Trade Policy Uncertainty, Offshoring, and the Environment: Evidence from US Manufacturing Establishments*

Jaerim Choi† University of Hawai'i

Jay Hyun‡ HEC Montréal

Gueyon Kim§ UC Santa Cruz

Ziho Park¶ National Taiwan University

This version: May 7, 2023

[Link to the latest version]

Abstract

We study long-run environmental impacts of trade liberalization on US manufacturing and the underlying mechanism by exploiting a plausibly exogenous reduction in US trade policy uncertainty: the conferral of Permanent Normal Trade Relations (PNTR) to China. Using detailed data on establishment-level pollution emissions and business characteristics—including trade activities and global subsidiary information—from 1997 to 2017, we show that establishments reduce toxic emissions in response to a reduction in trade policy uncertainty. Emission abatement is mainly driven by a decline in pollution emission intensity, and not by establishment exits or a reduction in production scale. Emission reduction is more pronounced for (i) establishments with foreign sourcing networks, (ii) those under more stringent environmental regulations, (iii) those operating in more upstream industries, and (iv) those that belong to a multi-sector firm. We provide comprehensive evidence that supports the pollution offshoring hypothesis: US manufacturers, especially those that emit pollutants more intensely, begin to source from abroad and establish more subsidiaries in China after PNTR.

JEL Codes: Q53, Q56, F14, F18, F23

Keywords: Pollution Haven Hypothesis, Pollution Offshoring Hypothesis, Toxics Release Inventory, Pollution Emissions, Trade and Environment, Trade Policy Uncertainty, Offshoring, PNTR

---

*We are grateful to Ryan Abman, Pol Antràs, Ed Balsdon, Johannes Boehm, Justin Caron, Yoosoon Chang, Sally Chen, Jevan Cherniwchan, Mijin Cho, Sunghoon Chung, Mario Crucini, Taeyoung Doh, Peter Egger, Masahiro Endoh, Federico Esposito, Don Fullerton, Sanjana Goswami, Jung Hur, Sam Hwang, Anna Ignatenko, Jota Ishikawa, Larry Karp, Minho Kim, Christopher Knittel, Yuzuru Kumon, Jennifer La'O, James Lake, Yang Liang, Guido Lorenzoni, Clark Lundberg, Ryo Makioka, Christos Makridis, Erin Mansur, Julien Martin, Camelia Minoiu, Jakob Munch, Ilan Noy, Rajesh Ramachandran, Natalia Ramondo, Akira Sasahara, Georg Schaur, Peter Schott, Joseph Shapiro, Yongseok Shin, Nori Tarui, Kensuke Teshima, Vladimir Tyazhelniko, Martin Uribe, Michael Weber, Jeremy West, and seminar participants in the NBER-Environment and Energy Economics Program Meeting (Cambridge), Hawaii-Hitotsubashi-Keio Workshop on International Economics, Keio University, KIEP-KAEA (Boulder), Korean Association of Trade and Industry Studies Conference (Korea University), Midwest International Trade Conference (University of Tennessee), San Diego State University, and the World Bank Conference on "Trade and Global Value Chains in Times of Insecurity" for their invaluable comments and suggestions. First draft: February 1, 2023.

†Department of Economics, University of Hawai'i, choijm@hawaii.edu

‡Department of Applied Economics, HEC Montréal, jungsik.hyun@hec.ca, Jungsik Hyun

§Department of Economics, University of California-Santa Cruz, gkim44@ucsc.edu

¶Department of Economics, National Taiwan University, zihopark@ntu.edu.tw

1 Introduction

One of the most salient features of global environmental issues since the late 20th century has been the two divergent paths for manufacturing pollution emissions between developed and developing countries. Toxic manufacturing emissions have declined in many developed countries, while they have increased in the industrializing middle- and low-income countries (Copeland et al., 2022). Over the same time period, there has been a spectacular integration of the global economy, especially through offshoring (Feenstra, 1998; Hummels et al., 2001). The *pollution offshoring hypothesis*—a version of the pollution haven hypothesis that emphasizes the role of offshoring mechanism—provides a compelling explanation for the tale of the two global trends, which posits that progress toward trade liberalization induced firms in developed countries to relocate high-polluting activities to developing countries with laxer environmental regulations (Copeland and Taylor, 2004; Cherniwchan et al., 2017; Copeland et al., 2022). Despite active discussions in the policy arena and the plausibility of the mechanism, however, the empirical evidence on whether and how trade liberalization *causally* drives such global relocation of pollution-intensive tasks still remains elusive.[1]

An important factor that has been often overlooked in the discussion of the pollution haven or offshoring hypothesis is that, as with any form of investment, foreign direct investment (FDI) or offshoring activity to relocate dirty production entails a significant investment cost—which is magnified in the presence of political and economic uncertainty (Dixit and Pindyck, 1994). Cross-country differences in environmental regulations and trade barriers are critical factors that firms consider but may not form a sufficient condition for the pollution offshoring hypothesis to hold. Given the importance of uncertainty in determining investment decisions made by firms (Aizenman and Marion, 2004; Bloom et al., 2007; Handley and Limao, 2015), we argue that a comprehensive analysis of the hypothesis requires incorporating how these relocation incentives are shaped by uncertainty. In particular, the degree of uncertainty surrounding international trade barriers—or *trade policy uncertainty*—that firms in developed countries encounter could critically influence these investment decisions, as forging business relationships to contract on inputs and organizing supply chains is predicated on stable business environments across borders.

In this paper, we draw on arguably the most significant trade liberalization episode that reduced trade policy uncertainty in early 2000s—the US granting permanent normal trade relations (PNTR) status to China—and present new causal evidence on the long-run environmental impacts of trade liberalization. A priori, it is unclear whether a reduction of trade policy uncertainty will increase or decrease US establishments' toxic emissions. For example, the surge of imports from China could

[1] As Copeland and Taylor (2004), and more recently, Copeland et al. (2022) noted, despite ample theoretical and empirical support on causal linkages between an environmental regulation change and cross-regional movement of pollution-intensive tasks, whether *a reduction of trade barrier* triggers such a movement has relatively received scant support. Importantly, the existing literature has largely overlooked the role of offshoring activities within a firm as an explanation of such global trends, which we fill the gap.

lead US manufacturers to increase emissions by saving costs associated with environment-friendly practices in order to cope with increasing competitive pressure from Chinese firms, whereas the same competitive pressure can lead to a reduction of production scale or establishments exits, which may reduce toxic emissions. Aside from increased competitive pressure, as aforementioned, it could facilitate the offshoring of pollution-intensive tasks and lead to a decrease in toxic emissions by US establishments.

To identify the causal impact, we adopt a generalized difference-in-differences identification strategy in Pierce and Schott (2016) in the context of pollution emissions for US manufacturing establishments. By exploiting rich longitudinal data from the Toxics Release Inventory (TRI), together with the National Establishment Time Series (NETS) database, we estimate the long-run effects of the reduction in trade policy uncertainty on establishment-level pollution outcomes over the two decades between 1997 and 2017. We find that US establishments reduce toxic emissions in response to a reduction in trade policy uncertainty. We then explore potential mechanisms behind the effect of trade policy uncertainty on pollution emissions and reveal that offshoring, not competitive pressure, played a pivotal role, thereby supporting the pollution offshoring hypothesis in US manufacturing.

We first present three stylized facts of US manufacturing pollution emissions over the sample period from 1997 to 2017 at the establishment level. First, US manufacturing exhibits a decline in aggregate levels of pollution emissions with increased effort in waste management. Second, adopting the decomposition exercise as in Melitz and Polanec (2015), we find that the aggregate decline in manufacturing toxic emissions is primarily driven by within-industry adjustments through surviving establishments. Third, within-establishment decreases in pollution emissions are more pronounced in industries comprising establishments that engaged intensively in imports, but not in exports.

We then use our main empirical strategy to estimate the causal effects of the conferral of PNTR on reductions in pollution emissions among US manufacturers. The conferral of PNTR to China offers a quasi-experimental setting to study the long-run environmental impacts of trade liberalization and the pollution offshoring hypothesis. Prior to 2000, the US Congress had voted annually on whether to raise the low normal trade relations (NTR) tariff rates applied to Chinese imported goods back to the higher non-NTR rates assigned to non-market economies, which acted as the main source of trade policy uncertainty between the two countries.[2] In October 2000, the US Congress granted China PNTR status that eliminated such uncertainty by permanently setting US duties on imported goods from China at low NTR tariff rates. Following Pierce and Schott (2016), we use the NTR gap to measure the unexpected reduction in trade policy uncertainty.[3]

Our estimates are both economically and statistically significant: Moving an establishment from

[2]The non-NTR rates assigned to non-market economies, including China, were set by the Smoot-Hawley Tariff Act of 1930.

[3]The NTR gap is defined as the difference between the non-NTR tariff rates to which tariffs would have risen had annual renewal failed and the low NTR tariff rates. This measure of uncertainty presents substantial variation across industries.

an NTR gap at the tenth (0.138) to the ninetieth percentile (0.424) of the observed distribution increases the implied relative reduction of pollutant emissions within an establishment by 34 percent. We find that the change in US trade policy had a prolonged effect on pollution emission reductions in US manufacturing over nearly two decades. Our results are not driven by pre-existing trends and are robust to a host of robustness checks such as different sample periods, controlling for NAFTA, excluding entry and exit, dropping outliers, and allowing various weighting schemes. We also confirm that establishments' exits or simple reductions of the production scale do not explain the reduction in pollution emissions, but instead, the results are primarily driven by a decline in pollution emission intensity within an establishment.

By further exploiting a triple difference-in-differences framework, we estimate heterogeneous treatment effects depending on various initial characteristics of US manufacturing establishments. The reductions in pollution emissions are more substantial for US establishments that were more able and willing to offshore production to China. That is, US manufacturers—(i) having existing foreign business relationships, (ii) having more incentives to move away from stricter environmental regulations (i.e., located in nonattainment counties), (iii) operating in upstream industries along the supply chains, and (iv) belonging to a multi-sector firm—indeed reduced pollution emissions more.

Motivated by the suggestive evidence supporting the offshoring mechanism, we next directly assess the importance of (i) global sourcing and (ii) FDI, in turn, as channels through which US establishments adjust and reduce domestic pollutant emissions. Our first approach uses time-varying importing status at the establishment level from the NETS database to proxy for global sourcing activities, and test whether PNTR induced US manufacturers to source from abroad. We find that US establishments initially associated with high-polluting tasks—(i) those that were located in counties with more stringent environmental regulations (i.e., nonattainment counties) and (ii) those that initially had higher pollution intensity—engage more in importing activities than other establishments after PNTR. Our second approach involves merging the Wharton Research Data Services (WRDS) Company Subsidiary Data with our primary dataset to examine the foreign subsidiary information of US firms to which US establishments belong. We find that PNTR induced US manufacturing establishments to begin sourcing from abroad and to establish more foreign subsidiaries in China, but not in other countries. Moreover, such impacts of sourcing and outward FDI into China are mostly driven by establishments with high-polluting activities, thereby suggesting that US establishments sent high-polluting tasks to China and consequently reduced domestic pollution emissions.

Finally, we directly assess whether US manufacturers increased imports of *dirtier* products from China. Our conjecture is that If US manufacturers shifted high-polluting activities to China after the conferral of PNTR, and such a shift was driven by the offshoring mechanism, we would expect that US manufacturers will increase dirty product imports from China relative to other countries. We test this hypothesis using HS 10-digit product-by-year-level data from the UN Comtrade database and find that, in response to the conferral of PNTR to China, the share of US imports from China increased, particularly for products produced by high-polluting industries.

Contributions to the Literature

To the best of our knowledge, our paper is the first to study the impact of trade liberalization on long-term, nearly two decades of *post-2000*, US manufacturing toxic emissions using detailed establishment-level data. By doing so, we provide direct and comprehensive evidence of the pollution offshoring hypothesis. The paper contributes to the literature in environmental economics and international trade in several dimensions.

First, we contribute to the literature in environmental economics regarding the mechanisms driving the reduction in pollution emissions in US manufacturing. Previous research dates back at least to Copeland and Taylor (1994) and Grossman and Krueger (1995). In these papers, they describe three channels (i.e., the scale, composition, and technique effects) through which macroeconomic changes including international trade may affect the environment.[4] More recent studies find that the reductions in pollution emissions are mostly attributed to the within-industry technique effect (e.g., Levinson, 2009; Shapiro and Walker, 2018; Holladay and LaPlue III, 2021). Equipped with a granular dataset at an establishment level from 1997 to 2017, we conduct a decomposition analysis. We confirm the previous finding that US manufacturing emission reductions are primarily driven by the within-industry component and further identify that surviving establishments, not the entry and exit of establishments, account for the majority of the within-industry technique effect. Our paper contributes to the literature by showing that the within-industry technique effect, which is primarily driven by surviving establishments' reduction of pollution emission intensity, is at least in part driven by trade-related activities (i.e., offshoring).

