Imported carbon emissions: Evidence from French manufacturing companies

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Abstract. This paper analyzes imported carbon emission at the firm level. To do so, we combine information on emissions, imports, imported emissions and energy prices for French manufacturing firms between 1997 and 2014. We document a significant increase of the carbon emissions embedded in imports of French manufacturing companies over the period 1997 to 2014 that is attributable mainly to a shift towards more carbon-intensive products and countries. We then estimate the impact of imported emissions on domestic emissions and emission intensity using a shift-share instrumental variable strategy based on third countries supply shocks. We do not find compelling evidence of an impact of carbon imports on total emissions, but emission efficiency improves significantly in companies offshoring emissions abroad. A 10% increase in carbon offshoring causes a 4% decline in emission intensity. In addition, we find that the elasticity of domestic emission intensity to imported emissions is stronger in energy-intensive sectors, on high-productivity companies and among exporters. Reassuringly, the relationship between imported emissions and emission intensity does not seem to be driven by a pollution haven motive.

Résumé. Émissions de carbone importées : données probantes provenant des entreprises de fabrication françaises. Cet article analyse les émissions de carbone importées au niveau des entreprises. Pour ce

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faire, nous regroupons des données sur les émissions, les importations, les émissions importées et les prix de l'énergie des entreprises manufacturières françaises entre 1997 et 2014. Nous documentons une hausse importante des émissions de carbone intégrées aux importations des entreprises des secteurs de l'industrie manufacturière française pendant la période allant de 1997 à 2014, hausse qui est principalement attribuée à l'augmentation des importations de produits plus intensifs en carbone et en provenance de pays où la production est davantage carbonnée. Nous estimons ensuite l'effet des émissions importées sur les émissions locales et l'intensité en émissions des entreprises à l'aide d'une stratégie de variable instrumentale fondée sur les chocs d'offre des pays tiers et la composition des importations passées au niveau firme. Nous ne trouvons pas de preuves convaincantes de la répercussion des importations de carbone sur les émissions totales. En revanche, 'efficience des émissions s'améliore considérablement pour les entreprises qui délocalisent leurs émissions à l'étranger. Une hausse de 10 % de la délocalisation du carbone se traduit par une diminution de 4 % de l'intensité en émissions. En outre, nous constatons que l'élasticité de l'intensité en émissions locales par rapport aux émissions importées est plus forte pour les secteurs énergivores, les entreprises à haute productivité et les exportateurs. Il est rassurant de voir que la relation entre les émissions importées et l'intensité en émissions ne semble pas être déterminée par un effet d'havre de pollution.

JEL classification: F18, F14, Q56

1. Introduction

Under the 2015 Paris Agreement, countries agreed on the ambitious global target of limiting the global temperature increase to well below 2°C above pre-industrial levels. However, the level of ambition to reduce carbon emissions differs markedly across countries, as illustrated by the vast heterogeneity in Nationally Determined Contributions. Divergent targets on emission reductions raise the concern that the introduction of ambitious policies in some countries or regions simply leads to a shift in emissions to less ambitious countries, following the so-called pollution haven hypothesis (Taylor 2004, Levinson and Taylor 2008). The potential for carbon leakage can undermine the effectiveness of climate policies at reducing global greenhouse gas (GHG) emissions (Fowlie and Reguant 2018)[1] and recently led the European Union to propose the introduction of "Carbon Border Adjustment Mechanism", a tariff on carbon-intensive products that adjusts for the difference in carbon pricing between the European Union and its trading partners.

Yet, while the relocation of dirty production from high-income to low-income countries may well have contributed to the clean-up of production in developed countries, a competing explanation is that environmental policies triggered an energy-saving technological change and associated reductions in the emission intensity of output (Levinson 2009, Shapiro and Walker 2018). Empirical evidence using sector- and country-level data lends strong support for this technological change explanation. Decomposition methods indicate that the contribution of the so-called within-sector "technique" effect is significantly larger than that of between-sector "compositional" effect induced by international trade for various pollutants and energy use (e.g., Cole and Elliott 2003, Levinson 2009, Brunel 2017, Shapiro and Walker 2018).

However, this result has been challenged by two pioneering works examining the impact of trade liberalization on emissions at the firm level (Li and Zhou 2017, Cherniwchan 2017). These studies reveal that the technique effect conflates true technological improvements with the offshoring of dirty tasks within narrowly defined production lines. While these new findings are thought-provoking, firm-level evidence on pollution offshoring is still scant,

1 Concerns over carbon leakage are motivated by the observation that the carbon intensity gap between high income and low and middle income countries has increased by 19% since 1990 (World Bank 2020).

confined mostly to the US and local pollutants. Moreover, due to data limitations on firm-level exposure to both import competition and domestic environmental policies, a comprehensive understanding of the underlined mechanisms remains limited so far.

Our paper provides new evidence on this question by examining a global pollutant, CO_2, and a different country, France, over a longer period of almost two decades, 1997−2014. To the best of our knowledge, we are the first to combine information on emissions, imports, imported emissions and environmental policy stringency, all at the firm level. The unique features of our data allow making substantial steps forward in understanding how companies' environmental performance responds to both trade liberalizations and changes in environmental policy stringency in the manufacturing sector.

The starting point of our analysis is a novel firm-level measure of imported emissions. We obtain our measure of imported emissions by weighting firm-level imports using data on the carbon intensity (direct and indirect) of each sector−country pair obtained from the combination of International Energy Agency (IEA) and the World Input−Output Database (WIOD) data. An advantage of our firm-level measure of imported emissions is that it can be extended to other firm-level datasets because it relies on publicly available data. Weighting imports by foreign emission intensity allows us to capture the environmentally-related motive of industrial relocation towards emerging economies and to go beyond Li and Zhou (2017) and Cherniwchan (2017), who focus on import volume from less-developed countries, assuming that they have less stringent regulations than the US (Ederington et al. 2005). By contrast, we measure actual differences in carbon intensity between sourcing countries. The idea is that companies already importing polluting goods from abroad find it easier to increase the extent of imported carbon emissions.

The subsequent step consists in correlating imported emissions with firm's domestic emissions and emission intensity, which are obtained from confidential data on the energy use and expenditures of French manufacturing establishments. To inspect the mechanisms behind the expected negative correlation between imported emissions and emission intensity, we conduct two types of econometric analyses. First, we isolate the exogenous component of import competition shocks using a shift-share instrumental variable strategy (Bartik 1991, Autor et al. 2013, Borusyak et al. 2022). The idea of this instrumental variable strategy is to rely on trade shocks that are driven by supply-side shocks occurring elsewhere than in France and in EU neighbouring countries. An advantage of our measurement framework is that we can compare the effect on emissions of total imports—as in Li and Zhou (2017) and Cherniwchan (2017)—with that of imported emissions that give more weight to carbon intensive products.

Another novelty of this paper is to jointly examine the impacts of environmental policies and imported emissions on emissions intensity. Following previous research (Aldy and Pizer 2015; Marin and Vona 2017, 2021; Sato et al. 2019; Dussaux 2020), we use the firm-level average unit energy cost as a proxy of environmental policy stringency. In absence of stringent and comparable carbon pricing policies around the world, this proxy rests on the assumption that companies adjust in a similar way to energy price changes that are policy-driven or driven by other factors (Marin and Vona 2021). The introduction of a firm-level measure of environmental policy stringency is critical not only to assess policy impacts on imported emissions, and thus the pollution haven hypothesis from a new angle, but also to test the relative incidence of the two main drivers of emission intensity improvements. Conditional on the impact of imported emissions, we interpret the effect of energy prices on emission intensity (properly instrumented, see section 4.4) as a proxy of technology inducement, thus revealing the relative importance of the trade and technology drivers (see Shapiro and Walker 2018 for a similar argument).

Our analysis provides the following set of results. First, we document a significant increase of the carbon emissions embedded in imports of French manufacturing companies over the period 1997–2014. A change in the composition of imports, namely a shift towards more carbon-intensive imported products and sourcing countries, explains the bulk of this increase together with the entry of new firms into the import market, which contributes to a scaling up of total emissions.

Second, we find a substantial impact of carbon imports on emission efficiency: a 10% increase in imported carbon emissions leads to a decrease in emission intensity (emissions over revenue) of approximately 4%. Importantly, we find that import values and imported emissions have a similar effect on firms' emission intensity. Therefore, the "carbon import effect" appears to be primarily the by-product of importing from abroad driven by other motives rather than the consequence of a pollution haven effect. We also show that the carbon import effect partly captures efficiency improvement related to exporting or to technological capabilities rather than to the offshoring of dirty tasks.

Third, we do not find compelling evidence of an impact of carbon imports on total emissions at the firm level, but this is due mostly to the difficulties to address the endogeneity due to firms' size. Nevertheless, we find that imported emissions are associated with a decrease in emission intensity and an increase in output suggesting that imported emissions are compensated by an increase in production scale. The negative relationship between domestic and imported emissions tends to be stronger in energy-intensive sectors.

Fourth, we find robust evidence that the effect of imported carbon emissions does not overlap with (and is not driven by) the effect of energy prices on emissions. In most cases, energy prices have a larger effect on the reduction of emission intensity than imported emissions for the average firm. However, this latter result is not robust across different specifications.

