence at the firm level that emission intensity falls for multi-product firms, and that both product skewness and product offerings increase with foreign demand. These results are consistent with previous work on multi-product firms (Mayer et al., 2014, 2016), and imply that product mix may explain the firm-level reductions in emission intensity.

Second, we estimate impacts on emission intensity at the firm-product level. With emissions computed at the product-line level, we need not condition on single product firms. In this sample, we again fail to reject the null of no change in emission intensity, which again implies that technological adoption does not drive the emission reductions. A potential concern is that firms do not adjust technology quickly enough to respond to year-to-year demand variation, but robustness checks reveal similar results in a long difference specification, which suggests that the baseline result is not merely an artifact of the time scale. As in Barrows & Ollivier (2018), these result caution against interpreting firm-level reductions in emission intensity as "technological upgrading", pointing rather to changes in allocations across product lines of heterogeneous emission intensity as the driver of within firm changes over time.

Our work is closely related to the literature on the carbon content of trade (see for example Aichele & Felbermayr (2015) and Sato (2014)). This literature computes emissions as the product of industry-specific carbon intensities in different countries and trade volumes and then estimates responses to environmental policy. A limitation of this approach is that exporters tend to be cleaner than non-exporters (Holladay, 2016), so applying industry average intensity (based on the pooled sample of exporters and non-exporters) overstates the carbon content of trade. Additionally, the carbon content approach computes intensity in value, not physical quantities, and ignores impacts on domestic production. By examining firm-level production data, we allow for heterogeneity in emission intensity and explicitly estimate endogenous response to domestic sales.

Beyond the leakage literature, our paper also contributes to literatures on the impacts of trade on emissions either at the firm level (Martin, 2012; Cherniwchan, 2017; Gutiérrez & Teshima, 2018) or at the regional and national levels (Antweiler et al., 2001; Frankel & Rose, 2005; Bombardini & Li, 2016). Most of these papers estimate import competition impacts via trade liberalization, or aggregate effects from trade openness (i.e., imports plus exports divided by GDP). We instead isolate the export demand side impacts to speak most directly to the leakage question. Two recent papers study the emission response from US manufacturing to changes in domestic and foreign regulation and competitiveness. Cherniwchan (2017) finds that SO_2 and $PM_{2.5}$ emissions levels from US manufacturing decline following tariff reductions on US goods entering Mexico. By contrast Shapiro & Walker (2018) find that changes in foreign competitiveness (which include environmental regulations abroad) had little effect on the reduction in criteria air pollutant emissions from US manufacturing over the 1990s and 2000s. A key difference between our study and both Cherniwchan (2017) and Shapiro & Walker (2018) is that we study CO_2 emissions in a developing country, where the concern for leakage is the greatest. Closer to our setting, Bombardini & Li (2016) studies how regional average export tariffs affect SO_2 and $PM_{2.5}$ concentrations in China. In contrast with all these papers, we are able to condition on the export status of the firm and compute emissions per physical unit of production.

Finally, in studying the underlying mechanisms of emission intensity reductions, we also relate to the large literature on the determinants of firm-level productivity. This literature mostly estimates the responsiveness of innovation or Hicks-neutral total factor productivity measures to various changes in trade, competitiveness and market conditions (Bernard et al., 2011; Lileeva & Trefler, 2010; Bustos, 2011; Bloom et al., 2016; De Loecker et al., 2016; Mayer et al., 2014, 2016). The literature hypothesizes that both technology adoption and product mix contribute to firm-level changes in productivity. Our estimates of emission intensity at the firm-product level represent the only product-level estimates of efficiency that we are aware of in the literature, and allow us to provide a direct test of the technological channel. Our results suggest like De Loecker et al. (2016); Bernard et al. (2011); Mayer et al. (2014, 2016) that product mix matters for efficiency.

2 Background and Data

In this section, we present the empirical context, the firm-level production data, and the international trade data from which we compute firm-level shocks.

2.1 Background

In 2016, India was the third largest emitter of CO_2 emissions (7% of world emissions), behind China (29% of world emissions) and the US (14% of world emissions). Over the period since 1980, India was also the fastest growing emitter of CO_2 among large emitters (initial share greater than 1% of world emissions), with an increase of 689% in total.[3] A large part of this growth was due to rapid expansion in manufacturing output. Following trade liberalization and other market reforms in the early 1990s, real output from manufacturing grew 313% between 1995 and 2011 (see Goldberg et al. (2010a) for a discussion of these reforms). Throughout most of the period, CO_2 emissions were completely unregulated in India. State governments made some efforts to regulate criteria air pollutants like $PM_{2.5}$ and NO_x, though regulation appears to have had limited effect (Greenstone & Hanna, 2014).[4]

Over the same period, exports from India also grew substantially, especially to developed countries. Between 1995 and 2011, real value of exports grew 658%. Exports to the US grew by a factor of 5, with similar increases in Belgium-Luxembourg, South Korea, Hong Kong, and Singapore. Among destinations accounting for at least 1% of Indian exports in 1995, 7 of the top 10 growth rates occurred in countries considered "high income" by the World Bank. By 2011, the United Arab Emirates accounted for the largest share of Indian exports (12.0%), with the US (11.9%), China (6.6%), Singapore (5.4%) and Hong Kong (3.7%) rounding out the top 5 destination markets. With much of the growth in exports occurring with developed countries, the case of India represents a good opportunity to study the potential of environmental regulation in rich countries to stimulate CO_2 emissions in a poor, unregulated country.

2.2 Manufacturing Data

Our manufacturing data comes from the Prowess dataset, maintained by the Center for Monitoring the Indian Economy (CMIE). This dataset is based on annual reports filed publicly by large Indian manufacturers, which CMIE collects and digitizes. Registered Indian firms are required to issue these reports annually as part of the Indian Companies Act of 1956. Not all firms file reports every year and not all reports are readily available,

[3] Data come from the International Energy Agency.

[4] More recently, in 2010, the government introduced a nationwide carbon tax of 50 rupees per tonne on coal, which has since increased to 450 rupees per tonne; though with our period of analysis ending in 2011, the tax is unlikely to have influenced the firms in our sample. We nevertheless include firm specific energy prices as controls in our regressions, thus potentially capturing local regulation effects.

but the sample in fact covers a very high share of output from the formal sector (around 80%) starting around the mid 1990s (De Loecker et al., 2016; Goldberg et al., 2010b).

In the annual reports, firms give detailed accounts of both inputs and outputs. On the output side, firms report both value and quantity of sales by product line along with the units of production. Firms list outputs by product names, which CMIE then assigns a standardized product classification code of their own design. Previous work has treated the CMIE product codes as unique identifiers of product offerings within the firm (De Loecker et al., 2016; Goldberg et al., 2010b). However, we found upon inspection that in many cases, CMIE assigns different product codes to products with identical or nearly identical text descriptions. This is particularly problematic when it happens within the same firm. Additionally, in many cases firms report multiple product offerings within the same CMIE product code. Hence, while we make use of the product classification for matching to trade shocks, we treat the product text description supplied by the firm as the individuating identifier of products (see appendix for details). Prowess also reports the share of revenue earned from exports, which will be useful for tracing demand shocks through export behavior to production, as well as identifying non-exporting firms to use as placebo checks.

On the input side, firms report most standard variables such as labor use, capital and material inputs in value each year. Firms do not directly report environmental emissions; but, they report detailed information about energy use.[5] In particular, firms report annual expenditure and consumption (with units) of different energy sources – coal, electricity, fuel, wood, etc. Additionally, due to an unusual reporting requirement, firms also report energy intensity of production (in units) *by product line*. This reporting requirement was formalized in the 1988 amendment to the Companies Act, presumably due to government interests in energy security. As a result, for many firms in the dataset, we observe annual energy intensity of production by product line. This allows us to track technological progress at the product-line level, which, to our knowledge, is not possible in any other dataset.

To compute CO_2 emissions, we follow previous work in multiplying energy consumption by source-specific CO_2 emissions factors (Martin, 2012; Marin & Vona, 2017; Forslid et al., 2018; Barrows & Ollivier, 2018). The underlying assumption behind this strategy is that a given source of energy (eg, coal, fuel, wood) has a fixed carbon content, and that burning the energy source releases that carbon content regardless of the technology used to burn it.

[5] Additionally, the location of production is not reported in Prowess. Hence, it is impossible to match production to ambient pollution levels.

This assumption seems reasonable for the case of CO_2 in India, where end-of-pipe carbon capture technology is not widely used. By contrast, one would not want to make the same assumption with respect to criteria pollutants such as NO_2 or $PM_{2.5}$, for example, for which the emission content can vary significantly with the technology used.

With the two different energy reports, we construct two different datasets. First, we construct firm-level CO_2 emissions by multiplying quantities of individual energy sources by CO_2 emissions factors from the US EPA and then summing over energy sources. By summing outputs over product line and merging, we then compute firm-level output, emissions, and emission intensity. We refer to this first dataset as the "firm-level" dataset because it is based on firm-level energy reports. Second, we compute product-level emission intensity by multiplying product-level energy intensity by the same CO_2 emissions factors from above and summing over energy types. We then merge these emission intensities to the product level output information. We refer to this second dataset as the "product-level" dataset. Whereas the firm-level dataset is useful for estimating impacts on total output, emissions, and emission intensity, the product-level dataset is helpful for decomposing average emission intensity changes into technology (within firm-product over time) vs product mix (changes in output shares of products with heterogeneous emission intensity). See the appendix for details on the data construction and discussion of the product-level energy intensity data.

After merging input and output variables and cleaning for outliers, we have 3,217 firms in the firm-level dataset. We report descriptive statistics in Table 1 by industry. Industry classifications are based on the CMIE product classification codes, but these map fairly well to the more broadly used National Industrial Codes, at the aggregate level at least. The data span the years 1989–2011, with coverage increasing through the early part of the 1990s. In Table 1, we find fairly broad coverage across the entire manufacturing sector. The average firm in the firm-level dataset generates 0.73 billion rupees in sales per year, or about 16 million USD. The average firm also earns 9.3% of revenue from exports and produces just under 2 different products per year.

In columns 5-7 of Table 1, we report descriptive statistics for the product-level dataset. Here, we count fewer firms overall – 2,121 firms in total. There are two reasons that this figure is smaller. First, not all firms report product-specific energy intensities. Second, merging product-line inputs to product-line outputs is a complicated process and is not possible in all cases, even when both data reports existed (see the appendix for details). In the product-level dataset, the average firm generates 0.83 billion rupees in sales on 1.59

Table 1: Firms by Industry

	Firm-Level Data				Product-Level Data		
		Average Values				Average Values	
Industry	# Firms (1)	Sales (2)	Export Share (3)	# Products (4)	#Firms (5)	Sales (6)	# Products (7)
Food products, beverages & tobacco	269	0.78	10.0	2.35	200	0.90	1.63
Textiles	685	0.50	11.7	1.56	620	0.55	1.45
Wood, Pulp & Paper Products	201	0.64	1.7	1.30	155	0.72	1.31
Chemicals	621	0.67	14.2	2.49	341	0.56	2.15
Plastics & Rubbers	210	0.70	9.3	1.73	120	0.58	1.61
Non metallic mineral products	182	1.11	4.5	1.33	148	1.53	1.42
Base Metals	719	0.85	4.9	1.96	417	1.15	1.54
Machinery	207	0.74	12.2	2.19	72	0.50	1.28
Transport equipment	123	1.01	10.4	2.06	48	2.62	1.40
Total	3217	0.73	9.3	1.94	2121	0.83	1.59

Notes: Table reports total number of firms by industry along with industry average values in firm-level dataset (columns 1-4) and product-level dataset (5-7). Firms are assigned to an industry based on the product that accounts for the greatest aggregate sales over the entire period (1989-2011). Sales are reported in billions of current year rupees.

products per year.

Table 2 presents descriptive statistics by export status for both datasets. Columns 1 and 2 report means and standard deviations of variables for firms that ever export over the whole period, while columns 3 and 4 report the same for firms that never export. In column 5, we test for difference in means between the two groups and reports the result of a two-sided t-test of the null of no difference. In panel A, we find that exporters in the firm-level dataset generate more sales, more emissions, produce more products, but also have lower emission intensity in value. Analogous results have been noted many times before in the trade literature: exporters are bigger and more productive than non-exporters. We also find that exporters produce more in terms of quantity and have lower emission intensity in quantity, but since units of quantity vary across firms, these numbers are nonsensical. In the regressions, we will condition on common units within the firm over time. Here, we merely report the aggregate statistics for completeness. The results for sales, emissions, and emission intensity in value also hold for the product-level dataset, though outliers appear to be more of an issue in the product-level data.