By adopting the identification strategy in Pierce and Schott (2016) and thus providing a causal link between international trade and pollution emission reduction, we emphasize the role of trade as a driving mechanism of the industry-level technique effect in US manufacturing's pollution emission dynamics. Our finding provides a new perspective on the role of trade on the environment, which is different from what economists have typically considered—i.e., international trade simply changes the composition of clean and dirty *industries*. Less attention has been paid to the role of trade in US manufacturing emission reductions because offshoring dirty industries has been considered to be associated with the between-industry composition effect at the industry level, while most of the decline in manufacturing emissions was explained by the within-industry effect.[5] Unlike the

[4]The scale effect refers to increases in a country's total production raising pollution emissions; the composition effect indicates that changing the share of output from cleaner to dirtier industries can affect pollution emissions; and the technique effect indicates that pollution emissions per unit of output can change within an industry.

[5]Instead of international trade, previous studies have regarded advances in production or abatement processes (Levinson, 2009) and changes in environmental regulation (Greenstone, 2002; Shapiro and Walker, 2018) as the main economic forces behind the decline in pollution emissions in US manufacturing.

conventional view, we show that international trade can be an important driver of the within-industry emission adjustments, especially through the reduction in emission intensity *within an establishment*.

By doing so, we also contribute to the literature that links international trade to environmental outcomes.[6] A few recent papers use firm-level or establishment-level data to look for a causal effect of international trade on emissions in India (Martin, 2011; Barrows and Ollivier, 2021), the United States (Holladay, 2016; Cherniwchan, 2017), European countries (Akerman et al., 2021; Dussaux et al., 2023; Leisner et al., 2023), Mexico (Gutiérrez and Teshima, 2018), and China (Bombardini and Li, 2020; Rodrigue et al., 2022). Our paper contributes to the literature by adopting a clean causal identification strategy and providing direct and comprehensive establishment-level evidence of offshoring as an emission reduction mechanism.[7] Most of the aforementioned research does not explore offshoring as an emission reduction mechanism, and a few recent papers (Cherniwchan, 2017; Dussaux et al., 2023; Leisner et al., 2023) that (partly) focus on offshoring either rely on indirect measures to infer about offshoring activities or rely on shift-share type of shocks using trade flow data to proxy exogenous shifts in offshoring.[8] Instead, we exploit a plausibly exogenous shift in trade policy uncertainty as a quasi-natural experiment and directly show how such a shock results in establishment-level adjustments in offshoring activities—which leads to emission reduction.

By providing establishment-level evidence supporting the pollution offshoring hypothesis, our paper also contributes to the literature that studies the pollution haven hypothesis or pollution haven effects.[9] To date, empirical evidence in this literature has been mixed. When we focus on studies in the US, some support pollution haven effects (e.g., Greenstone, 2002; List et al.,

[6]While decomposition studies are useful for understanding the role of international trade in affecting the environment, the decomposition analysis alone cannot identify causality. It simply gauges changes in manufacturing emissions from different channels over time and thus does not shed much light on what has driven these changes—especially regarding the role of international trade.

[7]The version of NETS database—the extended package—that we use is much more comprehensive than the version used in the previous literature, which allows us to better assess the offshoring mechanism. For example, we have establishment-year-level information on import and export status, which plays a key role in the mechanism identification. Also, we augment the NETS data with other data including global subsidiary information, which permits us to perform comprehensive analyses regarding offshoring activities.

[8]For example, Cherniwchan (2017) partly attributes the reduction in US manufacturing emission intensity in mid-1990s to offshoring by leveraging US tariffs preferences on dirty inputs, but does not show directly whether establishments adjusted their offshoring activities during the process. Dussaux et al. (2023) and Leisner et al. (2023) use shift-share type of measures to proxy exogenous shift in offshoring in the context of Europe. Some of these contemporaneous papers arrive at rather mixed conclusion on the role of offshoring (e.g., Akerman et al., 2021; Dussaux et al., 2023).

[9]Although previous studies have often used the two terms interchangeably, Copeland and Taylor (2004) formally distinguish the *pollution haven hypothesis* from *pollution haven effects*. The pollution haven hypothesis asserts that *a reduction in trade barriers* will lead to a shifting of pollution-intensive industry from countries with stringent regulations to countries with weaker regulations. The pollution haven effect states that *a tightening of pollution regulation* will influence plant location decisions and trade flows, which is also referred to as "carbon leakage" in the context of greenhouse gas emissions. Copeland and Taylor (2004), and more recently Cherniwchan et al. (2017) and Copeland et al. (2022), noted that the pollution haven hypothesis has relatively less theoretical and empirical support than the pollution haven effect because many other factors—in addition to environmental policy—can affect trade flows. As we study the impact of trade liberalization (driven by a decline in trade policy uncertainty), our paper is in line with the studies of the pollution haven hypothesis, and more specifically, the pollution offshoring hypothesis.

2003; Levinson and Taylor, 2008; Tanaka et al., 2022; Bartram et al., 2022), whereas others are broadly consistent with weak pollution haven effects (e.g., Eskeland and Harrison, 2003; Hanna, 2010).[10] Unlike previous studies, we leverage an episode of trade policy uncertainty reduction, neither variations in environmental regulations nor actual changes in tariffs, to study the pollution offshoring hypothesis. To our knowledge, our paper is the first to provide comprehensive evidence of the pollution offshoring hypothesis by directly investigating offshoring-related activities by establishments—using various establishment-, firm-, and county-level measures and linking them to global parent-subsidiary information—and by assessing dirty product imports from China to US using product-level data.

This paper contributes to the growing notion that trade policy uncertainty, even in the absence of actual changes in tariffs and other barriers, can have significant impacts on the economy (see, e.g., Handley and Limao, 2015; Handley and Limão, 2017, 2021; Caliendo and Parro, 2021). However, the literature devoted little attention to its impact on environmental outcomes. We contribute to the literature by filling this gap. Moreover, by emphasizing the notion of uncertainty in trade liberalization and investment decisions, we enrich the discussion on the pollution haven hypothesis and contribute by providing support for it.

Finally, we contribute to the literature on the China trade shock, which has significant impacts on labor market outcomes (Autor et al., 2013; Pierce and Schott, 2016; Choi and Xu, 2020; Kim, 2022), innovation (Bloom et al., 2016; Autor et al., 2020a), political outcomes (Che et al., 2016; Autor et al., 2020b), health (Pierce and Schott, 2020), product scope adjustment (Choi et al., 2022b), and internal migration (Greenland et al., 2019), among many others. Despite the vast literature on this topic, our paper is the first to formally explore US establishment-level environmental outcomes in response to the China trade shock. This is an important gap in the literature in light of the heated public and academic debates concerning the environmental impacts of globalization. This is also a curious gap in this vast literature since it is plausible that the labor and health outcomes of the China shock listed above can have environmental causes and consequences.[11]

The remainder of the paper is organized as follows. Section 2 provides the institutional background on the TRI program. Section 3 describes the data used for estimation. Section 4 presents some stylized facts on US manufacturing emission patterns. Section 5 details the empirical strategy. Section 6 presents our main estimation results and the heterogeneous effect analyses. Section 7 reports results on mechanisms, supporting the offshoring channel. Section 8 concludes the paper.

[10] In related studies, Chung (2014) and Cole et al. (2014) find supporting evidence for pollution haven effects in Korea and Japan, respectively. Also, Kahn (2003) uses three decades of historical bilateral US trade data to study trends in dirty and clean trade to characterize US pollution havens.

[11] For example, our findings provide a novel perspective on the finding in Pierce and Schott (2020) that the US regions hit more by the PNTR shock experienced larger declines in the rate of heart attacks. They suggest that safety in the workplace might have led to fewer heart attacks. Given the scientific findings that airborne toxic chemicals lead to heart attacks (Kim et al., 2015), however, our findings suggest that PNTR-induced environmental improvements are likely to have resulted in better heart-related health outcomes among the residents in US counties that experienced a greater decline in manufacturing toxic emissions.

2 Institutional Background: Toxics Release Inventory Program

In December 1984, a cloud of methyl isocyanate gas leaked from the Union Carbide India Limited (UCIL) pesticide plant at Bhopal, India, causing thousands of casualties and severe health effects in subsequent years. A few months after what is considered to be the worst industrial disaster in history, a similar accident involving toxic chemical leaks (aldicarb oxime and others) occurred in the US at another Union Carbide facility in West Virginia. Consequently, public concerns were raised about the importance of maintaining accurate information on how local facilities manage toxic chemicals and are prepared for any related emergencies.

In 1986, the US Congress passed the Emergency Planning and Community Right-to-Know Act (EPCRA). The Toxics Release Inventory (TRI) program was initiated under Section 313 of the EPCRA, which requires US facilities to report their annual releases of toxic chemicals. Under the Pollution Prevention Act of 1990, the reporting facilities must also include descriptions of the measures taken to prevent pollution, such as reducing pollutants at the source (e.g., substituting materials, modifying production methods), and managing waste in an environment-friendly manner (recycling, treating, combusting for energy recovery). The reports submitted by these facilities are compiled and archived as the TRI, which is maintained and publicly shared by the US Environmental Protection Agency (EPA).

The program is mandatory for facilities that meet the TRI reporting criteria. That is, a facility must report by July 1 of each year if it (i) operates in a TRI-covered sector (manufacturing, mining, electric utilities, and waste management) or is a federal facility; (ii) employs at least ten full-time workers; (iii) manufactures, processes, or otherwise uses more than the specified threshold amount of TRI-listed chemicals per year.[12] Facilities that are noncompliant are subject to further investigation and possible enforcement actions by the EPA.[13] The structure of the TRI program, designed to provide the public with accurate and timely information about the management of toxic chemicals, in turn, encourages facilities to move toward adopting environment-friendly and safer practices.

3 Data

We combine various data sources to assess the effect of the US trade policy change on establishment-level releases of pollutants. In this section, we describe the sources, data structure, and sample construction.

[12]According to the EPA, "facilities" refers to "all buildings, structures, and other stationary items which are located on a single site or on contiguous or adjacent sites and which are owned or operated by the same person (or by any person which controls, is controlled by, or under common control with, such person)", and "full-time employees" includes "all persons employed by a facility regardless of function (e.g., operational staff, administrative staff, contractors, etc.)."

[13]The following link provides press releases on TRI-related enforcement actions: https://www.epa.gov/toxics-release-inventory-tri-program/tri-compliance-and-enforcement

3.1 Data Sources

Toxics Release Inventory (TRI) We obtain facility-chemical-level releases of toxic materials (1987-2020) provided through the EPA's TRI database. For each reporting facility, we observe detailed information on the chemical (including chemical name, acuteness in human health effects, carcinogenicity, and the severity of environmental effects) and the chemical-specific amount of production waste generated on-site and transferred to off-site locations. The data add breakdowns of how each facility manages this chemical waste. One is the amount "released" (or emitted) to the air, water, (or placed into) land, which directly affects the environment. The other is the amount recycled, treated, or combusted for energy recovery, which speaks to facility-level effort in effectively managing waste. In addition, we also have information on the various types of pollution prevention (P2) activities that facilities conduct to reduce waste at the source. Detailed descriptions on such activities are available, which are categorized into the following broad groups: (i) material substitutions and modifications; (ii) product modifications, process, and equipment modifications; (iii) inventory and material management; and (iv) operating practices and training.[14]

The granularity of the data, along with the unique identifiers for facilities and chemicals, allows us to track changes in the amount of chemical-specific waste produced over time. However, it is important to note that the EPA has made a number of changes to the TRI program over the years: (i) expansion of the scope of TRI-covered sectors, chemicals, and geographic areas and (ii) changes in reporting criteria.[15] These updates were intended to better provide data on exposures to toxic chemicals and the environmental performances of US facilities. From an empirical perspective, the increasing list of TRI-covered chemicals, a subset of which face lower thresholds, can mechanically increase the reported amount after these policy changes. Therefore, our analyses carefully address these issues in the sample construction, and we conduct a series of robustness checks, which we describe in later sections. After restricting to a list of chemicals of interest, we apply a crosswalk obtained from the National Emissions Inventory (NEI) to map relevant chemicals to PM_{10}.[16] Throughout our analyses, we collapse the data and focus on facility-level waste production of this major pollutant.

