Foreign Demand, Developing Country Exports, and CO2 Emissions: Firm-Level Evidence from India

Hélène Ollivier, Geoffrey Barrows

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Abstract

Over the past few decades, wealthy countries have relied increasingly on imports from developing countries, prompting concerns regarding the environmental impacts of trade. Increased import demand in wealthy countries certainly increases export flows from developing countries, but emissions need not scale 1 for 1 with exports if domestic sales or emission intensity adjust endogenously to foreign demand. In the paper, we exploit detailed product-line information on production and emissions for Indian manufacturing firms to estimate how firms adjust their production decisions in response to demand shocks in trading partner markets. Using a shift-share instrument, we find that foreign demand growth increased growth in CO_2 emissions at the firm level via output growth (scale effect), but that endogenous reductions in emission intensity (technique effect) mitigated roughly 40% of this effect. With output denominated in physical units, both effects are estimated net of price adjustments. The overall impact on CO_2 emissions growth is positive, though statistically insignificant at conventional levels. We further document that the scale effect owes to increased growth in both export sales and domestic sales, and that firm-product emission intensity fell when expressed per physical unit of output. The latter result indicates that the firm-level technique effect owes at least in part to technological adoption.

Keywords: globalization; trade and environment; product mix; technological change
JEL codes: F14; F18; O3A; Q56

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1 Introduction

Economists, policy-makers, and the general public have been concerned about the environmental consequences of globalization for decades. Central to the debate is the fear that free trade encourages production to shift towards developing countries, where environmental regulation is weak and production less efficient (Antweiler et al., 2001; Frankel & Rose, 2005; Copeland & Taylor, 2004; Levinson, 2009). The rise in exports from developing countries in recent years makes this fear more salient,[1] especially in a context where international negotiations on climate change have stalled and where governments have been slow to regulate Greenhouse Gas (GHG) emissions.[2] But recent studies suggest that changes in international conditions also trigger endogenous response at the firm level beyond export sales growth (Forslid et al., 2018; Cui et al., 2015; Cherniwchan, 2017; Gutiérrez & Teshima, 2018; Shapiro & Walker, 2018; Barrows & Ollivier, 2018), which implies that even if globalization increases the demand for exports from the developing world, the impact on emissions is not clear. While there is a large theoretical literature on the impact of free trade on aggregate emissions, there is little empirical evidence of its impact on emissions of individual firms, especially in the developing world.

Three mechanisms in particular suggest that the impact of foreign demand on emissions is theoretically ambiguous. First, using a fixed cost model a la Bustos (2011), several papers argue that any positive shock to demand encourages technological adoption, which could lower firm-level emission intensity (Forslid et al., 2018; Cui et al., 2015; Cherniwchan, 2017; Gutiérrez & Teshima, 2018). The potential for technological upgrading is perhaps strongest specifically in the developing world, where firms operate on average far from the global technological frontier. Second, increased foreign demand may also incentivize multi-product firms to adjust their product mix towards lower-marginal-cost products (Mayer et al., 2014, 2020), which could lower firm emission intensity if these products also have lower emission intensity within firms (Barrows & Ollivier, 2018). Finally, the overall scale of output at the firm level may increase less (more) than the absolute growth in exports if export and domestic sales are substitutes (complements) (Berman et al., 2015). Together, these three mechanisms imply that firm-level emissions need not scale 1 for 1 with firm-level exports, and need not increase at all.

[1]See Figure A.3 for the evolution of developing-world export share in total world exports over time.

[2]Regional regulatory markets have been established in many places in the world, mostly in developed countries, 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.

In this paper, we estimate how foreign demand growth impacts the emissions of individual firms in the developing world. Our analysis leverages annual reports from Indian manufacturing firms that – due to an unusually detailed reporting requirement – allow us to track output and energy inputs both denominated in physical units over time at the firm and firm-product levels. From these reports, we compute CO_2 emissions by multiplying energy usage statistics by CO_2 content emission factors of different energy types and summing over energy types, as in other recent work (Martin, 2012; Marin & Vona, 2019; Forslid et al., 2018; Barrows & Ollivier, 2018). With these CO_2 emissions series, we first estimate an elasticity of firm-level emissions growth to foreign demand growth. We then decompose the overall impact into a scale effect and a technique effect. Since outputs are denominated in physical units, both effects are estimated net of price effects. At the firm level, the "technique" effect could include both across-product shifts and within-product over time adjustments; but at the product level, changes in emission intensity over time could only reflect changes in technology.[3] Thus, by studying product-level emission intensity expressed per physical unit of production, we can assess for the first time in the literature the magnitude of the technique effect net of both price and product-mix effects.

To separate the causal impact of foreign demand growth from unobservable correlated determinants of production and emissions of individual Indian firms, we construct firm-specific foreign demand shocks by combining aggregate (India-wide) product-specific foreign demand growth with firm-specific product sales shares, similarly to Hummels et al. (2014); Mayer et al. (2020); Aghion et al. (2019). As discussed in recent work on share-shift instruments (Borusyak et al., 2019; Goldsmith-Pinkham et al., 2020), identifying variation can be seen as stemming from the exogeneity of the firm-specific product shares or the aggregate demand shocks. We take the "shocks view" of Borusyak et al. (2019), and base identification on the exogeneity of the foreign demand growth shocks. Our preferred specification is estimated in growth rates, instrumenting current-year-weighted demand growth with base-year-weighted growth. Falsification tests of correlations between *future* demand shocks and contemporaneous growth in firm- and product-specific outcomes support the identifying assumption. Additionally, the granularity of the product classification as well as the observed variation in foreign demand suggests that the conditions articulated by Borusyak et al. (2019) for consistency in share-shift instrumental variable (SSIV) estimations are satisfied in our context.

[3]We refer to within-product over time adjustments as "technology", though changes in managerial practices, fuel source, or quality of inputs could also contribute to changes in emission intensity within a product.

With a panel of 7,817 Indian exporters over the period 1995-2011, we find that foreign demand growth increased year-over-year CO_2 emissions growth at the firm and firm-product levels up to two years after the foreign demand shock, though point estimates at the firm-level are not statistically significant at conventional levels. By contrast, we find that foreign demand growth had statistically significant and positive impacts on firm-level sales and real output growth, and negative impacts on CO_2 intensity growth expressed in both value and physical units. Hence, while foreign demand increased the growth of emissions via the scale effect, it also lowered emissions growth via the technique effect. Additionally, we find that both export sales growth and domestic sales growth responded to foreign demand shocks, suggesting that domestic and export sales are complementary, as in Berman et al. (2015). The results are robust to alternative constructions of foreign demand shocks, treatment of outliers, and sample specifications. The fact that domestic sales growth increased and that emission intensity growth decreased with foreign demand growth indicates that it is not sufficient to merely chart the increase in exports from developing countries in order to learn about trade's impact on the environment.

At the product level, we find that foreign demand growth lowered the growth rate of emission intensity expressed both in value and in quantity within the first two years after a shock. Both results are statistically significant at least at the 5% level. In contrast to the firm-level emission intensity response, which could include product-mix adjustments, the reductions in emission intensity in quantity observed at the firm-product level could only result from technological change (broadly construed). Thus, we find direct evidence of the technological-upgrading channel posited in Forslid et al. (2018); Cui et al. (2015); Cherniwchan (2017); Gutiérrez & Teshima (2018). As with the firm level, results are robust to alternative constructions of foreign demand shocks, treatment of outliers, and sample specifications. Long difference estimates yield similar qualitative results. Heterogeneity analysis indicates that emission intensity reductions are driven by below-median-sales firms, but there appear to be no major difference between clean vs dirty firms or clean vs dirty sectors.

To interpret these results, we provide year-by-year estimates of the average contribution of each channel to the overall impact of foreign demand growth on firm and firm-product CO_2 emissions growth. At the firm level, we find that foreign demand growth on average over the period 1998-2011 raised CO_2 growth rates by 1.77 percentage points annually via the scale effect and reduced CO_2 growth rates by 0.72 percentage points via the technique effect. Hence, the technique effect mitigated 41% of the scale effect. Combining the two together, we find that foreign demand growth increased CO_2 emissions growth rates by 1.05 percentage points annually. While the overall impact is not statistically significant, we cannot rule out substantial impacts on CO_2 emissions growth rates.

At the product level, with output denominated in quantity, we estimate that foreign demand growth raised CO_2 growth rates via the scale effect by 0.61 percentage points annually and reduced them via the technique effect by 0.35 percentage points annually, for an average net increase of 0.26 percentage points. Hence, technological upgrading offsets more than half of the scale effect at the firm-product level. Since the figures are denominated in quantity, the technique effect nets out price effects. By contrast, equivalent calculations based on output denominated in value overstate the magnitude of the scale and technique effects on average by factors of 2 and 3, respectively.

Our paper mainly contributes to the literature on the impacts of trade on emissions either at the firm level (Martin, 2012; Cherniwchan, 2017; Gutiérrez & Teshima, 2018; Holladay, 2016; Forslid et al., 2018) or at the regional and national levels (Antweiler et al., 2001; Frankel & Rose, 2005; Bombardini & Li, 2020). Most of these papers estimate import competition impacts via trade liberalization, or aggregate effects from trade openness. We instead isolate the export demand side impacts to speak most directly to the question of the contribution of developing countries' trade in global CO_2 emissions. Some of these papers have explicitly considered the impact of exporting on firm-level emissions, but empirical estimates are usually based on cross sections (Batrakova & Davies, 2012; Holladay, 2016; Forslid et al., 2018; Barrows & Ollivier, 2018), which conflate causal impacts with selection effects. By contrast, our paper exploits firm-year and firm-product-year variation over time to identify the response of exporters to foreign demand shocks.

Within this literature, our paper is most closely related to four papers that also study emission response to foreign market demand or access. Bombardini & Li (2020) study how regional average export tariff reductions affect SO_2 and $PM_{2.5}$ concentrations in China. In contrast to this paper, we focus on the firm-level mechanisms of response. With firm-level data, Cherniwchan (2017) finds that SO_2 and $PM_{2.5}$ emissions levels from US manufacturing declined following tariff reductions on US goods entering Mexico. This finding would suggest that export demand shocks *lower* emissions levels, at least in the US. Shapiro & Walker (2018) find that changes in foreign competitiveness (a determinant of foreign demand) had little effect on the evolution of criteria air pollutant emissions from US manufacturing over the 1990s and 2000s. Key differences between our paper and these two studies are that we are able to condition on the export status of the firm, and that we can abstract from price effects. Finally, in a structural model, Caron & Fally (2020) find that if preferences are non-homothetic, a 1% increase in income everywhere would lead to only a 0.88% increase in global CO_2 emissions. Our work complements this structural approach as we study the consequences of demand growth on production decisions at the micro level.

Our work also relates to the literature on the carbon content of trade (Sato, 2014; Aichele & Felbermayr, 2015). Looking at the Kyoto protocol, 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. 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 it computes intensity in value and ignores impacts on domestic production, as well as the endogenous response of firm emission intensity to trade shocks. By examining firm-level production data, we can test whether these responses matter for measuring carbon emissions.

Finally, in studying the underlying mechanisms of emission intensity reductions, we relate to the large literature on the determinants of firm-level productivity. This literature tends to estimate 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, 2020). The literature hypothesizes that both technology adoption and product mix contribute to firm-level changes in productivity. Our estimated impacts on 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.

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 demand 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.[4] 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).[5]

Over the same period, exports from India also grew substantially. 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 this huge growth in both CO_2 emissions and exports, the case of India represents a good opportunity to study the environmental consequences of the rise of exports from developing countries.

2.2 Manufacturing Data

To study production and emissions at the firm and firm-product level, we rely on a dataset of large Indian manufacturers compiled by the Center for Monitoring the Indian Economy (CMIE). This dataset, which CMIE calls Prowess, is based on annual reports filed publicly by Indian companies, which CMIE collected and digitized. 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, 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 and production by product line

[4] Data come from the International Energy Agency.

[5] 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.

along with units of production.[6] From these statistics, we can compute unit values. Firms also report the share of revenue earned from exports. We only include firms that report some positive export value at some point over the period in our sample.

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.[7] 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 Amendments to the Companies Act, presumably due to government interests in energy security. These annual product-specific energy intensity measures allow us to track technological progress at the product-line level, which, to our knowledge, is not possible in any other dataset.

While for some firms, it might be costly to compute these product-specific energy-use statistics, we learned from telephone interviews that in many cases, different products are produced in different "wings" of an establishment and that firms often install "sub-meters" to record electricity use by wings. Also, different product lines can utilize different machines or boilers with specific (known) energy input requirements. Finally, firms also reported using resource planning softwares (for example, the Enterprise Resource Planning tool from the firm SAP) to record input use for each product line.[8] Still, given that the product-specific energy data is self reported and that compliance with the reporting regulation is not costless, measurement error is a potential concern. To address this concern, we test and reject likely sources of misreporting (See Appendix B for further details).[9]

[6] Even though 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), we treat the product text description supplied by the firm as the individuating identifier of products (see appendix B for details). To give a sense of the product-level detail, examples of products listed by firms include "rounds and bars", "alloy steel castings and steel ingots", "sponge iron" and "steel billets" (for a steel producer) ; "cotton yarn", "towels", "paper", "processed yarn", and "sulphuric acid" (for a textile producer) ; "sugar", "cement", "deadburnt magnesite", and "bricks" (for a sugar producer).

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

[8] The software developed by SAP allows firms to follow their sales and distribution, their materials management, their production planning, as well as their human capital management. (https://www.sap.com/india/)

[9] We hypothesize that misreporting could take three forms. First, seeking to minimize the cost of reporting, firms could report pure noise for product-specific energy intensity. Second, firms could use a simple rule of thumb like attributing energy consumption in proportion to sales or production. Third, firms could pick some value for energy intensity (either accurate or not) and report the same value every

To compute CO_2 emissions, we multiply energy consumption by source-specific CO_2 emissions factors and sum over energy types, as in recent studies (Martin, 2012; Marin & Vona, 2019; 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. 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 output denominated in both value and quantity, we compute emission intensity as kilotonne (kt) of CO_2 per rupee of sales and per physical unit. There are advantages and disadvantages to both measures. When measuring emission intensity in quantity, we must restrict units to be common across products and over time to make comparisons, which reduces statistical power. However, this measure is free of price effects, which could be quite important. Previous research suggests that firms set higher prices in richer, larger and more distant destination markets (Manova & Zhang, 2012; Johnson, 2012) and upgrade quality to reach foreign markets (Fan et al., 2015). By contrast, when emission intensity is computed per rupee of sales, no extra restriction needs to be made for the purpose of comparison; but, with the denominator expressed in value, changes in emission intensity will partially reflect endogenous price changes. This price effect does not have any environmental meaning: a higher quality/price T-shirt with the same unit CO_2 requirements neither increases nor decreases environmental impact relative to a lower-quality version.