Panel A of Table 2 also reports firm-specific energy prices and the Thiel index for product output skewness. The energy prices are computed by dividing energy expenditures by energy consumption quantities for each energy source (electricity, coal, diesel). We will control for these firm-level prices in the regressions. These control variables capture at least to some degree differences across space in energy supply and environmental regulation. The number of products and the Thiel index will be used to measure product-mix effects, following Mayer et al. (2014, 2016). The Thiel index is computed for firm i in year t as

$$T_{it} = \frac{1}{N_{ijt}} \sum_{j \in \Delta_{it}} \frac{x_{ijt}}{\bar{x}_{it}} \log \left(\frac{x_{ijt}}{\bar{x}_{it}} \right) \tag{1}$$

with Δ_{it} the set of products produced by firm i in year t, x_{ijt} the sales of product j in firm i in year t, N_{ijt} the number of products produced, and $\bar{x}_{it}$ the average revenue of a product sold by firm i in year t. Higher values of T_{it} indicate greater skewness in the product mix.

2.3 Trade Data

We take international trade flows from CEPII's BACI dataset, which is a refinement of UN COMTRADE data. The data reports values of bilateral trade flows at the 6-digit Harmonized System (HS) product classification level from 1995 until 2011. For each product code, we compute a weighted average foreign demand shock faced by Indian firms, and

Table 2: Descriptive Statistics

	Exporters		Non-exporters		Difference
	Mean	Sd	Mean	Sd	
	(1)	(2)	(3)	(4)	(5)
Panel A : Firm-Level Data					
Sales Value Total (bill of rs)	1.096	(2.042)	0.401	(0.810)	0.695 ***
Sales Value Domestic (bill of rs)	0.961	(1.961)	0.401	(0.810)	0.560 ***
Sales Value Exports (bill of rs)	0.135	(0.284)	-		-
Production (various units)	123.7	(1184)	25.8	(434.6)	-
Emissions (kt CO_2)	55.99	(218.6)	34.1	(149.1)	21.94 ***
E/V (t/mill rs)	59.53	(473.9)	108.3	(538.4)	-48.79 ***
E/Q (t/unit)	341.2	(980.8)	209.2	(718.7)	-
# Products	2.116	(1.756)	1.733	(1.190)	0.383 ***
Thiel Index	0.955	(2.094)	0.734	(1.505)	0.221 ***
Elec Price (rs/kwh)	3.934	(0.890)	4.011	(0.883)	-0.077 ***
Coal Price (rs/kt)	0.009	(0.089)	0.008	(0.017)	0.000
Deisel Price (rs/Mls)	0.041	(0.152)	0.052	(0.476)	-0.011 **
# Firms	1759		1458		
# Firms Years	13700		7154		
Panel B : Product-Level Data					
Sales Value Total (bill of rs)	1.449	(5.398)	0.397	(0.831)	1.052 *
Production (various units)	12.08	(58.86)	6.489	(43.76)	-
Emissions (kt CO_2)	96.95	(385.4)	38.57	(204.6)	58.38
E/Q (t/unit)	220.6	(335.0)	153.6	(223.1)	-
E/V (t/mill rs)	770.1	(16691)	2642	(125067)	-1872.2 ***
# Firms	1327		794		
# Firms-products	2382		983		
# Firms-product Years	20338		4869		

Notes: Table reports firm-level (A) or product-level (B) descriptive statistics by export status. Exporters are firms that ever export positive quantity at any time over the period. Data covers 1989-2011. Column 5 reports difference in mean as well as statistical significance for t-test. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels. For most variable, top/bottom 1% of values have been removed.

then merge these shocks to Prowess via the CMIE product classification code.

Formally, let X_{odjt} be the value of bilateral trade between origin o and destination d in 6-digit HS code j in year t. We compute import demand shock in destination d as

$$X_{djt} = \sum_{o \in \Delta_o} X_{odjt} \tag{2}$$

where Δ_o is the set of exporting countries to d excluding India. Define s_{djt} as the share of exports that flow to destination d in the total exports of j from India in year t, $s_{djt} \equiv X_{djt} / \sum_{d \in \Delta_d} X_{djt}$. Then we compute weighted average demand shocks

$$\widetilde{FD}_{jt} = \sum_{d \in \Delta_d} s_{djt} X_{djt} \tag{3}$$

with Δ_d the set of all destinations India exports j to in year t. These are similar to the shocks computed by Mayer et al. (2014, 2016) to study the relationship between foreign competition, product mix, and productivity, and are meant to capture demand-side shifts in preference or income.

Two important points about identification bear mention. First, by leaving India's own exports out of equation (2), we have attempted to purge the equilibrium values X_{djt} of supply side effects that might jointly affect Indian exports and production. However, it is possible that product-specific technology changes affect both Indian and non-Indian producers similarly. In this case, equilibrium sales X_{djt} may still correlate with unobservable supply-side determinants of Indian production decisions. To address this possibility, we rely on placebo tests for non-exporters. We will return to this point below in the empirical strategy.

Second, even if X_{djt} is exogenous to trends in production on the supply side, the export shares s_{djt} from year to year may adjust endogenously to X_{djt}. To address this endogeneity concern, we compute an instrument for $\widetilde{FD}_{jt}$ using base-period Indian export weights:

$$\widetilde{Z}_{jt} = \sum_{d \in \Delta_d} s_{dj0} X_{djt} \tag{4}$$

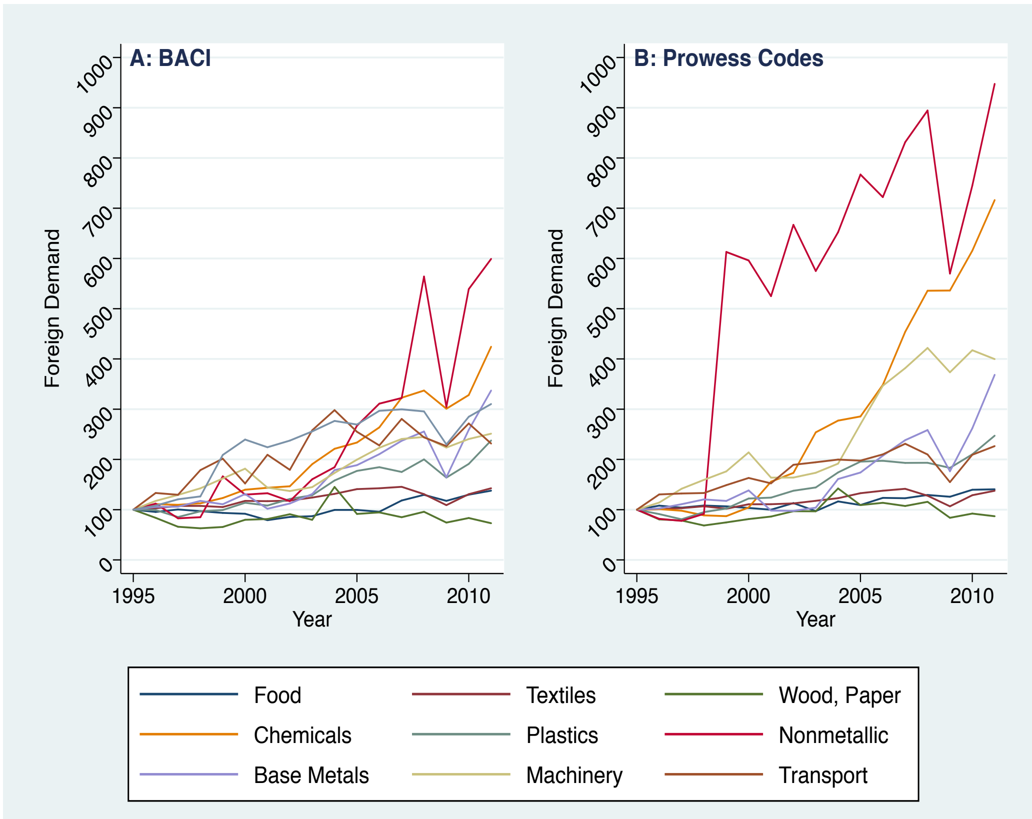
where the average values over 1995–1997 serve as the base-year weights for the beginning of the sample (1995–2004), and averages over 2002–2004 serve as base-year weights for the end of the sample (2005–2011). The reason to change the weights for the latter period is that trade patterns changed a lot over the period, and so export shares from 1995–1997 may not be very informative for Indian firms later in the sample. We take the split-sample weighting scheme as our baseline, though results are not substantially different if we leave weights fixed at 1995–1997 values throughout. With weights fixed to base-period, endogenous sorting across markets should not influence $\widetilde{Z}_{jt}$.

In Figure 1, panel A plots average $\widetilde{FD}_{jt}$ by industry-year. All values are deflated to the year 2000 and indexed to 1995. In panel A, we see that foreign demand increased dramatically from the vantage point of Indian firms over the period. The average $\widetilde{FD}_{jt}$ index value reached 275 by 2011, or almost three times higher than 1995 levels. We also see substantial variation across industries. Foreign demand for wood, paper, and printing was practically flat throughout the whole period, while demand for chemicals nearly quadrupled. The industry that saw the largest increase was nonmetallic minerals, which increased more than 6-fold. This growth was mostly driven by demand for concrete. Results below are robust to excluding this outlier.

Next, we map these product-code-level foreign demand shocks to manufacturing firms in Prowess via the CMIE product classification code system. CMIE classifies product names (reported by the firms) according to a 16-digit code they designed themselves. CMIE provides a cross-walk between their 16-digit codes and the more-commonly used National Industrial Codes (NIC), which can then be related to the HS codes via the cross-walk from Debroy & Santhanam (1993) (see De Loecker et al. (2016) for an example). However, the cross-walk from Debroy & Santhanam (1993) is fairly aggregated and relies on the version of the NIC from the early 1980s. Mapping between Prowess codes and HS codes via this cross-walk is probably sufficient when identifying variation is fairly constant across products within an industry, but we hope to exploit differential growth rates across products within an industry. As a result, we constructed our own cross-walk between the CMIE product codes and HS revision 1996 (see appendix for details). Industry-average indices are plotted in panel B of Figure 1. As in panel A, average foreign demand growth is significant, substantial variation exists across industries, and nonmetallic minerals remains an outlier.

Figure 2 presents descriptive statistics on annual growth rates in foreign demand after passing through the cross-walk. In a slight abuse of notation, we will continue to use j to index product codes in Prowess. There are 3,276 CMIE product codes in all to which we can assign foreign demand values. The left panel in Figure 2 plots the cumulative distribution function of year-over-year percentage growth rates. We find that about 25% of annual foreign demand decline year-over-year. The median growth over the whole sample is 5.7%

Figure 1: Foreign Demand Over Time



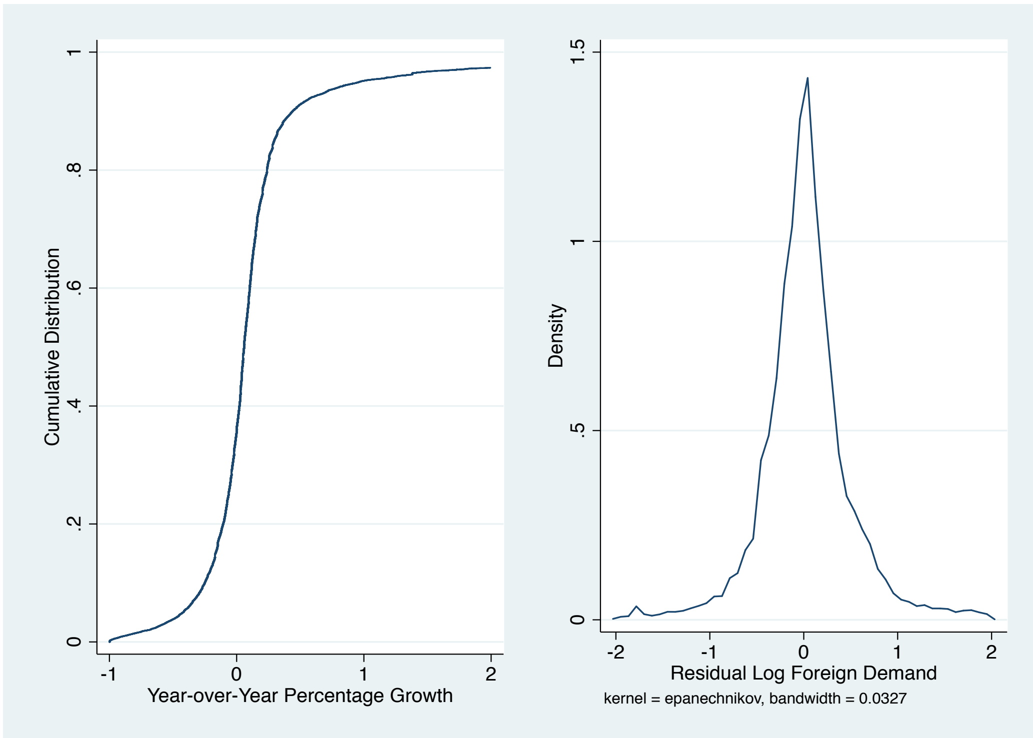
Notes: Figure reports average weighted average foreign demand ($\widetilde{FD}_{jt}$) indices by industry where goods are classified by HS rev 1996 (A) and Prowess product codes (B).