It is worth discussing the reason behind our decision to use TRI as our main data source for establishment-level toxic emissions over other potentially available data sources, such as the National Emissions Inventory (NEI). The primary reason for this choice is that TRI provides a common establishment identifier (DUNS number) that is readily linkable to the near-universe of the US establishment panel dataset—the NETS data—and that the emission information is at the yearly frequency. In addition, the transparency of TRI data in providing chemical-level details on reporting

[14] In 1990, Congress passed the Pollution Prevention Act (P2 Act), which stipulates that the EPA must establish a source reduction program that collects and disseminates information.

[15] The following link provides a full list of policy changes in the TRI program: https://www.epa.gov/toxics-release-inventory-tri-program/history-toxics-release-inventory-tri-program

[16] The crosswalk is available in Table 12 of the 2008 NEI Technical Support Document available at this link: https://www.epa.gov/sites/default/files/2015-07/documents/2008_neiv3_tsd_draft.pdf

criteria, release media, and amount, which aligns with its primary purpose to inform the public and policymakers about toxic emissions, facilitates a rigorous and consistent tracking of changes in emissions over a long time horizon.[17] To the best of our knowledge, TRI-NETS dataset is the only available data combination that provides the yearly frequency of establishment-chemical-level toxic emissions, together with a vast set of establishment and firm-level business characteristics.[18] Note that this is a critical data requirement for the purpose of our paper—identifying a reduced-form causality by adopting a difference-in-differences-type research design—compared to other types of exercises such as decomposition analyses or using data to calibrate or estimate a structural model. Finally, although the raw TRI data may include some erroneous or implausible emission records, we make sure that our results are not driven by any extreme observations (Table B.10) and clean the data in a way that the final sample contains a consistent set of chemicals throughout the sample period as discussed above (see Section 3.2 for detailed descriptions).[19] Reassuring the quality of our final sample, the aggregate patterns of toxic emissions from our sample are broadly consistent with those documented in the literature (e.g., Shapiro and Walker, 2018).[20]

National Establishment Time Series (NETS) To understand establishment-level responses in waste production relative to size (employment and sales) as well as various heterogeneity in the effects, we obtain establishment-specific characteristics from the NETS database, which is an annual panel of a near universe of US establishments (1990-2020). In NETS, we observe establishment-level industry codes (SIC and NAICS codes),[21] employment, sales, exporter and importer status, address, and headquarters identifier. Each establishment in the NETS database is assigned a unique identifier, thereby allowing us to track establishments consistently over time.

The source data for NETS are created by Dun & Bradstreet, which is among the largest credit rating companies in the world, and thus, it has a strong incentive and capacity to collect accurate

[17]Furthermore, TRI has several institutional features to optimize and maintain the quality of data, for example, through "built-in data quality alerts," "data quality call processes (ad hoc data quality calls)," and enforcement actions. See https://www.epa.gov/toxics-release-inventory-tri-program/tri-data-quality for further details.

[18]Although NEI could provide more accurate toxic emission information per se, it is only available at the triennial frequency and does not share a common establishment identifier with other establishment-level datasets (e.g., Annual Survey of Manufactures (ASM), Longitudinal Business Database (LBD), or NETS), which contrasts with the TRI-NETS dataset. Therefore, linking NEI to other business data, which requires algorithm-matching techniques, could introduce some additional measurement errors.

[19]Additionally, in Appendix C, we show that employment responses to the PNTR shock are fundamentally different between establishments with positive initial emissions and those with zero or negligible emissions, conditional on satisfying TRI-reporting criteria. We also document that the most important industries in terms of toxic emissions are very different from those in terms of employment (see Figure A.2). These exercises further support that our results are not spuriously driven by the restriction of sample induced by TRI-reporting criteria.

[20]Another advantage of the TRI-NETS data is that, since NETS independently maintains establishment-firm records and manually cross-checks the quality of linkage between TRI and NETS using various available information (e.g., name, address, etc.), the TRI-NETS merged data are much immune to the often raised issue on the quality of establishment-firm records in TRI (e.g., Khanna, 2019).

[21]The SIC and NAICS industry codes have been consistently maintained over time in NETS, so we do not need to perform any imputations of industry codes.

data through various records.[22] A number of studies have demonstrated the accuracy of information in NETS data (Neumark et al., 2006, 2011; Barnatchez et al., 2017).[23] Importantly, our version of the NETS database provides a match between the NETS establishment identifier (DUNS number) and the facility identifier in the TRI database. The matching process relies on company names and addresses and further involves eyes-on-the-records search efforts. Among the 61,907 unique facilities that are included in the TRI Database between 1987 and 2020, 91% (56,468 facilities) are matched with NETS' establishment identifiers. We focus on the one-to-one matches and use establishment (instead of a facility) as our unit of analysis.[24]

Wharton Research Data Services (WRDS) Company Subsidiary Data WRDS Company Subsidiary Data contain the parent company and its subsidiary information for companies filing with the US Securities and Exchange Commission (1995-2019). For a given parent company, the data allow us to identify the number of subsidiaries located in each country in a given year.[25] In our empirical analyses, we focus on parent companies located in the US. Thus, we track the number of subsidiaries in China (or other countries) at a yearly frequency to identify US companies' subsidiaries in China (or other countries).

U.S. Historical Tariff Rates We obtain NTR and non-NTR tariff rates provided by Pierce and Schott (2016), which sources data from Feenstra et al. (2002). We map the HS-level tariff rates to 4-digit-SIC industries using Pierce and Schott (2009) and use industry-level tariff rates in 1999 as in Pierce and Schott (2016).

[22]To maintain its quality, Dun & Bradstreet conducts an extensive array of analyses. For example, their analysts make phone calls to reliable sources such as the firms' legal personnel, CFOs, and CIOs. They also make use of publicly available government registries, legal filings, yellow pages, news, annual reports, company websites, and so forth. Note that the US government requires companies to report their information based on their DUNS number for procurement purposes. This also provides incentives for firms to report accurate information to Dun & Bradstreet.

[23]For example, Barnatchez et al. (2017) find that the county-level correlation between NETS and the Census Bureau's County Business Patterns (CBP) is above 0.99 regarding both employment counts and establishment counts, and Neumark et al. (2011) document the accuracy of entry and exit information of establishments. For recent studies that use the NETS database, see, e.g., Gray et al. (2015); Asquith et al. (2019); Rossi-Hansberg et al. (2021); Behrens et al. (2022); Hyun and Kim (2022); Choi et al. (2022a); Oberfield et al. (2022).

[24]A small share of the data is not one-to-one matches. In particular, 144 TRI facilities are matched to multiple NETS establishments, and 2,180 NETS establishments are matched with multiple TRI facilities. These one-to-many matches most likely come from slightly different definitions of "establishment" in NETS and "facility" in EPA. The concept of an establishment in NETS is defined as a line of business that has a fixed address so that there can be multiple NETS "establishments" at a single address under a single firm. On the other hand, a facility in EPA refers to "all buildings, equipment, structures, and other stationary items which are located on a single site or on contiguous or adjacent sites and which are owned or operated by the same person (or by any person which controls, is controlled by, or under common control with, such person)."

[25]We linked parent companies in WRDS Subsidiary data with headquarters companies in NETS data by using a probabilistic record linkage algorithm. We exploited company name and address information in the two datasets to perform record linking. We used the Stata command RECLINK2 to link the two datasets, where we exploited both the company name and address information. Then, we manually verified the quality of the linkage.

3.2 Sample Construction

The matching of the TRI-NETS data between 1987 and 2020 results in 2,809,810 observations with chemical-specific establishment-level release amounts for 54,224 establishments covering 660 chemicals, 27 of which are mapped to PM_{10}. We describe the detailed steps through which we trim the data and construct our baseline sample. First, we focus on 24 chemicals mapped to PM_{10} that have continued to exist since 1995. As discussed above, the EPA has (i) expanded the list of TRI-covered chemicals and (ii) changed the reporting criteria over time. In its continued efforts to include chemicals with adverse effects on human health and the environment, roughly 38 percent of the current list of chemicals (286 out of 750) were added in November 1994 and required in the reports beginning with the 1995 calendar year. Therefore, we exclude chemicals—Persistent Bioaccumulative and Toxic (PBT) chemicals, 1-Bromopropane, and chemicals in the Hexabromocyclododecane (HBCD) category—introduced in the subsequent years.[26]

We note that the reporting criteria applied to both PBT and non-PBT chemicals were relaxed during the period 2007-2009. The TRI Burden Reduction Rule (2006) expanded the use of reporting through Form A (a simpler form without quantity details on the produced waste); however, the Omnibus Appropriations Act in 2009 reverted the requirements to those that were effective before 2006. Given the value of understanding the long-run environmental consequences, we choose to keep these years in our sample but conduct robustness checks on whether our analysis is sensitive to the exclusion of these years. The final relevant component of the changes to the TRI program is the expansion in the geographic coverage to increase participation of Native Americans in 2012. To maintain consistency on this end, we keep establishments that are not located in *Indian country*.[27]

We exclude periods with prevailing impacts of major events that might have confounded the effects of our treatment. We do not include the years after 2018 due to the US-China Trade War and the pandemic, which substantially reshaped global trade flows and domestic production, thereby affecting manufacturing pollutant emissions. We also exclude a few years after the North American Free Trade Agreement (NAFTA) agreement (1994), given its impact on the reductions of establishment-level pollutant emissions (Cherniwchan, 2017). Hence, we restrict our sample period to years between 1997 and 2017. Note that including a few years (i.e., 1997, 1998, 1999, and 2000) before the US trade policy change in 2001 allows us to examine the pre-existing trends in our analysis. We address any remaining concerns related to the lagged responses of NAFTA by directly controlling for changes in the US tariffs on Mexican imports following Hakobyan and McLaren (2016). Lastly, we focus on manufacturing establishments that had positive emissions of chemicals of interest mapped to PM_{10} at least once during the sample period. Thus, our final sample is an unbalanced panel of establishment-year-level observations with positive PM_{10} Emissions. The final sample contains

[26] All additions to and deletions from the TRI chemical list can be found in the following link: https://www.epa.gov/system/files/documents/2022-03/tri-chemical-list-changes-03-07-2022.pdf

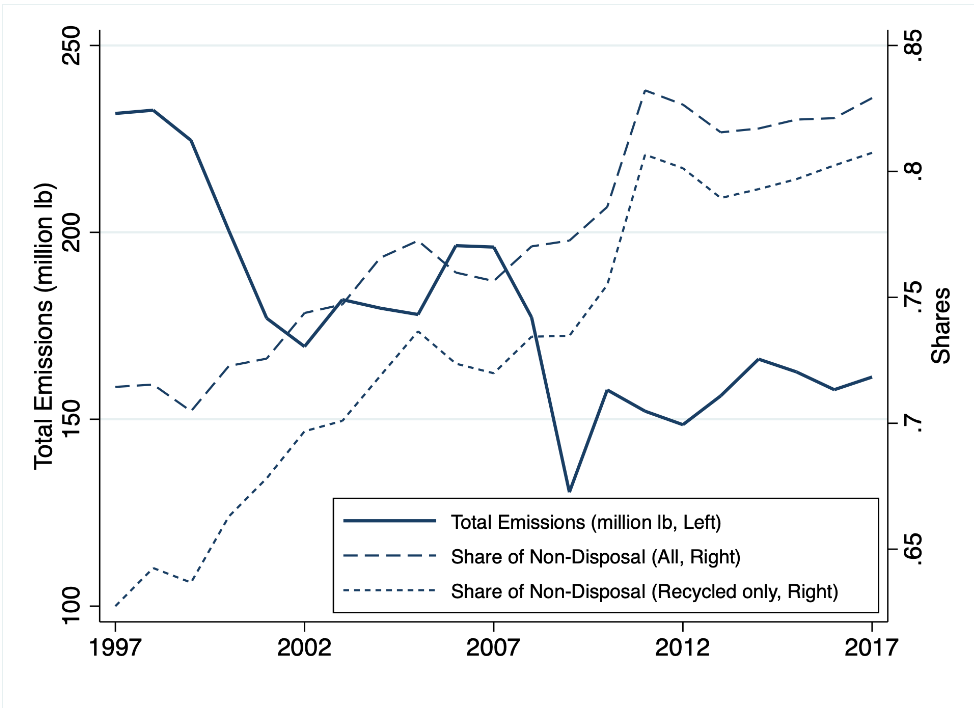
[27] Appendix Table B.1 provides further details related to these policy changes.

46,753 establishment-year-level observations with 4,946 unique manufacturing establishments.

4 Stylized Facts

Fact 1. US Manufacturing demonstrates a decline in aggregate levels of PM_{10} emissions with increased efforts in waste management.