Our findings are also connected to the empirical literature on the impact of environmental policies on carbon leakage that we briefly summarise here. Exploiting trade flows at the sector–country level, Aichele and Felbermayr (2015) document a substantial increase in the carbon content of trade between countries committed and uncommitted to the Kyoto protocol. Aldy and Pizer (2015) focus on energy prices in the US manufacturing sector and find a modest negative effect on trade flows, which is concentrated in energy-intensive industries. Saussay and Sato (2018) show that differences in energy prices affect FDI location decisions of multinational companies, but the effect is rather small, while Cole et al. (2021) find a larger effect of a self-reported measure of environmental regulation on the probability of outsourcing of Japanese companies. Ben-David et al. (2018) find that public companies facing more stringent environmental regulation in the headquarter country reduce their domestic emissions while increasing their foreign emissions. In contrast with these papers that suggest the existence of carbon leakage, two recent studies find no effect of the European Union emissions trading system (EU–ETS) on the carbon content of trade flows (Naegele and Zaklan 2019) and on carbon leakage within multinational companies having a foreign affiliate outside Europe (Dechezleprêtre et al. 2019).[2] A plausible explanation for this discrepancy is the well-known lack of stringency during the first phases of the EU–ETS, which justifies the focus on energy prices here. We add to this literature by assessing the effect of energy prices—possibly one of the best proxy of a stringent climate policy (e.g., Aldy and Pizer 2015, Cullen and Mansur 2017, Marin and Vona 2021)—on imported emissions, thus looking at the intensive margin shift of polluting activities within a company, using a larger sample of firms and a longer time span.

2 Two related papers using firm-level data find that the EU–ETS increased outward FDI in Italy (Borghesi et al. 2020) and Germany (Koch and Mama 2019). However, they do not directly focus on the pollution leakage effect of the EU–ETS.

The remainder of this paper is organized as follows. Section 2 briefly outlines the conceptual framework. Section 3 presents the data, measurement and descriptive statistics. Section 4 presents the empirical strategy used to identify both price and import effects. Section 5 contains the main results of the paper. Section 6 concludes.

2. Conceptual framework

Our empirical framework is motivated by a theoretical extension of the pollution haven hypothesis, known as pollution offshoring, which applies the concept of comparative advantages to product-level value chains (for a review, see Cole et al. 2014).

Cherniwchan et al. (2017) formally define pollution offshoring using the task-based approach to production. In this approach, output is produced by a set of related tasks, while production factors (energy, labour, etc.) compete in a Ricardian fashion to perform each task. A dirty task is a task where polluting inputs have a comparative advantage. Just like every task, a dirty task can be done within a company or be outsourced to other companies, either abroad or at home, depending on the relative cost of dirty inputs. Because the cost of dirty inputs is also related to environmental policy stringency, offshoring dirty tasks abroad may occur to escape an increase in environmental policy stringency at home.

Overall, the task approach highlights two channels through which a unilateral increase in environmental policy stringency can affect firms' emission intensity: (i) by relocating dirty tasks to pollution havens and (ii) by reducing or eliminating the use of dirty inputs required to perform a single task, i.e., the real technological effect.[3] A key contribution of our paper is to empirically assess the importance of these two margins at the firm level.

Importantly, as highlighted by Antweiler et al. (2001), a dirty task can be relocated abroad for other reasons than environmental policies. In particular, dirty tasks may complement a task in which cheaper unskilled labour or physical capital provides a comparative advantage.[4] Because labour and capital costs are larger than energy costs by an order of magnitude for the typical industrial sector, offshoring of dirty tasks may occur as a by-product of industrial relocation driven by differences in the costs of other production factors. In section 5, we extend our analysis to assess the role played by other factors in explaining the relationship between domestic and imported emissions.

Finally, the carbon offshoring effect may mask a technology inducement effect. A solid theoretical and empirical literature shows that accessing foreign markets boosts technological change and firm's productivity (e.g., Melitz 2003, Bustos 2011, Aghion et al. 2018). Several recent papers find that this happens also for energy and emission efficiency (e.g., Forslid et al. 2017; Barrows and Ollivier 2018a,b; Gutiérrez and Teshima 2018). Exporting and multinational firms have generally lower emission intensity than similar firms, and thus may respond differently to policy shocks.[5] Because most importing firms are also exporting

3 The "real" technological effect is not inflated by changes in task composition within the firm.

4 The task model of comparative advantage simplifies the analysis by assuming that each task is produced using only one input (e.g., Grossman and Rossi-Hansberg 2008, Acemoglu and Autor 2011). Complementarity and substitutability among tasks (and thus production factors) take place at the level of task production function.

5 On the relationship between export status and emission/energy intensity or investment in abatement technologies, see Batrakova and Davies (2012), Rodrigue and Soumonni (2014), Girma and Hanley (2015), Forslid et al. (2017), Jinji and Sakamoto (2015), Holladay (2016),

and our data are no exception to this, the export margin of adjustment to international competition can contaminate the interpretation of our results as revealing the avoidance of environmental regulation in France rather than the effect of trade on innovation. In section 5, we explore the possibility that the effect of carbon offshoring is driven by a positive relationship between exporting and productivity gains, but, as will be discussed there, a clear identification of the two margins remains problematic.

Though we observe imported emissions and not offshoring directly, the insights of this literature apply to our study. Imported emissions and offshoring differ by the fact that offshoring implies moving abroad productions that were previously done domestically (e.g., Olsen 2006). In contrast, an increase in imports can be the result of offshoring but also a consequence of an expansion in the scale of production. Because we do not observe offshoring or foreign direct investments directly, we will use the more neutral terminology of imported carbon emissions in this paper.

3. Data, measures and descriptive statistics

This project relies on the combination of several data sources. We focus on the manufacturing sector, which is both polluting and the most involved in international trade. Table 1 summarizes the sources and use of the data in the paper. Further details about the data are given in the following subsections.

3.1. Data and measures

Domestic emissions and energy prices. Domestic emissions and energy prices are obtained from the Enquête annuelle sur la consommation d'énergie dans l'industrie (EACEI) conducted by the French statistical office (INSEE). EACEI collects data on consumption of electricity, natural gas, coal, oil and other fuels (12 energy sources in total) for manufacturing establishments. As in similar plant-level surveys, sampling probabilities depend on size. All plants having at least 250 employees are included in EACEI, while plants with more than 20 employees are sampled through a two-level stratification procedure based on employment class and location. The response rate is very high: for example, 90% of the plants surveyed responded to the 2014 wave. To compute plant-level CO_2 emissions from fuel combustion, we follow the common practice of multiplying CO_2 emission factors from the French Environment and Energy Management Agency (Ademe) for each different fuel source available in EACEI (Marin and Vona 2017, Dussaux 2020).

The EACEI survey is used to compute the average unit energy cost, which is equal to the ratio between energy expenditure and energy consumption in tons of oil equivalent (i.e., toe). Following previous works (Davis et al. 2014, Aldy and Pizer 2015), we refer to the average unit energy cost as energy price and use it as a proxy of environmental policy stringency.

The main dependent variables used in this paper are CO_2 emissions (in tons) and emissions intensity. The latter is computed as emissions per unit of output, which has been deflated using sectoral deflators provided by the INSEE. We also checked the robustness of our results to the use of value added to rescale emissions because it is not clear which measure is theoretically better.

Cui et al. (2015), Barrows and Ollivier (2018a,b) and Gutiérrez and Teshima (2018). Fewer papers studied the environmental performance of multinationals compared to domestic firms, e.g., Eskeland and Harrison (2003), Cole et al. (2008), and Brucal et al. (2018).

TABLE 1
Map of data sources

Confidential data	Level	Years covered	Coverage	Main information as used in the paper
EACEI	Establishment	1994–2015	Survey on approx. 10,000 establishments per year	Domestic CO_2 emission, energy prices
Customs data (données Douanes)	Firm	1995–2014	Universe of importers and exporters	Import values and quantities
FARE–FICUS	Firm	1995–2015	Universe of companies	Revenue and value added
DADS	Establishment	1996–2015	Universe of establishments	Use employment weight to identify the companies with high employment coverage in EACEI (>90%)
Publicly available	**Level**	**Years covered**	**Coverage**	**Main information as used in the paper**
WIOD (release 2013 and 2016)	Sector-by-country	1995–2016	14 manufacturing sectors, 40 countries (OECD plus BRICS)	Revenue and input–output data to computed indirect emissions
IEA emission data	Sector-by-country	1994–2017	14 manufacturing sectors, 281 countries	Emissions of foreign countries
Sato (2014)	Product	Time invariant	4-digit products, SITC classification	Carbon content of product, definition of dirty products

NOTES: Access to confidential data through the French Secure Data Access Center (CASD). Detailed administrative procedures for accessing the data are available in the website: www.casd.eu/en. Sato's (2014) data are available upon request from the author.