(i.e. 0.057). About 5% of annual demands at least double, and a handful of growth rates reach enormous values (the right tale in Figure 2 left panel has been truncated at 200%). In the regressions, we take the logarithm of $\widetilde{FD}_{jt}$ to minimize the impact of outliers.

The right panel in Figure 2 presents the residual log weighted average foreign demand after stripping out product code and industry-by-year fixed effects. Here, the only variation remaining results from within product code changes over time, controlling for arbitrary industry-specific trends. This figure gives a sense of the variation that we exploit in the regressions below. The standard deviation of this distribution is 0.783, which means that the 90th percentile of residual demand is 7.4 times higher than residual foreign demand at the 10th percentile, assuming log residual demand is approximately normally distributed.[6] So

[6]The full calculation goes as follow: the 90th percentile of a normal distribution with mean 0 and

Figure 2: Foreign Demand Variation



Notes: Year-over-year and residual log foreign demand computed at product level (3,276 products) classified by Prowess product codes. Right panel plots residuals after regressing log $\widetilde{FD}_{jt}$ on product code fixed effects and industry-by-year fixed effects. Left panel truncates year-over-year growth at 200%.

not only is there substantial variation in growth rates across industries, there is substantial variation in growth within products over time, controlling for industry average growth.

3 Empirical Strategy

Our strategy is to estimate leakage by first estimating the elasticity of CO_2 emissions to foreign demand shocks in trading partner markets and then multiplying by an elasticity of foreign demand to environmental regulation. Taking import demand as the explanatory variable instead of environmental regulation circumvents the notoriously thorny problem of quantifying heterogeneous environmental regulations across countries and products (Sato

standard deviation is approximately 1. The 10th percentile is -1. Exponentiating, we have that the level of residual forewign demand at the 90th percentile is $exp(1) = 2.78$ and at the 10th percentile is $exp(-1) = 0.13$. Hence, the ratio of the 90th percentile level to the 10th percentile level is $2.78/0.13 = 7.4$

et al., 2015). Additionally, techniques from the trade literature deliver plausibly exogenous variation in foreign demand year-to-year, while environmental regulation tends to evolve slowly and potentially endogenously to other determinants of production outcomes.

Here, we describe how we link our measures of foreign demand $\widetilde{FD}_{jt}$ to firm level information in Prowess and estimate elasticities of exports, emissions, production, and emission intensity. We first describe our strategy for estimating impacts on firm-level average outcome variables. We then describe how to separate average impacts into a product-mix channel and a technology channel.

After passing demand shocks through our cross-walk between HS classification and CMIE product codes, we can relate foreign demand shocks to outcomes in the firm-level dataset by estimating

$$\text{Log } Y_{ikt} = \alpha_i + \alpha_{kt} + \beta * \text{Log } \widetilde{FD}_{it} + \gamma W_{it} + \epsilon_{ikt} \tag{5}$$

where Y_{ikt} is an outcome for firm i in industry k in year t, α_i is a firm fixed effect, and α_{kt} is an industry-by-year time shock that captures any common factor that affects all Indian firms in the same industry equally, such as labor regulations, income shocks, and general technological progress. Industries are defined as in Table 1. We associate each firm to a single industry based on the product code responsible for the largest share of sales for the firm over the whole period. The vector W_{it} represents firm-level controls for source-specific energy prices. Firm-level foreign demand shocks $\widetilde{FD}_{it}$ are computed as a weighted average of product-level shocks

$$\widetilde{FD}_{it} = \sum_{p \in \Delta_{it}} r_{ipjt} \widetilde{FD}_{jt} \tag{6}$$

where $r_{ipjt} \equiv \frac{X_{ipjt}}{\sum_{p \in \Delta_i} X_{ipjt}}$ the sales share of product p belonging to product classification j in firm i's total sales in year t, and Δ_{it} the set of products offered by firm i in year t. We distinguish between product p and product code j because sometimes firms list multiple product offerings that CMIE assigns to the same product code.[7]

Conditional on the assumption that firm-level demand shocks evolve exogenously to unobservable determinants of firm-level outcomes, OLS estimation yields an unbiased estimate of β in (5). However, given the endogeneity concern with $\widetilde{FD}_{jt}$, we would not expect

[7] For example, the firm KAREEMS SILK INTL. LTD. reports separate sales and production information for "Silk Tops" and "Silk Noils" in the same year, where CMIE codes both as 0601060000000000 "Silk fabrics, processed".

the assumption to hold. Additionally, the firm-level shares r_{ipjt} might also adjust endogenously to aggregate demand shocks. To control for endogenous shifts in Indian export shares as well as firm-level product shares, we construct

$$\widetilde{Z}_{it} = \sum_{p \in \Delta_{i0}} r_{ipj0} \widetilde{Z}_{jt} \tag{7}$$

where $\widetilde{Z}_{jt}$ is computed in (4), and $r_{ipj0} \equiv \frac{X_{ipj0}}{\sum_{p \in \Delta_{i0}} X_{ipj0}}$ the sales share of product p in firm i's total sales in base year $t = 0$, and Δ_{i0} is the set of products produced in base year $t = 0$. Practically, we take the first year of entry of each firm as the base year to define the product weights, and then exclude this year from the regressions. As in (4), by weighting product-specific demand shocks with base-year shares, endogenous reallocation should be limited in (7). We then instrument Log $\widetilde{FD}_{it}$ in (5) with Log $\widetilde{Z}_{it}$.

Having purged $\widetilde{FD}_{it}$ of endogenous share shifts with $\widetilde{Z}_{it}$, we can state the identification assumption as $E[\widetilde{Z}_{it} | \alpha_i, \alpha_{kt}, W_{it}, \epsilon_{ikt}] = 0$. The major outstanding challenges to this identification assumption revolve around product-specific technological trends. Though we control for industry-specific time trends, it is possible that some product-specific technological trends still drive both foreign demand and firm-level output and emissions. We address this concern in two ways. First we present placebo estimates for non-exporters, defined as firms that never export in any year throughout the sample. If there are product-specific time trends that affect both foreign demand and production, then foreign demand shocks should predict outcomes for non-exporting firms as well. Second, we test for differential outcome trends for firms prior to 1995 that will see smaller vs larger foreign demand shocks over the period 1995–2011. If firms operating in industries that happened to see large foreign demand shocks over the period 1995–2011 were on differential trends compared to firms that saw smaller demand shocks over the same period, then estimates of (5) would be biased. Additionally, we would expect to see differential growth trends by future foreign demand shocks in the pre-period (1990–1995).

We next estimate foreign demand impacts directly at the product level using the product-level dataset. Taking Y_{ipjkt} as output, emissions, or emission intensity generated by firm i to make product p in product category j in year t, we estimate

$$\text{Log } Y_{ipjkt} = \alpha_{ip} + \alpha_{kt} + \beta * \text{Log } \widetilde{FD}_{jt} + \gamma W_{it} + \epsilon_{ipjkt} \tag{8}$$

where α_{ip} is a firm-product fixed effect, α_{kt} represents industry-specific time-varying shocks, and W_{it} again represents firm-level controls for source-specific energy prices. Similarly to the firm-level regressions, we instrument Log $\widetilde{FD}_{jt}$ with Log $\widetilde{Z}_{jt}$. If firms adopt emission savings technology (Cui et al., 2015), then we should estimate $\beta < 0$ for emissions in (8).

Finally, we estimate separate impacts by country of origin of the foreign demand shocks. Considering the origin of the demand shock is important for two reasons. First, the leakage debate mainly centers on the erosion of competitiveness in rich countries due to relatively lax environmental regulations in less developed countries. Hence, to the extent that our results have implications for leakage, we want to check that foreign demand shocks specifically in rich countries induce changes in emissions in our developing-country firms. Second, changes to foreign demand may be more or less salient to Indian firms depending on market conditions of the destination (e.g., income), or there may be heterogeneous responses based on the destination market.[8] To separately identify effects based on destination markets, we compute destination-specific demand shocks for US/Canada, EU, and the rest of the world (ROW):

$$\widetilde{FD}^{l}_{jt} = \sum_{d \in \Delta_d} s_{djt} D^{l}_{djt} \tag{9}$$

for $l \in \{\text{US/Canada}, \text{EU}, \text{ROW}\}$. We then compute destination-specific instruments $\widetilde{Z}^{l}_{jt}$ in a similar fashion, and destination-specific firm-level foreign demand and instruments by replacing $\widetilde{FD}_{jt}$ and $\widetilde{Z}_{jt}$ with $\widetilde{FD}^{l}_{jt}$ and $\widetilde{Z}^{l}_{jt}$ in (6) and (7).

4 Results

In this section, we investigate how foreign demand affects the CO_2 emissions of individual Indian manufacturers. We first verify that foreign demand increases the export sales of Indian firms. We then trace these effects through to domestic sales, production, and finally CO_2 emissions. Lastly, we explore the channels of adjustment in emission intensity.

4.1 First Stage

We begin by verifying that $\widetilde{Z}_{it}$ meets the conditions of a valid instrument. Since $\widetilde{Z}_{it}$ utilizes base-year weights to weight both international trade flows and firm-level product-specific

[8]Multi-product models such as Mayer et al. (2014, 2016) allow for differential effects based on market conditions.

output shares, the exclusion restriction with respect to firm-level output and emissions should be satisfied. However, to qualify as a valid instrument, we must also have that $\widetilde{Z}_{it}$ predicts $\widetilde{FD}_{it}$.

Appendix Table A.1 presents estimates of the first stage. Panel A presents estimates with the overall average foreign demand, while panels B-D present estimates for demand originating in different countries. Columns 1-3 present results for the sample of exporters and columns 4-6 presents results for non-exporters. Data span from 1996 to 2011, since we exclude the first year a given firm is observed. All regressions include firm fixed effects, industry-year effects, and firm-specific controls for energy prices. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period.[9] In Table A.1, we find strong correlations between Log $\widetilde{FD}_{it}$ and Log $\widetilde{Z}_{it}$ across all samples, with F-statistics mostly above 20, which suggests that $\widetilde{Z}_{it}$ has sufficient power to serve as an instrument for $\widetilde{FD}_{it}$.

4.2 Impacts on Exports and Domestic Sales

Next, we estimate the relationship between foreign demand and firm-level exports. If exogenous variation in $\widetilde{FD}_{it}$ represents neutral growth in taste/income in foreign markets, then it is reasonable to expect that Indian exports should rise with foreign demand. However, it is possible that higher (instrumented) $\widetilde{FD}_{it}$ reflects shifts in taste towards India's competitors since India's own exports are left out of $\widetilde{FD}_{it}$. In this case, we might expect Indian exports to fall with foreign demand.

In Table 3, we present IV estimates of (5). In column 1, we take log export value as the dependent variable. We find that a positive foreign demand shock is associated with higher export value. This result holds in the full sample of exporters (panel A) and when broken down by multi-product vs single-product firms (panels B and C). In both the full sample and the multi-product sample, the point estimate is significant at the 1% level. A similar pattern holds in the OLS (see Table A.3), but our focus is on the IV results. Both independent and explanatory variables are expressed in logs, so the point estimates are interpretable as elasticities: a 10% increase in foreign demand leads to a 5.72% increase in firm-level export sales, conditional on the identification assumption.

[9]The CMIE product code follows a "tree-structure", so that all products that begin with the same string of digits belong to a common family. For example, all products that begin with the same 4 digits are part of a common group. We cluster on this aggregate product category to allow for correlation in the error term across closely related products, which is more precise than doing so on the industry.