Figure 1: Aggregate Levels of PM_{10} Emissions and Non-Disposal Shares, 1997 - 2017



Notes: The solid line shows the aggregate levels of PM_{10} waste released or emitted to the air. The long dashed line shows the non-disposal share, which is the total PM_{10} waste recycled, treated, or combusted for energy recovery (therefore, not released or emitted to the air or water) relative to total PM_{10} waste. The short dashed line shows the recycled share, which is the total PM_{10} waste recycled relative to total PM_{10} waste.

We begin by checking whether the clean-up of manufacturing found in previous studies (e.g., Levinson, 2009; Shapiro and Walker, 2018; Najjar and Cherniwchan, 2021) is also present in our data. The solid line in Figure 1 shows the time series of the aggregate levels of PM_{10} waste released or emitted into the air from 1997 to 2017, where we find a 30 percent drop. Appendix Figure A.1 reveals that most of these aggregate changes are largely driven by establishments in 2-digit-SICs 28 and 33, which are Chemicals and Allied Products and Primary Metal Industries, respectively.[28] In fact, these two industry categories represent a predominant share of the initial PM_{10} emissions from

[28] Appendix Table B.2 shows that the top 5 industries in PM_{10} emissions all belong to 2-digit-SICs 28 and 33.

manufacturing establishments.[29] However, we also note that there is also an overall decline in PM_{10} emissions in other industries.

The detailed breakdown of waste management in TRI allows us to understand the clean-up process from an alternative perspective: the extent to which establishments transition toward more environment-friendly waste management practices. The long dashed line in Figure 1 shows that the non-disposal share, which is the total PM_{10} waste recycled, treated, or combusted for energy recovery (therefore, not released or emitted into the air) relative to total PM_{10} waste, increases from 71 to 83 percent.[30] The EPA notes that the most sustainable and environmentally preferred management practice is to reduce waste at the source; however, for waste that has already been generated, recycling is the next best option (followed by combustion for energy recovery and treatment).[31] In sum, Figure 1 reveals that the aggregate emissions from manufacturing establishments declined during the past two decades, while the share of non-disposal (predominantly through recycling), which captures waste management efforts, steadily increased over time. In Section 7.1, we explore how the conferral of PNTR to China interacts with various initial characteristics and affects establishments' waste management efforts and PM_{10} emissions.[32]

Fact 2. The aggregate decline in PM_{10} emissions from manufacturing establishments is primarily driven by within-industry adjustments through surviving establishments.

Next, we further quantify the extent to which the changes in aggregate PM_{10} emissions are due to (i) changes in the size of the manufacturing sector (*scale*), (ii) changes in the mix of manufacturing industries (*composition*), and (iii) changes in the production technology employed within-industry (*technique*). The analysis below combines the approaches in Levinson (2009) and Melitz and Polanec (2015). Aggregate PM_{10} emissions in the manufacturing sector in year t, P_t equal the sum of PM_{10} emissions from each of the (SIC 4-digit) manufacturing industries, $p_{i,t}$. Defining industry shares using industry sales ($\theta_{i,t} = \nu_{i,t}/V_t$) and emission efficiency $z_{i,t}$ as the emission amount per dollar value of sales ($p_{i,t}/\nu_{i,t}$), we express the total PM_{10} emissions in a given year as the scale of the sector (V_t) times the weighted-average emission efficiency ($\sum_i \theta_{i,t} z_{i,t}$).

$$P_t = \sum_i p_{i,t} = \sum_i \nu_{i,t} z_{i,t} = V_t \sum_i \theta_{i,t} z_{i,t} \tag{4.1}$$

[29]Appendix Figure A.2 provides the industry distribution of our data using employment and total PM_{10} emissions.

[30]By construction, the share of PM_{10} waste released decreases from 29 to 17 percent.

[31]Further details on the non-hazardous materials and waste management hierarchy developed by EPA can be found at this link: https://www.epa.gov/smm/sustainable-materials-management-non-hazardous-materials-and-waste-management-hierarchy

[32]We explore whether the main effects we find are entirely driven by establishments in SIC-2-digit 28 and 33 given their importance in our sample. Appendix Table B.5 shows that, while the effects are stronger in these industries, we also estimate a significant impact for other industries.

Figure 2: Decomposition of Aggregate Manufacturing PM_{10} Emissions, 1997 - 2017

Notes: The graph illustrates changes in the aggregate manufacturing PM_{10} Emissions using equations (4.2) and (4.5). Line (1) shows the magnitude of the scale factor. The distances between lines (1) and (2), (2) and (4) show the magnitude of the composition and technique factors, respectively. The distances between lines (2) and (3), (3) and (4) capture the magnitude of the within-industry intensive and extensive margins, respectively.

The last part of Equation (4.1) can be represented in vector notation, $\mathbf{P} = V\theta'\mathbf{z}$, which we totally differentiate and obtain

$$d\mathbf{P} = \theta'\mathbf{z}dV + V\mathbf{z}'d\theta + V\theta'd\mathbf{z}. \tag{4.2}$$

Leveraging our establishment-level data, we further decompose the within-industry channel to examine the magnitude of the intensive and extensive margins following the methods in Melitz and Polanec (2015). That is, the extent to which within-industry changes are explained by changes in the way surviving establishments produce goods and emit PM_{10} pollutants and those that are attributed to the entry and exit of establishments. This part of the exercise requires identifying establishments that survive (s), enter (n), and exit (x) between the baseline year t_0 and year t.[33] Thus, the average emission efficiency of industry i in the baseline year and in year t are given as follows:

$$z_{i,t_0} = \theta_{s,t_0} z_{s,t_0} + \theta_{x,t_0} z_{x,t_0} \tag{4.3}$$

$$z_{i,t} = \theta_{s,t} z_{s,t} + \theta_{n,t} z_{n,t} \tag{4.4}$$

[33] We categorize firms that enter and exit between year t_0 and year t in the exit group.

where θ_{s,t_0} is the share of survivors in year t_0; and θ_{x,t_0} is the share of exiters in year t_0.[34] Finally, changes in the average emission efficiency for industry i between year t_0 and year t is

$$\Delta z = z_{i,t} - z_{i,t_0} = \underbrace{z_{s,t} - z_{s,t_0}}_{\text{surviving}} + \underbrace{\theta_{n,t}(z_{n,t} - z_{s,t}) + \theta_{x,t_0}(z_{s,t_0} - z_{x,t_0})}_{\text{entry and exit}} \tag{4.5}$$

where $z_{G,t} = \sum_{p \in G} (\theta_{p,t}/\theta_{G,t}) \times z_{p,t}$ is the average efficiency for each group ($G = s, n, x$) of establishments and $\theta_{G,t} = \sum_{p \in G} \theta_{p,t}$ is the aggregate market share of group G. As discussed in Melitz and Polanec (2015), one can further decompose the surviving firm channel into the within-establishment and reallocation across surviving establishments by applying the decomposition methods of Olley and Pakes (1996). As our focus is to compare the magnitude of the extensive and intensive margins of adjustments, we conduct our analysis based on Equation (4.5).

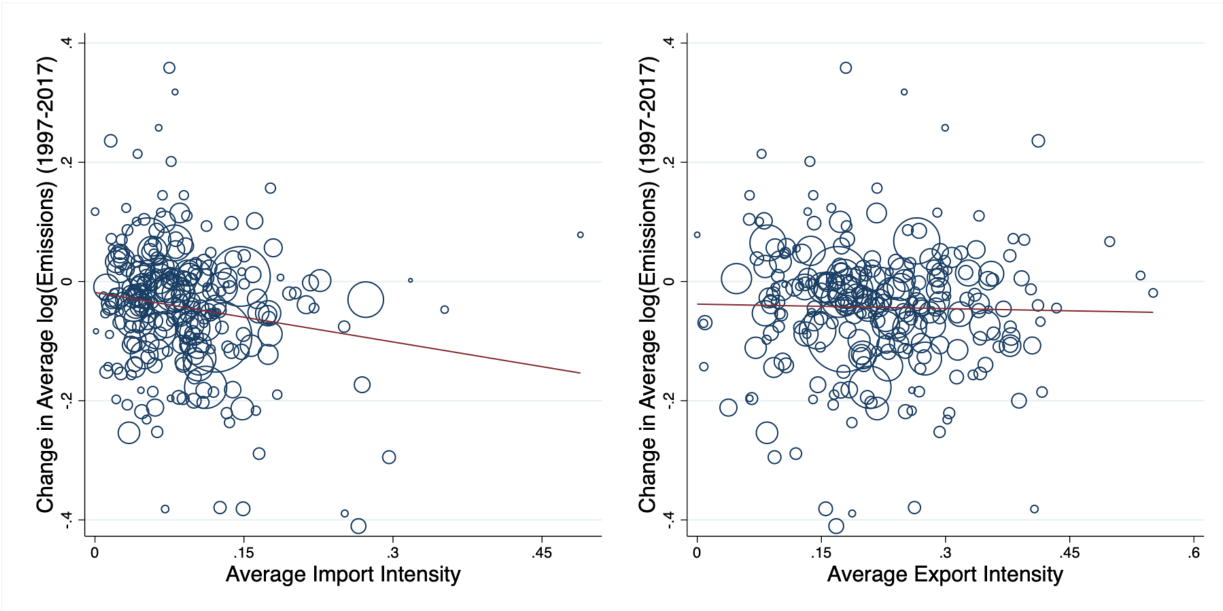
Figure 2 shows the decomposition results tracking changes in total manufacturing emissions of PM_{10} relative to 1997 for 20 years and the contribution of each channel. The aggregate change, which exhibits a downward trend in total manufacturing emissions of PM_{10} over time, is captured using line (4). Line (1) isolates the change attributed to the *scale* factor. Line (2) adds the *composition* factor to the first factor. The sum of these two channels makes limited contributions in the downward aggregate trend in PM_{10} Emissions we observe in Figure 1. The remaining two lines add the *technique* factor, which explains most of the decline in PM_{10} emissions over time. Consistent with previous studies (e.g., Levinson, 2009; Holladay and LaPlue III, 2021), the magnitude of change due to the adjustments that occur through surviving establishments (intensive margin) is larger than those caused by the entry and exit of establishments (extensive margin). According to our calculations, roughly two-thirds of the 30 percent decline in 2017 relative to 1997 is due to surviving establishments.

Fact 3. Within-establishment decreases in PM_{10} emissions are more pronounced in industries with establishments that actively engaged in imports, not exports.

To understand the clean-up of manufacturing establishments in the context of globalization, we examine how the initial trade status relates to changes in PM_{10} emissions. Figure 3 plots the industry-level average growth of PM_{10} emissions against measures of industry-level import intensity (left panel) versus export intensity (right panel). Specifically, from the establishment-year-level data, we calculate for each industry (i) the growth in the average PM_{10} emissions between 1997 and 2017; (ii) the average initial within-firm employment share of importing establishments (*import intensity*); and (iii) the average initial within-firm employment share of exporting establishment (*export intensity*). We observe a stark asymmetry between import-intensive and export-intensive industries on their PM_{10} emission dynamics. That is, we find a clear negative correlation between

[34] By construction, $\theta_{s,t_0} + \theta_{x,t_0} = 1$ and $\theta_{s,t} + \theta_{n,t} = 1$ hold.

Figure 3: Correlations between Changes in Average PM_{10} Emissions and Initial Trade Status



Notes: The graph on the left (right) illustrates the correlations between the industry-level averages of changes in the within-establishment log(emissions) of PM_{10} from 1997 to 2017 and the industry-level averages of import (export) intensity in 1997. Import (export) intensity is defined as an employment share of importing (exporting) establishments within a firm. The sizes of the circles are proportional to the industry-level log(employment) in 1997.

the changes in average PM_{10} emissions and the measure of import intensity, while such a correlation does not exist for the measure of export intensity.[35] A possible interpretation of such asymmetry is that offshoring of manufacturing associated with importing activities led to emission declines. We revisit such relationships in a formal regression setting in Section 7.1.

5 Empirical Strategy and Descriptive Statistics

5.1 Empirical Strategy

Our empirical approach builds on the pioneering work of Pierce and Schott (2016), which exploits a sudden US trade policy change—PNTR to China in October 2000—to investigate the impact of trade liberalization on US manufacturing employment. The conferral of PNTR to China (i) eliminated uncertainty associated with the tariff rates faced by Chinese exporters and (ii) allowed China guaranteed access to NTR tariffs, which were primarily applied to World Trade Organization (WTO) members. Prior to 2000, Chinese firms received NTR tariff rates based on the US president granting NTR (US Trade Act of 1974), which also required annual renewals by the US Congress.

[35] Appendix Figure A.3 robustly demonstrates a similar asymmetry between import-intensive and export-intensive industries by using industry-level import-to-value-added and export-to-value-added ratios as measures of import and export intensities.