Imports and imported emissions

We use data on imports by product and destination from customs offices (so-called *données Douanes*). Import data are available at the firm level, so we aggregate the plant-level EACEI data at the firm level to analyze the relationship between trade liberalization and emissions at the level of aggregation where trade shocks occur. Previous studies perform plant-level regressions, but they measure imports at a more aggregated level, using either the sector (Cherniwchan 2017) or the firm (Li and Zhou 2017) import levels. An advantage of our study is that we observe both the dependent and the main variable of interest at the same level of aggregation, thus reducing possible measurement error.

Practically, to aggregate EACEI data at the company level, we need to retain only those companies whose establishments are fully (or almost fully) covered in EACEI. To do this, we compute the proportion of each firm's employees working at all of the firm's establishments observed in the EACEI survey, using the Déclaration annuelle des données sociales (DADS), a database containing information on employment-related variables for the universe of French establishments. As a threshold for inclusion in our baseline estimation sample, we keep firms with a share of employment covered in EACEI of 90% or more and impute carbon emission and all other relevant variables proportionally.[6]

To calculate imported carbon emissions, we combine confidential customs office data with data on foreign emission intensity at the country−sector level computed using emissions data from the International Energy Agency (IEA) and real output data from the World Input−Output Database (WIOD) socio-economic accounts released in 2013 and 2016 (see table 1). We use the 2018 edition of the IEA CO2 Emissions From Fuel Combustion database to compute emission intensities by country, sector and year. The IEA database covers annual data for 143 countries, 12 industrial sectors for more than 40 years (1971−2016).[7] Emissions are calculated using the IEA energy database and emission factors similar to those used for domestic emissions. The IEA energy database provides emissions for each fuel separately as well as for all fuels altogether. The IEA emission data have two key advantages compared with the emission data of the WIOD environmental accounts: (i) a greater coverage in terms of countries and years because WIOD emissions are available from 1995 to 2009 and only for a limited number of countries and (ii) a criterion of measuring CO_2 emissions that is consistent with that used in the EACEI data set. Indeed, IEA data consider only CO_2 emissions from fuel combustion, which represent 95% of total CO_2 emissions worldwide.[8]

The imported emissions of firm i are computed as follows:

$$ImpE_{it} = \sum_{k}\sum_{j} M_{ijt,k\in s}\, EI_{jt,k\in s}, \tag{1}$$

where $EI_{jt,k\in s}$ is the total (direct plus indirect) emissions intensity (i.e., tons of CO_2 per unit of real output) of product k of sector s in sourcing country j, while $M_{ijt,k\in s}$ is the imported quantity of firm i of products k of sector s from sourcing country j. Import values

6 For example, if only 92% of a firm's employment is covered in EACEI, we multiply observed emissions by 1/0.92.

7 The sectors are chemical and petrochemical; food and tobacco; iron and steel; machinery, mining and quarrying; non-ferrous metals; non-metallic minerals; non-specified industry; paper, pulp and printing; textile and leather; transport equipment; wood and wood products.

8 Calculations based on the World Development Indicators database. The ratio between CO_2 emissions from gaseous fuel consumption, liquid fuel consumption, solid fuel consumption and total CO_2 emissions equals 95% for the 1994−2014 period.

are deflated using the methodology proposed by Gaulier et al. (2008). Online appendix A.1 contains further details on this methodology, on the crosswalk between IEA and WIOD sectoral classifications and the management of missing data and outliers on foreign countries' emissions.

Two issues are worth discussing at this point. First, direct carbon emissions capture only part of the emissions generated to produce a unit of output inside the foreign country. Therefore, to compute EI_{jst} through equation (2), we follow Aichele and Felbermayr (2015) by applying the single-region input−output method that sums emissions directly generated by sector s in country j and emissions indirectly generated through the use of intermediate inputs from upstream sectors in country j.[9] We use the WIOD tables to allocate emissions to countries and production stage.[10]

Second, because detailed data on emission intensity at the product−country−year level are not available, our working assumption in equation (1) is to assign a uniform level of emissions per unit of output to all products k imported from the same sector−destination pair in year t. We are aware that this is a limitation of our study in light of what found in the only study with product-level emission factors (Barrows and Ollivier 2018a). We relax this assumption using an alternative measure of emission intensity, which also exploits time- and country-invariant carbon content of products derived from life-cycle assessments (see Sato 2014 and online appendix A for further details on this alternative measure). Given that results barely change when using this measure, we decided not to use this alternative measure because the quality of product-level data on carbon content is not certified by international organizations and such data do not vary over time and across countries.

3.2. Descriptive evidence

We motivate our research with two pieces of descriptive evidence. First, table 2 illustrates a well-known fact: emission intensity in developed countries, in our case France, is significantly lower than emission intensity in emerging and developing economies (we choose China as an example). The emission efficiency gap is significantly larger for indirect emissions, due primarily to the large gap in CO_2 emissions in the electricity sectors of the two countries. This suggests an obvious fact and a less obvious one. On the one hand, the massive relocation of industrial activities towards China and other emerging economies contributed mechanically to explaining the exponential increase in global emissions in last two decades. On the other hand, the gap is large in both energy and non-energy-intensive sectors such as the textile sector. This indicates that differences in environmental regulations, which affects mostly energy-intensive industries, may not be the main factor behind the emission intensity gap.

9 The emission intensities computed with the single-region input−output method account for 98% of the emissions intensities computed with the multi-region input−output method. See online appendix A3 for more details.

10 More specifically, we compute total emission intensity in foreign country as follows:

$$EI_{jst} = \frac{E_{jst}}{Y_{jst}} = \frac{1}{Y_{jst}} \left(E^D_{jst} + E^I_{jst} \right),$$

where E_{jst} is the total CO_2 emissions from fuel combustion of sector s in country j for year t, Y_{jst} is total real output of sector s in country j for year t, E^D_{jst} are direct emissions and E^I_{jst} are indirect emissions, computed as $E^I_{jst} = \sum_l Y_{jslt} E^D_{jlt}$. Y_{jslt} is the amount of output of sector j used as input in sector s in the same country (from the World Input−Output tables). Therefore, indirect emissions are only those generated within the same country.

TABLE 2
Gap in emission intensity between China and France

	Direct emission intensity			Indirect emissions		
Sector	China	France	Ratio	China	France	Ratio
Mining and quarrying	147	172	0.9	1,316	279	4.7
Food and tobacco	124	51	2.4	268	87	3.1
Textile and leather	69	24	2.9	229	48	4.8
Wood and wood products	56	17	3.3	254	46	5.5
Paper, pulp and printing	230	90	2.6	538	141	3.8
Chemical and petrochemical	328	45	7.3	910	77	11.8
Non-metallic minerals	1,285	406	3.2	2,066	506	4.1
Basic metals and fabricated metals	907	107	8.5	1701	162	10.5
Machinery	90	55	1.6	379	78	4.9
Transport equipment	20	13	1.5	179	42	4.3
Non-specified industry	116	5	23.2	316	20	15.8

NOTES: Authors' elaboration from IEA and WIOD data, year 2013. CO_2 emissions in tons per millions of euro. Indirect emissions are computed considering only domestic emissions from other sectors.

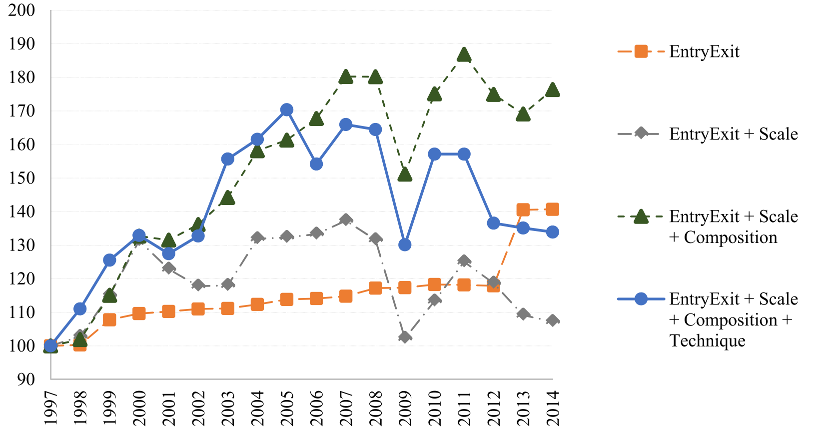

FIGURE 1 Decomposition of imported emissions, 1997–2014
NOTES: Authors' calculation based on trade flows from the French customs data and emission intensity computed using IEA and WIOD data. These statistics are for all firms in the French manufacturing sector.

The central idea behind the theory of pollution offshoring is that compositional effects, namely the relocation of polluting tasks abroad, occur both within and between firms. In our specific case, this implies that changes in the product mix within the firm are the main force driving the increase in imported emissions. As a first step to understand whether foreign emissions have contributed to the decrease in domestic ones, we decompose the trend in imported emissions into its main components: scale, composition, entry and exit, and technique (see online appendix B for details).