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 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.

year. As reported in Appendix B.4, we test and reject all three types of misreporting.

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, Bev. & Tob.	562	1.556	0.276	5.590	99	1.215	2.868
Textiles	1398	0.959	0.331	3.782	413	0.864	1.727
Wood, Pulp & Paper	368	0.807	0.223	3.274	68	1.443	1.532
Chemicals	1403	1.463	0.262	6.758	239	0.883	3.814
Plastics & Rubbers	592	1.038	0.249	3.941	85	0.915	2.768
Non Metallic Mineral	440	2.091	0.318	3.551	79	1.869	2.597
Base Metals	1123	1.470	0.218	4.372	219	1.236	1.740
Machinery	1418	1.177	0.234	5.918	56	0.743	1.383
Transport Equipment	513	1.684	0.222	5.207	34	1.148	1.686
Total	7817	1.312	0.261	4.995	1292	1.054	2.426

Notes: Table reports total number of firms by industry along with industry average values per year in firm-level dataset (columns 1-4) and product-level dataset (5-7). Data covers the period 1995-2011. Firms are assigned to an industry based on the product that accounts for the greatest aggregate sales over the entire period. Sales are reported in billions of current year rupees.

We report descriptive statistics for both datasets in Table 1 by industry. Industry classifications are based on the CMIE product classification codes, which map closely to the more broadly used National Industrial Codes, at the aggregate level at least. The entire Prowess dataset spans the years 1989 - 2017, but our aggregate trade statistics only cover the years 1995 - 2011, so we only include this period in the analysis. After merging input and output variables, we have 7,817 exporting firms in the firm-level dataset. Coverage is fairly broad across the entire manufacturing sector. The average firm in the firm-level dataset generates 1.31 billion rupees in sales per year, or about 17 millions USD. The average firm also earns 26% of revenue from exports and produces just under 5 different products per year. In the product-specific dataset we have fewer firms overall – 1,292 exporters in total. The firm count is smaller in the product-specific dataset for two reasons. 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 1.05 billion rupees in sales on 2.42 products per year.

Table 2 presents descriptive statistics in levels (columns 1 to 5) and in growth rates (columns 6 to 10) for both datasets. Columns (1)-(2) and (6)-(7) report means and standard deviations of variables. Columns (5) and (8) report the number of observations for each variable, columns (4) and (9) the number of firm-products, and columns (5) and (10) the number of firms. In panel A, we find sales grew by 8.7% annually for the average firm in the sample, while emission intensity in value (in quantity) declined by 2.3% (resp. by 1.6%). Since units of quantity vary across products and firms, we cannot interpret the numbers for production and unit value. In the regressions, we will condition on common units within the firm over time. At the product level (panel B), we find that sales grew by 5.5% annually, on average. Emission intensity declined by 2.6% annually when expressed in value, and by 0.6% annually when computed in quantity.

2.3 Trade Data and Construction of Foreign Demand Shocks

Our goal is to relate exports, CO_2 emissions, production, and emission intensity to foreign demand shocks in trading partner markets. To do so, we employ techniques from the trade literature that suggest plausibly exogenous measures of foreign demand changes year to year.

Consider an Indian exporter producing a product j at date t. Using aggregate trade data, we observe over the entire period that Indian products j are sold in destinations $d \in \Omega_d$, where Ω_d is the set of all destinations India exports j to. Let X_{djt} denote the aggregate import flow in product j into destination d from all countries except India at time t. Thus, X_{djt} reflects the size of the (j, d) export market at time t. The intuition is that subsequent changes in destination d's imports of product j from the world (except from India) serve as a good proxy for the change in export demand faced by Indian firms operating in market j. By leaving India's own exports out of X_{djt}, we seek to purge the equilibrium values X_{djt} of supply side effects that might jointly affect Indian exports and production. We then compute the year-to-year change in (j, d) demand as the Davis-Haltiwanger growth rate and sum across destinations d weighted by the current-year relative importance of destination d for Indian firms:

$$\Delta FD_{jt,t-1} = \sum_{d \in \Omega_d} s_{djt,t-1} \left(\frac{X_{djt} - X_{djt-1}}{\frac{1}{2}(X_{djt} + X_{djt-1})} \right), \tag{1}$$

where $s_{djt,t-1} \equiv \frac{1}{2}[X^{\text{from India}}_{djt-1} / \sum_\ell X^{\text{from India}}_{\ell jt-1} + X^{\text{from India}}_{djt} / \sum_\ell X^{\text{from India}}_{\ell jt}]$ is the share of exports that flow to destination d in the total exports of j from India in the combined years

$t-1$ and t, with $\sum_d s_{djt,t-1} = 1$. The Davis-Haltiwanger growth rate operates similarly to a log first difference, but preserves observations when the shock switches from 0 to a positive number or vice versa (a notorious feature of international trade statistics), and takes a maximum value of -2 and 2.

The measure $\Delta FD_{jt,t-1}$ reflects the current-year shock (between $t-1$ and t) to foreign demand faced by Indian producers of j. While the change in X_{djt} can arguably be taken as exogenous, the export shares $s_{djt,t-1}$ are likely determined in part by unobserved shocks to production in India. To address this endogeneity concern, we compute an instrument for $\Delta FD_{jt,t-1}$ using base-period (t_0) Indian export weights:

$$\Delta Z_{jt,t-1} = \sum_{d \in \Omega_d} s_{djt_0} \left(\frac{X_{djt} - X_{djt-1}}{\frac{1}{2}(X_{djt} + X_{djt-1})} \right), \tag{2}$$

where $s_{djt_0} \equiv X_{djt_0}^{\text{from India}} / \sum_\ell X_{\ell jt_0}^{\text{from India}}$. We use for base-year weights s_{djt_0} the average values over 1995–1997 for the beginning of the sample (1995–2004), and the average over 2002–2004 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.

To construct firm-level foreign demand shocks, we weight product-level shocks by the relative importance of each product in the firm's total sales, and aggregate. For the current-year-weighted foreign demand shocks, we compute for firm i,

$$\Delta FD_{it,t-1} = \sum_{j \in \Omega_{it,t-1}} r_{ijt,t-1} \Delta FD_{jt,t-1}, \tag{3}$$

where $r_{ijt,t-1} \equiv \frac{1}{2}[V_{ijt-1}/(\sum_{h \in \Omega_{it-1}} V_{iht-1}) + V_{ijt}/(\sum_{h \in \Omega_{it}} V_{iht})]$ is the sales share of product j in firm i's total sales in the combined years t and $t-1$, and $\Omega_{it,t-1}$ is the set of products offered by firm i in years t and $t-1$. For the base-year-weighted foreign demand instruments, we compute:

$$\Delta Z_{it,t-1} = \sum_{j \in \Omega_{it_0}} r_{ijt_0} \Delta Z_{jt,t-1} \tag{4}$$

where $\Delta Z_{jt,t-1}$ is computed in (2), and $r_{ijt_0} \equiv V_{ijt_0} / \sum_{h \in \Omega_{it_0}} V_{iht_0}$ the sales share of product

j in firm i's total sales in base year t_0, and Ω_{it_0} is the set of products produced in base year t_0. In practice, 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. This ensures that product sales shares do not respond to contemporaneous foreign demand shocks.

To compute demand shocks, we take international trade flows from CEPII's BACI dataset, which 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 then merge these shocks to Prowess via the CMIE product classification code. CMIE classifies product names (reported by the firms) according to a 16-digit code of their own design. There are 3,276 CMIE product codes to which we can assign foreign demand values. To exploit differential growth rates in foreign demand at a granular level – across varieties of products within an industry – we construct our own cross-walk between the CMIE product codes and HS revision 1996 (see appendix B.5 for details).[10] In a slight abuse of notation, we use j to index either HS product categories or product codes in Prowess. Industry average indices of foreign demand are plotted in Figure A.1 using BACI product codes (Panel A) and Prowess product codes (Panel B). In both panels, we see that foreign demand increased dramatically from the vantage point of Indian firms over the period. The industry that saw the largest increase was nonmetallic minerals, which increased between 4- and 6-fold.[11]

3 Empirical Strategy

To identify the impact of foreign demand on firm and firm-product level outcomes, we estimate a difference-in-difference model in growth rates, instrumenting current-year-weighted foreign demand shocks with base-year-weighted shocks and controlling for arbitrary industry-

[10] An alternative mapping relies on the CMIE cross-walk between their 16-digit codes and 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. Hence, the resulting variation would be fairly constant across products within an industry.

[11] This growth was mostly driven by the demand for concrete. Results below are robust to excluding this outlier. See Appendix Figures A.7 and A.10.

by-year trends:[12]

$$\Delta Y_{ikt,t-1} \;=\; \sum_{\tau=-z'}^{z} \beta_\tau \Delta FD_{it-\tau,t-1-\tau} + \gamma \alpha_k \chi_{t,t-1} + \epsilon_{ikt} \tag{5}$$

where $\Delta Y_{ikt,t-1}$ is an outcome for firm i in industry k measured using the Davis-Haltiwanger growth rate between t and $t-1$: $\Delta Y_{ikt,t-1} = (Y_{ikt} - Y_{ikt-1})/[\frac{1}{2}(Y_{ikt} + Y_{ikt-1})]$, and where we instrument each $\Delta FD_{it-\tau,t-1-\tau}$ by $\Delta Z_{it-\tau,t-1-\tau}$. We include an industry indicator α_k interacted with year interval fixed effects $\chi_{t,t-1}$ to capture industry-specific trends, such as labor regulations, income shocks, and general technological progress.[13] 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.

At the product level, we estimate

$$\Delta Y_{ijkt,t-1} \;=\; \sum_{\tau=-z'}^{z} \beta_\tau \Delta FD_{jt-\tau,t-1-\tau} + \gamma \alpha_k \chi_{t,t-1} + \epsilon_{ijkt} \tag{6}$$

where $\Delta Y_{ijkt,t-1}$ is the firm-product equivalent to firm-level growth rates $\Delta Y_{ikt,t-1}$, and where we instrument each $\Delta FD_{jt-\tau,t-1-\tau}$ by $\Delta Z_{jt-\tau,t-1-\tau}$. We include an industry indicator α_k interacted with year interval fixed effects $\chi_{t,t-1}$ to capture industry-specific trends. Note that specification (6) identifies a causal impact from changes in product-specific demand shocks $\Delta FD_{jt,t-1}$, not from firm-level demand shocks $\Delta FD_{it,t-1}$.

As explained in Borusyak et al. (2019), the exogeneity of shift-share instruments can stem from the exogeneity of either the shares or the shocks. In the "shares view", we would need to assume that unobserved determinants of year-to-year growth rates of exports, sales, and emissions are unrelated to the choice of initial product offerings of firms, conditional on industry trends. This assumption seems unlikely in our context. Indeed, any product-specific trend would violate the assumption. For instance, import competition

[12] Our first-difference estimator yields identical results to a fixed effect model specified in levels when there are only two periods. For more than two periods, the two models do not in general yield identical results (Wooldridge, 2002). If the outcome variable is stationary, then both estimators are consistent. However, when the outcome variable is non-stationary, the fixed effect estimator may be biased. In macro studies, it is widely acknowledged that output is non-stationary, and hence should be expressed in first difference before estimating time series models. The practice is less common in micro studies, such as ours; though recent work in international trade treats firm-level outcomes as non-stationary, hence favoring first-difference models (Mayer et al., 2020; Aghion et al., 2019).

[13] Unlike in Mayer et al. (2020) and Aghion et al. (2019), our exposure shares s_{djt_0} and r_{ijt_0} sum to one, which implies that we do not need to control for them in our regressions (as explained in Borusyak et al. 2019).

has been shown to increase productivity in Indian manufacturing (De Loecker et al., 2016). Thus, firms heavily specialized in products that saw greater import tariff reductions over the period would likely have grown faster than other firms, even without positive foreign demand shocks. This is true even if the omitted shock is uncorrelated with the studied shock (foreign demand, in our case).[14]

Instead, we adopt the view that foreign demand shocks are as good as randomly assigned with respect to firm and product-level growth outcomes, after controlling for industry trends. Hence, we rest identification on two assumptions formulated by Borusyak et al. (2019): (1) $\Delta FD_{jt,t-1}$ are orthogonal to unobserved determinants of firm and product-level growth rates, conditional on industry trends, and (2) there are many uncorrelated shocks or shock residuals.[15] In this case, even if initial product shares are correlated with future growth rates through some omitted variable, our weighted average foreign demand shocks are exogenous to unobserved determinants of firm and firm-product outcomes.

Considering the quasi-random assignment of shocks, the main threat to identification is that some unobservable firm characteristic could affect the trend of firm-level outcomes and correlate with the shocks. For example, firms with better market research departments may better identify growing foreign markets *and* may grow faster for reasons independent from trade. This would lead to a mechanical correlation between positive foreign demand shocks and firm-level growth. To address this concern, we run falsification tests of correlations between *future* foreign demand growth with current growth rates for all our dependent variables at both the firm and firm-product levels, as recommended by Borusyak et al. (2019).[16] If omitted variables connect growing firms to growing foreign markets, then we would expect to see differential trends for firms that will see larger foreign demand shocks in the future, even before the demand shocks are realized.

With respect to the second assumption from Borusyak et al. (2019), we must verify that we have a large number of shocks and sufficient dispersion in their average exposure. Over the period, we count 8,391 product-level foreign demand growth rates $\Delta FD_{jt,t-1}$, which validates the fact that shocks are available in large numbers. Additionally, we find substantial variation in the shocks. In Appendix Figure A.2, we plot the cumulative distri-

[14]See Appendix A.3 in Borusyak et al. (2019) for the demonstration.

[15]The formal requirements are that (1) the shocks $\Delta FD_{jt,t-1}$ must have constant expected value conditional on exposure-weighted averages of firm or product-level unobservables, average shock exposure, and a vector of shock-level observables, and (2) the expectation of the sum of average exposure shares squared must tend toward zero and product-level shocks must be uncorrelated with each other after stripping out industry-period fixed effects (see assumptions 3 and 4 and proposition 4 in Borusyak et al. 2019).

[16]Mayer et al. (2020); Aghion et al. (2019) run the same test to check for omitted variables.

bution of Davis-Haltiwanger growth rates in foreign demand (left panel) and the residual growth rates after stripping out product code and industry-by-year fixed effects (right panel). The inner quartile of absolute (residualized) current-year-weighted foreign demand growth ranges from -.043 (-.062) to .125 (.063), indicating that, even after controlling for average levels and arbitrary industry trends, there is still a substantial amount of variation in year-over-year product-specific foreign demand growth.