Table 3: Foreign Demand and Exports

	Exporters		Non-Exporters
Dep Var:	Log(Exp. Val) (1)	Log(Dom. Val) (2)	Log(Dom. Val) (3)
Panel A : Full Sample			
Log $\widetilde{FD}_{it}$	0.572***	0.127***	-0.061
	(0.153)	(0.044)	(0.090)
R squared	0.777	0.890	0.905
mdv	0.342	2.096	1.448
# Obs	5647	8400	3337
# Firms	957	1203	661
Panel B: >1-prod Firms			
Log $\widetilde{FD}_{it}$	0.539***	0.101**	-0.044
	(0.180)	(0.050)	(0.091)
R squared	0.769	0.882	0.913
mdv	0.427	2.295	1.634
# Obs	3880	5737	1992
# Firms	620	776	381
Panel C: 1-prod Firms			
Log $\widetilde{FD}_{it}$	0.433*	0.287*	-0.342
	(0.252)	(0.152)	(0.287)
R squared	0.829	0.903	0.890
mdv	0.151	1.667	1.178
# Obs	1761	2663	1336
# Firms	337	427	279

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ on log export and log domestic value for exporters and log domestic value for non-exporters using the firm-level dataset. All regressions instrument log $\widetilde{FD}_{it}$ with base-year-weighted foreign demand shocks log $\widetilde{Z}_{it}$. Data span 1996–2011. Panel B restricts to multi-product firms, and Panel C to single-product firms. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Top/bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

In Table A.4 , we break out elasticities by origin of the foreign demand shocks, considering shocks in the US/Canada in column 1, shocks in the EU in column 2, and shocks from everywhere else in column 3. Concerns for leakage tend to center on regulation in rich countries displacing production to unregulated poor countries. In accordance with this scenario, we find in column 1 that demand shocks in the US and Canada have large and statistically significant impacts on Indian exports in the full sample and for multi-product firms. By contrast, demand shocks in the EU have only modest effects on Indian exports. Due to their trade patterns, Indian firms seem to be more responsive to US/Canadian shocks than to the EU shocks.

Next, we test for impacts on domestic sales. Most leading models of international trade (such as Mayer et al. (2016); Bernard et al. (2011), among others) feature segmented markets, which imply that conditions in foreign markets do not affect sales in the domestic market. However, some recent empirical work suggests that complementarities or frictions may cause foreign and domestic sales to be determined jointly. Berman et al. (2015) hypothesize that increased export sales relaxes liquidity constraints for the firm, which lowers the marginal cost of production overall, thus increasing domestic sales. Using a panel of French exporters, they find robust empirical evidence in support of the complementarity of foreign and domestic sales. Alternatively, if firms face short-run credit constraints, then a firm might not be able to increase production year-to-year in the face of higher foreign demand. Instead, to meet higher foreign demand, firms might reallocate sales away from the domestic market. Evidence of credit constraints from Feenstra et al. (2014) would be consistent with such a mechanism.

In column 2 of Table 3, we estimate the impact of foreign demand shocks on domestic sales for exporters. We find that not only do export sales increase with foreign demand, but domestic sales increase as well: a 10% increase in foreign demand leads to a 1.27% increase in domestic value of sales. We fail to reject the null of no impact at the 1% level. OLS results also yield positive and statistically significant impacts on domestic sales (see Table A.3). These estimates point to complementarity between export and domestic sales, as hypothesized by Berman et al. (2015), and imply that import flows alone are insufficient to estimate leakage. In particular, total physical production and emissions should increase by more than what is needed simply to meet the export demand, all else equal (holding emission intensity constant). Since we do not observe production broken down by market destination, we cannot test this claim directly. Nevertheless, the estimates in column 2 gives good reasons to believe the overall scale of production increased by more than just the increase in foreign shipments.

The results in columns 1 and 2 rely on the parallel trends assumption – that absent foreign demand shocks, the firms that were exposed to larger foreign demand shocks would have developed similarly to firms that saw smaller demand shocks. As mentioned above, a possible problem with this assumption could be that differential pre-existing product-specific trends lead both export sales and domestic sales to increase with foreign import demand. To address this concern, we first estimate placebo impacts on domestic sales for non-exporters. If demand shocks stimulate increases in output and emissions purely through exports, and if demand shocks are orthogonal to pre-existing trends, then we should not see any impact on non-exporters.[10] Second, we test for pre-period trends directly. Our production data in Prowess go back to 1989, while the trade data only start in 1995. Hence, we have 6 years of production data prior to the beginning of the period for which we can measure trade shocks. Relating the firm-level trends in exports between 1989 and 1995 to the growth in foreign demand between 1995 and 2011, we can test whether firms that saw greater increases in foreign demand from the mid-1990s through the 2000s were already trending differently in the early 1990s relative to the other firms.

For both placebo tests, we fail to reject the null of no differential trends. First, in column 3 of Table 3, we find negative point estimates on domestic sales of non-exporters, which we fail to reject as different from 0. Had common technological trends driven the IV estimates in columns 1 and 2, we would have found positive coefficients. Second, in Table A.2 panel A, we fail to reject that exports of firms that experienced greater foreign demand growth between 1995 and 2011 weren't already differentially trending up before 1995. In column 1 of Table A.2, we compute the change in average log export value between 1991–1992 and 1994–1995 for exporters that appear in both periods. We exclude the years 1989 and 1990 because of very low coverage, and we take averages to get a better reading of export value growth and to include as many firms as possible. The explanatory variable is the change in average log foreign demand between 1995–1997 and 2009–2011, instrumented with the analogous difference in averages for base-year weighted demand. Restricting to exporters with positive export flows in both periods leaves very few firms with which to test for pre-period trends. Nevertheless, we fail to reject no difference in pre-trends.

[10] This placebo check relies on the assumption that non-exporters have access to similar technological trends as exporters. While there is evidence that one of the channels through which exporters gain access to new technologies is precisely through exporting (De Loecker, 2013), there is also evidence that technological adoption spills over to firms in the same industry/region (Bloom et al., 2013). Thus, even if there is some "learning by exporting", one still might expect at least some of the technological trend to spill over to domestically-oriented firms.

4.3 Impacts on Production and Emissions

With exports and domestic sales responding positively to foreign demand, we might expect production and emissions to increase as well. But even with an increase in production, emissions need not increase if emission intensity adjusts endogenously to foreign demand. Here, we trace the export and domestic sales impacts through to CO_2 emissions, production, and CO_2 intensity of production.

In Table 4, we estimate (5) by IV taking CO_2 emissions, production (in physical units), and CO_2 intensity of production as dependent variables. Columns 1-3 report estimates for exporters and columns 4-6 report placebo checks on non-exporters. To give meaningful interpretations to estimates involving production, we restrict the sample to firms that report outputs in the same physical units both across products and over time. Most physical units are reported in tonnes, so this restriction does not drop many firms. For consistency, we make this restriction throughout, even when estimating impacts on export and domestic sales. As in Table 3, we present estimates on the full sample (panel A), multi-product firms (panel B), and single-product firms (panel C). All regressions include firm fixed effects, industry-specific time trends, and firm-specific energy price controls. The top and bottom 1% of values are excluded, and standard errors are clustered on the 4-digit product category responsible for the greatest share of sales for the firm throughout the period.

In panel A of Table 4, we find that firm-level CO_2 emissions and physical quantity of production both increase with foreign demand shocks, while CO_2 intensity declines slightly. Point estimates imply that a 10% increase in foreign demand leads to 1.64% higher CO_2 emissions and to 2.14% higher production volumes, both statistically significant at the 1% level. The impact on emission intensity is not precise in the full sample, but it is precisely estimated for the multi-product firm sample. OLS estimates in Table A.5 yield similar findings, though smaller coefficients. Additionally, in columns 4-6, we find small and statistically insignificant impacts on non-exporters, which again supports the identification assumption. Moreover, in Table A.2 panel A, we fail to reject no difference in pre-period trends in all 3 outcome variables (columns 2-4). These estimates imply that to the extent that environmental regulation erodes competitiveness and increases net imports from developing countries, CO_2 leakage is a legitimate concern in India.

A few recent studies provide useful benchmarks for our results. Aichele & Felbermayr (2015) estimate that trade flows increased by 5% and embodied carbon emissions by 8% between committed and non-committed trade partners after Kyoto was signed. Our re-

Table 4: Emissions in the Firm-Level Dataset

	Exporters			Non-Exporters		
	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full sample						
Log $\widetilde{FD}_{it}$	0.164***	0.214***	-0.050	-0.037	-0.019	-0.017
	(0.045)	(0.060)	(0.036)	(0.078)	(0.081)	(0.070)
R squared	0.945	0.974	0.982	0.944	0.963	0.969
mdv	9.104	10.065	-0.961	8.626	9.701	-1.075
# Obs	8400	8400	8400	3337	3337	3337
# Firms	1203	1203	1203	661	661	661
Panel B: >1-prod Firms						
Log $\widetilde{FD}_{it}$	0.094*	0.179**	-0.085**	-0.035	0.012	-0.047
	(0.056)	(0.071)	(0.042)	(0.109)	(0.096)	(0.074)
R squared	0.942	0.968	0.977	0.947	0.959	0.966
mdv	9.309	10.114	-0.805	8.866	9.929	-1.063
# Obs	5737	5737	5737	1992	1992	1992
# Firms	776	776	776	381	381	381
Panel C: 1-prod Firms						
Log $\widetilde{FD}_{it}$	0.407**	0.278*	0.130	-0.151	-0.302	0.151
	(0.189)	(0.147)	(0.095)	(0.200)	(0.260)	(0.130)
R squared	0.949	0.986	0.990	0.943	0.969	0.975
mdv	8.662	9.960	-1.298	8.277	9.345	-1.068
# Obs	2663	2663	2663	1336	1336	1336
# Firms	427	427	427	279	279	279

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ on log CO_2 emissions, log production in quantity (Q), and log CO_2 emission intensity in quantity ($\frac{CO_2}{Q}$) for exporters and non-exporters using the firm-level dataset. All regressions instrument log $\widetilde{FD}_{it}$ with base-year-weighted foreign demand log $\widetilde{Z}_{it}$. Data span 1996–2011. Panel B restricts to multi-product firms, and Panel C to single-product firms. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Top/bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

sults indicate that impacts on CO_2 from production could be either higher or lower than the estimate on embodied carbon emissions from Aichele & Felbermayr (2015). First, we find that exporters are cleaner than non-exporters, hence taking an average sectoral emission intensity would over-estimate carbon leakage. Second, domestic and export sales

Table 5: Percentage Change in Emissions from Median Foreign Demand Growth

	Annualized Δ in CO_2 for Exporters (%) (1)	Total Δ in CO_2 1995-2011 (%) (2)	Share of Total Δ in CO_2 from Manufac. (%) (3)
Full Adjustment	1.39	6.69	4.58
Constant Emission Intensity	1.82	8.73	5.98
+ Constant Domestic Sales	0.80	3.86	2.64

Notes: Table reports estimated percentage change in annualized CO_2 emissions for a representative exporter (column 1) and total change in CO_2 emissions from manufacturing over the period 1995-2011 (column 2) due to the median annualized increase in foreign demand over the period 1995 - 2011 (8.5% annually). Column 3 reports the share of (2) in total observed change in CO_2 emissions from manufacturing over 1995-2011 (145% increase).

evolve symmetrically, hence ignoring the domestic response would under-estimate carbon leakage. Third, firms' emission intensity decline with trade shocks, hence assuming an exogenous emission intensity would over-estimate carbon leakage. Our study thus provides a complementary perspective to Aichele & Felbermayr (2015), acknowledging that firm heterogeneity matters for carbon emissions.

Next, in the context of US manufacturing, Cherniwchan (2017) finds that firm-level emission levels of $PM_{2.5}$ and SO_2 *fall* with greater access to the Mexican market as a result of NAFTA, while Shapiro & Walker (2018) find no change in emissions as a result of changes in foreign competitiveness. Cherniwchan (2017) ascribes the decline in emissions levels to an enormous drop in emission intensity. While we also find that emission intensity falls with foreign demand (column 3), the effect is not nearly large enough to overturn a large scale effect. Hence, in our developing-world setting, we find a positive increase in CO_2 emission levels. The results from Shapiro & Walker (2018) are harder to compare to our findings because "foreign competitiveness" in Shapiro & Walker (2018) is driven mostly by productivity growth abroad, historically. Still, to the extent that environmental regulations reduce foreign competitiveness abroad, the results from Shapiro & Walker (2018) suggest that they should have hardly any impact on US emissions. For the case of CO_2 emissions from India, we find that this is not the case.