In figure 1, the blue line with circle corresponds to the actual evolution of imported emissions that are by definition identical to the sum of the different components of the decomposition. The bottom line of the decomposition is that changes in the product mix have been the main drivers of the 34% increase in imported emissions that we observe in the French manufacturing sector. In the absence of the other effects, changes in the product and destination mix would have increased the carbon content of imports by 69% (see online appendix table B1). Interestingly, technical improvements in foreign countries contributed to mitigate the increase in the carbon content of trade for French manufacturing. This result is consistent with those of papers showing that FDI and trade participation decrease emission intensity in developing countries (Brucal et al. 2018), but make the product mix in those countries dirtier (Barrows and Ollivier 2018a). Over the years, there have been more firms that started importing than firms that stopped importing, thus contributing to increase the scale effect along the extensive margin. This is consistent with decreasing trade costs, the extension of the European Single Market and the numerous trade agreements introduced between European member states and other countries during that period. In contrast, the emissions of firms already in the market due to change in import volume decreased over time. This is because the number of French manufacturing firms got smaller over time (Insee 2018).[11]

Note that the results of the decomposition do not change if we use the alternative measure of imported emissions, which corrects for time-invariant differences in the carbon content of products (online appendix table B2). Both our favourite measure and the alternative one are imperfect, but for different reasons. Therefore, we are reassured by fact that we find similar results using either.

3.3. Estimation sample

Before turning to the empirical analysis of the relationship between imported and domestic emissions, it is important to briefly discuss the way the estimation sample is selected. As in previous research using firm-level emission data (e.g., Marin and Vona 2021) combined with trade data (e.g., Barrows and Ollivier 2018a), our estimation sample is not representative of the entire population for four reasons. First, the information on emissions is obtained from the EACEI survey on approximately 10,000 establishments per year.[12] Second, only a subset of firms imports for at least three years, which, as will be explained in the next section, is the minimum required to compute the firm fixed effect and the instrument. Third, imports are available at the firm level; thus, we need to aggregate emissions at this level, retaining only firms for which we can observe at least 90% of establishments. For instance, firms in our estimation sample cover a modest share of imported emissions in manufacturing (approximately 20%) because many importing companies are not systematically surveyed in EACEI or are not retained after filtering the sample as just described. Finally, for the sake of a clean interpretation of the econometric results, we focus on importing firms (and in particular on firms always importing to isolate the intensive margin effect), further reducing the representativeness of our estimation sample relative to the population of French manufacturing firms. Nevertheless, the selected firms covered in our analysis exhibit similar trends in terms of emissions, imported emissions and emission intensity as the overall population. We report in the online appendix the evolution of the dependent variable for the whole population of French manufacturing companies (figure B1, using IEA−WIOD data) and our estimation sample (figure B2, using EACEI and FARE−FICUS data) to compare the estimation sample with the population of French manufacturing companies. The main takeaway is that CO_2 emissions, emission intensity and output exhibit qualitatively similar trends in

11 Notably, firms having more than 1,000 employees—more likely to import—decreased their size, as measured by their average number of employees, by 9% between 2006 and 2015, suggesting that on average their production and import volume decreased as well over the same period.

12 In the EACEI survey, large firms are always surveyed while small companies are randomly surveyed. Thus, the panel dimension of the data is often more available for large firms than for small ones.

the whole population and in our main estimations sample. In addition, the magnitude of the changes is similar, although revenue grows less in our sample than in the whole population. We observe an increasing trend in imported emissions both in the entire population and in our estimation sample, which is driven mostly by importing emissions from non-OECD countries (figure B3).

Table B3 in the online appendix contains detailed descriptive statistics on the evolution of the main variables of interest for our main estimation sample of "always importers" (panel A) and the 281 companies always present in our estimation sample (panel B). By using firms always importing, we can precisely identify the intensive margin shift triggered by trade liberalization. Including occasional importers would also reveal the direction of the extensive margin shifts. However, we cannot satisfactorily address the issue of self-selection into trade because it is extremely difficult to find a suitable instrumental variable for trade participation (see, e.g., Carluccio et al. 2015). In what follows, our empirical strategy is designed primarily to identify the within-firm intensive margin shift. This within-firm intensive margin shift captures the imported carbon emissions linked to intermediate rather than final goods. If the production of polluting companies is entirely relocated abroad in response to differences in environmental policy stringency, then our estimate will represent a lower bound of the effect of imported carbon emissions driven by a pollution haven motive because it does not capture that extensive margin shift.

4. Empirical strategy

This section presents the empirical strategy to estimate the effect of imported emissions. We first focus on the identification of this effect. Second, we turn to the second main contribution of this paper: assessing the relative importance of carbon imports and policy-induced effect in reducing CO_2 emission intensity.

4.1. Estimation equation

Our starting point is the following reduced-form specification:

$$ln(e_{it}) = \alpha \cdot ln(ImpE_{it}) + \tau_{kt} + \theta_{rt} + d_{i,t=0} \times \varphi_t + \mu_i + \varepsilon_{it}. \tag{2}$$

At this stage of the discussion, the main coefficient of interest α capture the contemporaneous association between emission intensity e_{it} (or emissions E_{it}) and imported emissions $ImpE_{it}$. The log–log specification corrects for the skewed distributions of both imports and emissions, while allowing a straightforward interpretation of α as an elasticity of domestic to imported emissions.[13]

We include firm fixed effects μ_i to control for time-invariant unobservable characteristics that are correlated with both imported and domestic emissions. Similarly, region (of France)-by-years θ_{rt} and sector-by-year dummies τ_{kt} absorb demand and supply shocks in the local labour market (NUTS2 regions) or sector of activity (two-digit NACE rev. 2), respectively.

Controlling for firm size is important because size is a well-known determinant of both productivity improvements and firms' engagement in the global markets (e.g., Melitz 2003,

13 To deal with the skewness of the distribution of CO_2 emissions, our main results are obtained by dropping the top and the bottom 1% of observations. Results are, however, consistent when keeping those outliers.

Bustos 2011). However, the direct inclusion of firm's revenue among the covariates is problematic because revenue is endogenous and thus a bad control, leading to biased estimates (Angrist and Pischke 2008). To break the dynamic association between size, productivity and imports while comparing firms of similar size, our favourite specification allows for differential trends in emissions depending on initial size dummies $d_{i,t=0}$, measured as the deciles of the initial distribution of revenue. However, allowing for flexible time trends for different decile of initial size may not be enough to predict time-varying size effects, especially in a long panel as ours. Consequently, we cannot exclude that the relationship between emissions and imported emissions is spurious, reflecting the simultaneous correlation of these two variables with size. By contrast, the residual influence of size is mitigated when we estimate the association between emission intensity and imported emissions, especially after controlling for trends that depend on initial size dummies. Therefore, although we are interested in the effect of import competition on both emissions and emission intensity, our econometric analysis focuses primarily on the latter.

The main source of variation left to identify the carbon import effect is the within-firm one, depurated from any shocks common to firms in the same sector, region and size class. This is similar to the approach followed in related papers of Li and Zhou (2017) and Cherniwchan (2017), which focus on the within- rather than the between-firm variation as a first step to isolate a causal effect.

4.2. Endogeneity issues

Ideally, we would like to use our estimates of α to answer policy relevant questions such as the effect of a tariff reduction for dirty products on domestic carbon intensity. Estimating α through equation (2) is not enough to answer these questions for well-known endogeneity concerns. Time-varying unobservables such as demand and supply shocks may affect both emission intensity and imported emissions, leading to inconsistent estimates of the carbon import effect. Moreover, as discussed in previous section, imported emissions are measured with error leading to an attenuation bias in our OLS estimates. Finally, reverse causality can be an issue as long as forward looking managers adjust their product mix (and so the imported product mix) to reduce current and future emissions in response to anticipated regulatory changes.

The direction of the estimation bias is not straightforward a priori. Unobserved shocks to the French product market are positively correlated with imports, but their association with emission intensity is unclear. Domestic supply shocks relevant for emission intensity and imports are related primarily to the adoption of energy-saving technologies. Theoretically, adopting energy-saving technologies is the main alternative strategy to the offshoring of dirty tasks.[14] Therefore, firms that adopt such technologies should have both lower emission intensity and lower imported emissions. If α is negative, we expect a bias toward zero which is amplified by the attenuation bias due to measurement error discussed above. Overall, we expect the OLS estimate of α to go against the existence of the carbon import effect.

Following a well-established literature on the labour market effects of trade liberalizations (Autor et al. 2013, Hummels et al. 2014, Carluccio et al. 2015), we use global supply shocks directed to other countries but France and its neighbouring countries to mitigate the bias in

14 Obviously, a firm can simultaneously offshore and innovate. The stark distinction is made here only to illustrate the direction estimation bias.

the estimate of the carbon import effect. This instrument captures the potential exposure to such shocks and has a typical shift-share structure:

$$IV\_ImpE_{it} = \sum_k \left(s_{ik,t<t_0} \sum_j \left(e_{jk,t<t_0} \, WS_{jk,t} \right) \right). \tag{3}$$

The shift component is $WS_{jk,t}$, namely world exports of product k from origin country j in year t. To isolate supply shocks outside core EU countries, we consider world export $WS_{jk,t}$ directed to all countries except France and countries' bordering France (i.e., Germany, Spain, Italy, UK and Belgium). As for imported emissions (equation (1)), the shift $WS_{jk,t}$ are reweighted by the pre-sample emission intensity of origin country j in product k (i.e., in sector s to which product k belong to) $e_{jk,t<t_0}$ in all countries except France. This reweighting emphasize the environmentally related motive of industry relocation.[15] With this method, the instrument and thus the exposure to supply shocks takes a higher value if product k is more carbon-intensive.