Finally, inference for SSIV regressions must account for the correlation in the error terms across observations with similar exposure profiles (Borusyak et al., 2019). In our context, firms with similar product mix would have "correlated exposure." To address correlation in the error terms over time and across units, we cluster standard errors at the 4-digit product-group category that corresponds to the main activity of the firm over time.[17] Borusyak et al. (2019) refers to these standard errors as "exposure robust," a term we adopt as well.

4 Results

In this section, we estimate the response of CO_2 emissions to foreign demand shocks via instrumental variables. We begin at the firm level, and then proceed to the firm-product level.

4.1 Firm-Level Response to Foreign Demand Shocks

We first show that our foreign demand shocks impact firms' total sales and production. We estimate equation (5) via GMM, instrumenting $\Delta FD_{it-\tau,t-1-\tau}$ by $\Delta Z_{it-\tau,t-1-\tau}$ for $\tau \in \{-2, 3\}$, and taking the growth rate of total sales, respectively domestic and foreign sales, unit value and total output (in physical units) as our outcome variable $\Delta Y_{ikt,t-1}$. When estimating effects on output in physical units and unit values, we include only firm-year observations for which all products produced by the firm in consecutive years are denominated in the same units. Most physical units are reported in tonnes, so this restriction does not drop many firms in the regression. Since we include 3 years of lagged trade shocks and 2 years of leads, the outcome data span years 1999 - 2009, while trade shocks span 1995 - 2011. We exclude the top and bottom 1% of all outcome values and trade shocks, and control for industry-by-period fixed effects in all regressions.

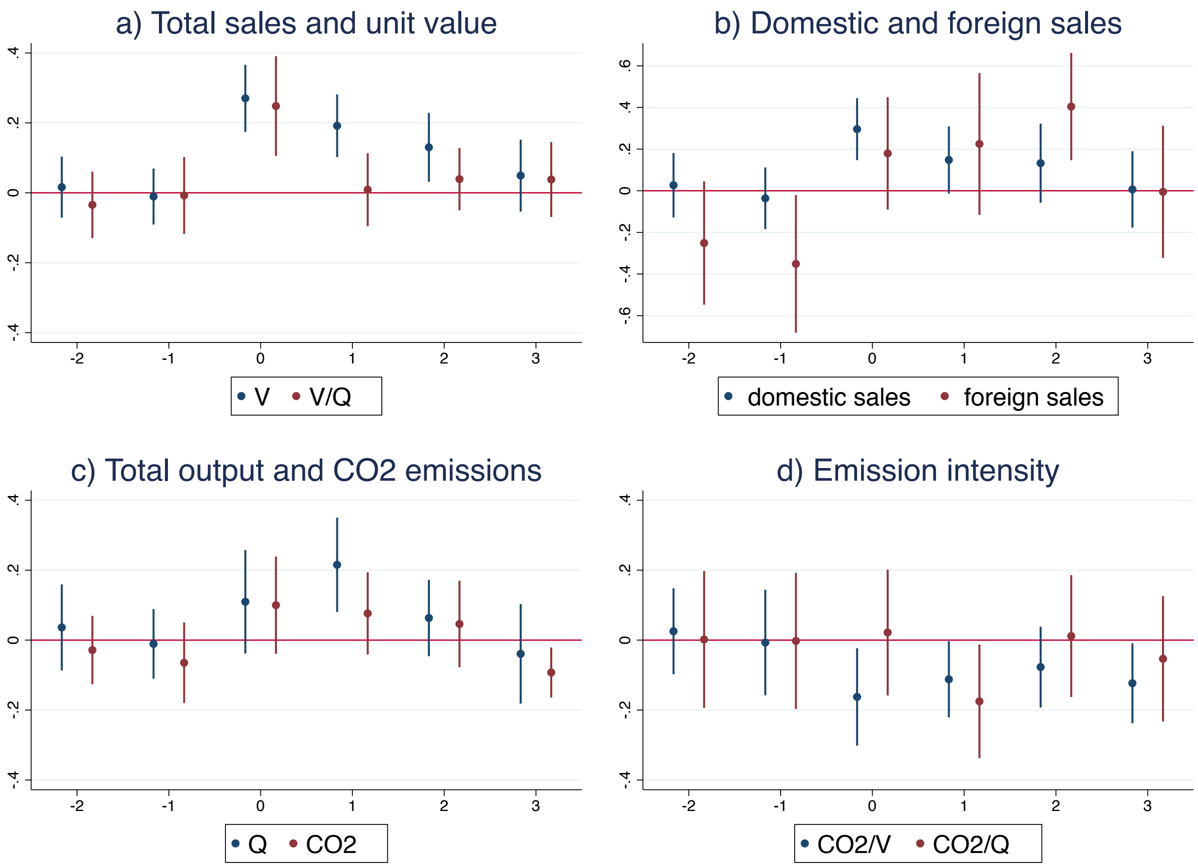
[17] The CMIE product code follows a "tree-structure", so that all products that begin with the same string of digits belong to a common family.

We report results for estimated coefficients β_τ (dots) and their 95% confidence intervals (bars) graphically in Figure 1 for $\tau = -2, ..., 3$. The β_τ coefficients for $\tau > 0$ represent a response of the outcome variable $\Delta Y_{ikt,t-1}$ to a demand shock $\Delta FD_{ikt-\tau,t-1-\tau}$ τ years earlier, whereas coefficients for $\tau < 0$ represent a response to a demand shock $-\tau$ years later. Note that in India the accounting year spans from April 1st of year t to March 31st of year $t+1$, while trade data spans from January 1st to December 31st. Thus, the coefficient associated with $\tau = 1$ corresponds to the main contemporaneous effect (including 9 months of synchronous changes) whereas the coefficient associated with $\tau = 0$ corresponds to the early month effect (including 3 months of synchronous changes).

Panel a) of Figure 1 shows a strong contemporaneous and lagged response in total sales to foreign demand shocks. Based on a sample of 20,031 firm-year observations, our point estimates imply that a 1 percentage point (p.p.) increase in foreign demand growth (e.g., from 5% to 6%) led to a 0.27 p.p. increase in firm-level sales growth in period $\tau = 0$, 0.19 p.p. increase in period $\tau = 1$, and 0.13 p.p. increase in period $\tau = 2$, all statistically significant at the 1% level. The Kleibergen-Paap rk LM statistic for the estimation is 19.03, which easily surpasses the critical value necessary to reject that the correlation matrix between endogenous and exogenous regressors has less than full rank. The impact of *future* shocks (β_{-1} and β_{-2}) on current growth rates is small and statistically indistinguishable from zero, which is inconsistent with differential pre-trends. This lends support to the plausibility of the identification assumption of our specification. We also find similar patterns in both the OLS (Figure A.5) and the reduced form (Figure A.4). These results essentially replicate the findings in Mayer et al. (2020) and Aghion et al. (2019) for Indian firms: foreign demand shocks raised firm-level sales.

We next examine to what extent this sales response reflect changes in real production vs price effects. In panel c) of Figure 1, with a sample of 13,249 observations, we estimate that a 1 percentage point increase in foreign demand growth led to a 0.11 p.p. increase in real output growth in period $\tau = 0$ and 0.22 p.p. increase in period $\tau = 1$. The point estimate for β_1 is statistically significant at the 1% level. The Kleibergen-Paap rk LM statistic for the estimation is 18.12; and, again, small and statistically insignificant estimates of β_{-1} and β_{-2} imply no differential trends prior to demand shocks. Taking unit value as the outcome variable, we find in panel a) a strong and statistically significant impact of contemporaneous shocks ($\tau = 0$), with small and statistically insignificant estimates for all other τ's. Hence, part of the sales effect can be explained by higher unit values, though there remains a strong impact on physical production as well.

Figure 1: Firm-level results, Instrumental Variables



Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where current-year-weighted trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group level. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 10.96 (for CO2/Q) to 19.03 (for V). Number of firm-year observations range from 6,463 (for CO2/Q) to 20,031 (for V).

Next, we decompose the total sales effect into foreign vs domestic sales. Most leading models of international trade (such as Mayer et al. 2020; 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 empirical work suggests that complementarities 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.[18] Alternatively, if firms face short-run credit constraints, then a firm might not be able to increase production year-to-year to meet higher foreign demand. Instead, 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. Thus, positive trade shocks could produce either positive or negative effects to domestic sales.

In panel b) of Figure 1, with a sample of 16,358 firm-year observations, we find that positive foreign demand shocks tend to raise export sales. Point estimates imply that a 1 percentage point increase in foreign demand growth led to a 0.17 p.p. increase in export growth in period $\tau = 0$, 0.22 p.p. increase in period $\tau = 1$, and 0.40 p.p. increase in period $\tau = 2$. The parameters β_0 and β_1 are imprecisely estimated, but the estimate of β_2 is statistically significant at the 1% level. The Kleibergen-Paap rk LM statistic for the estimation is 18.96. There appears to be a negative impact from the lead trade shock (β_{-1}), which could indicate differential trends in product-specific factors, though we find no such pattern in the reduced form (see Figure A.4).

Turning to domestic sales, with a sample of 19,742 observations, we estimate that a 1 p.p. increase in foreign demand growth led to a 0.30 p.p. increase in period $\tau = 0$ and 0.15 p.p. increase in period $\tau = 1$, statistically significant at the 1% and 10% levels, respectively. The Kleibergen-Paap rk LM statistic for the estimation is 18.85. Impacts from future shocks are small and statistically insignificant. The positive domestic sales response is consistent with the liquidity constraint channel posited by Berman et al. (2015).[19] Our result reveals complementarities between domestic and foreign sales, and indicates that the scale effect from domestic sales should be accounted for in evaluating the environmental impacts of trade shocks.

Finally, we turn to CO_2 emissions and CO_2 emission intensity at the firm level. Panel c) reports estimates for CO_2 emissions, while panel d) reports impacts on emission intensity in value (CO2/V) and in quantity (CO2/Q). When treating CO2/Q as the dependent variable, we restrict to constant units across products and consecutive years. All the same restrictions and controls apply as in panels a) and b). Responses of CO2/V to foreign de-

[18] For instance, when foreign demand expands, the increase in exports could allow firms to use their order book as a collateral or as a signal to obtain external financing.

[19] Complementarities in domestic and foreign sales could also arise if firms use the same technology to manufacture products sold in different markets. Indeed, if foreign demand growth stimulates the adoption of a marginal-cost-saving technology as in Bustos (2011), firms see their marginal costs of production decline irrespective of the market where they sell their products. These lower marginal costs may induce higher sales in both the domestic and the foreign markets.

mand will include price impacts, whereas impacts on CO2/Q reflect real emission savings. Both measures can reflect product-mix effects (across-product shifts) and technological effects (within product-line effects over time), such as fuel switching or technological upgrades.

In panel c), with a sample of 10,116 observations, we find that CO_2 emissions growth increased with positive foreign demand shocks, but that the impacts are not statistically significant (p-values .15 and .20 for $\tau = 0$ and 1, respectively). By contrast, growth in both CO2/V and CO2/Q fell with foreign demand shocks, and point estimates are statistically significant. With samples of 10,120 and 6,463, respectively, we estimate that a 1 percentage point increase in foreign demand growth led to 0.16 and 0.11 p.p. lower growth in periods $\tau = 0$ and $\tau = 1$ for CO2/V, and to 0.18 p.p. lower growth in period $\tau = 1$ for CO2/Q, all statistically significant at the 5% level. The Kleibergen-Paap rk LM statistic are 11.99 and 10.96. This result indicates that firms endogenously adjusted emission intensity to foreign demand, even when emission intensity is measured per physical unit of output.[20] For all firm-level results, we present robustness checks in the Appendix in which we alternatively suppress the top and bottom 2% of firm-year outcome values and demand shocks (Fig. A.6), and exclude nonmetallic minerals (Fig. A.7). We find that results are qualitatively the same, though we note that the 95% confidence interval for the emission intensity in quantity result includes zero in the case where we exclude top and bottom 2% outliers.

The emission intensity reductions measured per unit of physical output observed in panel d) could owe to the product-mix effect hypothesized in Barrows & Ollivier (2018), Cherniwchan (2017) and Cherniwchan et al. (2017), and/or to the technology channel posited by Forslid et al. (2018); Cui et al. (2015); Cherniwchan (2017); Gutiérrez & Teshima (2018); Shapiro & Walker (2018). Presumably, the technology channel would deliver greater benefit to the environment, as technological investment has an element of irreversibility, whereas product mix can be adjusted year to year. In order to isolate the technological channel, we can estimate impacts at the firm-product level using our detailed dataset, holding product-mix and price channels fixed.

[20] Previous research has tied various measures of trade to emission intensity in value (CO2/V) at the firm level. For example Cherniwchan (2017) estimates that NAFTA-induced reductions in tariffs on US goods entering Mexico resulted in lower SO_2 and $PM_{2.5}$ emission intensity in value at US plants, Cui et al. (2015) find that exporters have lower emission intensity in value than non-exporters, also in the US, and Gutiérrez & Teshima (2018) find that increased import competition in Mexico leads to reductions in plant-level energy intensity in value. But we are not aware of any other research that connects trade measures to emission intensity *in quantity*.

4.2 Firm-Product-Level Response to Foreign Demand Shocks

At the firm-product level, we estimate equation (6) by GMM, instrumenting $\Delta FD_{jt-\tau,t-1-\tau}$ by $\Delta Z_{jt-\tau,t-1-\tau}$ for $\tau \in \{-2, 3\}$. As before, we exclude the top and bottom 1% of trade shocks and outcome values, we control for industry trends, and we compute exposure-robust standard errors by clustering on both the 4-digit product group (to account for similar exposure of firms producing similar products) and on firm (to account for the fact that the firm-level profile in trade exposure across products matters).