To put the magnitude of these point estimates in perspective, we compute in Table 5 the change in annualized CO_2 emissions for an exporter who would have seen the median annualized growth in foreign demand for its products (column 1), and the resulting change in total CO_2 emissions over the period 1995–2011 (column 2) from the manufacturing sector. In the first row of Table 5, we allow for endogenous adjustment of both domestic sales and CO_2 intensity to foreign demand growth. The annualized median growth in foreign demand over the period 1995–2011 was 8.5%. Multiplying this figure by the point estimate in column 1 of Table 4 yields 0.085*0.164*100= 1.39% annual increase in CO_2 emissions for exporters facing the median foreign demand growth. Multiplying this number by the share of output in manufacturing that is generated by exporters in an average year and summing over the 16-year period (1995–2011), we estimate that total CO_2 emissions from manufacturing increased 6.69% as a result of foreign demand growth.[11] In column 3, we divide the total increase in CO_2 emissions from foreign demand by the observed increase in CO_2 emissions from manufacturing as reported by the International Energy Agency (145% growth between 1995 and 2011).[12] Hence, the increase due to foreign demand represents 4.58% of the total increase.

In rows 2 and 3 of Table 5, we illustrate the importance of accounting for endogenous changes in emission intensity and domestic sales by repeating the aggregate computations under different constraints. In the second row, we assume that exporter CO_2 emission intensity does not adjust with foreign demand growth. Here, we multiply average annualized foreign demand growth by the point estimate in column 2 of Table 4 – implicitly assuming that emissions scale 1-for-1 with production. We find that the annualized emissions from exporters increase by 1.82% and total CO_2 emissions from manufacturing increase by 8.73%, or 5.98% of the observed increase, under this assumption. Finally, in the third row of Table 4, we further impose that only production for exports are affected by foreign demand growth. Thus, we multiply median annualized demand growth by the export elasticity from column 1 in Table 3, and by the average export share in revenue for an exporter (16.5%). The assumption in this scenario is that CO_2 emissions scale 1-for-1 with export sales growth. Under this assumption, we find that annualized emissions from exporters increase 0.80% and total CO_2 emissions from manufacturing increase 3.86%, or 2.64% of the observed increase. Hence, had we ignored endogenous CO_2 intensity adjustments, we would have over-estimated the total increase in emissions by about 30%, and had we also ignored the complementary increase in domestic sales, we would have under-estimated the total increase by 58%.

One drawback of the estimates in Table 5 is that we implicitly assume no entry or

[11] We compute the share of sales that comes from exporters from auxiliary data in the nationally representative Annual Survey of Industries (ASI). The ASI reports export share only for the year 2009. In this year, we calculate that exporters account for 30% of total sales. Hence, we compute 0.3*1.39*16 = 6.69% increase.

[12] We compute CO_2 emissions from manufacturing as the CO_2 emissions from "Other industrial combustion" as reported by the IEA.

exit.[13] A large literature on heterogeneous firms shows that changes in market conditions can have complex effects on firm entry and exit (Melitz, 2003; Melitz & Ottaviano, 2008; Bernard et al., 2011). An increase in regulation or general demand growth in one country will likely encourage entry in other countries, especially entry of multinational affiliates (Eskeland & Harrison, 2003; Kellenberg, 2009; Hanna, 2010). In this case, the estimates of Table 5 would tend to understate the emissions response as well as the emission intensity changes, since multinationals tend to use cleaner production processes.

4.4 Channels of Emission Intensity Adjustment

We next study the underlying causes of firm-average emission intensity adjustments. We begin by separately estimating emission intensity effects on multi-product and single-product firms in the firm-level dataset. If product mix and technology yield different impacts on emission intensity, then we might expect heterogeneous impacts on single vs multi product firms. We then estimate product-mix impacts and the technology channel directly.

First, in Table 4, we separately estimate emission intensity effects on multi-product and single-product firms. In panel C, we restrict to firms that only produce a single product over the entire period. Since product mix is ruled out by construction, any change in emission intensity can be attributed to technological change. In contrast, in panel B, we estimate impacts for multi-product firms only, for which both technology and product-mix could play a role. In column 3 of Table 4 panel C, we find that emission intensity for single-product firms *increases* with foreign demand, though the point estimate is statistically indistinguishable from 0. By contrast, in panel B, we find that the emission intensity of multi-product firms falls for exporters, and we can reject the null of no impact at the 5% level. The point estimates in column 3 imply that a 10% increase in foreign demand translates into 0.5% lower emission intensity overall, and 0.85% lower emission intensity for multi-product firms only. The fact that emission intensity falls for the multi-product sample and not the single-product sample suggests that product mix likely plays an important role in determining average emission intensity.

Next, we address product mix directly by estimating impacts on skewness and number of products offered in the firm-level dataset. In Table 6, we regress the Thiel index and the number of products on instrumented foreign demand for exporters (columns 1-2) and non-

[13] Our sample, however, allows us to consider entry in the export market, since we call an exporter a firm that ever exports during our period of analysis.

exporters (column 3-4) for multi-product firms only. Foreign demand shocks could lead to changes in both measures, and if different product lines are produced with different levels of emission intensity, then product mix could explain the average reductions in emission intensity observed in Table 4. All regressions include firm fixed effects, industry-by-year effects, and firm-specific energy prices.

In panel A, we find that both skewness and number of products increase with foreign demand for exporters, though the coefficients are imprecisely estimated. By contrast, in panel B, we find that both skewness and number of products increase strongly when demand shocks originate in the US/Canada, and that the null of no impact is rejected at the 1% level. Also, in panel B, we find no such impact on non-exporters. The results echo findings from Mayer et al. (2016), in which it was also found that foreign demand growth leads to increased skewness in production. If output shares skew towards lower-emission intensity products or if newly added products have lower emission intensity, these product-mix effects could explain the result in panel B of Table 4.

Lastly, we explore the technology channel directly by exploiting the product-specific energy reports. With outcomes already computed at the firm-product level, we need not make any restriction on the sample. In Table 7, we estimate (8) by IV for exporters (columns 1-3) and non-exporters (columns 4-6). All regressions control for firm-product and industry-by-year effects, as well as energy prices. The industry corresponds to the 2-digit CMIE code of the product. Standard errors are clustered on the 4-digit CMIE code and the firm.

In columns 1 and 2, we find that both CO_2 emissions and physical output at the product level increase with foreign demand for exporters, as in the firm-level dataset. We can reject the null of no impact at the 1% and 5% level, respectively. OLS results yield similar results (see Table A.7), though smaller point estimates. In the non-exporter sample, we find small and statistically insignificant impacts (columns 4 and 5), which again suggests that product-specific trends do not drive the result. Also, in panel B of Table A.2, we fail to reject no difference in pre-1996 trends in emissions or output in the product-level dataset.

Moving to emission intensity, in column 3 we fail to reject the null of no impact for exporters. The point estimate in column 3 is very near 0, and statistically indistinguishable from 0 at conventional levels. Thus, similarly to the single-product firm results, the findings suggest that technological change is not responsible for the CO_2 emissions reductions in Table 4. This conclusion contrasts with previous work that ascribes changes in firm-level

Table 6: Product Mix

	Exporters		Non-Exporters	
	Thiel	# Products	Thiel	# Products
	(1)	(2)	(3)	(4)
Panel A				
Log $\widetilde{FD}_{it}$	0.156	0.221	0.129	0.004
	(0.193)	(0.215)	(0.204)	(0.150)
R squared	0.810	0.849	0.829	0.835
mdv	1.367	2.652	1.163	2.184
# Obs	5737	5737	1992	1992
# Firms	776	776	381	381
Panel B				
Log $\widetilde{FD}_{it}^{US/CAN}$	0.218***	0.259***	0.077	0.020
	(0.068)	(0.051)	(0.078)	(0.067)
R squared	0.810	0.850	0.832	0.838
mdv	1.390	2.678	1.217	2.239
# Obs	5533	5533	1832	1832
# Firms	764	764	369	369
Panel C				
Log $\widetilde{FD}_{it}^{EU}$	0.116	0.148	0.357**	0.303
	(0.076)	(0.102)	(0.164)	(0.189)
R squared	0.813	0.851	0.822	0.825
mdv	1.374	2.664	1.220	2.232
# Obs	5562	5562	1843	1843
# Firms	768	768	374	374

*Notes:*Table reports estimated impacts of log $\widetilde{FD}_{it}$ on Thiel index and # products for exporters and non-exporters using the firm-level dataset. All regressions instrument log $\widetilde{FD}_{it}$ with base-year-weighted foreign demand shocks log $\widetilde{Z}_{it}$. Data span 1996-2011. Panel B restricts to demand shocks from the US/Canada, and Panel C to EU shocks. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

emission intensity to changes in technology (Cui et al., 2015; Cherniwchan, 2017; Gutiérrez & Teshima, 2018). In this rare case where we observe input intensity directly in the data,

Table 7: Emissions in the Product-Level Dataset

	Exporters			Non-Exporters		
	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Log $\widetilde{FD}_{jt}$	0.055***	0.056**	-0.001	0.019	-0.026	0.044
	(0.016)	(0.025)	(0.015)	(0.101)	(0.062)	(0.052)
R squared	0.941	0.968	0.989	0.938	0.962	0.986
mdv	9.332	9.998	6.242	8.544	9.427	6.025
# Obs	13013	13013	13013	2666	2666	2666
# Firm-Products	1653	1653	1653	523	523	523
# Firms	1062	1062	1062	455	455	455

Notes: Table reports estimated impacts of log $\widetilde{FD}_{jt}$ on log(CO_2), log production in quantity (Q), and log CO_2 emission intensity in quantity ($\frac{CO_2}{Q}$) for exporters and non-exporters using the product-level dataset. All regressions instrument log $\widetilde{FD}_{jt}$ with base-year-weighted foreign demand shocks log $\widetilde{Z}_{jt}$. Data span 1995-2011. All regressions include firm-product fixed effects, industry-by-year fixed effects, and energy prices controls. Regressions are clustered on the 4-digit product code and the firm. Top/bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

and hence can isolate technological effects from product-mix, we fail to find evidence of the technological upgrade channel.[14]

One potential concern is that firms do not adjust technology fast enough to respond to year-to-year fluctuations in demand. To address this possibility, we follow recent works in the climate impacts literature that estimate technological adoption from "long difference" estimates using annual data (Dell et al., 2012; Burke & Emerick, 2016). In Table 8, we relate the change in exports, emissions, production, and emission intensity over the entire period to similar long-difference changes in foreign demand for exporters. For all variables, we take averages over 3-year periods from the beginning of the period (1995-1997) and the end of the period (2009-2011), and take the log difference between the two. Hence, we compare changes in average outcomes to changes in average foreign demand conditions. Given the length of the period, one would expect that firms have enough time to adjust their technology. All regressions control for long differences in energy prices, and the change in

[14] Another possible explanation for the null result on emission intensity is that firms *do* adopt new technologies that both increases total input efficiency and lowers energy-use efficiency. A flexible production function such as CES would allow for such countervailing effects. However, the negative across-firm correlation between total factor productivity and emission intensity found in many studies would suggest that input efficiency and emissions efficiency are complementary (Martin, 2012; Shapiro & Walker, 2018; Holladay, 2016).

average current-year foreign demand is instrumented with the change in base-year-weighted demand.

Table 8: Long Difference Results

Dep Var:	Δ Log(Exports)	ΔLog CO2	ΔLog Q	ΔLog (E/Q)
Δ= 2009-2011 / 1995-1997	(1)	(2)	(3)	(4)
Panel A: Firm Level				
Δ Log $\widetilde{FD}$	0.482	0.165*	0.277*	-0.113
	(0.335)	(0.097)	(0.142)	(0.103)
# Firms	144	220	220	220
Panel B: Firm-Product Level				
Δ Log $\widetilde{FD}$		0.138	0.171**	-0.033
		(0.090)	(0.085)	(0.039)
# Firm-Products		360	360	360

Notes: Table presents long difference estimates in the firm-level dataset (panel A) and product-level dataset (panel B). All explanatory variables and dependent variables are computed as the difference between 2009-2011 averages and 1995-1997 averages. In panel A, Δ Log $\widetilde{FD}_i$ is instrumented with Δ Log $\widetilde{Z}_i$, while in panel B Δ Log $\widetilde{FD}_j$ is instrumented with Δ Log $\widetilde{Z}_j$. Long differences for energy prices are included in all regressions. Sample is restricted to exporters, and panel A restricts to firms with constant units over the period. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

In panel A of Table 8, we find that exports, emissions, and production all increase in the long difference in the firm-level dataset. Emission intensity also declines, though the impact is imprecisely estimated. In panel B, we estimate the long difference effect on emissions, production, and emission intensity in the product-level dataset. Production and emissions also increase, and the impact on emission intensity is small and statistically insignificant. Again, these results indicate that investments in emission-saving technology are not a primary driver of demand-induced emission reductions.