The outcomes of these reviews were sensitive to political tensions between the two countries and, therefore, highly uncertain. In the event of unsuccessful outcomes, which potentially resulted in the withdrawal of China's Most Favored Nations (MFN) status, Chinese imports were subject to non-NTR rates—substantially higher rates applied to nonmarket economies. The policy uncertainty also imposed challenges for US firms doing business with China because they faced an excessively risky environment for trade and investment.[36]

The change in China's PNTR status generated heterogeneous implications across different manufacturing industries: Those that experienced a larger expected drop in the tariff rates also benefited more from a reduction in trade policy uncertainty. As in Pierce and Schott (2016), we define *NTR Gap*, the magnitude of the trade policy shock faced by industry i, using the difference between the observed NTR rates and the potential non-NTR rates for each industry i in 1999,

$$NTR\ Gap_i = Non\ NTR\ Rate_i - NTR\ Rate_i. \tag{5.1}$$

As summarized in Panel (B) of Appendix Table B.3, we observe sufficient variation in industry-level *NTR Gap* in our sample.[37] Note that the differences in the tariff rates faced by Chinese firms due to the policy change are mainly driven by the initial rates set under the Smoot-Hawley Tariff Act of 1930. We thus mitigate endogeneity concerns related to the *NTR Gap* responding to the rate at which establishment-level emissions changed across industries during the period 1997-2017.

We leverage industry-level variations in *NTR Gap*'s to examine the impact of the trade policy shock on establishment-level PM_{10} emissions in a difference-in-differences research design. Conceptually, the first difference compares establishments in high-*NTR Gap* industries versus low-*NTR Gap* industries. The second difference compares years before and after 2001 when Congress passed the bill that granted China's PNTR status and the change in US trade policy became effective. Figure 4 visualizes our identification strategy where we demonstrate trends in the log of average establishment-level PM_{10} emissions for industries in the 75^{th} percentile (solid line) and the 25^{th} percentile (dashed line) of *NTR Gap*. We show that the high-exposure industries exhibit a larger decline in their PM_{10} emissions compared to the low-exposure industries. The differences between the two groups substantially increase after the policy change relative to the observed differences in the pre-shock period.

We now formally estimate the impact of the US trade policy change on establishment-level PM_{10} Emissions using the following empirical specification:

$$\mathrm{y}_{p,t} = \beta_0 + \beta_1 NTR\ Gap_i \times Post_t + \delta Z_i \times Post_t + \gamma X_{i,t} + \eta_p + \eta_{c,t} + \varepsilon_{p,t}, \tag{5.2}$$

where the dependent variable is the log of PM_{10} emissions from establishment p in industry i in year

[36]See Pierce and Schott (2016, 2020) for a comprehensive description of the policy background.

[37]The average is 0.329 and the standard deviation is 0.142.

Figure 4: Research Design: Difference-in-Differences

Notes: The graph illustrates the trends in the log of average establishment-level PM_{10} emissions for industries in the 25^{th} (dashed line) and 75^{th} percentile (solid line) of *NTR Gap*'s. The vertical line indicates the timing of the shock, October 2000, which is when Congress passed the bill that granted PNTR status to China.

t.[38] The second term interacts our measure of the shock $NTR\ Gap_i$ with $Post_t$, an indicator for the post-PNTR period (years from 2001 forward). The third term is an interaction of time-invariant industry-level characteristics (Z_i) with the post-PNTR period. As in Pierce and Schott (2016, 2020), these variables include Chinese policy variables—exposure to changes in Chinese import tariffs from 1996 to 2005 and exposure to changes in Chinese domestic production subsidies from 1998 to 2005—and initial industry characteristics, including capital intensity (capital-to-labor ratio) and skill intensity (the proportion of non-production workers in total employment) in 1997. The fourth term controls for time-varying industry characteristics ($X_{i,t}$)—the phasing out of Multi-Fiber Arrangement (MFA) quotas and the US import tariff rates. We also include establishment fixed effects (η_p) to control for time-invariant establishment characteristics. We add county-by-year fixed effects ($\eta_{c,t}$), which are the most flexible way of controlling for any time-varying (and time-invariant) observed and unobserved common component at the county level. These fixed effects absorb any time-varying local environmental regulatory conditions and any common variation within a county-by-year pair that

[38] Appendix Table B.6 also considers emissions of sulfur dioxide (SO_2) and volatile organic compounds (VOC). We find a negative impact of PNTR on these emissions. However, note that we lack sufficient observations in the sample for SO_2, which limits the power of our estimates. Similarly, for VOC, there is insufficient variation in our shock measure, resulting in imprecise estimates.

is due to a time-varying regional shock—e.g., local demand and supply shocks, local labor market shocks, regional housing market shocks—regardless of whether the shock is county-year-specific or correlated with shocks in other regions. They also account for spillovers from one region to another, e.g., due to price or other general equilibrium effects, as long as such spillovers generate common impact across establishments within a county-by-year pair. We allow for arbitrary correlations in the error term across establishments and years within the same 4-digit industry and county—thus, standard errors are two-way clustered at the industry level and the county level. The coefficient of interest is β_1, which captures the within-establishment effects of the change in trade policy on pollutant emissions.

Identification rests on the assumption that manufacturing industries that face a greater $NTR\ Gap$ do not show differential trends in PM_{10} emissions in the pre-shock period. To check for parallel trends, we estimate

$$\mathrm{y}_{p,t} = \beta_0 + \sum_t \beta_t \mathbb{1}\{year = t\} \times NTR\ Gap_i + \sum_t \delta_t \mathbb{1}\{year = t\} \times Z_i + \gamma X_{i,t} + \eta_p + \eta_{c,t} + \varepsilon_{i,t}, \quad (5.3)$$

where the second term now interacts $NTR\ Gap$ with a full set of year dummies excluding 2000. Therefore, each β_t coefficient estimates the effect in year t relative to 2000. The full sequence of the estimated parameters not only allows us to examine pre-existing trends but also to further examine the dynamic effects and the persistence of trends in PM_{10} emissions caused by the trade policy shock.

As discussed above, our sample period overlaps with major events that possibly confound the effects of the shock. We address this concern in the following way. First, we include county-by-year fixed effects to control for lagged responses from the 1990 Clean Air Act Amendments (CAAA), the stringency of the regulatory enforcement of which varied across counties and time.[39] Second, we mitigate concerns related to the confounding effects of NAFTA in two ways. One is to restrict our sample to begin in 1997, dropping a few years that are immediately affected by the trade liberalization with Canada and Mexico. Another is to directly control for the change in US import tariffs from Mexico and check whether our main estimates are sensitive to the inclusion of this control. Finally, we repeat our main specification using alternative sample periods to assess whether the estimated effects are robust to a shorter sample period that excludes the financial crisis (2007-2009), which also coincides with the period when the TRI reporting criteria temporarily changed.[40]

5.2 Descriptive Statistics

Table 1 presents the summary statistics of the key variables used in our analyses at the establishment-year level. The sample consists of 46,753 establishment-year-level observations, which include a total

[39]The EPA classifies US counties into attainment and nonattainment based on the ambient concentrations of pollutants, and counties in the nonattainment category face stricter regulation (Hanna, 2010).

[40]Section 6.2 includes results for the second and third points.

of 4,946 unbalanced establishments and 3,666 unbalanced firms between 1997 and 2017. Subscripts t, p, f, i, and c indicate year, establishment, firm, SIC-4-digit industry, and county, respectively. For the summary statistics at various aggregation levels (i.e., industry-year, industry, firm, establishment, and county), see Appendix Table B.3.

Table 1: Summary Statistics

Establishment-Year Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
PM Emissions$_{p,t}$ (lb)	46753	50838	450609	10	719	36605
NTR Gap$_{i,99}$	46753	0.294	0.119	0.138	0.304	0.424
NTR$_{i,t}$	46753	2.480	2.037	0.000	2.342	5.162
MFA Exposure$_{i,t}$	46753	0.098	1.493	0.000	0.000	0.000
NP$_{i,95}$/Emp$_{i,95}$	46753	0.281	0.096	0.176	0.259	0.435
K$_{i,95}$/Emp$_{i,95}$	46753	137	150	37	81	324
ΔChinese Tariff$_i$	46753	-0.097	0.083	-0.175	-0.077	-0.029
ΔChinese Subsidies$_i$	46753	-0.000	0.002	-0.002	-0.000	0.001
Import Intensity (Unconditional)$_{f,97}$	37763	0.135	0.203	0.000	0.028	0.404
Import Intensity$_{f,97}$	17373	0.250	0.218	0.034	0.196	0.514
Export Intensity (Unconditional)$_{f,97}$	37763	0.276	0.331	0.000	0.132	0.965
Export Intensity$_{f,97}$	28347	0.346	0.337	0.033	0.202	1.000
Firm Employment$_{f,97}$	37763	21655	76745	82	1870	41640
Num. Establishment$_{f,97}$	37763	164	472	1	19	402
Num. 4-digit Sectors$_{f,97}$	37763	24	37	1	8	73
Age$_{p,97}$	37763	57	42	9	52	110
PM Emissions$_{p,97}$	37763	59213	514114	0	254	38195
PM Emissions$_{p,97}$/Sales$_{p,97}$ (lb/million dollar)	37763	3145.4	38071.1	0.0	5.1	960.4
I(Num. P2$_{p,95-97}$>0)	37763	0.282	0.450	0	0	1
I(Num. P2 Clean-Tech$_{p,95-97}$>0)	37763	0.146	0.353	0	0	1
Establishment Employment$_{p,97}$	37763	477	1050	34	185	1000
Establishment Sales$_{p,97}$ (million dollar)	37763	113	286	4	29	239
CAA Nonattainment$_{c,95-97}$	37763	0.118	0.323	0	0	1

Notes. This table presents the summary statistics of the key variables used in our analyses. The sample consists of 46,753 establishment-year-level observations, which include a total of 4,946 unbalanced establishments and 3,666 unbalanced firms between 1997 and 2017. Subscripts t, p, f, i, and c indicate year, establishment, firm, SIC-4-digit industry, and county, respectively.

A first notable feature is that there exists significant variation in PM_{10} emissions across manufacturing establishments and years. The average establishment-year-level emissions are 50,838 pounds with a standard deviation of 450,609 pounds. The emissions are highly skewed. The median emissions are only 719 pounds, which implies that some establishments produce extreme amounts of emissions.[41] Another notable feature is that the NTR gap also has substantial variation—with an average of 0.294 and a standard deviation of 0.119. This provides a source of variation that allows us to identify the impact of the conferral of PNTR to China on environmental outcomes.

Turning to initial firm characteristics, the average unconditional import intensity in 1997 is 13.5 percent, which is measured as the within-firm employment share of establishments that engaged in import activities in 1997.[42] After conditioning on having at least one establishment that engaged in import activities, the average conditional import-establishment share in 1997 is 25.0 percent. We observe a slightly higher value for export activities within a firm, where the average unconditional (conditional) export-establishment share in 1997 is 27.6 percent (34.6 percent).

Regarding the size of sample firms, consistent with the TRI's reporting threshold of 10 or more full-time employees, the sample firms are relatively large compared to the entire distribution (see Appendix Table B.4 for the comparison of our final sample distribution with the manufacturing sample distribution from the original NETS data).[43] For the establishment-year-level observations, the average number of firm employees in 1997 is 21,655 with a median of 1,870, meaning that the firm size distribution is also highly right-skewed.[44]

6 Main Results

6.1 Within-Establishment Emission Adjustment

Difference-in-Differences Table 2 presents the estimates of Equation (5.2). Column (1) includes the DID term and simple two-way fixed effects, i.e., establishment and year fixed effects. Columns (2) through (4) replace year fixed effects with county-by-year fixed effects. Column (3) adds time-varying industry characteristics. Column (4), which is our baseline specification, includes an interaction between the post-PNTR dummy variable and time-invariant industry characteristics.

[41] Appendix Table B.10 shows that our results are not driven by these extreme observations.

[42] This measure captures the importance of import activities within a firm. We cannot weight by import values since the NETS provides information on whether an establishment engages in import activities (a dummy variable) but not import values.

[43] Based on Appendix Table B.4, the firm-level summary statistics of our final sample show that the mean and median number of firm employees are 5,566 and 388, respectively, while those of the entire NETS manufacturing sample are only 74 and 5, respectively.

[44] A similar pattern holds for the establishment size distribution. The distribution is highly right-skewed. Based on the establishment-level summary statistics in Appendix Table B.4, the mean and median numbers of establishment employees are 410 and 160, respectively, whereas those of the entire NETS manufacturing sample are 31 and 5, respectively. This is because NETS includes a near-universe of US establishments with no size threshold including individual proprietors without any paid employee.