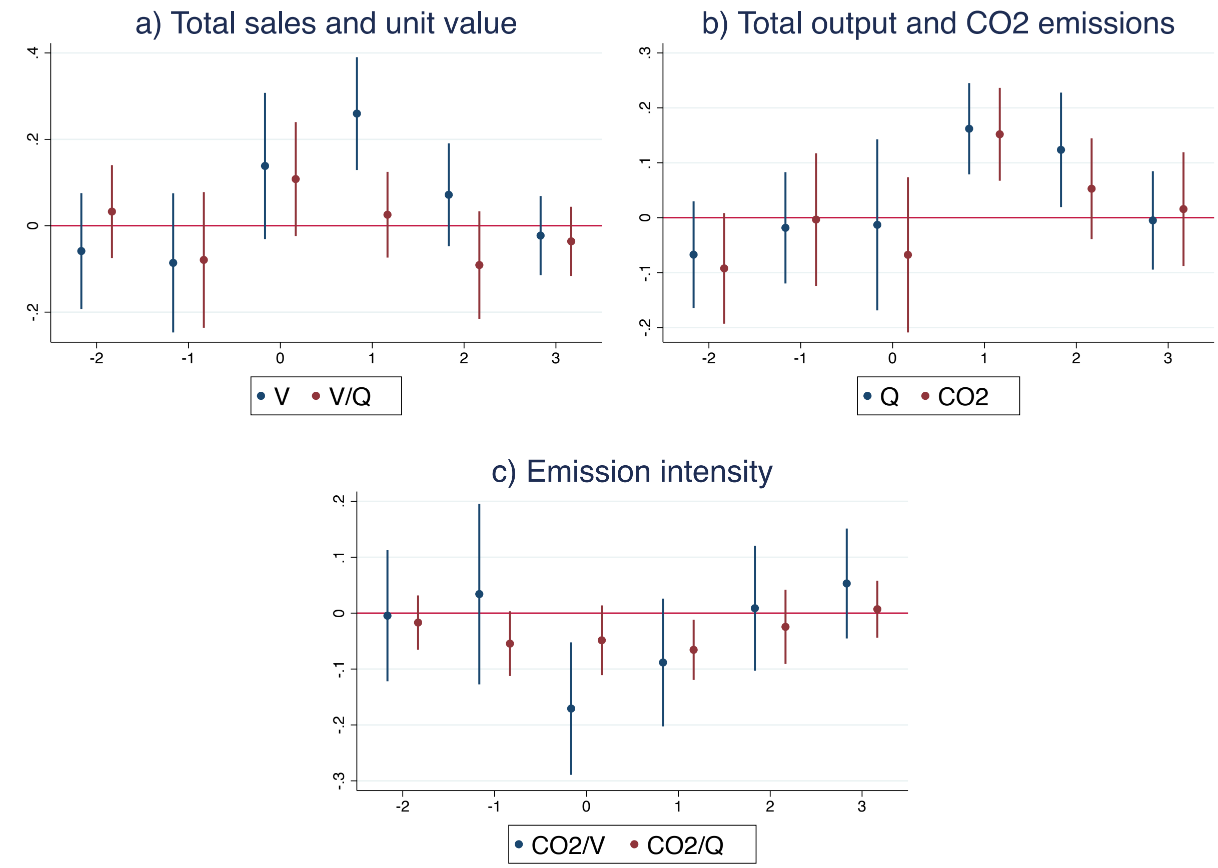
Figure 2 presents results graphically. Panels a) and b) show statistically significant and economically meaningful responses of firm-product sales (V) and output (Q) to foreign demand shocks. With a sample of 7,835 firm-product-year observations across 1,501 firm-products, we find that a 1 percentage point (p.p.) increase in foreign demand growth led to 0.14 and 0.26 p.p. higher sales growth in periods $\tau = 0$ and $\tau = 1$, statistically significant at the 10% and 1% levels, respectively. The Kleibergen-Paap rk LM statistic is 7.57, which again easily surpasses the critical value necessary to reject that the correlation matrix between endogenous and exogenous regressors has less than full rank. For physical production, we find that a 1 p.p. increase in foreign demand growth led to 0.16 and 0.12 p.p. higher growth in periods $\tau = 1$ and $\tau = 2$, statistically significant at the 1% and 5% levels, respectively. The Kleibergen-Paap rk LM statistic is 7.96. For production and sales, we can reject pre-trends.[21] Unit values increased slightly, especially in period $\tau = 0$, though the point estimate is not statistically significant at the 10% level.

For CO_2 emissions, with a sample of 8,046 firm-product-year observations across 1,539 firm-products, we find in panel b) that a 1 p.p. increase in foreign demand growth led to 0.15 and 0.05 p.p. higher emissions growth in periods $\tau = 1$ and $\tau = 2$. The former estimate is statistically significant at the 1% level, while the latter has a p-value of 0.25. The Kleibergen-Paap rk LM statistic is 7.99. Again, we can rule out pre-trends.

Lastly, panel c) presents the results for emission intensity at the firm-product level. We estimate that a 1 p.p. increase in foreign demand growth led to 0.17 p.p. lower growth in emission intensity in value in period $\tau = 0$ (significant at the 1% level), and to 0.06 p.p. lower growth in emission intensity in quantity in period $\tau = 1$ (significant at the 1% level), in samples of 7,750 and 8,032 observations, respectively. Kleibergen-Paap rk LM statistic are 7.72 and 7.89. The impact on emission intensity in value includes any endogenous price adjustment at the firm-product level. But the impact on emission

[21] Even with a non-trivial impact from lead shocks for sales, the negative sign is hard to reconcile with a differential pre-trend explanation of the post-shock results.

Figure 2: Firm-product-level results, Instrumental Variables



Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (6) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for total output (Q) and total CO_2 emissions (CO2), and panel c) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm-product level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where current-year-weighted trade shocks are instrumented by $\Delta Z_{jt-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group and firm levels. Top and bottom 1% of firm-product-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 7.64 (for V) to 8.07 (CO_2). Number of observations range from 7,835 (for V) to 8,046 (CO_2).

intensity in quantity abstracts from product-mix and price effects, and thus confirms that technological adjustments contribute to lower average firm-level emission intensity.[22] This result, and all other firm-product-level results, are robust to excluding nonmetallic minerals

[22]We also explore long difference specifications to allow more time for adjustment, as in the climate change literature (Dell et al., 2012; Burke & Emerick, 2016). See Table A.1 for results. Signs and magnitudes are similar, but with smaller sample size, estimates are not statistically significant at the 10% level for emission intensity in quantity.

(Fig. A.10), and alternatively suppressing the top and bottom 2% of firm-year outcome values and foreign demand shocks (Fig. A.11).

5 Heterogeneous Impacts

In this section, we study heterogeneous impacts from foreign demand shocks on single-product versus multi-product firms, on large vs small firms, on clean vs polluting firms within an industry, and on firms belonging to heavily polluting industries vs belonging to cleaner industries.

5.1 Multi-product versus single-product firms

Firms that supply multiple products may adjust their average emission intensity by changing their product mix. In fact, if each product within a firm generates a specific amount of CO_2 emissions per unit of output, then adjusting product mix can affect firm emission intensity without any investment made at the firm-product level. However, multi-product firms may also be intrinsically different from single-product firms: multi-product firms are often larger, more productive, and less likely to face liquidity constraints. We define single-product firms as firms that only produce a single product over the entire period of analysis. We test for heterogeneous responses to foreign demand shocks for single-product and multi-product firms by running equations (5) and (6) for each sub-sample.

Panels A and B in Table A.2 report the results from these regressions for the sub-samples of multi-product vs single-product firms, respectively, while focusing only on the contemporaneous shock ($\tau = 1$). Indeed, we found earlier that most of the firms' response is contemporaneous. We only report results for total sales (V), output in quantity (Q), emission intensity in value (CO_2/V) and in quantity (CO_2/Q) both at the firm level and at the firm-product level, since we search for heterogeneity in firms' response that can impact their emission intensity. Overall, we do not find that multi-product firms behave significantly differently to single-product firms. The firm-level response in emission intensity is negative but statistically significant in neither sub-samples, whereas the firm-product response in emission intensity is negative and statistically significant at the 5% level only for CO_2/V for multi-product firms. This suggests that multi-product firms could have a stronger price effect that will influence the decline in emission intensity in value, but we cannot conclude that single-product and multi-product firms show heterogeneous responses in technology upgrading.

5.2 Heterogeneity in size and in emission intensity

Large firms may be more prone to make technological investments as they face lesser financial constraints or have better access to credit. We test for heterogeneous response to foreign demand shocks by interacting these shocks with a dummy variable identifying firms that have average annual total sales above the median within their industry. Panel C in Table A.2 reports the results from estimating equations (5) and (6) for contemporaneous shocks ($\tau = 1$) and considering heterogeneity in size. We find that most of our outcome variables, even total sales and production, lose their statistical significance when we contrast small versus large firms, except firm-product emission intensity in quantity for which we observe heterogeneous responses (significant at the 1% level). Results in column 8 of panel C indicate that emission intensity reductions at the firm-product level were driven by below-median-sales firms.

We also explore heterogeneity in terms of emission intensity either across firms within a sector or across sectors. Panel D of Table A.2 reports the results from estimating equations (5) and (6) for contemporaneous shocks ($\tau = 1$) and interacting foreign demand shocks with a dummy variable identifying firms that have average annual total emission intensity (in value) above the median within their industry. Panel E reports the results from interacting demand shocks with a dummy variable identifying industries with average annual total emission intensity (in value) above the median. In either case, we do not find any significant heterogeneous response in emission intensity. We observe a significant contrast only for production at the firm level and sales at the product level for clean vs dirty firms within an industry: dirty firms respond less strongly to foreign demand shocks.

6 Counterfactuals

To quantify the impacts of trade shocks over the period 1995 - 2011 and assess the relative magnitude of the different channels through which they influenced emissions, we build counterfactual emission growth rates firm by firm or product by product and compare them to observed growth rates. We first identify the full trade effect on emissions growth by comparing observed growth rates with equivalent metrics that counterfactually set foreign demand growth to 0. We then decompose the contribution of a scale effect (due to the growth in total sales), a technique effect, and an export sales effect.

6.1 Computing Counterfactual Growth Rates

To compute the magnitude of the full trade effect – that is, the overall causal impact of foreign demand shocks on CO_2 emissions growth in Indian firms – we compute counterfactual growth rates year by year for firms and firm-products as

$$\Delta Y'_{ikt,t-1} = \Delta Y_{ikt,t-1} - \sum_{\tau=0}^{2} \widehat{\beta}_\tau * \Delta FD_{it-\tau,t-1-\tau} \quad (7)$$

$$\Delta Y'_{ijkt,t-1} = \Delta Y_{ijkt,t-1} - \sum_{\tau=0}^{2} \widehat{\beta}_\tau * \Delta FD_{jt-\tau,t-1-\tau} \quad (8)$$

where $\widehat{\beta}_\tau$ are the instrumental variables estimates from Figures 1 and 2. We consider two years of lagged trade shocks and no leads; hence, the period of analysis runs from 1998 to 2011.[23] For the full trade effect, Y corresponds to CO_2 emissions. Denoting the counterfactual emissions growth rates obtained from (7) and (8) by $\Delta CO2'_{ikt,t-1}$ and $\Delta CO2'_{ijkt,t-1}$, we can measure the *full trade effect* – which includes a scale effect and a technique effect (encompassing technology and product mix effects at the firm level, and reflecting only technology at the firm-product level) – by comparing these counterfactual growth rates to the observed growth rates.

Next, we compute counterfactual emissions growth rates under different scenarios to decompose the contribution of each channel. In the first scenario, we assume that trade shocks result only in a scale effect measured in physical output growth. Implicitly, we thus assume that any improvement in emission intensity in quantity observed over the period would have happened anyway. To do so, we compute counterfactual levels of an outcome variable Y by inverting the Davis-Haltiwanger growth rate:

$$Y'_{ikt} = Y'_{ik,t-1} * \frac{2 + \Delta Y'_{ikt,t-1}}{2 - \Delta Y'_{ikt,t-1}} \; , \; Y'_{ijkt} = Y'_{jik,t-1} * \frac{2 + \Delta Y'_{ijkt,t-1}}{2 - \Delta Y'_{ijkt,t-1}}, \quad (9)$$

starting at observed base year t_0 levels, and then proceeding iteratively year after year.[24] Using the counterfactual levels for output (Q'_{ikt} and Q'_{ijkt}) obtained from (9), we compute

[23] We consider two years of lagged trade shocks because, in our results above, we observe that statistically significant impacts are concentrated in the first two years after the shock.

[24] These measures explode as the counterfactual growth rate approaches -2 or 2, so we censor growth rates at -1.95 and 1.95.

counterfactual CO_2 levels as:

$$CO2''_{i,t} = \left(\frac{CO2}{Q}\right)_{it} * (Q'_{it}) \ , \ CO2''_{ij,t} = \left(\frac{CO2}{Q}\right)_{ijt} * (Q'_{ijt}), \tag{10}$$

where $(CO2/Q)_{it}$ and $(CO2/Q)_{ijt}$ are the observed emission intensities in quantity for firm i and firm-product ij in year t. After computing these levels (10) for all years, we take the Davis-Haltiwanger growth rates. The difference between these counterfactual growth rates and the observed growth rates identifies the *scale effect* of trade measured in quantity. Additionally, the difference between these counterfactuals and the full trade effect counterfactuals ($\Delta CO2'_{ikt,t-1}$ and $\Delta CO2'_{ijkt,t-1}$) identifies the technique effect in quantity.

In the second scenario, we assume that trade shocks result only in a scale effect measured in value. We proceed as before, computing the following counterfactual levels:

$$CO2'''_{i,t} = \left(\frac{CO2}{V}\right)_{it} * (V'_{it}) \ , \ CO2'''_{ij,t} = \left(\frac{CO2}{V}\right)_{ijt} * (V'_{ijt}), \tag{11}$$

where $(CO2/V)_{it}$ and $(CO2/V)_{ijt}$ are the observed emission intensities in value in t, and V'_{it} and V'_{ijt} are counterfactual total sales computed from (9). Comparing the resulting counterfactual growth rates to observed growth rates identifies the scale effect of trade in value, while comparison to $\Delta CO2'_{ikt,t-1}$ and $\Delta CO2'_{ijkt,t-1}$ identifies the technique effect expressed in value. These measures obviously include price effects, but they are useful benchmarks to consider for settings in which quantities are unobserved.

The third scenario aims at isolating the effect of trade-induced export growth on emissions. For this scenario, we assume that trade only impacts export sales, thereby attributing all domestic sales growth and emission intensity in value growth to factors other than foreign trade. As exports are only observed at the firm level, we compute

$$CO2''''_{i,t} \ = \ \left(\frac{CO2}{V}\right)_{it} * (EXV'_{it} + DOMV_{it}), \tag{12}$$

where $(CO2/V)_{it}$ and $DOMV_{it}$ are observed emission intensity in value and domestic sales for firm i in year t, and EXV'_{it} is counterfactual export sales computed from (9). We then compute growth rates. Comparing these counterfactuals to observed growth rates identifies the *export sales effect* of trade.