The other share component of the instrument ($s_{ik,t<t_0}$) is the average pre-sample share of product k imported by firm i in the three periods before the first period t_0 in which the firm is observed in the sample. Using pre-sample share mitigates the reverse causality bias discussed above, but implies that we reduce the time span used to estimate the carbon import effect to 2000−2014.

4.3. Validation of the instrumental variable strategy

Our instrument identifies the carbon import effect provided that: (i) it is a good predictor of firm's imported emissions and (ii) it excludes the components of import shocks that are dependent on shocks in the French economy as well as on forward-looking behaviour of managers. As stated formally by Borusyak et al. (2022), the latter assumption is equivalent to random assignment of import shocks conditional on the vector of covariates.

On the first issue, table 3 illustrates that the instrument is a good predictor of imported emissions (and a classical instrument not weighted by emission intensity is a good predictor of imports in value) with an F-test passing the usual threshold of 10. The only exception shown in the second two of table 3 is for a specification where we control for revenue on total emissions and will be discussed in section 5.1. As would be expected by inspecting the first-stage results, imported emissions grow faster in firms with a dirtier initial mix of imports. The strength of the instrument is consistent with two well-known facts of the French customs data: (i) the set of imported product is very stable over time and (ii) there is little overlapping of product-specific shocks across firms (Carluccio et al. 2015).

On the second assumption, we use the insights from Borusyak et al. (2022) to test for the plausibility of conditional random assignment of carbon import shocks. First, this assumption may be violated because the weighted average of the shocks (i.e., our instrument) is correlated with pre-existing trends in CO_2 emissions. A positive correlation between our instrument and pre-sample emission trend indicates that the carbon import effect may just capture pre-existing trends. We make full use of our long panel to assess violations of the parallel trend assumption. We capture pre-trend in dependent variables using the average of the change in carbon emissions (in log) over the period 1995−1999 and the average of

15 In our set-up, pre-sample shares are computed as the average between $t_0 - 1$, $t_0 - 2$ and $t_0 - 3$, where t_0 is the first year in which we observe the firm. This implies that a firm should be observed for at least 3 years to be included in our main estimation sample.

TABLE 3
First-stage results

Specification	Endogenous variable	Coefficient instrument on the endogenous	Standard error	F-statistics of excluded instruments
Emissions (table 6)	Imported emissions	0.254	0.066	52.2
Emissions, adding revenue (table 6)	Imported emissions	0.058	0.061	3.1
Emissions intensity (table 7)	Imported emissions	0.254	0.066	52.2
Emissions intensity (table 8)	Imports and emission Intensity of imports	0.197	0.069	35
Emissions intensity (table 9)	Energy prices	0.046	0.008	65.8

NOTES: Only firms always importing are included in the estimation sample. All rows include firm fixed effects, industry (two-digit) × year dummies, region × year dummies and size class × year dummies. The instrumental variables for imported emissions is defined in the text (equation 3) and is a weighted average of supply shocks, measured as imported emissions, from all countries except France and neighbouring countries of France (Italy, Belgium, Spain, Germany and the UK) towards all countries except France and neighbouring countries of France. The weights, firm-specific, equal the product share of the firm total imports in the first three years of trade data available. The instrumental variable for emission intensity is constructed similarly, but the supply shocks are the emission intensities of the supplying countries. The instrument for imports is not weighted by emission intensity in foreign countries. Robust standard errors clustered at the firm level.

carbon emissions (in log) over the same period. We regress the instrument on the standard controls of equation (2) and the interactions between the two pre-trend variables and a time trend.[16]

Table 4 shows that past trends in emission intensity are uncorrelated with the instrument (column (1)), while past changes in emissions display a positive and weakly significant association (p-value = 0.1) with the instrument (column (2)). Although these results reassure us on the validity of our identification strategy, we add pre-trend to the set of covariates of equation (2) in a key extension of our main specification.

Second, we conduct a balance test of the distribution of the carbon import shocks. Random assignment of such shocks would require that covariates affecting emissions are not correlated with the import shocks. If this was the case, unobserved shocks would be more likely to be correlated with import shocks (Altonji et al. 2005). The balance test is carried out on a set of potential confounders that include capital intensity, value added per capita, total employment and revenue. We first aggregate the data at the level of the sector of origin of the shock at the two-digit level; then we regress the instruments and the endogenous on these covariates. Table 5 shows that, while imported emissions and imports are positively correlated with some of these covariates (columns (1) and (2)), the corresponding instruments are not correlated with them (columns (3) and (4)). This analysis shows that the instruments mitigate possible unbalances in the exposure to unobservable shocks. In addition, it justifies the fact of not including these covariates in our main specification of equation (2), although we check that our results remain robust to their inclusion in what follows.

Third, we consider slightly different IV strategies that exploit different sources of variation to estimate the carbon offshoring effect. This allows to assess the sensitivity of the results to

16 Note that we can focus on only the subsample of firms observed before 2000 for this exercise. Using this subsample also allows to test whether results are driven by firms that are less likely to incorporate future regulatory shocks, in particular related to the approval of the EU–ETS, in their choices.

TABLE 4

Instruments and pre-trend in emissions, 1995–1999

	Main instrument of			
	Imported emissions		Energy prices	
Dependent variable	(1)	(2)	(3)	(4)
Time × pre-sample average emission intensity (in log)	0.0008		0.0024***	
	(0.0005)		(0.0003)	
Time × pre-sample average changes in emission intensity (in log)	0.0025		−0.0025**	
	(0.0015)		(0.0011)	
Time × pre-sample average emissions (in log)		0.0007		0.0021***
		(0.0005)		(0.0003)
Time × pre-sample average changes in emissions (in log)		0.0027*		−0.0022**
		(0.0014)		(0.0009)
Constant	16.39***	2.457	−17.39***	−56.07***
	(3.927)	(12.84)	(1.983)	(6.896)
Observations	23,530	24,247	20,549	21,215
R-squared	0.991	0.991	0.792	0.792
Number of firms	2,762	2,857	2,434	2,521

NOTES: Only firms always importing are included in the estimation sample. Sector–year, region–year and size–year fixed effects always included. These estimates are performed on firms for which we have at least two observations before 2000 to build pre-sample changes in emission (or emission intensity). Standard errors clustered at the firm level are in parentheses. Statistical significance: ***p<0.01, **p<0.05, *p<0.1.

TABLE 5

Balance of covariates at the level of the shock (exporting sector in foreign country)

	Regressors			
Covariates	(1) Imported emission	(2) Imports	(3) IV imported emission	(4) IV imports
Capital (in log)	2.055**	1.206**	−0.158	−0.245
	(0.779)	(0.460)	(0.273)	(0.267)
Value added (in log)	0.110	0.0262	0.0540	−0.0475
	(0.160)	(0.130)	(0.106)	(0.0990)
Employees (in log)	2.804***	1.913***	0.278	0.186
	(0.873)	(0.527)	(0.362)	(0.290)
Revenue (in log)	−0.517	0.258	0.143	0.217
	(0.760)	(0.444)	(0.394)	(0.340)
Revenue (in log) in t0 × Year	−0.0004*	−0.0004***	0.0001	3.82e−05
	(0.0002)	(0.0001)	(0.00015)	(0.00013)
Number of sectors	30	30	30	30
Observations	510	510	510	510
R-squared	0.609	0.708	0.443	0.426

NOTES: Data are aggregated at the level of the sector of origin in the foreign country, at the two-digit level. Standard errors clustered at the foreign sector level in parentheses. Statistical significance: ***p<0.01, **p<0.05, *p<0.1.

subsets of most salient shocks. As in Carluccio et al. (2015), we use origin–destination variation in the initial shares. Furthermore, we exclude top 10 dirty products, top 10 imported products and fossil fuel products from the instrument. More details are provided in section 5.2.

Last, as suggested by Jaeger et al. (2018), the interpretation α in equation (2) is not straightforward because it may conflate past and present responses to trade shocks. For sake of interpretation, we explicitly account for the adjustment dynamics by adding lags of imported emissions to equation (2) and instrumenting each lag with the corresponding lagged instruments, built as in equation (3).

4.4. Role of policies

We use energy prices as proxies of policies following the lead of several recent papers in in the literature on the evaluation of environmental economics (e.g., Kahn and Mansur 2013, Aldy and Pizer 2015, Cullen and Mansur 2017, Sato et al. 2019, Dussaux 2020, Marin and Vona 2021). In absence of stringent carbon pricing policies around the world, this approach becomes very popular to shed light on the plausible impact of ambitious carbon pricing policies. The key assumption for the validity of this exercise is that companies adjust in a similar way to energy price changes that are policy-driven (i.e., an increase in a carbon tax) or driven by other factors (i.e., demand and supply). Conceptually, there are no reason to believe that such adjustments are different in the long run because what really matters for companies is whether the price change is perceived as permanent or not (Marin and Vona 2021).[17] Using the same data and empirical strategy as this paper, Marin and Vona (2021) show that long-term effect of energy prices on various outcomes is not significantly different from (although larger than) the short-term effect.