Table 2: PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)	(4)
	Log(PM Emissions)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.161***	-1.049**	-1.031**	-1.191***
	(0.428)	(0.422)	(0.425)	(0.387)
$\text{NTR}_{i,t}$			-0.019	-0.008
			(0.034)	(0.036)
$\text{MFA Exposure}_{i,t}$			-0.011	-0.009
			(0.016)	(0.016)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				0.305**
				(0.118)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$				0.050
				(0.054)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$				-0.740
				(0.459)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$				-33.097
				(27.109)
Establishment FE	✓	✓	✓	✓
Year FE	✓	-	-	-
County x Year FE	-	✓	✓	✓
Observations	46753	46753	46753	46753

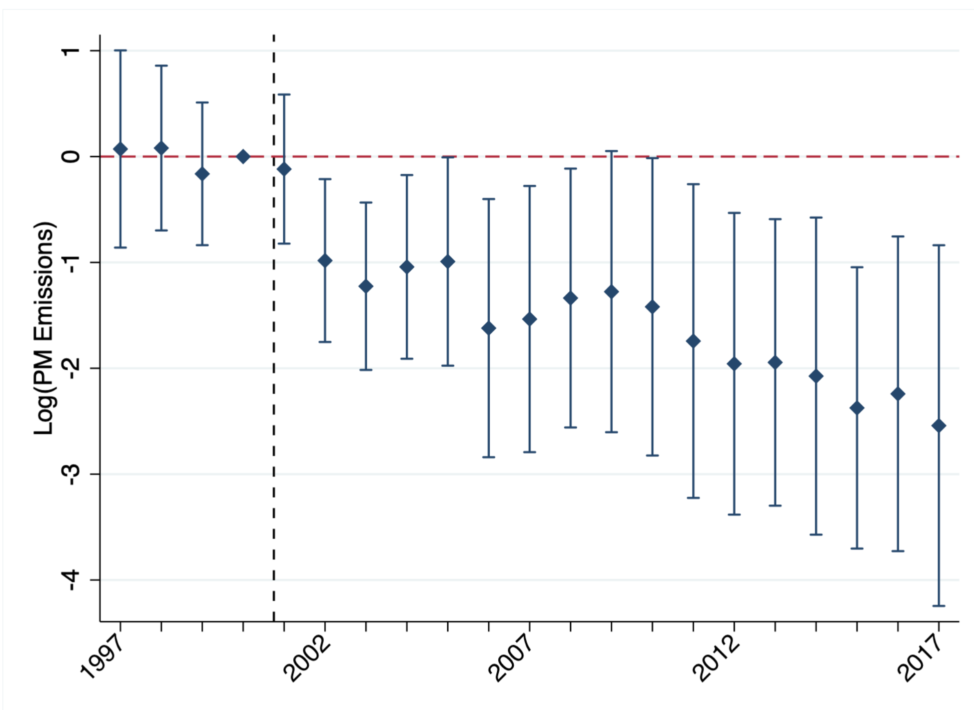
Notes. This table shows how the conferral of PNTR to China affected the establishment-year-level pollution emissions. The dependent variable is the log of establishment-year PM_{10} Emissions (Log(PM Emissions)) and the independent variable representing the effect of PNTR is the interaction of a post-PNTR indicator and the NTR gap ($\text{Post}_t \times \text{NTR Gap}_{i,99}$). Subscripts t and i indicate year and SIC-4-digit industry, respectively. Additional controls include time-varying variables—NTR tariff rates ($\text{NTR}_{i,t}$), MFA exposure ($\text{MFA Exposure}_{i,t}$)—as well as interactions of the post-PNTR indicator with time-invariant controls including the industry-level log of 1995 skill and capital intensity ($\text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$ and $\text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$, respectively), changes in Chinese import tariffs from 1996 to 2005 ($\Delta\text{Chinese Tariff}_i$), and changes in Chinese production subsidies per total sales from 1999 to 2005 ($\Delta\text{Chinese Subsidies}_i$). The sample period is from 1997 to 2017. Standard errors (in parentheses) are two-way clustered at the industry level and county level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Across all columns, we find negative coefficients with statistical significance at the 5 percent (or 1 percent) level. The results suggest that the change in China's PNTR status induced US manufacturing establishments to reduce emissions of particulate matter (PM_{10}). Quantitatively, the coefficients are highly stable across columns, ranging from -1.19 to -1.03. The baseline specification in Column (4) indicates that moving an establishment from an NTR gap at the tenth (0.138) to the ninetieth percentile (0.424) of the observed distribution increases the implied relative reduction of emissions of particulate matter (PM_{10}) within an establishment by 0.341 (= -1.191 $\times$ (0.424 - 0.138)) log points—or 34 percent.

In Column (4), while most control variables are statistically insignificant, the coefficient of the interaction of the post-PNTR dummy variable and industries' initial skill intensity (defined as the ratio of non-production workers to total employment) is positive and statistically significant. This indicates that less skill-intensive industries reduce relatively more emissions after 2000. The result is somewhat related to the finding in Pierce and Schott (2016) such that skill-intensive industries more in keeping with US comparative advantage perform relatively well in terms of employment after 2000. If less skill-intensive industries perform more poorly in terms of employment partly due to offshoring and simultaneously those industries entail more emissions, then we would expect to see a larger decline in pollution emissions in less skill-intensive industries as in Column (4).

Pre-Existing Trends and Dynamic Treatment Effects Figure 5 plots the coefficient estimates, along with their 95 confidence intervals, from the regression in Equation (5.3). We do not detect any differential pretrends in that point estimates are statistically indistinguishable from zero leading up to 2000. This pattern is in line with the parallel trends assumption, giving further credence to our identification strategy.

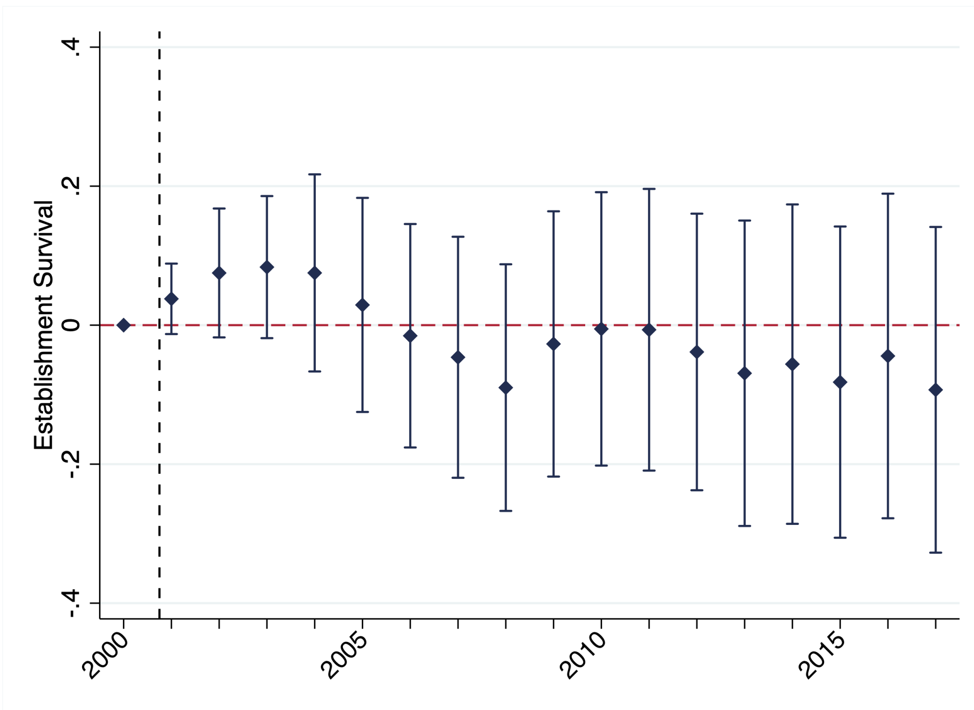
Figure 5: Dynamic Treatment Effects at the Establishment Level



Notes: This figure displays the estimated difference-in-differences (DID) dynamic coefficients with their 95 percent confidence intervals, for interactions of year dummies with the NTR gap from Equation (5.3). The dashed vertical line denotes October 2000, in which the conferral of PNTR status to China was passed by the US Congress. All controls in Column (4) of Table 2 are included in the regression. Standard errors are two-way clustered by industry and county.

The point estimate for 2001 is negative but statistically insignificant, but it becomes significant from 2002 forward. Note that while Congress passed the bill in October 2000, the change in PNTR status became effective in January 2002. The estimated coefficient declined by -0.983 log points in 2002 (the first year PNTR became effective) and remained stable until 2005. There is an overall downward trend in the estimated coefficients for the subsequent years with an uptick from 2007 to 2009.[45] The magnitude of estimates increased over time from -1.419 log points in 2010 to -2.541 log points in 2017. Overall, the dynamic treatment effects indicate that trade policy had a prolonged effect on the reductions of pollution emissions in US manufacturing establishments.

Figure 6: PNTR and Establishment Survival, 2001 - 2017



Notes: This figure shows the cumulative effect of the imposition of PNTR on establishment survivals, conditional on positive PM_{10} Emissions in 2000. Each point reflects an individual regression coefficient, β_t, following Equation (6.1). The estimated coefficients are displayed with their 95 percent confidence intervals. The dependent variable, $y_{p,t}$, is an indicator variable that equals one if establishment p exists in year t and 0 otherwise. Note that we restrict the sample to establishments that had positive PM_{10} Emissions in 2000, so $y_{p,2000} = 1$ holds for all establishments. The independent variable is the industry-level NTR Gap (NTRGap_i). All regressions include county fixed effects and control for the log of establishment employment in 2000, the log of firm employment in 2000, firm age in 2000, the industry-level NTR tariff rates in 2000, the industry-level MFA exposure in 2000, the industry-level log of 1995 skill and capital intensity, changes in Chinese import tariffs from 1996 to 2005, and changes in Chinese production subsidies per total sales from 1999 to 2005. Standard errors are two-way clustered by industry and county.

[45]Please refer to Section 3 regarding the reporting criteria change during the years 2007-2009 for further details. We cautiously interpret that the upticks shown in this period because they may be attributed to the change in reporting chemicals or the global financial crisis. In the following, we further subject our empirical specification to an alternative approach that excludes the years 2007, 2008, and 2009 to determine whether the results remain robust.

Establishment Survival Pierce and Schott (2016) note that the change in trade policy may induce Chinese producers to invest in entering or expanding into the US market, thereby increasing competition for US manufacturers. If so, less competitive domestic manufacturers would be squeezed out of the market by heightened import competition, meaning that pollution emissions would decrease from the exit of establishments, in addition to the within-establishment emission adjustment. To assess this possibility, we use the following empirical specification to compare the evolution of establishment survival in industries facing large NTR gaps to those in industries facing smaller NTR gaps:[46]

$$\mathrm{y}_{p,t} = \beta_t NTRGap_i + \alpha V_p + \gamma X_i + \delta Z_i + \eta_c + \varepsilon_{p,t}. \tag{6.1}$$

The sample is restricted to establishments that release positive amounts of PM_{10} in 2000 (the reference year in the analysis). We estimate this equation separately for each year $t \in [2001, 2017]$. The dependent variable, $y_{p,t}$, is an indicator variable that equals one if establishment p exists in year t and 0 otherwise. β_t measures the cumulative effect of the imposition of PNTR on establishment survival by year t. V_p captures establishment- and firm-level initial characteristics (measured in 2000), including the log of establishment employment, the log of firm employment, and firm age. X_i and Z_i capture industry-level characteristics, which are analogous to $X_{i,t}$ and Z_i, respectively, in our main difference-in-differences specification in Equation (5.2).[47] η_c is the county to which establishment p belongs in 2000. As in Dix-Carneiro and Kovak (2017), each year's β_t captures one point on the empirical impulse response function describing the cumulative effects of the imposition of PNTR as of each post-PNTR year.

Figure 6 plots the coefficients on $NTRGap_i$ for each year. The survival rates initially increased in the early 2000s; showed a downward trend until the year 2008; rebounded during the period 2008-2010; and then slightly declined thereafter. However, all the coefficients are statistically insignificant, meaning that the imposition of PNTR did not induce US manufacturers that reported positive amounts of PM_{10} in 2000 to exit the market. Therefore, establishment exits are not the primary factor behind the reduction in pollution emissions in US manufacturing.[48]

Note that this result does not necessarily mean that the imposition of PNTR did not induce US manufacturers to leave the market *in general*. In Appendix C, we show that manufacturing establishments that generate positive amounts of emissions are fundamentally different from those

[46]The empirical specification is similar to that of Dix-Carneiro and Kovak (2017) in which they study the evolution of trade liberalization's effects on Brazilian local labor markets.