5 Implications for Leakage

The elasticities estimated in the previous section bear directly on the leakage question only to the extent that environmental regulation erodes competitiveness of domestic firms, and hence raises import demand. We rely on the conclusion of a recent review by Dechezleprêtre & Sato (2017) that environmental regulation in fact erodes the competitiveness of firms to motivate our question, but we still need an estimate of the elasticity of foreign demand to environmental regulation in our context to carry our results through to leakage. To this end, we adapt the methodology recommended by Sato et al. (2015) of using industry-specific energy prices as a proxy for environmental regulation to estimate the elasticity of foreign demand in India's trading partner markets to regulations in those countries. The argument in Sato et al. (2015) is that environmental regulations primarily impact the energy prices faced by manufacturers, so the elasticity of competitiveness to energy prices proxies fairly well for the elasticity of competitiveness to regulation. Additionally, energy prices depend on international prices of fuel, which are plausibly exogenous for individual firms.

Formally, we multiply fuel-share-weighted energy prices EP_{j_kdt} for 6-digit HS product j belonging to industry k in destination market d in year t by India's export weights to compute the weighted average foreign energy prices paid by competitors to Indian firms operating in product market j, exactly as we did to construct foreign demand shocks:

$$\widetilde{EP}_{jt} = \sum_{d \in \Delta_d} s_{djt} EP_{dj_kt} \tag{10}$$

with s_{djt} the export share in product j of Indian sales to destination d computed from BACI. Country-by-industry specific fuel prices come from Sato et al. (2015) for the years 1995–2011 for 12 sectors and 46 countries. We match each 6-digit HS codes to one of the 12 sectors in Sato et al. (2015) and compute $\widetilde{EP}_{jt}$. We then compute base-year-weighted instruments for $\widetilde{EP}_{jt}$ as we did for foreign demand

$$\widetilde{ZEP}_{jt} = \sum_{d \in \Delta_d} s_{dj0} EP_{dj_kt} \tag{11}$$

with base-year weights s_{dj0} computed as averages over 1995–1997 for the early period and 2002–2004 for the later period, as before. We then estimate

$$\text{Log } Y_{jt} \quad = \quad \alpha_j + \alpha_t + \beta * \text{Log } \widetilde{EP}_{jt} + \epsilon_{jt} \tag{12}$$

where Y_{jt} is either $\widetilde{FD}_{jt}$ or aggregate Indian exports from BACI. We estimate (12) by OLS and by IV while instrumenting $\widetilde{EP}_{jt}$ with $\widetilde{ZEP}_{jt}$.

Before we proceed to the estimates of (12), it is important to note that $\widetilde{EP}_{jt}$ is not nearly as precisely estimated as $\widetilde{FD}_{jt}$. This is a key reason that we focus on the elasticity with respect to foreign demand, and not international energy prices, at the firm level. Energy prices are only available for 46 markets and 12 industries in Sato et al. (2015). Hence, many destination markets are left out of the computation of $\widetilde{EP}_{jt}$, and there is much less variation across product categories. Additionally, energy prices are missing for many country-industry-years. This means that impacts are identified from fewer observations. We therefore use (12) only to get a sense of the relationship between foreign demand and regulation in our context, but rely on $\widetilde{FD}_{jt}$ to estimate elasticities at the firm level.

Table 9: Aggregate Trade Flows and Energy Prices

	Log(Foreign Demand)		Log(Exports)			
	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)
Log $\widetilde{EP}_{jt}$	0.267***	0.307***	0.004	0.163***		
	(0.006)	(0.045)	(0.007)	(0.055)		
Log $\widetilde{FD}_{jt}$					0.286***	0.709***
					(0.009)	(0.019)
R squared	0.835	0.842	0.833	0.829	0.826	0.800
mdv	9.324	9.351	7.644	7.691	7.378	7.581
# Obs	52599	51771	52636	51800	79905	75357
# HS6 Codes	3365	3325	3366	3326	4976	4892

Notes: Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

In Table 9, we present estimates of (12). Data cover the years 1995–2011. An observation is a 6-digit HS code by year. All regressions include 6-digit HS code fixed effects and year fixed effects. Standard errors are clustered on the 6-digit HS code. Columns 1 and 2 present impacts on Log $\widetilde{FD}_{jt}$, while columns 3 and 4 present impacts on aggregate Indian exports. Columns 1 and 3 present OLS estimates, while columns 2 and 4 instrument current-year energy prices with base-year-share weighted averages.

In column 1 of Table 9, we find that current-year average demand indeed increases with energy prices. We can reject the null of no impact at the 1% level. IV estimates in column 2 yield slightly larger impacts, also statistically significant at the 1% level. These estimates provide a reassuring check that environmental regulation would likely stimulate import demand for India's trading partners. Additionally, in column 4, we find direct evidence that energy prices raise Indian exports. Indeed, the point estimate is statistically significant at the 1% level and economically important: doubling energy prices abroad raises Indian exports by 16.3%. Finally, it is instructive to compare the results in columns 3 and 4 to analogous results in columns 5 and 6 taking Log $\widetilde{FD}_{jt}$ as the explanatory variable. We find that foreign demand growth has a much stronger impact on Indian exports than energy prices. The IV estimates in column 6 are statistically significant at the 1% level, and more than 4 times larger than in column 4. This finding echoes results in Dechezleprêtre & Sato (2017): environmental regulation matters for trade flows, but the effect is overwhelmed by other determinants of trade. This is another reason to estimate emission response at the firm level with respect to foreign demand instead of regulation or energy prices.

Taking the point estimates seriously, the results imply that if environmental regulation doubled energy prices everywhere in the world except for India, import demand in the average Indian destination market would increase by 30.7%. Multiplying this estimate by the results in Section 5 yield that doubling energy prices everywhere in the world except for India would raise emissions from Indian manufacturing by 1.5% annually, or about 6.8 Megatons of CO_2 each year. While this figure is not trivial, it seems likely that the reduction in CO_2 emissions in regulated countries from doubling energy prices would overwhelm this value. This allows us to conclude that leakage fears for India are real, but not necessarily of large magnitudes.

6 Conclusion

In this paper, we study the second part of the leakage mechanism – i.e., the relationship between foreign demand and emissions from production. Previous work finds significant impacts of environmental regulation on domestic competitiveness (Dechezleprêtre & Sato, 2017) and import demand (Aichele & Felbermayr, 2015; Levinson & Taylor, 2008). We take these impacts as given and ask: What do they imply for CO_2 emissions from manufacturing in a developing country? Research suggests that CO_2 emissions need not scale 1-for-1 with exports because of complementarities between foreign and domestic sales, and because emission intensity adjusts endogenously with foreign demand. Using detailed firm and firm-product level output and energy-use data from Indian manufacturing firms, we estimate the elasticity of CO_2 emissions to foreign demand and study the underlying mechanisms of adjustments to CO_2 intensity.

We find that foreign demand increases firm-level exports, domestic sales, production, and CO_2 emissions. Back of the envelope calculations suggest that the magnitudes are economically significant. We estimate that a representative export firm that saw foreign demand grow at the median observed rate over the period 1995–2011 would have increased CO_2 emissions by 1.39% annually. This figure translates into 6.69% total increase in CO_2 emissions from manufacturing over the period 1995-2011 because of foreign demand growth, which equals 4.58% of the observed growth in CO_2 emissions from manufacturing in India. Had we ignored endogenous changes to domestic sales and emission intensity, we would have underestimated this figure by 58%. We also find that emissions are most sensitive to demand originating in the US and Canada, which suggests that leakage fears are most warranted in those countries, at least with respect to India.

Next, we find that foreign demand growth triggered a modest reduction in firm-level average emissions intensity, especially for multi-product firms and for demand shocks originating in the US and Canada. Decomposing this firm-level average effect into an across-product effect and a within-product effect, we find some evidence of endogenous reallocation towards cleaner products, but fail to reject the null of no impact for technological change within firm product. Using long-difference estimates yields similar results. Therefore, these results suggest, like Barrows & Ollivier (2018), that researchers should take caution in ascribing firm-level average changes in emission intensity to technological adoption.

In a final exercise, we compute leakage as the product of the elasticity of CO_2 emissions to foreign demand and the elasticity of foreign demand to environmental regulation. Proxying environmental regulation with weighted average energy prices in India's trading partner markets, we find that an environmental regulation that doubles average energy prices would only increase CO_2 emissions from manufacturing in India by 1.5%, or 6.8 Megatons per year. This result echoes previous conclusions from the literature that environmental regulation matters for trade flows, but the effect is overwhelmed by other determinants of trade. In our context, we can see that CO_2 emissions in India are sensitive to regulation elsewhere in the world, but also that leakage rates are fairly modest.

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Appendix

A Additional Results

Table A.1: First Stage Impacts on Current-year Foreign Demand

	Exporters			Non-Exporters		
	All Firms	MP Firms	SP Firms	All Firms	MP Firms	SP Firms
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All Country Average						
Log $\widetilde{Z}_{it}$	0.306***	0.292***	0.396***	0.279***	0.280***	0.314***
	(0.028)	(0.031)	(0.083)	(0.028)	(0.037)	(0.089)
R squared	0.923	0.901	0.960	0.927	0.911	0.960
F-stat	45.5	27.9	6.7	28.5	19.1	55.7
# Obs	8400	5737	2663	3337	1992	1336
# Firms	1203	776	427	661	381	279
Panel B: US/Canada						
Log $\widetilde{Z}_{it}$	0.567***	0.528***	0.759***	0.584***	0.487***	0.840***
	(0.052)	(0.057)	(0.079)	(0.107)	(0.098)	(0.100)
R squared	0.962	0.945	0.994	0.967	0.951	0.994
F-stat	34.5	22.5	88.7	39.2	32.3	87.0
# Obs	8122	5533	2589	3108	1832	1263
# Firms	1186	764	422	636	369	265
Panel C: EU						
Log $\widetilde{Z}_{it}$	0.376***	0.367***	0.447***	0.275***	0.265***	0.314***
	(0.041)	(0.042)	(0.092)	(0.055)	(0.050)	(0.104)
R squared	0.943	0.925	0.978	0.943	0.918	0.982
F-stat	21.0	20.1	13.9	8.5	21.3	11.4
# Obs	8173	5562	2611	3137	1843	1284
# Firms	1191	768	423	645	374	270
Panel D: Other Countries						
Log $\widetilde{Z}_{it}$	0.315***	0.294***	0.432***	0.302***	0.289***	0.393***
	(0.027)	(0.022)	(0.073)	(0.031)	(0.027)	(0.099)
R squared	0.890	0.864	0.937	0.903	0.890	0.932
F-stat	45.0	63.3	10.4	25.7	31.2	107.5
# Obs	8391	5728	2663	3336	1991	1336
# Firms	1203	776	427	661	381	279

Notes: Table reports estimated first-stage impacts of log $\widetilde{Z}_{it}$ on log $\widetilde{FD}_{it}$ for exporters and non-exporters using the firm-level dataset. Data span 1996-2011. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.2: Testing for Pre-Period Trends

Dep Var:	Δ Log Exp. Val.	ΔLog CO2	ΔLog Q	ΔLog (E/Q)
Δ= 1994-1995 / 1991-1992	(1)	(2)	(3)	(4)
Panel A: Firm Level				
Δ Log $\widetilde{FD}$	-0.144	-0.023	0.057	-0.080
	(0.606)	(0.089)	(0.080)	(0.082)
# Firm	60	109	109	109
Panel B: Firm-Product Level				
Δ Log $\widetilde{FD}$		0.075	0.055	0.020
		(0.078)	(0.068)	(0.033)
# Firm-Products		143	143	143