Our measure of energy prices is the average cost of energy from the EACEI data set. We first use energy prices to explain differences in imported emissions. If the pollution haven motive is the primary driver of emission reduction within a company, we expect to observe that firm responds to higher prices in France by relocating polluting tasks abroad.

Next, we include energy prices in equation (2) so we can compare the role of policy with that of carbon imports:

$$ln(e_{it}) = \alpha \cdot ln(ImpE_{it}) + \beta \cdot ln(p_{it}) + \tau_{kt} + \theta_{rt} + d_{i,t=0} \times \varphi_t + \mu_i + \varepsilon_{it}. \tag{4}$$

Conditional on imported emissions, we interpret the coefficient of energy prices on emission intensity β as an induced innovation effect. It is worth emphasizing here that the main goal of this extension is not to estimate the precise effect of energy prices on emissions, but to assess the plausibility of the pollution haven hypothesis and the robustness of the carbon import effect to an important confounder.

In this setting, energy prices are endogenous due to omitted variables, such as managerial capabilities and unobservable demand and supply shocks. Marin and Vona (2017) show that quantity discounts are a typical source of endogeneity. We follow a now standard approach in the literature and use also a shift-share instrument for energy prices (Lynn 2008, Sato et al. 2019, Dussaux 2020, Marin and Vona 2021). The instrument is:

$$IV\_p_{it} = \sum_f w^f_{i,t=t_0} \ln\left(p^f_{kt,-i}\right), \tag{5}$$

where $w^f_{i,t=t_0}$ is the share of fuel f (electricity, gas, oil, etc.) in total energy use of firm i at the pre-sample year 0 and $p^f_{kt,-i}$ is the median price of fuel f for the three-digit industry k in which firm i operates at year t. The shift has a leave-one-out structure, that is, the median price of the sector is computed without considering the price paid by the company i

17 Note also that because the energy price may be highly volatile, carbon taxes or the price of emission allowances can be highly uncertain, so in both instances, estimates of long-term effects are preferred.

for fuel f. As for the instrument of imported emissions, using pre-sample weights mitigate concerns related to reverse causality and simultaneity biases. The identifying assumption is again that shocks are as good as randomly assigned and it is difficult to test in practice. The key challenge is to test whether the parallel trend assumption is violated (Marin and Vona 2021).

Columns (3) and (4) of table 4 replicates for the instrument of energy prices the analysis on the influence of emission pre-trends done for the instrument of imported emissions. Consistent with results at establishment-level analysis (Marin and Vona 2021) and firm-level analysis (Dussaux 2020), the table shows that past trends in both emissions and emission intensity are highly correlated with IV_p_{it}, thus raising concerns on the credibility of the energy price instrument.[18] To mitigate possible violations of the parallel trend assumption, we always present an alternative specification where we directly control for pre-trends in emissions or emission intensity. Clearly, this is not the definitive solution for the endogeneity of energy prices, but, as already stated, the goal of this exercise is just to assess the robustness of the imported emission results and the plausibility of the pollution haven hypothesis.

5. Estimation results

This section is divided into four subsections. In section 5.1, we present the effect of imported emissions on total emissions. We then move to our main results on emission intensity in section 5.2 and to two critical extensions in section 5.3. Finally, we examine the extent to which results on emission intensity are driven by changes in energy prices in section 5.4.

All results are obtained estimating equations (2), (4) and (5), in which we cluster standard errors at the company level. We do not weight the estimates in our favourite specification because, as we discussed above, our sample is not representative of the French population of manufacturing company, but we check the robustness of our findings to weighting by firm revenue.

5.1. Results on emissions

Table 6 presents the carbon offshoring effect for total emissions. We find a positive and significant, although very small, association in the OLS specification (column (1)). The effect remains positive, but becomes insignificant in our favorite IV specification (column (2)). The absence of a negative correlation between domestic and imported emissions is at odds with the descriptive evidence presented in section 3. While on average there is a concomitant decline in domestic emissions and an increase in imported emissions, these do not seem to occur within the same company. Previous papers of Li and Zhou (2017) and Cherniwchan (2017) find a negative effect of importing but for local pollutants,[19] thus it is not necessarily surprising that these effects do not hold for global pollutants such as CO_2.

18 The parallel trend tests conducted by Marin and Vona (2021) are done for each fuel share, so implicitly for each sub-instrument composing the main energy price instrument. The results are very similar to those presented here.

19 Cherniwchan (2017) focuses on particulate matter and sulphur dioxide while Li and Zhou (2017) focus on toxic emissions equal to the all-media release of designated toxic chemicals.

TABLE 6
Emissions and imported emissions

	Dependent variable: Emissions (in log)			
	(1)	(2)	(3)	(4)
	OLS	IV	IV revenue	IV revenue until 2005
Imported emissions (in log)	0.0361***	0.134	−0.574	−0.437
	(0.0057)	(0.129)	(0.784)	(0.271)
Revenue (in log)			1.110	0.740**
			(0.968)	(0.290)
Observations	35,537	35,537	35,540	12,500
Number of firms	4,962	4,962	4,962	3,239
F-test excluded instrument		52.18	3.088	15.52

NOTES: Only firms always importing are included in the estimation sample. All rows include firm fixed effects, industry (two-digit) × year dummies, region × year dummies and size class × year dummies. In columns (3) and (4), we replace size class × year dummies with the log of revenue. In column (4), we run the model for the short time period 2000−2005. The instrumental variable defined in the main text is a weighted average of supply shocks from all countries, except France and neighbouring countries of France (Italy, Belgium, Spain, Germany and the UK) towards all countries except France and neighbouring countries of France. The weights, firm-specific, equal the product share of the firm total imports in the first three years of trade data available and are adjusted for the average emission intensity of the product. Robust standard errors clustered at the firm level. *p<0.1, **p<0.05, ***p<0.01.

A possible explanation for obtaining a different result is that we do not properly account for the scale effect by using time trends specific to the deciles of initial revenue. Because average revenue increased substantially in the period of our analysis, a scale effect may fully offset the substitution effect associated with the offshoring of domestic emissions abroad. To tackle this issue, we amend our main estimation equation (1) by directly including revenue instead of initial revenue dummies interacted with year dummies. Column (3) presents this extension. Although the negative sign and the magnitude of elasticity between domestic and imported emissions indicates some offshoring of carbon emissions, the lack of statistical significance and the weakness of the instrument prevent us from drawing any solid conclusion. Column (4) shows that both the precision of the estimate (p-value = 0.110) and the predictive power of the instrument improves considerably when we consider a shorter time span 2000−2005. This is consistent with the structure of shift-share instruments, whereby the initial share is fixed before 2000 and thus loses predictive power the farther the year is from 2000.

In the online appendix, we show that explicitly controlling for pre-trends in emissions leads to similar results (table 1C). To explore whether this lack of effect of imported emissions on total emissions masks heterogeneous effects, we extend the model of equation (2) by allowing the effect of imported emissions to differ in energy-intensive sectors.[20] We expect energy-intensive sectors to have higher incentives to relocate carbon-intensive productions in foreign countries as a response to current and expected climate policies. Online appendix table C2 shows that this expectation is corroborated by the data. The interaction

20 In this paper, an energy-intensive sector is defined as a sector that has an emission intensity that is higher or equal to the median emission intensity across all French manufacturing sectors. The emission intensities are averaged over the entire observation period.

TABLE 7
Emission intensity and imported emissions

	Dependent variable: Emissions/revenue (in log)			
	(1) OLS	(2) IV	(3) IV, including pre-trend	(4) IV, pre-trend sample
Imported emissions (in log)	−0.0615***	−0.489***	−0.392***	−0.460***
	(0.0061)	(0.164)	(0.137)	(0.147)
Time × pre-sample average emission intensity (in log)			−0.0161***	
			(0.0016)	
Time × pre-sample average changes in emission intensity (in log)			−0.0141***	
			(0.0048)	
Observations	35,537	35,537	23,530	23,530
Number of firms	4,962	4,962	2,762	2,762
F-test excluded instrument		52.18	56.52	57.43

NOTES: Only firms always importing are included in the estimation sample. All rows include firm fixed effects, industry (two-digit) × year dummies, region × year dummies, and size class × year dummies. All columns include firm fixed effects, industry (two-digit) × year dummies, region × year dummies and size class × year dummies. The instrumental variable defined in the main text is a weighted average of supply shocks from all countries, except France and neighbouring countries of France (Italy, Belgium, Spain, Germany and the UK) towards all countries except France and neighbouring countries of France. The weights, firm-specific, equal the product share of the firm total imports in the first three years of trade data available and are adjusted for the average emission intensity of the product. Robust standard errors clustered at the firm level. *p<0.1, **p<0.05, ***p<0.01.

term between the dummy for energy-intensive sectors and imported emissions is negative in most specifications, but precisely estimated only in the specification controlling for pre-trends.

Overall, the firm-level analysis highlights an important methodological challenge in estimating the effect of trade liberalization on emissions. The result of table 6 confirms that reduced-form strategies fail to control satisfactorily for size effects when the dependent variable is not rescaled, such as for the case total emissions, and especially in a long panel. In light of this result, the reminder of the paper will focus on emission intensity, which by incorporating revenue into the dependent variable, captures the net effect of import competition that results from the combination of a scale and a substitution effect.