[47]These industry controls include the industry-level NTR tariff rates in 2000 ($\mathrm{NTR}_{i,00}$), the industry-level MFA exposure in 2000 (MFA $\mathrm{Exposure}_{i,00}$), the industry-level log of 1995 skill and capital intensity ($\mathrm{Log}(\mathrm{NP}_{i,95}/\mathrm{Emp}_{i,95})$ and $\mathrm{Log}(\mathrm{K}_{i,95}/\mathrm{Emp}_{i,95})$, respectively), changes in Chinese import tariffs from 1996 to 2005 (ΔChinese Tariff_i), and changes in Chinese production subsidies per total sales from 1999 to 2005 (ΔChinese $\mathrm{Subsidies}_i$).

[48]In Appendix Table B.14, we accommodate observations with zero reported emission using PPML regressions and show that the estimated impact of PNTR on PM emissions are more or less stable across (i) accommodating establishment entry and exit margins, (ii) restricting the analysis to surviving establishments but allowing zero emission, and (iii) restricting the analysis to observations with positive emissions. This further shows that our results are not particularly driven by the extensive margin of establishment exits.

with zero or negligible emissions, and our result that attributes emission abatement to surviving establishments is not a spurious result driven by the restriction of sample induced by TRI-reporting criteria.

Emission Intensity Adjustment The emission reduction effects could be explained simply by a scale effect within an establishment. In other words, the observed emission effects could be interpreted as reductions in production, not abatements in emissions. To check for this possibility, we construct an establishment-level emission intensity, which is defined as the ratio of PM_{10} Emissions to sales. Using this new dependent variable, we repeat the baseline analysis in Table 2.

Table 3: PNTR and Establishment-level Pollution Emission Intensity, 1997 - 2017: Log(PM Emissions/Sales)

	(1)	(2)	(3)	(4)
	Log(PM Emissions/Sales)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.743***	-1.621***	-1.595***	-1.635***
	(0.514)	(0.597)	(0.544)	(0.535)
$\text{NTR}_{i,t}$			0.013	0.041
			(0.042)	(0.045)
$\text{MFA Exposure}_{i,t}$			-0.010	-0.008
			(0.018)	(0.018)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				0.312**
				(0.155)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$				0.172***
				(0.062)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$				-0.855
				(0.572)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$				-74.688**
				(30.637)
Establishment FE	✓	✓	✓	✓
Year FE	✓	-	-	-
County x Year FE	-	✓	✓	✓
Observations	46751	46751	46751	46751

Notes. This table repeats the specifications in Columns (1)-(4) of Table 2, where we use a measure of establishment-year-level pollution emission intensity—measured by log of PM_{10} emissions-to-sales ratio—as a dependent variable. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 3 reports the estimation results. Across all columns, we find negative coefficients with statistical significance at the 1 percent level. This means that establishments that are more exposed to the change in trade policy reduce not only pollution but also pollution per unit of sales within an establishment. Quantitatively, the coefficients range from -1.74 to -1.60. The magnitudes of emission intensity reduction are much larger than those of emission reduction. This implies that the results go against the scale effect channel within an establishment.[49] Therefore, we can rule out the hypothesis that trade liberalization simply drives down the scale of output and reduces pollution emissions among surviving US manufacturers accordingly. Appendix Figure A.8 plots the dynamic treatment effects using emission intensity as a dependent variable. Reassuringly, we do not detect any differential pretrends. Furthermore, as in the emission variable, we also find a lingering effect of the imposition of PNTR on emission intensity. Unlike Figure 5, in the case of emissions, we do not detect any uptick during the period between 2007 and 2009:[50] The emission intensity declines substantially in 2002 and thereafter exhibits a smooth, downward trend until 2017.

Non-Disposal Activities As discussed in Section 3, production waste can be either disposed of or managed through non-disposal activities. To understand whether the establishment-level adjustments to reduce PM_{10} emissions are mechanically driven by increases in non-disposal activities, we use the log amount of PM_{10} that is recycled, treated, or combusted for energy recovery as the dependent variable and repeat the baseline analysis in Table 2. We separately construct the waste amount transferred to off-site facilities and processed on-site. Note that recycling, which the EPA ranks as the most environmentally preferred among the available non-disposal methods, accounts for the vast majority of non-disposal shares in our sample: 99 percent of off-site and 64 percent of on-site non-disposal. The first columns of Appendix Tables B.18 (off-site non-disposal) and B.19 (on-site non-disposal) present the estimation results. Here, we do not find statistically significant effects of PNTR on off-site or on-site non-disposal activities. That is, we find limited evidence that PNTR-led within-establishment adjustments are mechanically driven by establishments increasingly resorting to these waste-management methods. Instead, the results imply that establishments are responding by potentially reducing waste production at the source through, for example, adopting green technology or relocating high-polluting tasks abroad. We revisit this discussion in Section 7 where we study mechanisms in detail.

[49] Appendix Figure A.9 is generally consistent with this pattern such that sales within establishments increased after PNTR but they are almost statistically indistinguishable from zero. While most of the coefficients are statistically insignificant, sales within surviving establishments that release positive amounts of pollution emissions can actually increase in response to PNTR at the intensive margin. One potential explanation for the increase in sales after PNTR can be found in Pierce and Schott (2016), where they detected PNTR is associated with capital deepening for continuing plants. They also provided anecdotal evidence supporting this mechanism such that US firms expanded production capacity significantly more in order to compete with Chinese firms. Another reason for the mild increase of scale after PNTR for surviving establishments can be explained in Appendix C. Here, we find that establishments that release toxic emissions are fundamentally different from typical establishments, which suggests that sales response (at the intensive margin) does not necessarily coincide with those two different groups.

[50] It appears that the TRI Burden Reduction Rule and the Great Recession may have differentially affected US manufacturing establishments in terms of emissions and sales, respectively. However, we conjecture that the normalization (i.e., pollution per unit of sales) may have addressed the differential impacts. This may be why we observe a smooth, downward trend in Appendix Figure A.8.

6.2 Robustness Checks

In this section, we conduct several robustness tests to corroborate our main difference-in-differences results in Section 6.1: (i) alternative sample periods; (ii) controlling for NAFTA; (iii) excluding entry and exit; (iv) dropping outliers; and (v) weighted regressions and toxicity-weighted emissions. At the end of this section, we also briefly summarize additional robustness exercises that we conducted in Appendix B.

Alternative Sample Periods We alter the sample period beyond the baseline setting (from 1997 to 2017). First, we add two earlier years (i.e., 1995 and 1996). Initially, we excluded those two years to avoid the potentially confounding effects of NAFTA, which came into force on January 1, 1994. Column (1) of Appendix Table B.7 shows the estimated results using the sample period from 1995 to 2017. They are nearly indistinguishable from our baseline results. While the magnitude of the coefficient increases, i.e., from -1.19 to -1.32, we continue to reject zero emission effects. Appendix Figure A.4 illustrates corresponding dynamic treatment effects in the sample period from 1995 to 2017, confirming the robustness of the baseline results summarized in Figure 5: We again reject differential pretrends in emissions; the long-term effects we find in the main results are robust to including the two earlier years.

Next, we check the robustness of our results by excluding three years (2007, 2008, and 2009) from our baseline sample. As discussed above, there was a major change in reporting criteria in 2007, which was revoked in 2009.[51] Another related concern about this period is the overlapping of our sample with the Great Recession. If US manufacturing establishments were differentially affected by the Great Recession, the observed emission effects could be ascribed to the Great Recession instead of trade policy. For instance, this would be the case if unobserved demand or supply shocks caused by the Great Recession are also correlated with our shock, which we might fail to address through the set of control variables including the county-year fixed effects.

Column (2) of Appendix Table B.7 repeats the baseline analysis when dropping years from 2007 to 2017; Column (3) drops from 2007 to 2017 and adds two earlier years (1995 and 1996); Column (4) drops from 2007 to 2009. The magnitude of coefficients remains quantitatively similar, i.e., ranging from -1.22 to -0.98, further solidifying our baseline results. Appendix Figures A.5 and A.6 present corresponding dynamic treatment effects for the sample periods (i) from 1995 to 2006 and (ii) from 1997 to 2006, respectively. Once again, the results confirm the robustness of the baseline results summarized in Figure 5, verifying the validity of the parallel trends assumption. In addition, the observed emission reductions are noticeable from 2002 onward, indicating that a structural change may have happened between 2001 and 2002—the timing overlaps well with China becoming a member of the WTO on December 11, 2001, and with PNTR becoming effective on January 1, 2002.

[51] See Section 3 for further details regarding the TRI Burden Reduction Rule and the Omnibus Appropriations Act.

Controlling for NAFTA A more direct way to address concerns related to the lagged responses to NAFTA is to control for changes in US tariffs on imports from Mexico in our baseline regression. In particular, we include an interaction term of industry-level changes in US tariffs on imports from Mexico from 1990 to 2000 and the post-PNTR dummy variable.[52] Appendix Table B.8 presents the estimation results.[53] Column (1) of Appendix Table B.8 includes the interaction of the post-PNTR indicator and the industry-level NAFTA tariff changes with US total imports as trade value weights, whereas Column (2) uses US imports from Mexico as trade value weights. The estimated coefficients remain negative and statistically significant but decrease slightly in magnitude in comparison with the main DID coefficient in Column (4) of Table 2. Appendix Figure A.7 plots the dynamic treatment effects after controlling for the NAFTA tariff changes. Again, we obtain quantitatively similar effects to our main results presented in Section 6.1.

Excluding Entry and Exit Due to the entry and exit of establishments, our main empirical specification in Equation (5.2) may not fully capture within-establishment emission adjustment. If the observed emission effects are entirely driven by reallocations along the extensive margin, then the imposition of PNTR should have no impacts on establishments that had operated throughout the entire sample period (i.e., 1997 - 2017). To alleviate this concern, we restrict establishments that had positive employment for the entire sample period from 1997 to 2017. Appendix Table B.9 presents the estimation results using the restricted sample. The estimated coefficients are all negative with statistical significance at the one percent level. In addition, the magnitudes of the coefficients become even larger, ranging from -1.57 to -1.43. These results reject the hypothesis that the emission effects are fully driven by the entry and exit of establishments, whereas they support the within-establishment emission reductions as a consequence of the imposition of PNTR.

Dropping Outliers As discussed in Section 5.2, the distribution of PM_{10} emissions is highly skewed such that there are a small number of establishments that produce extreme emissions. A similar pattern holds for the firm size and establishment size distributions, which are also well-documented in the literature (e.g., Gabaix 2011; Haltiwanger et al. 2013). To ensure that these extreme observations are not driving our results, we perform a robustness check by dropping extreme values. Specifically, in Columns (1)-(3) of Appendix Table B.10, we drop observations from the top and the bottom 2.5 percent of the distribution of (i) PM_{10} emissions, (ii) firm size, and (iii)

[52]Following Hakobyan and McLaren (2016), we construct the industry-level tariff changes as follows: first, we collect HS-8-digit-level US tariffs on imports from Mexico in 1990 and 2000; second, we obtain trade-value-weighted (in 1990) average tariffs for each 4-digit-industry using within-industry product shares; and third, we then compute the industry-level average US tariffs on imports from Mexico between 1990 and 2000. Note that the within-industry product shares are constructed in two different ways: using trade flows between (i) the US and the rest of the world; (ii) the US and Mexico.

[53]Note that all controls in Column (4) of Table 2, our baseline specification, are included in these regressions.

establishment size, respectively. The results are barely affected by dropping those outliers.

Weighted Regressions and Toxicity-Weighted Emissions We also show that our results are robust to allowing alternative weighting schemes. Column (1) of Appendix Table B.11 considers a weighted regression weighted by the establishment's initial PM_{10} emissions. In Column (2), we instead weigh each observation by an establishment's initial employment. Last, in Column (3), we use the log of toxicity-weighted PM_{10} emissions as a dependent variable and run a weighted regression weighted by initial emissions.[54] Our results are robust to the alternative specifications.

Additional Analyses In Appendix B, we additionally show that our main results are robust to controlling for the indirect impact of PNTR through input-output linkages—i.e., upstream- and downstream-specific NTR gaps (Table B.12), upstream-specific time trends (Table B.13), and accommodating observations with zero reported emission using PPML regression (Table B.14).