Notes: Table presents long difference estimates in the firm-level dataset (panel A) and product-level dataset (panel B). All dependent variables and controls are computed as the difference between 1994–1995 averages minus 1991–1992 averages. Current-year foreign demand shocks are computed as the difference between 2009–2011 averages minus 1995–1997 averages and instrumented with base-year weighted foreign demand. Sample is restricted to exporters, and panel A restricts to firms with constant units over the period. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.3: Foreign Demand and Exports, OLS

	Exporters		Non-Exporters
Dep Var:	Log(Exp. Val)	Log(Dom. Val)	Log(Dom. Val)
	(1)	(2)	(3)
Panel A : Full Sample			
Log $\widetilde{FD}_{it}$	0.224***	0.067***	0.033
	(0.048)	(0.016)	(0.023)
R squared	0.782	0.878	0.901
mdv	0.349	2.042	1.391
# Obs	6592	9801	4381
# Firms	1103	1362	864
Panel B: >1-prod Firms			
Log $\widetilde{FD}_{it}$	0.290***	0.065***	0.035
	(0.054)	(0.020)	(0.026)
R squared	0.770	0.870	0.903
mdv	0.434	2.225	1.570
# Obs	4573	6764	2633
# Firms	725	886	497
Panel C: 1-prod Firms			
Log $\widetilde{FD}_{it}$	0.029	0.091***	0.001
	(0.076)	(0.033)	(0.051)
R squared	0.831	0.893	0.904
mdv	0.158	1.634	1.118
# Obs	2016	3037	1740
# Firms	378	476	366

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ on log export and log domestic value for exporters and log domestic value for non-exporters using the firm-level dataset. Data span 1996–2011. Panel B restricts to multi-product firms, and Panel C to single-product firms. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.4: Foreign Demand and Exports, By Destination

Dep Var:	Log(Export Value)			Log(Domestic Value)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A : Full Sample						
Log $\widetilde{FD}_{it}^{US/CAN}$	0.238***			0.016		
	(0.078)			(0.025)		
Log $\widetilde{FD}_{it}^{EU}$		0.156*			0.022	
		(0.085)			(0.031)	
Log $\widetilde{FD}_{it}^{Other}$			0.429***			0.079**
			(0.109)			(0.038)
R squared	0.787	0.787	0.782	0.889	0.888	0.890
# Obs	5478	5526	5641	8122	8173	8391
# Firms	936	947	957	1186	1191	1203
Panel B: >1-prod Firms						
Log $\widetilde{FD}_{it}^{US/CAN}$	0.253***			-0.002		
	(0.089)			(0.028)		
Log $\widetilde{FD}_{it}^{EU}$		0.105			0.031	
		(0.096)			(0.035)	
Log $\widetilde{FD}_{it}^{Other}$			0.438***			0.074
			(0.104)			(0.049)
R squared	0.777	0.776	0.773	0.881	0.880	0.882
# Obs	3763	3793	3874	5533	5562	5728
# Firms	606	613	620	764	768	776
Panel C : 1-prod Firms						
Log $\widetilde{FD}_{it}^{US/CAN}$	0.137			0.095*		
	(0.128)			(0.055)		
Log $\widetilde{FD}_{it}^{EU}$		0.375***			-0.060	
		(0.093)			(0.080)	
Log $\widetilde{FD}_{it}^{Other}$			0.013			0.156
			(0.211)			(0.114)
R squared	0.836	0.836	0.833	0.903	0.903	0.904
# Obs	1707	1727	1761	2589	2611	2663
# Firms	330	334	337	422	423	427

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ disaggregated by export destination on log export value and log domestic value for exporters and non-exporters using the firm-level dataset. All regressions instrument log $\widetilde{FD}_{it}$ with base-year-weighted foreign demand shocks log $\widetilde{Z}_{it}$. Data span 1996–2011. Panel B restricts to multi-product firms, and Panel C to single-product firms. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest

Table A.5: Emissions in the Firm-Level Dataset, OLS

	Exporters			Non-Exporters		
	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Full sample						
Log $\widetilde{FD}_{it}$	0.067***	0.065***	0.001	0.017	0.014	0.002
	(0.018)	(0.023)	(0.021)	(0.015)	(0.027)	(0.023)
R squared	0.939	0.972	0.980	0.938	0.962	0.969
mdv	9.025	9.986	-0.961	8.511	9.727	-1.216
# Obs	9801	9801	9801	4381	4381	4381
# Firms	1362	1362	1362	864	864	864
Panel B: MP Firms						
Log $\widetilde{FD}_{it}$	0.075***	0.067**	0.008	0.009	0.012	-0.003
	(0.019)	(0.029)	(0.027)	(0.021)	(0.034)	(0.028)
R squared	0.936	0.965	0.974	0.938	0.954	0.963
mdv	9.213	10.015	-0.801	8.744	9.896	-1.152
# Obs	6764	6764	6764	2633	2633	2633
# Firms	886	886	886	497	497	497
Panel C: SP Firms						
Log $\widetilde{FD}_{it}$	0.050	0.068***	-0.018	0.018	-0.007	0.025
	(0.032)	(0.021)	(0.025)	(0.043)	(0.044)	(0.017)
R squared	0.947	0.985	0.989	0.945	0.974	0.979
mdv	8.605	9.923	-1.318	8.162	9.463	-1.300
# Obs	3037	3037	3037	1740	1740	1740
# Firms	476	476	476	366	366	366

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ on Log(CO_2), Log production in quantity (Q), and Log CO_2 emission intensity in quantity ($\frac{CO_2}{Q}$) for exporters and non-exporters using the firm-level dataset. Data span 1996–2011. Panel B restricts to multi-product firms, and Panel C to single-product firms. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.6: Emissions in the Firm-Level Dataset, By Destination

	Exporters			Non-Exporters		
	$\text{Log}(CO_2)$	Log(Q)	$\text{Log}(\frac{CO_2}{Q})$	$\text{Log}(CO_2)$	Log(Q)	$\text{Log}(\frac{CO_2}{Q})$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A						
Log $\widetilde{FD}_{it}^{US/CAN}$	0.049	0.093**	-0.044*	0.044	0.005	0.039
	(0.033)	(0.040)	(0.024)	(0.031)	(0.024)	(0.036)
R squared	0.943	0.974	0.983	0.943	0.962	0.969
mdv	9.041	10.012	-0.971	8.539	9.548	-1.009
# Obs	8122	8122	8122	3108	3108	3108
# Firms	1186	1186	1186	636	636	636
Panel B						
Log $\widetilde{FD}_{it}^{EU}$	0.044	0.040	0.004	0.074	-0.000	0.075
	(0.030)	(0.033)	(0.025)	(0.078)	(0.091)	(0.059)
R squared	0.943	0.975	0.982	0.942	0.963	0.969
mdv	9.054	10.016	-0.962	8.565	9.583	-1.019
# Obs	8173	8173	8173	3137	3137	3137
# Firms	1191	1191	1191	645	645	645
Panel C						
Log $\widetilde{FD}_{it}^{other}$	0.142***	0.166***	-0.024	-0.005	0.028	-0.033
	(0.041)	(0.050)	(0.041)	(0.053)	(0.072)	(0.057)
R squared	0.945	0.975	0.982	0.944	0.963	0.969
mdv	9.107	10.068	-0.961	8.626	9.700	-1.074
# Obs	8391	8391	8391	3336	3336	3336
# Firms	1203	1203	1203	661	661	661

Notes: Table reports estimated impacts of log $\widetilde{FD}_{it}$ by export destination on $\text{Log}(CO_2)$, Log production in quantity (Q), and Log CO_2 emission intensity ($\frac{CO_2}{Q}$) for exporters and non-exporters using the firm-level dataset. All regressions instrument log $\widetilde{FD}_{it}$ with base-year-weighted foreign demand shocks log $\widetilde{Z}_{it}$. Data span 1996–2011. All regressions include firm fixed effects, controls for energy prices, and industry-by-year fixed effects. Standard errors are clustered on the 4-digit product code responsible for the largest share of firm sales over the period. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.7: Emissions in the Product-Level Dataset, OLS

	Exporters			Non-Exporters		
	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)	Log(CO_2)	Log(Q)	Log($\frac{CO_2}{Q}$)
	(1)	(2)	(3)	(4)	(5)	(6)
Log $\widetilde{FD}_{jt}$	0.020***	0.021**	-0.001	-0.000	-0.005	0.005
	(0.007)	(0.010)	(0.005)	(0.021)	(0.018)	(0.006)
R squared	0.941	0.968	0.989	0.938	0.962	0.986
mdv	9.323	9.997	6.234	8.528	9.439	5.997
# Obs	13157	13157	13157	2703	2703	2703
# Firm-Products	1659	1659	1659	527	527	527
# Firms	1062	1062	1062	456	456	456

Notes: Table reports estimated impacts of log $\widetilde{FD}_{jt}$ on Log(CO_2), Log production in quantity (Q), and Log CO_2 emission intensity ($\frac{CO_2}{Q}$) for exporters and non-exporters using the product-level dataset. Data span 1995–2011. All regressions include firm-product fixed effects, controls for energy prices, and industry-by-year fixed effects. Regressions are clustered on the 4-digit product code and the firm. Top and bottom 1% of outcome variable values have been removed. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

B Data Appendix

In this appendix, we discuss individuating products in the output data, computing CO_2 emissions from energy-use data, merging product-specific emission intensity to product-specific outputs, diagnostic checks on the product-specific emissions calculations, and constructing trade shocks from trade data. The first four steps rely on our previous work (Barrows & Ollivier, 2018). Hence, we provide summaries of the procedures here and direct the reader to Appendix A of Barrows & Ollivier (2018) for more details. As the construction of the trade shock is novel, we describe it in more depth.

B.1 Individuating Products in the Output Data

Firms report value and quantity of sales each year individuated by text descriptions (e.g. “t-shirts”). CMIE assigns each product string a single 16-digit product classification code, which we will use to map to trade shocks. However, the CMIE codes are not ideal for individuating products. First, CMIE sometimes assigns different product codes to the same text description over time. Second, CMIE sometimes assigns the same product codes to multiple text descriptions within the same firm-year. Our assumption is that if the firm separately reports output information for two (potentially closely related) product descriptions, then we should treat them as different products, even if CMIE does not distinguish between them in terms of product codes. Hence, we take the firm-supplied product string name as the identifier of a firm-product.

As described in Barrows & Ollivier (2018), an issue with the output data is that output units are not always constant within firm-product over time. We attempt to standardize units as much as possible, but then drop any observations from the analysis which we cannot compute in constant units. See Barrows & Ollivier (2018) for more details.

B.2 Computing Emissions from Energy-Use Data

While firms in Prowess do not report emissions directly, we can compute CO_2 emissions from energy-use data conditional on the assumption that CO_2 emissions are directly proportional to the quantity of an energy source consumed (Martin, 2012; Marin & Vona, 2017; Forslid et al., 2018; Barrows & Ollivier, 2018). At the firm level, firms report the total quantity of each energy source consumed each year (e.g., liters of diesel, KwH of electricity, etc.). At the product level, firms report energy intensity of production by output product – the amount of each energy source used to generate a single unit of the good. For both reports, we translate physical quantities of energy consumed into physical quantities of CO_2 emissions and sum over energy sources to compute firm-level or product-level emissions. Source specific emissions factors come from the US EPA 2012 Climate Registry Default Emissions Factors (http://theclimateregistry.org/wp-content/uploads/2015/01/2012-Climate-Registry-Default-Emissions-Factors.pdf), and are reported in Table B.1.[15]

In computing CO_2 emissions, several issues arise. We describe in detail each issue and our treatment of it in Barrows & Ollivier (2018), but mention them briefly again here. First, output units are not always the same across energy sources within the firm-year or firm-product-year. We standardize output units as much as possible, but must in the end drop observations for which standardization is not possible. Second, we are not able in every case to assign a meaningful CO_2 emissions factor to all energy reports. Emissions factors are reported for a specific unit of energy source consumed or mmBTU of energy. For a given energy source reported, if we can not convert the reported unit to match the unit in Table B.1, then we can not convert energy consumption into CO_2 emissions. We first attempt to standardize units, and then drop any observations for which we cannot match units with the EPA report. Third, we drop outputs which appear to be intermediate inputs used by the firm in later stages of production.

[15]In the EPA report, CO_2 intensities are reported per unit of energy source (e.g., short ton of Lignite), and per mmBTU of energy. The energy types and CO_2 emissions factors from Table 12.1 in the US EPA 2012 Climate Registry Default Emissions Factors are listed in Table B.1. There are 25 energy sources described in Table 12.1. Electricity generation for India is reported in Table 14.4. The table reports 951 g CO2 per KwH for Indian electricity. Applying conversion of 1 KwH equals 0.0034095 mmBTU yields 278 kg CO2 per mmBTU.