5.2. Results on emission intensity

Table 7 exposes the main results of the impact of imported carbon emissions on emission intensity. The key finding is that importing carbon-intensive products improves the domestic efficiency in the use of dirty inputs. In our baseline IV model (column (2)), the elasticity is quite large (−0.49), but declines slightly to −0.39 if we control for pre-trends in the restricted sample of firms present before 2000 (column (3)). Column (4) shows that it is controlling for pre-trends, and not considering firms present before 2000, that reduces the size of the elasticity. Using the conservative estimates of −0.39, domestic emission intensity would have been 33% higher if imported emissions remained at the level of the initial years (2000−2002).[21]

21 We use in-sample figures for the evolution of emission intensity and imported emissions for always importers that are reported in online appendix table B3, panel A. To obtained the

Our findings that carbon offshoring reduces emission intensity but not total emissions suggest that offshored emissions are compensated by an increase in the production scale. The firm's choice of importing dirty products is expected to lead to reductions in production costs, allowing carbon importers to capture a higher share of domestic market. To illustrate this mechanism, we regress revenue (in log) on imported emissions (in log) using the same specification of equation (2). We find an elasticity of 0.62 that is statistically different from zero.[22] This provides some empirical support that offshored emissions are offset by a positive scale effect at the firm level, leaving firms' total emissions unaffected.

When we compare the OLS (column (1)) and the IV (column (2)) estimates for emissions and emission intensity in table 7, the bias towards zero of OLS estimates becomes evident. As explained in section 4.2, we interpret this bias as the resultant of unobservable technological choices correlated with both emission intensity and carbon imports. In response to external regulatory pressure, public opinion and stakeholders, managers can reduce the carbon content of production either by innovating or by relocating polluting tasks abroad. By construction, our two-stage IV strategy estimates the effect on the compliers (Angrist and Imbens 2003), that is, those who decide to offshore in response to a reduction in the implicit cost of dirty tasks' relocation. Non-compliers, instead, are insensitive to the new offshoring opportunity and, being "innovators," have low levels of both emissions intensity and import of polluting goods, thus explaining the direction of the estimation bias.

In online appendix C, we conduct a series of robustness checks that confirm the presence of a significant effect of imported CO_2 emissions on domestic emission efficiency. The first battery of checks is linked to the discussion in section 4.3 and is a direct assessment of the identification strategy. Online appendix table C3 illustrates the robustness of our results to the use of slightly different sources of identifying variation in the instrument.[23] As expected from the balance tests conducted in table 5, adding controls for capital intensity, total employment and labour productivity does not change the estimated effect of imported emissions (online appendix table C4).[24] Finally, we do not loose precision and statistical significance in the estimates of the imported emission effects when clustering standard errors by domestic or foreign sectors (online appendix table C5).

Our results are also unchanged when measuring emission intensity as emissions over value added rather than revenue (online appendix table C6), weighting the regression by

historical variation in emission intensity explained by carbon offshoring for the sample of the always importers, we multiply the unweighted growth rate of imported emissions based on the difference between the moving average of the last three years (2012−2013−2014) and the moving average of three first years (2000−2001−2002). We take the moving averages before computing the growth rates to avoid the influence of outlier years (e.g., 2001) in our quantification. The growth rate of imported emissions is equal to 25.7% (online appendix table B3), that is multiplied by the estimated elasticity of −0.39 to obtain predicted change in domestic emissions intensity. Then, we divide this predicted change in emission intensity with the historical one in the same sample and also computed using the moving average of the first three and the last three years (−30.2%).

22 Detailed results are available upon request by the authors.

23 The only exception is a weak instrument problem emerging when we exploit the full product-by-country variation available in our data as in Carluccio et al. (2015) (row 4), but this instrument becomes stronger when we use imports rather than imported emissions as the main variable of interest (row 5).

24 In online appendix table C6, we show that firms that are more productive are also less emission-intensive, while capital intensity and emission intensity are positively correlated.

TABLE 8
Emission intensity and imports

	Dependent variable: Emissions/revenue (in log)					
	Imports		Emission intensity of imports		Both imports and emission intensity of imports	
	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Imports (in log)	−0.0935***	−0.860***			−0.094***	−0.704**
	(0.0074)	(0.315)			(0.007)	(0.279)
Emission intensity of imports (in log)			0.0007	0.255	0.0132	0.825
			(0.0099)	(0.513)	(0.0097)	(0.746)
Observations	35,537	35,537	35,537	35,537	35,537	35,537
Number of firms	4,962	4,962	4,962	4,962	4,962	4,962
F-test excluded instrument		34.98		16.02		8.845

NOTES: Only firms always importing are included in the estimation sample. All rows include firm fixed effects, industry (two-digit) × year dummies, region × year dummies, and size class × year dummies. The instrumental variables for imports is a weighted average of supply shocks, measured as import value, from all countries, except France and neighbouring countries of France: Italy, Belgium, Spain, Germany and the UK, towards all countries except France and neighbouring countries of France. The weights, firm-specific, equal the product share of the firm total imports in the first three years of trade data available. The instrumental variable for emission intensity is constructed similarly but the supply shocks are the emission intensities of the supplying countries. Robust standard errors clustered at the firm level. *p<0.1, **p<0.05, ***p<0.01.

average revenue (table C7) and considering the larger sample of companies importing for at least three years (table C8). As would be expected, the carbon import elasticity is larger when we include occasional importers, suggesting that the extensive margin shift adds to the intensive margin effect that we estimate in our main specification. Interestingly, the carbon import effect is also larger when weighting, although, for the reason discussed in section 3.3, we cannot consider weighted regressions as representative of the entire population of French manufacturing establishments. Finally, using the Jaeger et al. (2018) approach to distinguish long- and short-term effects, we find that the effects estimated without including lagged terms in imported emissions are slightly smaller than the long-term effect, implying that our favourite specification provides a conservative estimation of imported carbon emissions (online appendix table C9). Note, however, that the instruments become weak in this specification so this conclusion should be taken with cautious.

5.3. Interpreting the effect of imported emissions

This section discusses a series of extensions that dig deeper into the mechanisms behind the carbon import effect. In the first extension, we aim at understanding whether the carbon import effect is driven primarily by an increase in the import volume by the average firm in our sample. In doing so, we replace imported emissions with imports in the model of equation (2) (and we modify the instrument accordingly). Results, presented in columns (1) and (2) of table 8, reveal that the estimated elasticity of emission intensity to imports is 1.7 times larger than that of imported emissions.[25] However, because the measurement error for imported emissions and its instrumental variable is likely larger than for total imports, the estimated coefficient for imported emissions might still suffer from an attenuation bias that overestimates the difference between the two coefficients. In any case, the quantified

25 As for our main results on the impact of imported emissions on emissions, we find no effect of imports on emissions. Results are available upon request by the authors.

impact of imports on emission intensity is of similar magnitude of imported emissions because imports increased by only 18% in our primary estimation sample while imported emissions increased by 25.7%. This finding provides a first indication that the carbon import effect is unlikely to be driven by a Pollution Haven effect.

To further dig into this issue, in columns (3) and (4), we estimate the association between emission intensity and emission intensity of imports, modifying the endogenous and the instrument accordingly.[26] We find no association between the domestic and imported emission intensity, even when we add both imports and the emission intensity of imports together (columns (5) and (6)). Conditional on firm fixed effects, this lack of association implies that companies becoming less carbon-intensive do not import products that become more (or less) carbon-intensive abroad. Because the changes in the emission intensity of imports largely depends on technological choices of foreign companies, it would have been surprising to find a statistically significant association.

Next, we explore several sources of heterogeneity in the negative relationship between imported emissions and domestic emission efficiency. Results are briefly exposed here and presented in details in online appendix C. In a nutshell, we find that the elasticity of domestic emission intensity to imported emissions is stronger in energy-intensive sectors (table C10), among exporters (table C11) and on high-productivity companies (table C12).[27] All these results are expected from findings in previous works. While the stronger elasticity in energy-intensive sectors reveals the greater opportunity cost for improving emission efficiency through the offshoring of dirty tasks in these sectors, exporters reap the benefits of trade liberalization by becoming more efficient along all dimensions, including an environmental one (e.g., Forslid et al. 2017, Barrows and Ollivier 2018a and Gutiérrez and Teshima 2018). The same occurs for high-productivity companies that arguably have better technological capabilities. In these specifications with interaction terms, the coefficient associated with imported emissions is often estimated less precisely becoming nearly significant with p-values marginally above 0.1. We interpret this result as indicating that an important, but hard to quantify, fraction of the carbon import effect estimated in table 7 is conflating a productivity-enhancing effect, which is also correlated with the exporting status of the firm.