To account for uncertainty in the estimation of the β_τ's, we build confidence intervals for these trade effects by randomly sampling β_τ from a normal distribution with mean and standard deviation equal to the point estimates and standard errors of $\widehat{\beta_\tau}$ from Figures 1 and 2, and then recomputing $\Delta CO2'_{ikt,t-1}$, $\Delta CO2''_{ikt,t-1}$, $\Delta CO2'''_{ikt,t-1}$,$\Delta CO2''''_{ikt,t-1}$, $\Delta CO2'_{ijkt,t-1}$,$\Delta CO2''_{ijkt,t-1}$, $\Delta CO2'''_{ijkt,t-1}$ for each draw. We then compute the lower (upper) bound of a confidence interval for each average counterfactual growth rate as the 2.5^{th} (97.5^{th}) percentile of the distribution resulting from these random draws.

6.2 Results

Figure 3 reports graphically the decomposition of the different firm-level channels for quantity (top) and value (bottom) year by year. To aggregate over firms, we compute a sales-weighted average of annual growth rates. Observed emission growth rates are reported by horizontal red lines. Average trade shocks are reported with diamonds. Dark gray bars indicate the full trade effect, which is positive if the bar is below the corresponding red line and negative otherwise. These estimate are based on regression coefficients from Figure 1 panel (c), taking emissions as the outcome variable. Light gray bars indicate the scale effect of trade. These estimate allow for endogenous change to scale, but attribute any change in emission intensity to factors other than foreign demand. Black bars indicate the export sales effect of trade. Confidence intervals are depicted with gray vertical lines.

In Figure 3, we find that foreign demand growth increased emissions growth rates substantially via the scale effect. In the top panel – in which outputs are denominated in quantities – we calculate that observed emissions increased annually at a rate of 3.56% on average. By contrast, looking at the light gray bars, we estimate that emissions *would have* grown at an average annualized rate of only 1.78% absent foreign demand growth, if we attribute all emission intensity growth changes to factors other than foreign demand. Hence, we find that foreign demand growth increased emissions growth rates via the scale effect by 1.77 percentage points per year, or almost 50% of the observed emissions growth rate. We observe that for many years – especially when foreign demand growth rates are high – the scale effect is statistically significant at the 5% level (the gray line does not cross the horizontal red bar). In the bottom panel, where outputs are denominated in value, we find that observed emissions increased annually at a rate of 4.50% on average, while counterfactual emissions increased only 1.89% annually, stripping out any endogenous technique effect. Hence, the scale effect increased emissions growth rates by 2.61 percentage points, or 58% of the average observed emissions growth rate.[25]

[25]Differences in average observed emissions growth rates between the top and bottom panel are due to differences in weighting only, while differences in the magnitude of the scale effect reflect differences in

To assess the magnitude of the technique effect, we compute the magnitude of the overall trade effect and then compare to the scale effect. The residual represents the firm-level technique effect. In the top panel of Figure 3, we estimate that foreign demand growth overall increased emissions growth by 1.05 percentage points annually on average (dark gray bars), but this effect is almost never statistically significant at the 5% level. Comparing the scale effect to the full trade effect yields a technique effect of 0.72 percentage points, or 41% of the scale effect. Corresponding estimates from the bottom panel yield a full trade effect of 1.02 p.p. and a technique effect of 1.59 p.p.; but again, the full trade effect is usually statistically indistinguishable from zero. Thus, we find that the scale effect increased emissions growth, but that the technique effect mitigated this impact to some degree.

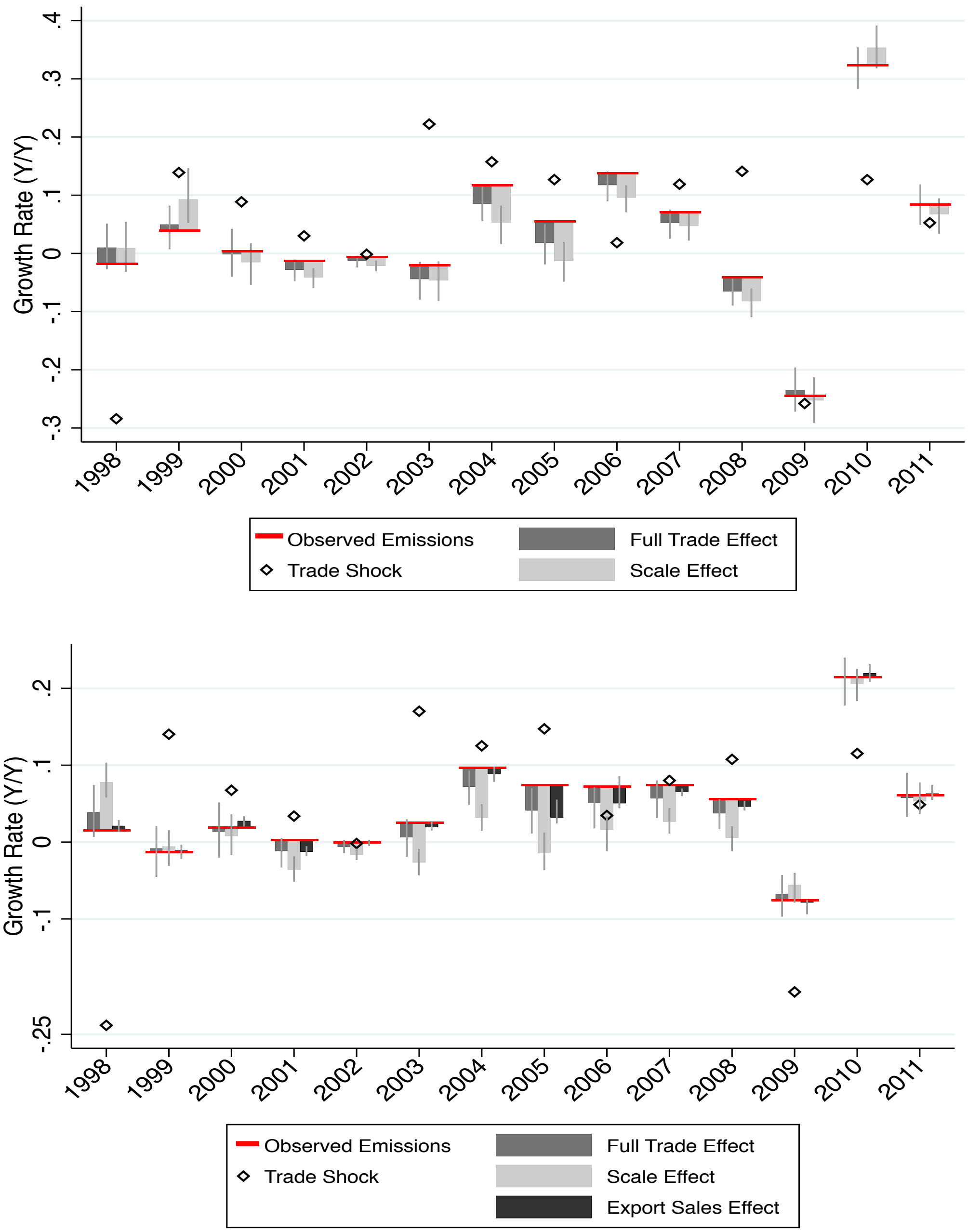
In the bottom panel of Figure 3, we can also assess the magnitude of the export sales channel separately from the domestic sales channel. The black bar indicates the contribution of foreign demand growth to observed emissions growth stemming just from the exports sales channel. In this case, we attribute all changes in year-over-year emission intensity growth and domestic sales growth to factors other than foreign demand growth. We find that emissions would have grown at an average annualized rate of 3.82%, yielding an average export sales effect of 0.68 percentage points. For years with high foreign demand growth, this effect can be quite large. For example, in 2005, we estimate that the export sales channel contributed 3.94 percentage points to the growth rate, which is statistically significant at the 5% level.

In magnitude, the export sales effect is very close to the full trade effect on average, which indicates that the emission intensity effect in value almost perfectly offsets the domestic sales effect. However, this relationship does not hold each year: for instance, the export sales effect is larger than the full trade effect in 2005, smaller in 2004, and even of opposite sign in 2009. Export sales alone are thus not sufficient to assess the full environmental impact of trade shocks.

Figure 4 reports graphically the contributions of the full trade effect, scale effect, and technique effect at the product level in quantity (top) and value (bottom), along with weighted-average foreign demand growth (diamonds). In the top panel, we find that foreign demand growth increased the emissions growth rate via the scale effect by 0.61 p.p. and slowed emissions growth rate via the technique effect by 0.35 p.p, for an overall trade effect of 0.26 p.p.. Hence, the technique effect mitigated 57% of the scale effect, on average. At

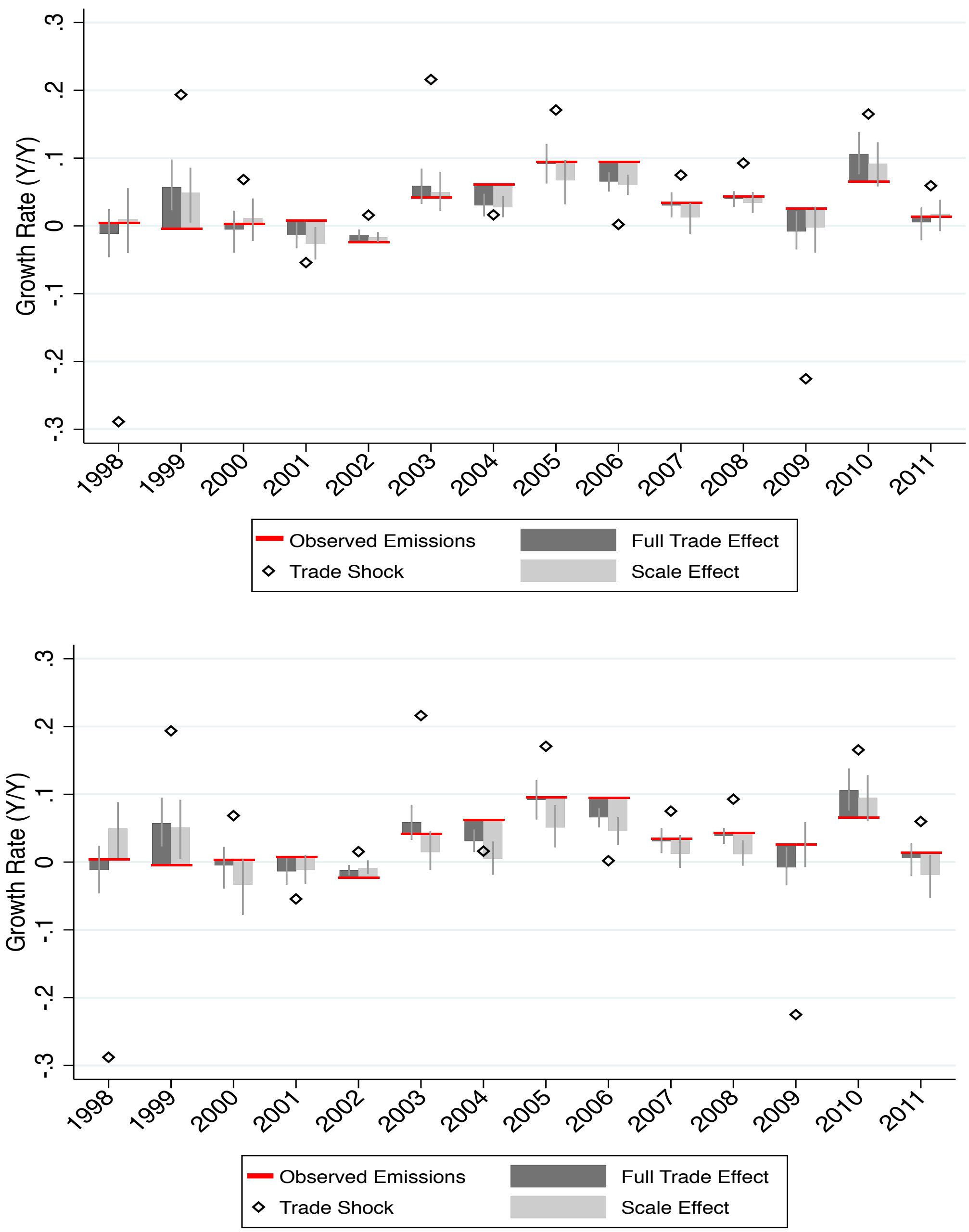
weighting and differences in estimated coefficients when outputs are denominated in quantity vs value.

Figure 3: Firm Level Channels, Quantity (top) and Value (bottom)



Notes: Red lines indicate the observed weighted average growth rate in CO_2 emissions. Dark gray bars indicate the full impact of foreign demand shocks including contemporaneous shocks and two years of lags. Light gray bars indicate the average scale effect, i.e., change in emissions due to changes in output quantity (top) or in total sales (bottom). Black bars indicate the average export scale effect, ruling out any impact on domestic sales. Vertical lines indicate confidence intervals. Diamonds indicate the weighted average foreign demand shock experienced by firms in a given year.

Figure 4: Product Level Channels, Quantity (top) and Value (bottom)



Notes: Red lines indicates the observed weighted average growth rate in CO_2 emissions. Dark gray bars indicate the average full impact of foreign demand shocks including contemporaneous shocks and two years of lags. Light gray bars indicate the average scale effect, i.e., change in emissions due to changes in output quantity (top) or in total sales (bottom). Vertical lines indicate confidence intervals. Diamonds indicate the weighted average foreign demand shock in the sample in a given year.

an average annual growth rate of 3.36% in the sample, the scale, technique, and overall impacts of trade shocks amount to 18.1%, -10.3%, and 7.7% of the observed growth rate, respectively.

In the bottom panel of Figure 4, we find similar magnitudes from the full impact of trade, but with output denominated in value, we see that price effects magnify both the scale and technique effects. On average, we estimate that foreign demand growth raised the emissions growth rate via the scale effect by 1.3 p.p. and slowed emissions growth rate via the technique effect by 1.1 p.p, for an overall trade effect of 0.26 p.p.. Hence, the technique effect mitigated 80% of the scale effect, on average. At an average annual growth rate of 3.38% in the sample, the scale, technique, and overall impacts amount to 38.7%, -31.0%, and 7.7% of the observed growth rate, respectively. Comparing the top and bottom panels, we see that, had we ignored price effects, we would have overestimated the scale effect by a factor of 2 and the technique effect by a factor of 3, on average.