Table B.1: CO_2 emission factors

Energy Source	Kg CO_2 per Unit of Energy Source	Unit of Energy Source	Kg CO_2 per MMBTU of Energy Source
Acetylene	0.1053	scf	71.61
Agricultural Byproducts	974.9	short ton	118.17
Anthracite	2597.82	short ton	103.54
Biogas (Captured Methane)	0.0438	scf	52.07
Coke	2530.59	short ton	102.04
Coke Oven Gas	0.0281	scf	46.85
Distillate Fuel Oil No. 1	10.18	gallon	73.25
Distillate Fuel Oil No. 2	10.21	gallon	73.96
Electricity			278.00
Fuel Gas	0.0819	scf	59.00
Kerosene	10.15	gallon	75.20
Kraft Black Liquor	1131.11	short ton	94.42
LPG	5.79	gallon	62.98
Lignite	1369.28	short ton	96.36
Lubricants	10.69	gallon	74.27
Motor Gasoline	8.78	gallon	70.22
Naptha (<401 deg F)	8.5	gallon	68.02
Natural Gas (US average)	0.0545	scf	53.02
Petroleum Coke (Liquid)	14.64	gallon	102.41
Petroleum Coke (Solid)	3072.3	short ton	102.41
Propane (Liquid)	5.59	gallon	61.46
Residual Fuel Oil No. 6	11.27	gallon	75.10
Solid Byproducts	2725.32	short ton	105.51
Wastewater Treatment Biogas			52.07
Waxes	9.57	gallon	72.60
Wood and Wood Residuals	1442.64	short ton	93.80

Notes: The first column lists the energy source as named by the EPA. Prowess does not use exactly the same naming convention, so we mapped by hand these energy types to the energy types listed in Prowess. The second column reports kg CO_2 associated with a given unit of energy type in column 1, where the unit is reported in column 3. For most energy types, we use the CO_2 intensity listed in column 2. However, for some observations, we were unable to standardize units across the two datasets. In some cases, we were able to use an alternative CO_2 intensity reported per mmBTU. We list this alternative CO_2 intensity in column 4.

B.3 Merging Product-specific data to Output data

To compute firm-product-level emissions, we merge CO_2 emission intensity to product-level outputs. While there is no unique product-level identifying code on which to match, both energy intensity and product-level outputs report text descriptives of the products and CMIE has labeled products in both datasets with the 16-digit product codes. Hence, we could match either on exact string name or on the 16-digit product code. However, upon inspection, it seems clear that neither string names nor product codes are consistent across the two datasets.

Our strategy is first to match on exact string name of the product. Then, with all the products that fail to match on exact string name, we match by hand the inputs to the outputs based on the product descriptions. For example, in one case, a product described as "Shopping Bags/CarryBags" in the output dataset is merged to a product called "Plastic Bags" in the energy dataset. Though the names are not exactly the same, it seems clear from looking at the range of products described for the given firm that these two reports refer to the same outputs. By considering approximate matches such as this example, we increase the size of the matched input-output product-level dataset substantially.

B.4 Diagnostic checks of Product-specific Emissions Calculations

Our test of the technological channel relies mostly on the product-specific energy reports, from which we compute emission intensity and emissions above.[16] While firms are required by the 1988 Amendments to the Companies Act to report product-specific energy intensities, there are no formal mechanism to ensure accurate reporting. Additionally, there may be significant costs to breaking down energy use by product line for the firms. Hence, firms may not have strong incentives to report product-wise energy use accurately. Lacking independent audit reports of the energy-use data, we cannot say how accurate the reports are. However, we can perform diagnostic checks on the product-specific energy data and test alternative assumptions. We perform these tests in the appendix of Barrows & Ollivier (2018), but summarize them here.

Suppose that firms want to comply with the reporting requirement but do not want to pay the cost to learn how energy-use breaks down by product. Three reasonable hypothesis emerge. First, the firms could report pure noise for the energy intensity figures. If there is no penalty for false reporting and/or no mechanism for ensuring accurate reporting, then

[16] We also test for technology effects using the firm-level energy reports for single-product firms, though this sample is by definition not representative.

it is certainly possible that firms could follow this strategy. Second, firms might employ some cheap heuristic for determining product-specific energy use. The most obvious choice would be to break down energy use by sales share of the products. Sales share is not difficult to calculate (and is in fact already required in the reports). So simply dividing total energy use by sales share would be a very cheap way to determine the product-wise energy intensity. Finally, firms might pick some value for energy intensity (either accurate or not) and report the same value every year. If firms followed the first or the third strategy, one would not expect the reported energy intensity to respond to foreign trade shocks, regardless of whether firms adjusted their technology.

To address these three hypothesis, we perform several tests in Barrows & Ollivier (2018). First, if firms report pure noise, then the computed emission intensity should not correlate with any variable. This hypothesis is easily rejected in Barrows & Ollivier (2018) by the strong correlation between emission intensity and product sales share rank within the firm. In Barrows & Ollivier (2018), we find that larger products have lower emission intensity. This relationship would be highly unlikely if the product-specific energy reports were pure noise.

Second, in Barrows & Ollivier (2018), we test for whether product-specific energy use is driven entirely by sales share. It is quite likely that higher-sales products should use more energy. However, if the energy reports are accurate, we would not expect sales share to explain all the variation in energy use. In Barrows & Ollivier (2018), we compute for each energy source (e.g., electricity, coal, diesel) the share of energy use devoted to a given product based on the product-specific energy reports. We then regress this variable on the sales share of the product within the firm-year. In Barrows & Ollivier (2018), we find that energy-use share is increasing in sales share, but that sales share does not perfectly predict energy-use share. To address measurement errors, we also instrument sales share with lagged sales share. In all specifications, we found point estimates away from 1. We take this as evidence that the product-specific energy data reflect more than just the sales share.

Finally, we can reject the hypothesis that firms do not adjust energy-use intensity year-to-year simply by noting the large amount of variation in emission intensities within product-line over time.

In summary, while we cannot say for sure how accurate the product-specific energy reports are without an audit, we test the three most obvious hypothesis for how the firms could misreport the information, and find compelling evidence against all three hypothesis.

B.5 Merging Trade Data to Prowess

To test for impacts of foreign demand, we must merge trade shocks to the product-level information in Prowess. International trade flows are classified in BACI according to the Harmonized System (HS) revision 1996, of which there are 5,132 6-digit codes (sections 1-21), while products in Prowess are classified according to CMIE's own 16-digit coding system. Previous work has merged trade data to Prowess by first mapping HS codes to National Industrial Classification codes (NIC) via a crosswalk from Debroy & Santhanam (1993), and then to CMIE's codes via a crosswalk provided by CMIE (see De Loecker et al. (2016) for an example). However, the cross-walk from Debroy & Santhanam (1993) is aggregated to the 3-digit level (for the most part), and relies on the version of the NIC from the early 1980s. Hoping to exploit differential growth rates in foreign demand at a more granular level, we construct our own cross-walk between the CMIE product codes and HS revision 1996.

We aim to assign one or more HS codes to each of 3,324 distinct 16-digit CMIE product codes based on the descriptions of the products. While descriptions in the two datasets are usually not exactly the same, both classifications hew fairly closely to the ISIC classification, which means that product ordering and text descriptions are often quite similar in the two datasets. We thus match HS codes to CMIE product codes by hand as follows.

We first attempt to match one or multiple 6-digit HS codes to a given 16-digit CMIE product code. Sometimes, there is no obvious 6-digit match. In these cases, we exploit the fact that the HS follows a tree-like structure, so that all products with the same first four digits belong to a common family of products. Thus, while there may be no 6-digit code that matches to a 16-digit CMIE code, there may be a 4-digit HS code. Finally, if no 4-digit code can be matched to a CMIE code, we match to the 2-digit HS code. See Table B.2 for an example. Here, one can see that some CMIE products match to 6-digit HS codes, while other products can only be matched to the broader 4-digit group. In the full crosswalk, we match 3,276 distinct product codes to at least a 2-digit HS code.

Next, we translate foreign demand computed for 6-digit HS codes in BACI into 16-digit CMIE codes. When a single 6-digit HS code matches to a 16-digit CMIE code, then translating between the two classification systems is simple. However, as is illustrated in Table B.2, in some cases, multiple 6-digit codes match to the same 16-digit CMIE code, and sometimes CMIE codes only match to a 4-digit or even 2-digit HS code. In these cases, we must take averages over shocks computed at the 6-digit level.

Index 6-digit HS codes by $h6$, 4-digit HS codes by $h4$, 2-digit HS codes by $h2$, and 16-

digit CMIE codes by c. Foreign demand and instruments $\widetilde{FD}_{h6,t}$ and $\widetilde{Z}_{h6,t}$ are computed in Section 2.2. Suppose that a given CMIE product c matches to multiple 6-digit HS codes. This could be because the CMIE code is less detailed than the 6-digit HS codes, or because there is uncertainty with respect to which 6-digit HS code best describes the CMIE product code. To assign a foreign demand in this case, we take a simple average over shocks computed at the 6-digit level:

$$\widetilde{\Theta}_{c,t} = \sum_{h6 \in \Delta_c} \widetilde{\Theta}_{h6,t} \tag{B.1}$$

for each $\Theta \in \{FD, Z\}$ and each $h6$ that matches to the CMIE code c.

Next, suppose we cannot match any 6-digit codes to a CMIE code, but can match an entire 4-digit category. In this case, we simply take the simple average of foreign demand and instruments over all 6-digit codes in the 4-digit code:

$$\widetilde{\Theta}_{c,t} = \sum_{h6 \in \Delta_{h4}} \widetilde{\Theta}_{h6,t} \tag{B.2}$$

for each $\Theta \in \{FD, Z\}$ and each $h6$ in the aggregate $h4$. Then, if multiple 4-digit codes match to a CMIE code, we again take a simple average over the 4-digit codes

$$\widetilde{\Theta}_{c,t} = \sum_{h4 \in \Delta_c} \widetilde{\Theta}_{h4,t} \tag{B.3}$$

We then follow the same procedure to compute shocks for CMIE codes that match to 2-digit HS codes.

In an abuse of notation, in the main text we refer to both BACI codes and CMIE codes as j, though in reality when considering shocks computed in the CMIE coding system, a product j potentially refers to simple averages over multiple 2-digit, 4-digit, or 6-digit HS codes.

Table B.2: Cross-Walk Example

4-Digit HS Desc	HS4 Code	6-Digit HS Desc	HS6 Code	CMIE Desc	CMIE Code
Synthetic filament yarn, not put up for retail sale	5402			Synthetic filament yarn other than sewing threads	0605010200000000
		High tenacity yarn of nylon or other polyamides	540210	High tenacity yarn of nylon or other polyamides	0605010201000000
		Tyre cord fabric of nylon or other polyamides	590210	Nylon tyre yarn	0605010201009999
		Other yarn, single, untwisted or w/twist not exc. 50 turns per Of nylon or other polyamides	540241	Nylon filament yarn	0605010201020000
Synthetic filament yarn, not put up for retail sale	5402			Yarn of other polyamides , excluding nylon	0605010201030000
		High tenacity yarn of polyesters	540220	High tenacity yarn of polyesters	0605010202000000
Synthetic filament yarn, not put up for retail sale	5402			Polyester filament yarn (PFY)	0605010202009999
Synthetic filament yarn, not put up for retail sale	5402			Other polyester, excluding terylene dacron	0605010202019999
Synthetic filament yarn, not put up for retail sale	5402			Partially oriented yarn (POY)	0605010202040000
Synthetic filament yarn, not put up for retail sale	5402			Drawn textured yarn (DTY)	0605010202060000
		Textured yarn :– Other	540239	Textured yarn of synthetic filament yarn	0605010203000000
Synthetic filament yarn, not put up for retail sale	5402			Other synthetic filament yarns	0605010204000000
Synthetic filament yarn, not put up for retail sale	5402			Polyvinyl acetate filament yarn	0605010204009999
Synthetic filament yarn, not put up for retail sale	5402			Polyvinyl chloride filament yarn	0605010204019999
Synthetic filament yarn, not put up for retail sale	5402			Polypropylene filament yarn (PPFY)	0605010204030000
Synthetic filament yarn, not put up for retail sale	5402			Acrylic filament yarn (AFY)	0605010204040000