Although the relationship between domestic emission intensity (and emissions) and imported emissions is stronger in energy-intensive sectors, the evidence presented so far seems to downplay the "pollution haven" driver of the negative imported emission elasticity. To corroborate this interpretation, we directly test of the pollution haven hypothesis by regressing imported emission on energy prices, instrumented as described in section 4.4. We find a positive but statistically insignificant impact of energy prices, on imported emissions (table C13), which is consistent with existing results on the EU−ETS (Martin et al. 2014, Naegele and Zaklan 2019). However, the effect becomes statistically significant (and large) on the share of imported on domestic emissions (columns (3) and (5)). Again, the pollution

26 In particular, imported emission intensity is computed as $ImpE_{it} = \sum_k \sum_j (M_{ijt,k\in s}/M_{it,tot})$ $EI_{jt,k\in s}$, where $M_{it,tot} = \sum_j M_{ijt}$. The instrument is $IV_{it} = \sum_p s_{ip0} \left(\sum_j e_{jpt}\right)$, where notation of equation (2) applies.

27 In our estimation sample, most firms are both importers and exporters, thus we cannot exclude that such effect contaminates the imported emission effect. Empirically, it is not easy to tackle this issue because export status and intensity are also endogenous, thus it is difficult to find strong instruments for both imports and exports. By instrumenting both export (instrument built as in Carluccio et al. 2015) and imported emissions, the F of excluded instruments is below the cut-off level of 10. These results are available upon request.

haven effect is more evident in energy-intensive sectors, except for the share of imported on domestic emissions (table 14C). Taken these results together, the effect of energy prices is driven clearly by the denominator of the ratio on between imported and domestic emission, a result consistent with previous findings using the same data (Marin and Vona 2021, Dussaux 2020).[28]

Overall, the analysis of this section leads to two main conclusions regarding the mechanisms behind carbon offshoring. First, a large share of emission intensity improvements due to trade are driven by other motives than pollution haven. Second, trade liberalizations have two effects on emission intensity that both play a role: a carbon import effect and a productivity-enhancing effect, which is driven partly by exporting. This latter result deserves further scrutiny in future research to precisely quantify the importance of these two margins through which trade affects emission intensity.

5.4. Energy price vs. carbon offshoring impacts

The final step of this paper is to go back to the fundamental question of the inducement effect of environmental policies and the relative role of imported emissions and technology in reducing the carbon footprint of French production (Levinson 2009, Shapiro and Walker 2018). The richness of the data used in this paper allows to tackle this issue looking at firm-level reactions. We add a proxy of environmental policies, energy prices, and properly instrument it as described in section 4.4. Conditional on carbon imports, the effect of energy prices on emission intensity can be interpreted as a technological inducement effect as in, e.g., Shapiro and Walker (2018).

Table 9 presents the main results of this analysis. Because the instrument of energy prices is positively correlated with pre-trends in emission intensity, the main table shows also the results controlling for pre-trend (columns (4) to (6)). To assess the extent to which the inclusion of carbon imports alters the effects of energy prices, we present both the results of without carbon imports (columns (1), (2), (4) and (5)) and with carbon imports (columns (3) and (6)).

The main takeaway from this analysis is that carbon import and price inducement effect are quite independent. The policy inducement effect remains very similar if we include or not carbon offshoring (e.g., column (6) vs. column (5)), which is consistent with the small and insignificant effect of energy prices on imported emissions that we have discussed in the previous section.[29] However, the policy inducement effect becomes imprecisely estimated if

28 Following Ederington et al. (2005), an alternative way to address the extent to which broad differences in environmental policy stringency drives our results is to compare the carbon import effect for OECD and non-OECD countries. Trade between OECD countries is usually associated to technological improvements than to cost savings. Conversely, trade in polluting industries between France and non-OECD countries is more likely to respond to a pollution haven effect and, in general, to cost-saving considerations. Online appendix table C15 shows that the carbon offshoring effect is present in trade with both groups of countries, but it is significantly stronger for OECD countries, contrary to what one would expect if importing is driven by differences in environmental regulation. Note, however, that the volume of imported emissions increased significantly more for non-OECD (+61%) than for OECD countries (0.3%) over the sample period. Thus, the overall effect is larger for non-OECD countries than for OECD countries.

29 The magnitude of the price effect is in line with what found by Marin and Vona (2021) and Dussaux (2020) on emissions, but slightly larger because our estimation sample is even more bias towards large companies involved in international trade.

TABLE 9
Emission intensity, imported emissions and energy prices, controlling for pre-trends

	Dependent variable: Emissions/revenue (in log)					
	(1) OLS	(2) IV	(3) IV	(4) OLS, including pre-trend	(5) IV, including pre-trend	(6) IV, including pre-trend
Energy prices (in log)	−1.196***	−1.176***	−1.185**	−1.104***	−1.303*	−1.261
	(0.0565)	(0.408)	(0.524)	(0.0751)	(0.674)	(0.828)
Time × pre-sample average emission intensity (in log)				−0.0102***	−0.00895**	−0.00833
				(0.00158)	(0.00441)	(0.00550)
Time × pre-sample average changes in emission intensity (in log)				−0.00898*	−0.00855*	−0.0121**
				(0.00462)	(0.00469)	(0.00535)
Imported emissions (in log)			−0.528***			−0.497***
			(0.181)			(0.148)
Observations	25,915	25,915	25,915	17,121	17,121	17,121
Number of firms	4,120	4,120	4,120	2,315	2,315	2,315
F-test excluded instrument		65.77	20		33.08	16.28

NOTES: Only firms always importing are included in the estimation sample. All rows include firm fixed effects, industry (two-digit) × year dummies, region × year dummies and size class × year dummies. In columns (3) and (4), we replace size class × year dummies with the log of revenue. The instrumental variable defined in the main text is a weighted average of supply shocks from all countries, except France and neighbouring countries of France (Italy, Belgium, Spain, Germany and the UK) towards all countries except France and neighbouring countries of France. The weights, firm-specific, equal the product share of the firm total imports in the first three years of trade data available and are adjusted for the average emission intensity of the product. The instrumental variable for energy prices is a weighted (leave-one-out) average of industry level fuel prices. The fuel weights, firm-specific, are the share of the fuel in total energy use of the firm. The industry-level fuel prices are the median price at the three-digit industry level. Robust standard errors in parentheses clustered at the firm level. *p<0.1, **p<0.05, ***p<0.01.

we control for pre-trends in emissions intensity. Interestingly, when weighting by revenue (table C16), the effect of energy prices is dominated by the effect of imported emissions in the favourite specification with pre-trends. The latter remains statistically significant at conventional level, while the former is far from being significant. This indicates that the carbon import channel for reducing emissions is more important than the policy inducement channel for larger companies.

While these estimates should be taken with caution as tackling two causal problems in a reduced-form econometric model is always problematic, the important finding of this extension is that the carbon import effect remains unchanged if we control or not for a proxy of environmental policy stringency. This result reinforces the main interpretation of our finding, which is that the offshoring of carbon emissions abroad is not explained primarily by differences in environmental policy stringency.

6. Conclusions

In this paper, we use a unique dataset that combines information on carbon emissions, imports, imported emissions and environmental policy stringency, all at the firm level, on a panel of around 5,000 firms operating in the French manufacturing sector to show that imported carbon emissions cause a decrease in French firms' domestic emission intensity. Most importantly, we provide evidence suggesting that this carbon import effect is not due primarily to a pollution haven effect (except perhaps in energy-intensive sectors) but to a general increase in the propensity to import of French companies. Finally, we find that the stringency of domestic carbon pricing policies (as proxied by energy prices) has a larger effect on firms' domestic emission intensity than carbon offshoring. However, these results should be taken cautiously because the identification of energy price effects on emission presents some unresolved challenges.

One policy implication of our results is that, within the firm's boundaries, increased energy costs does not lead to a substantial increase in imported emissions and thus to an increase in carbon emissions in foreign countries, i.e., carbon leakage. Carbon leakage might still occur through competition on the final products market and firm exit, but the finding that it does not seem to happen within the firm—at least at the current level of carbon policy stringency gap across countries driving part of the difference in relative energy costs—is certainly reassuring as regards the effectiveness of unilateral carbon pricing policies.

The issue of introducing carbon border adjustment mechanism (CBAM) is the subject of renewed interest and policy discussions in a context of increased divergence in climate policy ambition, where many countries and regions have decided to move towards carbon neutrality by 2050. Our results, combined with the complexity of designing CBAMs that are both effective and compatible with the current multilateral system of trade rules together with their potential to increase trade tensions, suggest that this policy instrument should be considered with caution, at least as long as the pollution haven effect is used as primary justification for its implementation. Further widening of the policy stringency gap may however alter this conclusion. Moreover, a CBAM may still be an excellent policy instrument to create incentives to improve emission efficiency in emerging economies and to ensure an equal distribution of abatement efforts across countries.

Our paper has a number of limitations. First, although we cover a wide range of firms in terms of size and sector, our sample is overrepresented by large firms. This is an unavoidable feature of energy consumption surveys, amplified by the fact that firms engaged in international trade are also much larger than the average. Second, in our reduced-form specification, we do not explicitly model other factors affecting importing such as labour costs. Teasing out the impact of these various factors behind production cost differences on the location of carbon emissions is an interesting avenue for future research. Finally, we do not explore the role of the extensive margin of imports mainly because the empirical setting does not offer a suitable instrumental variable varying at the firm level. This is also left for future analyses.

Supporting information

Supplementary material accompanies this article. The data and code that support the findings of this study are available in the Canadian Journal of Economics Dataverse at https://doi.org/10.5683/SP3/4BIGZS.

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