7 Conclusion

In this paper, we leverage detailed output and energy reports from Indian manufacturing firms to study how foreign demand shocks impact CO_2 emissions growth rates for individual firms. We find that, over the period 1998 - 2011, foreign demand growth substantially increased firm-level CO_2 emissions growth rates for Indian manufacturers via the output scale effect, but that an endogenous technique effect mitigated roughly half of this increase. We also find that a substantial fraction of the scale effect owes to increased domestic sales. These results confirm that export sales growth alone is insufficient to measure trade's impacts on the environment. At the product level, we find that emission intensity growth per physical unit of output declined with foreign demand shocks. Since these estimates are net of product mix and price effects, we conclude that foreign demand indeed stimulated technological upgrading, as posited by earlier work.

The environmental impact of the rise in exports from developing countries is a hotly debated topic. Our results indicate that, even if foreign demand increases emissions via the scale effect, exporting to foreign countries can lower the emission intensity of production of firms in the developing world. Since the primary concern with these export flows is that emission intensity in the developing world is so much higher than in the developed world, the endogenous technique effect should mitigate the fears associated with these trade flows, to some extent. Fruitful avenues for future research would be to investigate to what extent the technique effect owes to adoption of existing technologies vs new research and development. Additionally, it would be useful to know under what conditions technological adoption is more likely and what policies can help accelerate convergence with developed-world practices.

8 Acknowledgment

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Table 2: Descriptive Statistics

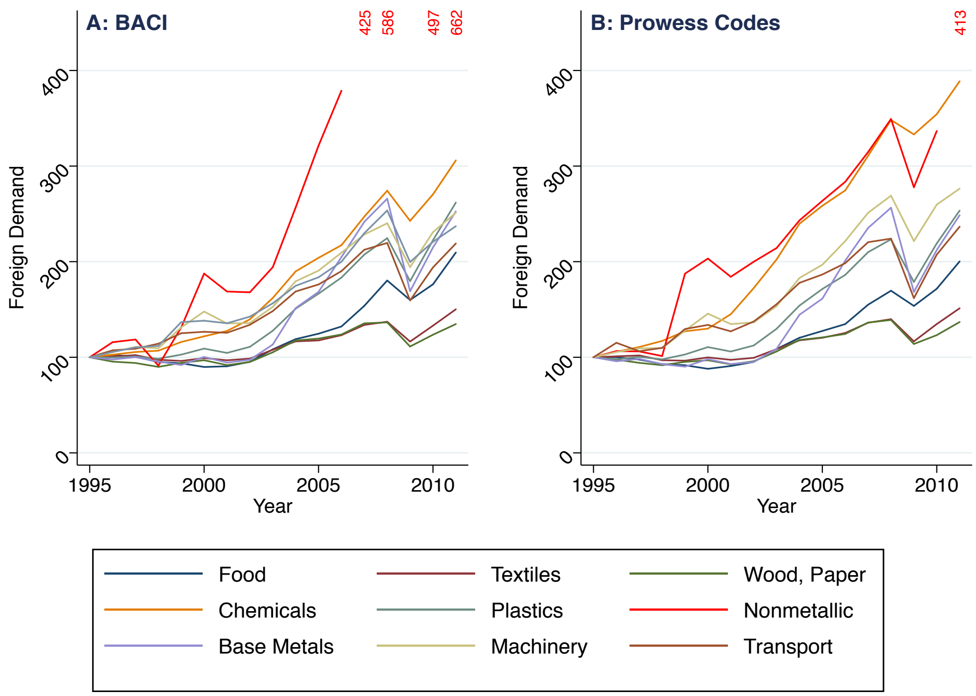
	Levels					Growth Rates				
	Mean	Sd	# Obs	# F-P	# Firms	Mean	Sd	# Obs	# F-P	# Firms
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A : Firm-Level Data										
Sales Value Total (bill of rs)	1.312	2.787	70265		7817	0.087	0.419	60337		7521
Sales Value Domestic (bill of rs)	1.016	2.275	69515		7810	0.099	0.751	57624		7375
Sales Value Exports (bill of rs)	0.218	0.595	69876		7806	0.087	1.068	47416		7504
Production (various units)	415	2144	69000		7736	0.073	0.506	60176		6427
Unit Value (various units)	9.43	30.83	67620		7630	0.015	0.447	60296		6424
Emissions (kt CO_2)	80.3	303.2	32999		4069	0.058	0.367	28348		3866
E/V (t/mill rs)	44.4	84.4	32340		4003	-0.023	0.383	28070		3841
E/Q (t/unit)	200.8	580.8	31914		3981	-0.016	0.479	28104		3141
Panel B : Product-Level Data										
Sales Value Total (bill of rs)	1.054	1.950	16722	2269	1292	0.055	0.433	14023	2018	1230
Production (various units)	17	86	16555	2239	1282	0.038	0.349	14497	2085	1240
Unit Value (various units)	0.002	0.004	16721	2248	1280	0.019	0.309	14023	2013	1229
Emissions (kt CO_2)	100.9	433.1	16554	2238	1275	0.033	0.382	14496	2087	1240
E/V (t/mill rs)	191.9	1149.0	15895	2150	1259	-0.026	0.345	13854	2006	1227
E/Q (t/unit)	227.5	380.6	16553	2229	1270	-0.006	0.199	14496	2091	1237

Notes: Table reports firm-level (A) or product-level (B) descriptive statistics in levels (columns 1-5) and growth rates (columns 6-10). Data covers 1995-2011. F-P stands for firm-products. For each variable, top/bottom 1% of firm-year or firm-product-year values have been removed. For production, unit values, and emission intensity in quantity, growth rates are only computed if output is denominated in the same units across products within the firm-year and across consecutive firm-years. Currency values are billions of current year rupees.

Appendix

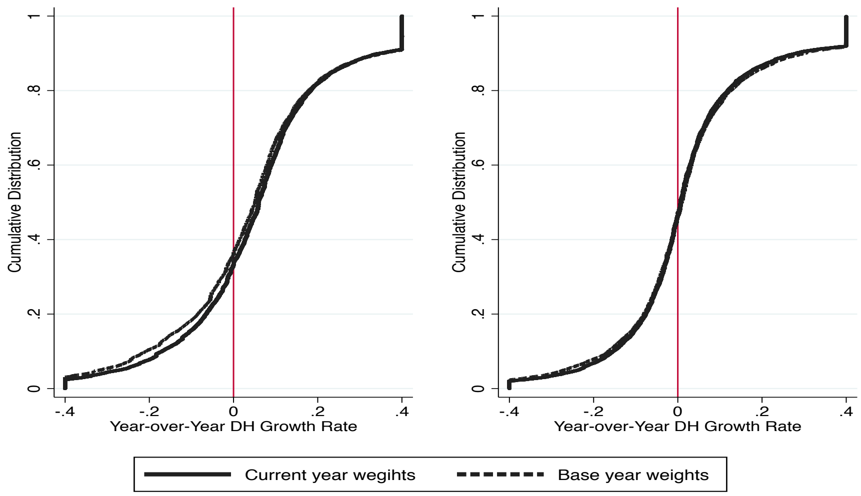
A Additional Results

Figure A.1: Foreign Demand Over Time



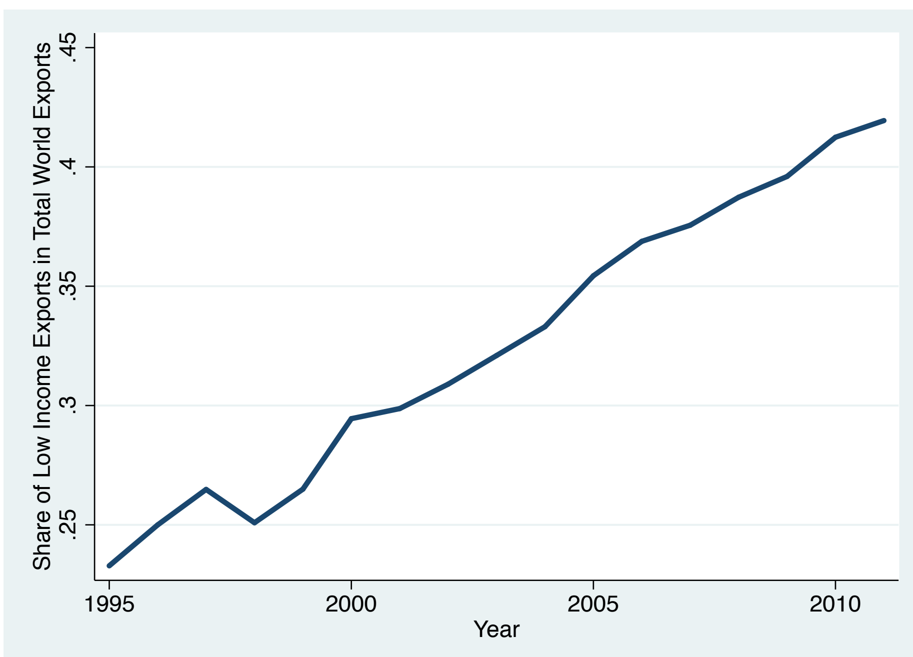
Notes: Figure reports weighted average foreign demand (FD_{jt}) indices by industry where goods are classified by HS rev 1996 (A) and Prowess product codes (B).

Figure A.2: Foreign Demand Variation



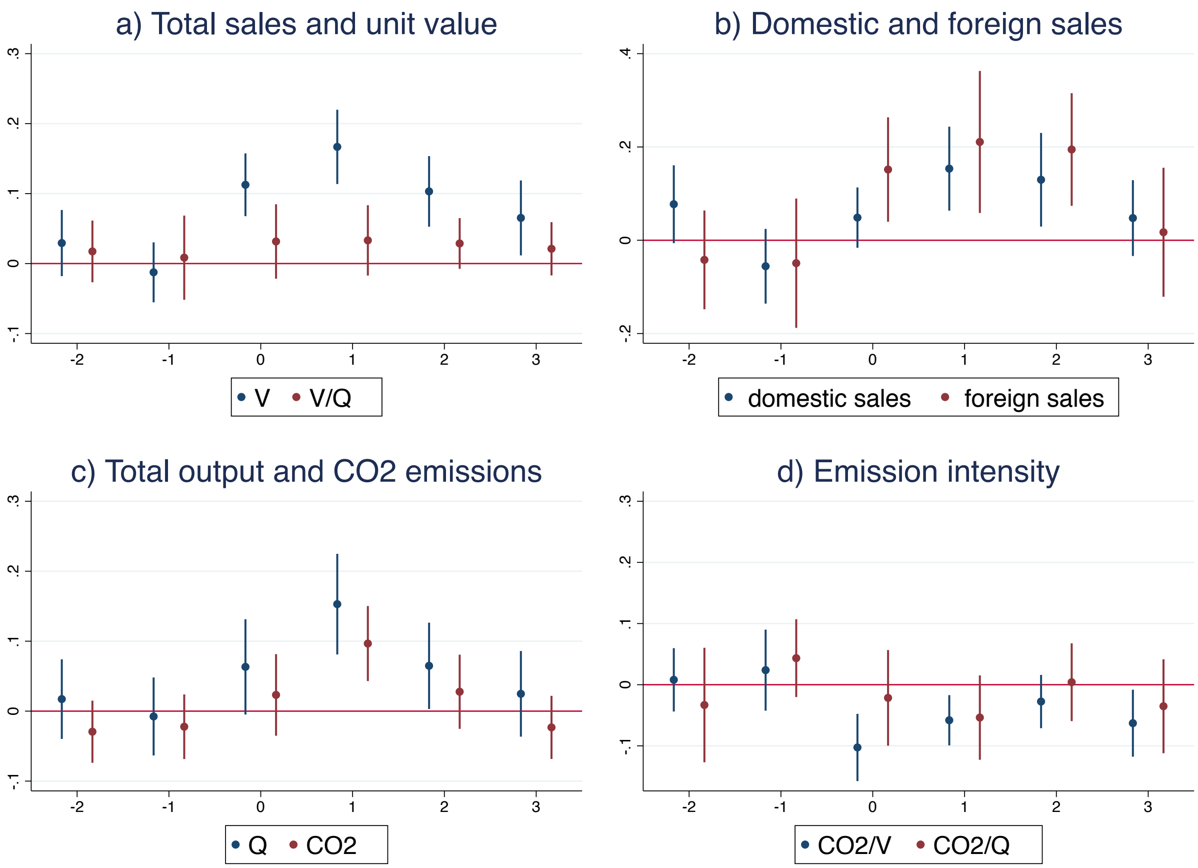
Notes: The Davis-Haltiwanger (DH) growth rate and residual growth rate are computed at the product level (3,276 products). Right panel plots residuals after regressing $\Delta FD_{jt,t-1}$ on product code fixed effects and industry-by-year fixed effects. In both panels, growth rates are truncated at -40% and 40% for ease of viewing.

Figure A.3: Developing World Export Share Over Time



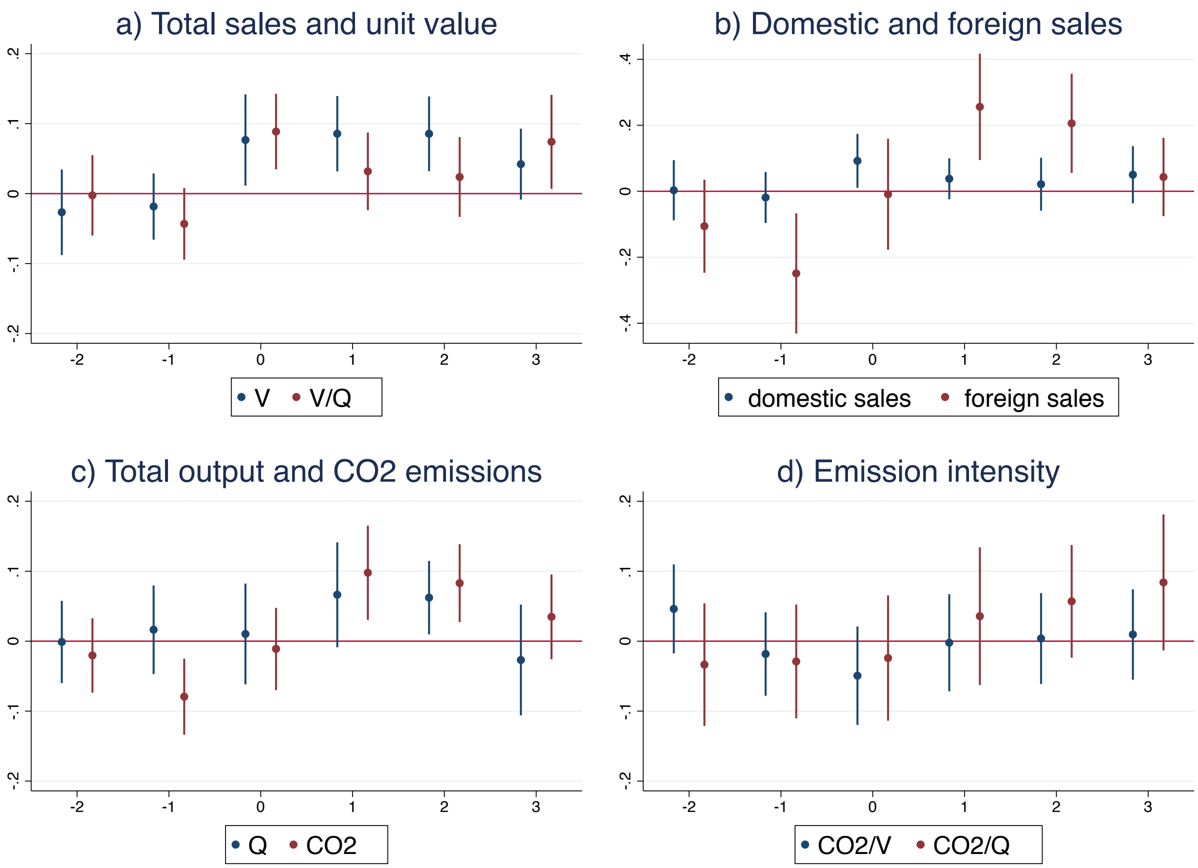
Notes: Computed from BACI Export Data set. Developing Country is defined as any country not defined by the World Bank in 2006 as "High Income OECD" or "High Income Other".

Figure A.4: Firm-level results, Reduced Form



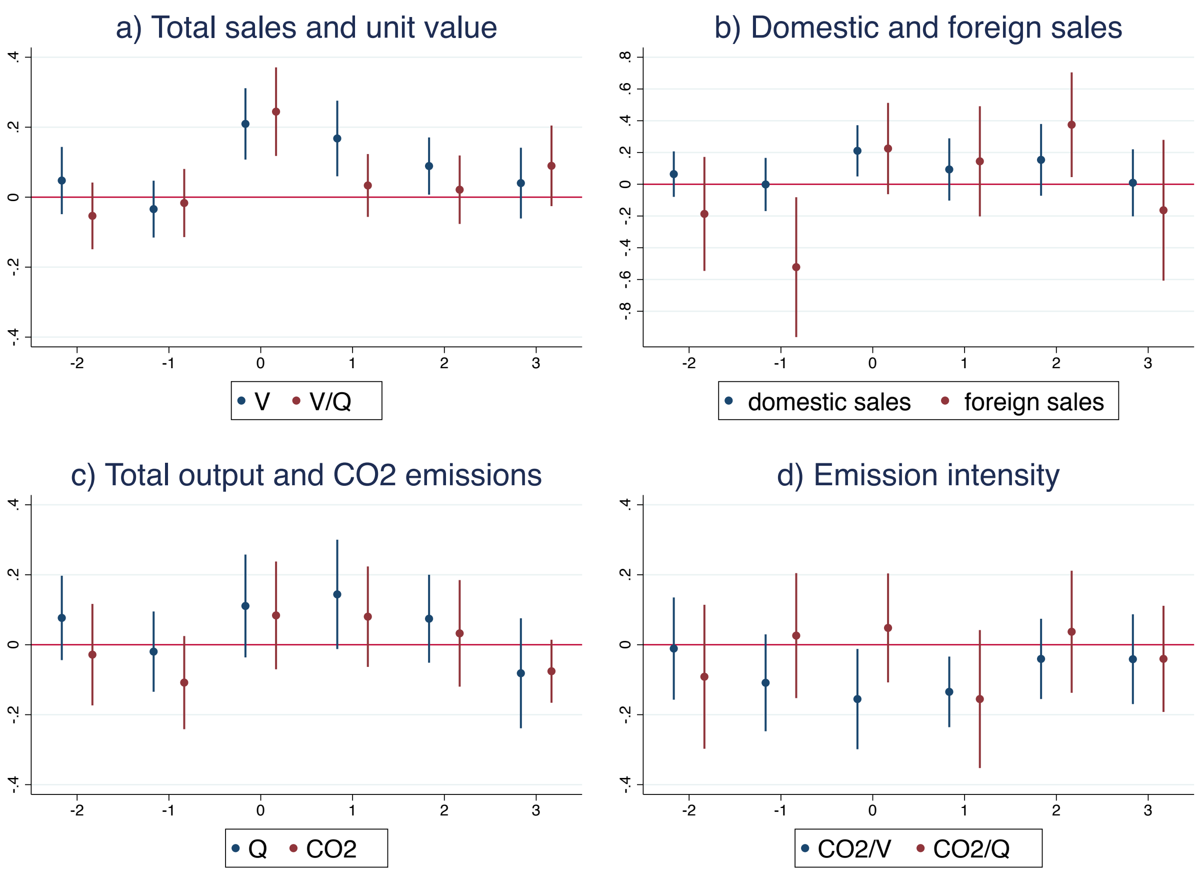
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from OLS regressions of dependent variables on base-year weighted instruments $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group level. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Number of firm-year observations range from 11,107 (for CO2/Q) to 36,157 (for V).

Figure A.5: Firm-level results, OLS



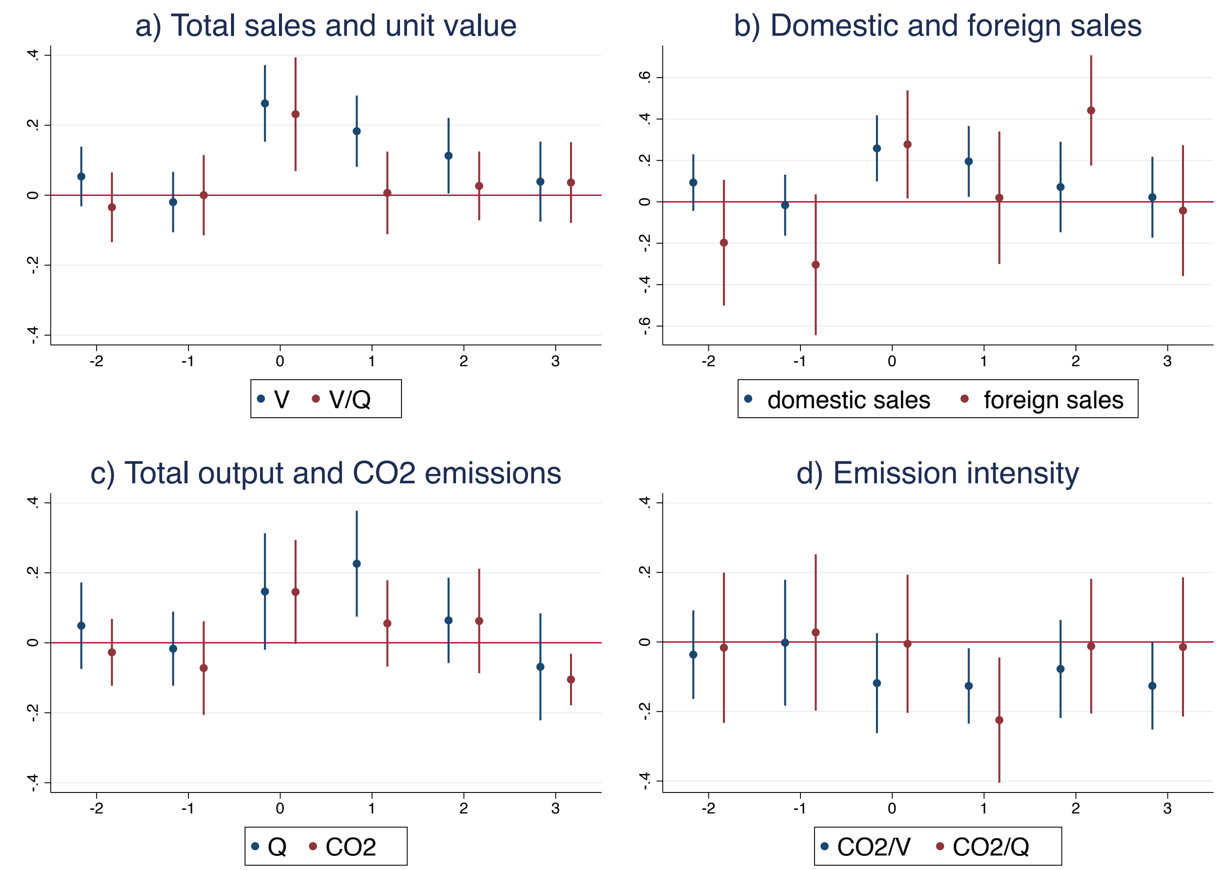
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from OLS regressions of dependent variables on contemporaneous foreign demand shocks. Standard errors are clustered at the 4-digit product-group level. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Number of firm-year observations range from 6,582 (for CO2/Q) to 21,183 (for V).

Figure A.6: Firm-level results, Excluding top/bottom 2%



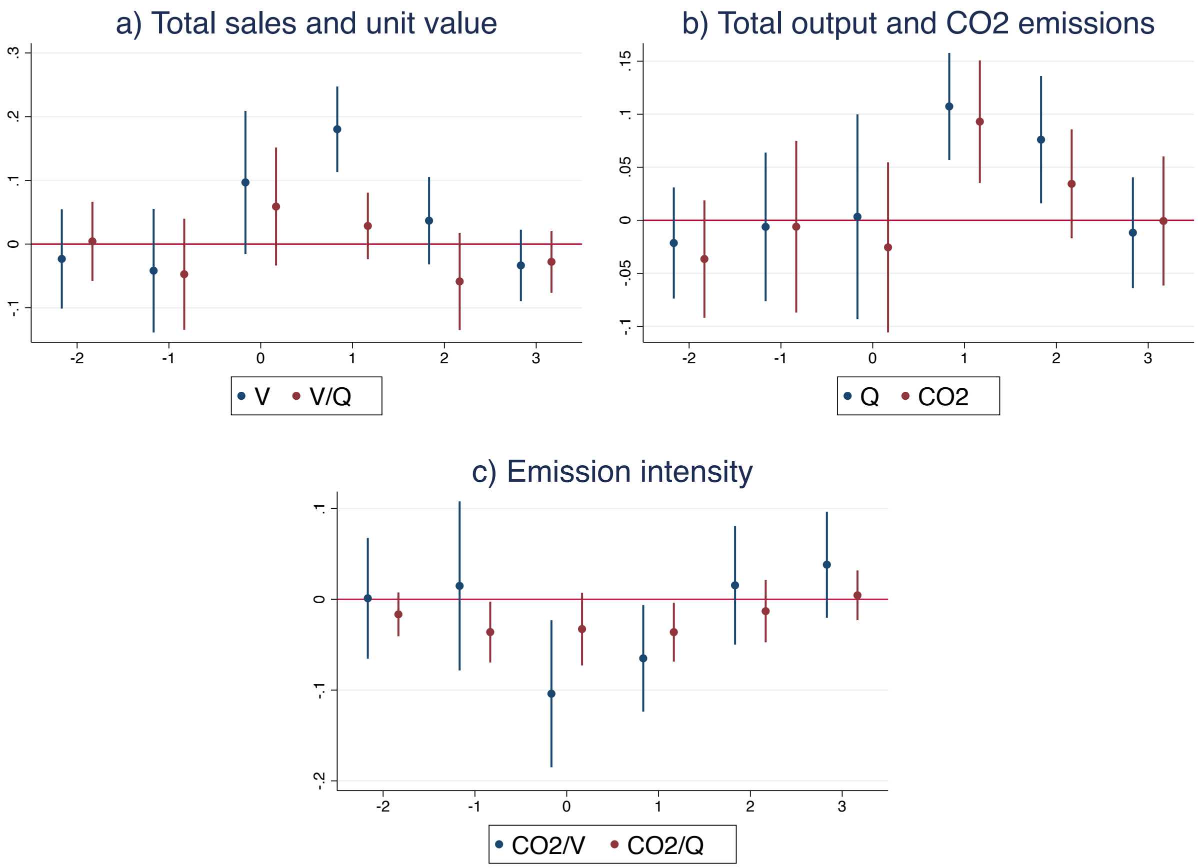
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group level. Top and bottom 2% of firm-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 10.66 (for CO2/Q) to 19.65 (for export value). Number of firm-year observations range from 5,704 (for CO2/Q) to 18,134 (for V).

Figure A.7: Firm-level results, Excluding Nonmetallic Minerals



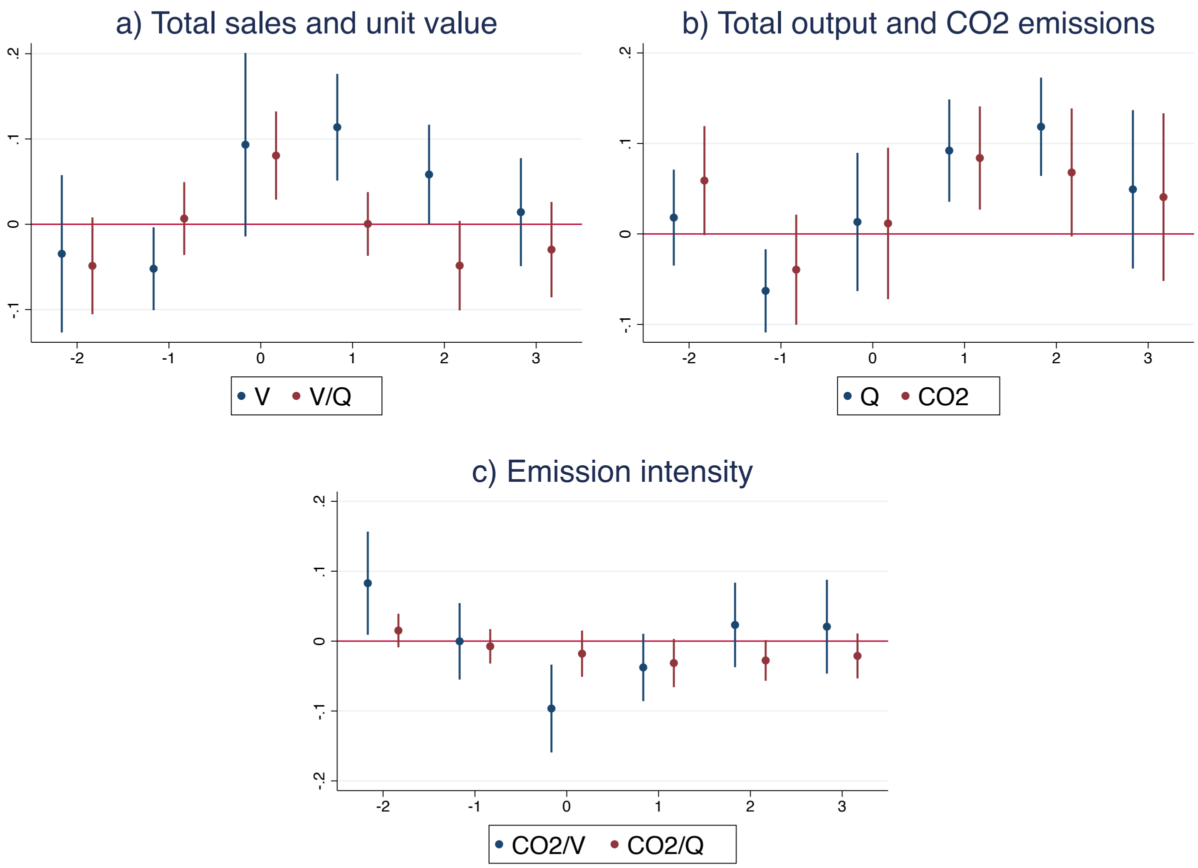
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group level. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 9.86 (for CO2/Q) to 17.39 (for V). Number of firm-year observations range from 6,048 (for CO2/Q) to 18,900 (for V). Sample excludes Nonmetallic Minerals.

Figure A.8: Firm-product level, Reduced Form



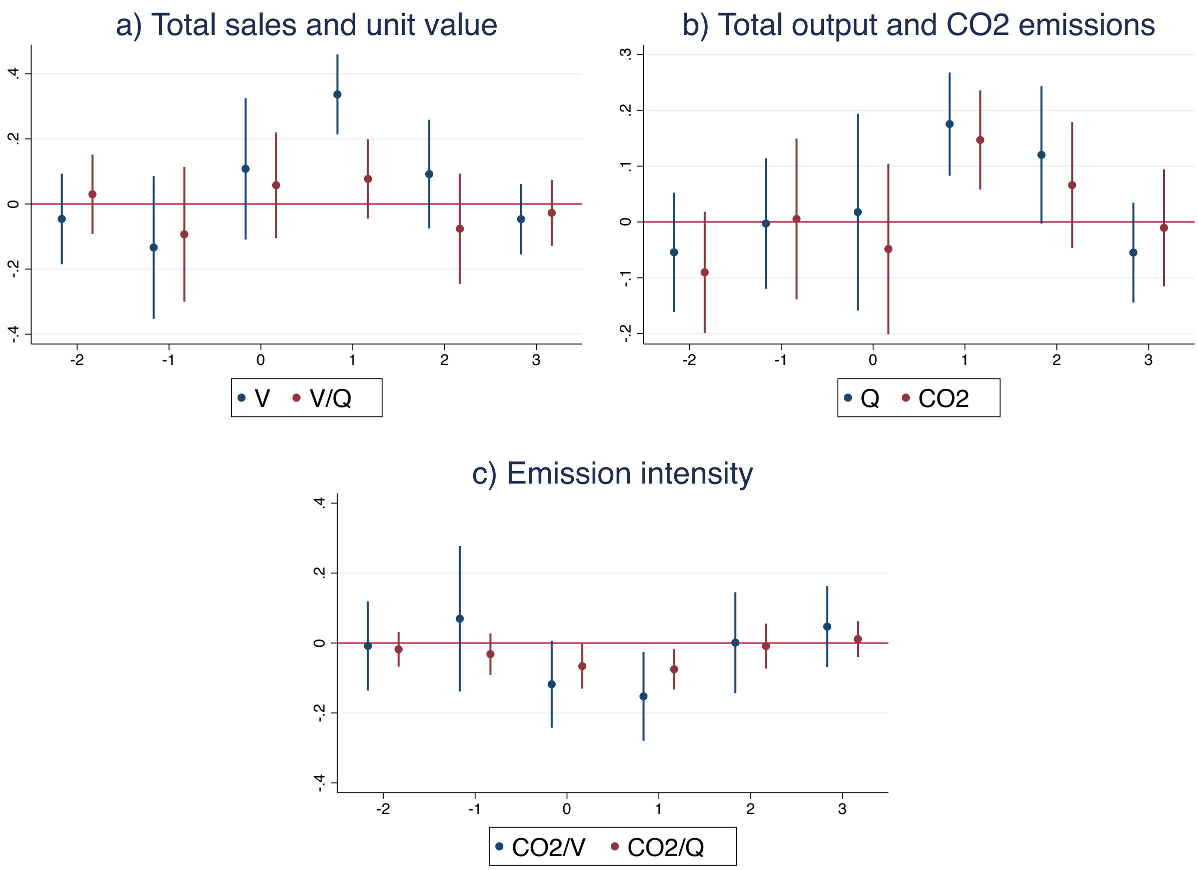
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group and firm levels. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Number of observations range from 7,835 (for V) to 8,046 (CO_2).

Figure A.9: Firm-product level, OLS



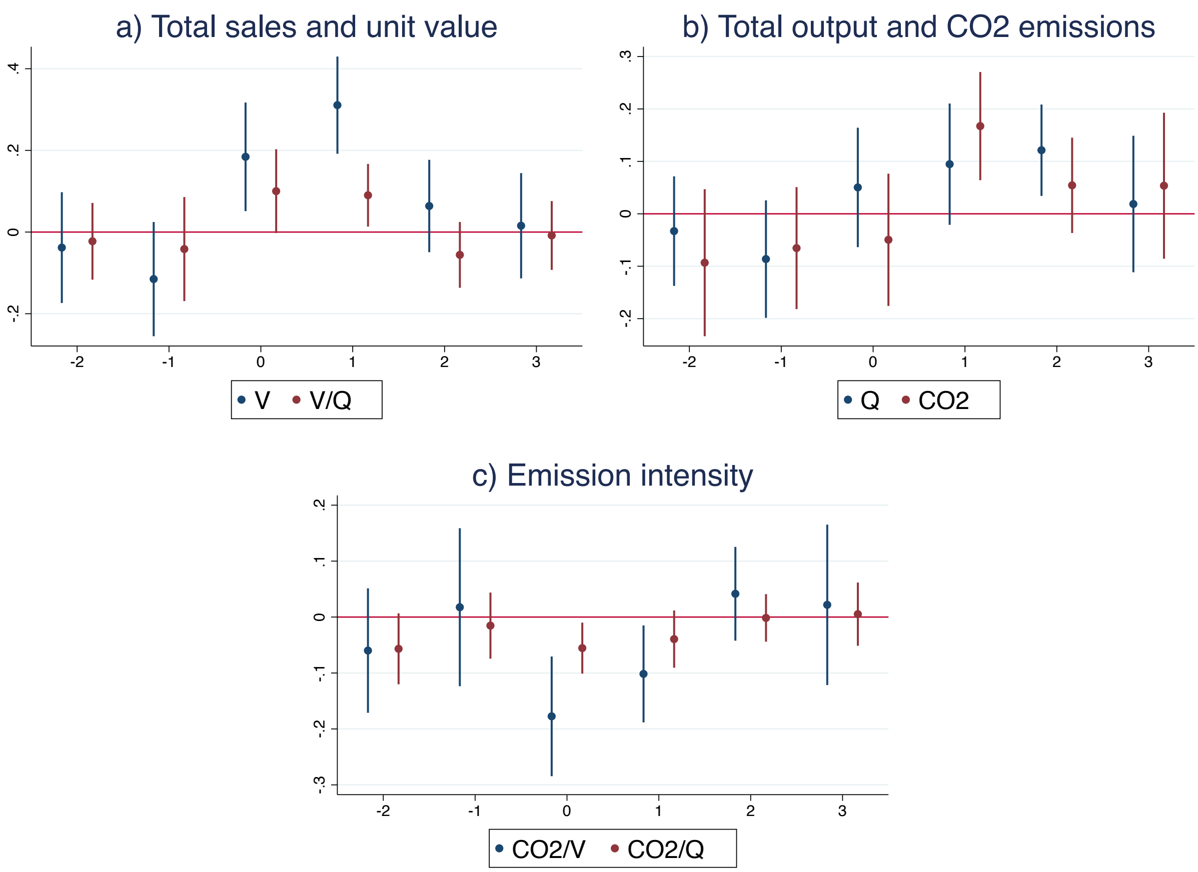
Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group and firm levels. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Number of observations range from 7,835 (for V) to 8,046 (CO_2).

Figure A.10: Firm-product level, Excluding Nonmetallic Minerals



Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group and firm levels. Top and bottom 1% of firm-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 8.07 (for CO2/Q) to 8.41 (V/Q). Number of firm-year observations range from 7,213 (for CO2/V) to 7,503 (CO2).

Figure A.11: Firm-product level, Excluding top/bottom 2%



Notes: Estimates of coefficients β_τ for $\tau = -2, ..., 3$ from equation (5) are reported graphically. Panel a) reports these coefficients for total sales (V) and unit value (V/Q), panel b) for domestic and foreign sales, panel c) for total output (Q) and total CO_2 emissions (CO2), and panel d) for emission intensity in value (CO2/V) and in quantity (CO2/Q), all at the firm level. The x-axis represent the value of τ, the dots the point estimates of β_τ, the bar the 95% confidence intervals. Estimates are obtained from instrumental variable regressions where endogenous trade shocks are instrumented by $\Delta Z_{it-\tau,t-1-\tau}$. Standard errors are clustered at the 4-digit product-group and firm levels. Top and bottom 2% of firm-year outcomes and trade shocks have been excluded. Kleibergen-Paap rk LM statistics range from 9.41 (for V/Q) to 9.87 (for CO2/Q). Number of firm-year observations range from 6,996 (for CO2/V) to 7,241 (for Q).

Table A.1: Long Difference Results at the Firm-Product Level

Dep Var:	ΔV (1)	ΔQ (2)	Δ(V/Q) (3)	ΔCO2 (4)	Δ(CO2/V) (5)	Δ(CO2/Q) (6)
Panel A: 3-year intervals						
$\Delta FD_{jt,t-3}$	0.176***	0.113***	0.050*	0.115***	-0.056	-0.006
	(0.044)	(0.040)	(0.030)	(0.043)	(0.037)	(0.025)
# Obs	2850	2931	2845	2932	2816	2922
Panel B: 5-year intervals						
$\Delta FD_{jt,t-5}$	0.231***	0.143***	0.095**	0.092	-0.122**	-0.056
	(0.061)	(0.054)	(0.042)	(0.057)	(0.051)	(0.035)
# Obs	1113	1153	1113	1155	1099	1158

Notes: Table presents long difference estimates from regression (6) for contemporaneous shocks only. All explanatory and dependent variables are computed as the Davis-Haltiwanger growth rate for 3-year intervals (panel A) or 5-year intervals (panel B). Top and bottom 1% of outcomes and trade shocks have been excluded. Asterisks indicate statistical significance at the 1% ***, 5% **, and 10% * levels.

Table A.2: Heterogeneous Impacts

	Firm level				Firm-product level			
	ΔV	ΔQ	$\Delta\frac{CO2}{V}$	$\Delta\frac{CO2}{Q}$	ΔV	ΔQ	$\Delta\frac{CO2}{V}$	$\Delta\frac{CO2}{Q}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A : Multi-product firms								
Δ FD$_{it}$	0.103***	0.094***	-0.016	-0.020	0.122***	0.051**	-0.060**	-0.013
	(0.022)	(0.029)	(0.025)	(0.036)	(0.026)	(0.020)	(0.026)	(0.015)
# Obs	36714	23080	17193	10366	9330	9621	9227	9610
Panel B : Single-product firms								
Δ FD$_{it}$	0.126***	0.142***	-0.021	-0.007	0.261***	0.264***	0.048	-0.018
	(0.040)	(0.047)	(0.049)	(0.048)	(0.095)	(0.081)	(0.088)	(0.050)
# Obs	10848	10974	4303	4366	1349	1350	1346	1338
Panel C : Large firms								
Δ FD$_{it}$	0.057	0.072*	-0.030	0.004	0.119	0.111	-0.074	-0.105***
	(0.036)	(0.043)	(0.034)	(0.052)	(0.081)	(0.070)	(0.058)	(0.033)
Δ FD$_{it}$ * Large	0.073**	0.058	0.010	-0.028	0.012	-0.055	0.019	0.100***
	(0.036)	(0.040)	(0.042)	(0.049)	(0.080)	(0.076)	(0.056)	(0.037)
# Obs	47562	34054	21496	14732	10694	10985	10586	10961
Panel D : Dirty firms								
Δ FD$_{it}$	0.138***	0.189***	-0.034	-0.101	0.305***	0.261	-0.026	-0.061
	(0.037)	(0.051)	(0.039)	(0.072)	(0.094)	(0.161)	(0.114)	(0.083)
Δ FD$_{it}$ * Dirty	-0.040	-0.106**	0.013	0.095	-0.184*	-0.208	-0.033	0.049
	(0.036)	(0.051)	(0.042)	(0.068)	(0.101)	(0.163)	(0.116)	(0.084)
# Obs	47562	34054	21496	14732	10694	10985	10586	10961
Panel E : Dirty sectors								
Δ FD$_{it}$	0.107***	0.109***	0.002	0.012	0.132**	0.074**	-0.052	-0.022
	(0.029)	(0.036)	(0.028)	(0.034)	(0.053)	(0.032)	(0.036)	(0.016)
Δ FD$_{it}$ * Dirtysec	0.001	-0.000	-0.047	-0.053	-0.005	-0.021	-0.010	0.014
	(0.039)	(0.053)	(0.039)	(0.051)	(0.063)	(0.039)	(0.041)	(0.024)
# Obs	47562	34054	21496	14732	10694	10985	10586	10961

Notes: We report estimates of β_1 from equation (5) in columns 1 to 4 and from (6) in columns 5 to 8. Panel A uses a sub-sample of multi-product firms, and panel B of single-product firms. Panel C reports results on firms being larger than the median in size (denoted 'large'), panel D on firms being dirtier than the median within their own sector (denoted 'dirty') and panel E on firms being part of a dirtier sector (denoted 'dirtysector'). 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, 2019; 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[26], and are reported in Table B.1. 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. We use 25 energy sources and associated CO_2 emissions factors from Table 12.1 in the US EPA 2012 Climate Registry Default Emissions Factors, as well as the CO2 factor for electricity generation in India reported in Table 14.4. The table reports 951 g CO2 per KwH for Indian electricity as the average intensity for grid electricity in the period 2000-2010.

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.

[26] Source: http://theclimateregistry.org/wp-content/uploads/2015/01/2012-Climate-Registry-Default-Emissions-Factors.pdf

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

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 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 $FD_{h6,t}$ and $Z_{h6,t}$ 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:

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

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:

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

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

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

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