6.3 Heterogeneous Adjustments Across Establishments

We extend Equation (5.2) to a triple difference-in-differences design to investigate heterogeneous responses across establishment *groups* defined by their initial characteristics. We consider firm-level import and export intensities (measured using the within-firm employment share of establishments that engaged in import and export activities), counts of 4-digit sectors, counts of establishments, and size. We also consider establishment-level exposure to environmental regulation stringency using the county-specific *nonattainment* status designated through the 1990 Clean Air Act Amendments, age of establishment, and establishment-level adoption of environment-friendly practices in production and waste management (or green technology) using pollution prevention (P2) activities.[55] Finally, we consider industry-level upstreamness (constructed using Input-Output tables on US production linkages as in Antras et al. (2012)). Following Burchardi et al. (2019), we work with a binary indicator that takes value one if the upstreamness index is larger than 2. Table 4 presents estimates of the triple-difference estimator. Columns (1) through (9) separately examine the differential effects across these initial characteristics, and Column (10) combines all eight of them.[56]

There are four notable results in Table 4, which, for visibility, we placed in the first four columns

[54] We use toxicity weights that the EPA constructed using the Risk-Screening Environmental Indicators (RSEI) Methodology. These measures are useful in terms of understanding our results with respect to potential long-term health risks associated with the pollutants.

[55] Under the 1990 Clean Air Act Amendments, the EPA established a minimum level of air quality standard that all US counties are required to meet for four pollutants: carbon monoxide (CO), ozone (O3), sulfur dioxide (SO2), and particulate matter (PM). Each year, if a county exceeds the minimum level for a specific pollutant, then it receives a nonattainment designation for that pollutant. Otherwise, a county receives an attainment designation. In our analysis, we define nonattainment counties designated specifically for particulate matter (PM). See Hanna (2010) for comprehensive coverage of the institutional details.

[56] We consider the log of PM_{10} Emissions as a dependent variable in Table 4, but we find broadly consistent results with the log of pollution emission intensity. See Appendix Table B.15.

Table 4: Heterogeneous Treatment Effects:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017: Log(PM Emissions)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Log(PM Emissions)									
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.221	-1.090***	0.350	-2.392	-1.252**	-1.588**	-1.986	-1.274*	-1.664***	4.611
	(0.969)	(0.393)	(0.887)	(1.751)	(0.629)	(0.676)	(1.340)	(0.688)	(0.421)	(5.360)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Import Intensity}_{f,97}$	-4.452*									-10.944***
	(2.365)									(3.081)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Nonattainment}_{c,95-97}$		-2.316***								-3.995***
		(0.772)								(0.986)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Upstream}_{i,97}$			-2.187**							-3.172**
			(0.959)							(1.596)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Log(Num. 4-digit Sectors}_{f,97})$				-0.105						-2.934**
				(0.486)						(1.355)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Export Intensity}_{f,97}$					-0.454					-5.922
					(1.358)					(4.367)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Log(Num. Establishment}_{f,97})$						0.057				0.397
						(0.179)				(1.125)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Log(Firm Employment}_{f,97})$							0.076			0.786
							(0.170)			(0.860)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{Age}_{p,97}$								-0.002		0.007
								(0.009)		(0.010)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$ $\times \text{I(Num. P2}_{p,95-97} > 0)$									0.532	0.977
									(0.663)	(0.952)
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County x Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	17373	37763	37701	37763	28347	37763	37763	37763	37763	15611

Notes. This table shows how the conferral of PNTR to China heterogeneously affected the establishment-level pollution emissions depending on various initial characteristics by including triple interactions of a post-PNTR indicator, the NTR gap, and a given initial characteristic. Column (1) considers a firm's initial import intensity—measured as a within-firm employment share of establishments that engaged in import activities in 1997—conditional on the firm being an importer ($\text{Import Intensity}_{f,97}$>0) to capture the intensive margin of intensity. Column (2) considers a county-level measure of strict regulatory oversight under the Clean Air Act Amendments (CAAA). Specifically, we consider a nonattainment dummy variable that takes value one if a given county has a record of nonattainment during 1995-1997 to achieve the national standards for PM emissions under CAAA. Column (3) considers an industry-level upstreamness dummy as in Burchardi et al. (2019), which takes value one if the upstreamness index (Antras et al., 2012) is larger than 2. Column (4) considers the log of the initial number of SIC-4-digit sectors within a firm. Column (5) considers a firm's initial export intensity—measured as a within-firm employment share of establishments that engaged in export activities in 1997—conditional on the firm being an exporter ($\text{Export Intensity}_{f,97}$>0) to capture the intensive margin of intensity. Columns (6)-(8) consider the log of the initial number of establishments within a firm, the log of initial firm employment, and the initial age of establishment. Column (9) considers a measure of an establishment's initial pollution prevention-related activities (P2), which equals one if there were at least one toxic chemical between 1995-1997 that the establishment had taken any pollution prevention-related activities. Column (10) includes all triple interactions simultaneously. All columns include interactions of the column-specific initial characteristic(s) with (i) post-PNTR indicator and (ii) NTR gap, respectively. The rest of the specifications are identical to those in Column (4) of Table 2: We include all set of controls and fixed effects as in Column (4) of Table 2. The sample is restricted to establishments whose initial firm characteristics are well defined (i.e., establishments whose parent firms existed in 1997), which results in 37,763 observations. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

in the table (i.e., Columns (1)-(4) and (10)). First, the estimated coefficient for import intensity (Import Intensity$_{f,97}$) is negative and statistically significant while that of export intensity (Export Intensity$_{f,97}$) is also negative but lacks statistical precision. Consistent with Fact 3 of the stylized facts in Section 4, establishments of firms that are initially more engaged in import activities have substantially more reduced emissions than others.[57] We have limited information on the nature of these import (or export) activities since we do not observe the type of products establishments import (or export) or their trading partners in the data. However, as long as manufacturing firms do not purchase goods from abroad to resell to consumers, it is most likely that these imports consist of intermediate goods (Hummels et al., 2014), thereby possibly capturing offshoring activities. In this context, one plausible mechanism is that PNTR encourages establishments leveraging existing foreign sourcing networks to import instead of produce intermediate goods that require high-polluting activities and end up reducing pollutant emissions domestically.[58]

Second, the estimated coefficient for the initial nonattainment status of the county in which each establishment is located (Nonattainment$_{c,95-97}$) is negative and statistically significant. That is, in response to PNTR, establishments that were initially facing tougher environmental regulations decreased emissions by a greater magnitude than others facing more lenient standards. In fact, Hanna (2010) finds that strengthened US environmental regulations, proxied by nonattainment county status of establishment locations, induce US-based multinationals to increase their FDI activities. Consistent with this finding, a possible interpretation of our result is that establishments in nonattainment counties, in search of ways to reduce abatement costs, leveraged the PNTR-induced opportunities for FDI, including offshoring high-polluting tasks abroad.

Third, the estimated coefficient for upstream industries (Upstream$_{i,97}$) is negative and statistically significant. In response to PNTR, establishments operating in industries that produce goods considered as intermediate inputs in the final goods production process reduced emissions by a greater amount than those operating in more downstream industries. Together with the first result using import intensities, this result offers complementary evidence in support of the offshoring mechanism. If PNTR facilitated firms with available sourcing networks to purchase inputs from abroad, then by virtue of the same mechanism, establishments in upstream industries may reduce pollution-intensive

[57] In this exercise, we condition on firms being importers (i.e., Import Intensity$_{f,97}>0$) to capture the intensive margin of intensity. In Appendix Table B.16, we consider the unconditional import intensity that includes non-importers. We obtain negative coefficients, but the estimate is less precise. The importance of intensive margin is consistent with Martin et al. (2021), who find that products with higher relationship stickiness—in particular, industrial (specialty) chemical and pharmaceutical products—have larger intrafirm trade and exhibit stronger trade dynamics in response to uncertainty shock. In our data, we find that chemicals and allied products (SIC 2-digit 28), which include industrial chemical and pharmaceutical products, are among industries with the highest emission share in the US (Figure A.2) and exhibit strong response to PNTR shock (Table B.5).

[58] Note that we find qualitatively similar results when using the log amount of PM_{10} processed through off-site non-disposal methods as the dependent variable (See Column (11) of Appendix Table B.18). While we did not find any significance in the main effects, we report important complementarity between an establishment's access to foreign sourcing networks and off-site non-disposal activities. Appendix Table B.19 presents the results for on-site non-disposal, where we do not find analogous patterns.

production of intermediate goods as PNTR induces domestic firms to switch to Chinese suppliers. For multi-establishment firms operating in both upstream and downstream industries, this speaks to a scenario in which firms offshore dirty tasks previously performed through their own establishments in upstream industries, thereby reducing emissions.

Finally, the estimated coefficient for multi-sector establishments (Num. 4-digit Sectors$_{f,97}$) is negative and statistically significant, conditional on other interactions (Column (10)). Establishments that belong to a multi-establishment firm operating in different sectors are more diversified and might be more resilient to shocks through flexible reallocation of resources across establishments (Hyun et al., 2022). Together with the third result, it is possible that such flexibility allows these multi-sector firms to easily offshore upstream and dirty production and reallocate their resources toward cleaner production, resulting in reduced pollution emissions.

7 Mechanisms

7.1 Pollution Offshoring Hypothesis

Motivated by the suggestive evidence in support of the offshoring mechanism in the heterogeneous treatment effect analyses, we directly assess the importance of the offshoring channel—global sourcing and FDI—in explaining our main findings on the PNTR-induced reductions in the PM_{10} emissions within US manufacturing. We further examine whether consistent patterns are shown when studying the PNTR-induced US imports of high-polluting products from China.

Global Sourcing and FDI Activities Offshoring occurs when parts of the multi-stage production process are performed abroad. Such offshoring activities involve sourcing foreign intermediate inputs (Hummels et al., 2001), creating vertical production networks to perform offshored tasks, and establishing foreign affiliates to serve the market of the host country or to export to other markets outside the host country (Hanson et al., 2005; Garetto, 2013; Tintelnot, 2017). In the data, however, it is challenging to construct a single measure that comprehensively captures these offshoring activities (Monarch et al., 2017), let alone relocation of dirty tasks.

Our analysis relies on two separate proxies of offshoring, which leverage detailed data from NETS and WRDS Company Subsidiary data, respectively.[59] Specifically, we first use time-varying importing status at the establishment level from the NETS database to capture US manufacturers' purchasing task outputs from China that are combined in the final goods production. Next, we use WRDS Company Subsidiary data linked to our main dataset and count the number of foreign subsidiaries in China (and other countries) to measure US multinationals' FDI activities at the establishment-year

[59] While the coverage of the WRDS Company Subsidiary Data is confined to publicly listed companies, it is the best available dataset that enables us to directly test the offshoring mechanism via FDI activities. Refer to Section 3.1 for more detailed descriptions of WRDS Company Subsidiary Data.

level. Using these two measures—importing status and FDI—as dependent variables, we estimate the main equation (5.2). In each exercise, we use the triple difference-in-differences framework to test whether such offshoring activities are more pronounced for establishments associated with high-polluting tasks—measured as whether establishments were located in nonattainment counties or whether establishments had higher initial pollution intensity. Note that establishments in counties with a nonattainment designation are likely heavy emitters during the initial period, as this designation is granted to counties with air pollution concentrations that exceed federal standards (Hanna, 2010).

Table 5: PNTR and Import Status, 1997 - 2017

	(1) Import	(2) Import	(3) Import
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.288**	0.154	1.183***
	(0.119)	(0.115)	(0.444)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Nonattainment}_{c,95-97}$		0.731***	
		(0.278)	
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(PM Emissions/Sales}_{p,97})$			0.090**
			(0.040)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Controls	✓	✓	✓
Margin	Intensive	Intensive	Intensive
Observations	13760	13760	9164

Notes. This table investigates the average and heterogeneous treatment effects of the conferral of PNTR to China on establishment-level import status. The dependent variable, Import, is a dummy variable that equals to one if establishment p engages in importing activities in year t. We focus on the intensive margin adjustment of importing activities within a firm by restricting the sample to establishments that belonged to an importing firm in 1997 (i.e., Import $\text{Intensity}_{f,97} > 0$). Column (1) shows the average treatment effect. Columns (2) and (3) investigate the heterogeneous treatment effects depending on (i) a county-level initial measure of strict regulatory oversight under the Clean Air Act Amendments (CAAA) and (ii) a measure of the establishment's initial pollution emission intensity—measured by the log of PM_{10} emissions-to-sales ratio. Specifically, we include triple interactions of a post-PNTR indicator, the NTR gap, and a given initial characteristic. Columns (2)-(3) also include interactions of the column-specific initial characteristic with (i) post-PNTR indicator and (ii) NTR gap, respectively. The rest of the specifications in Columns (1)-(3) are identical to Column (4) of Table 2. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 5 reports the estimates for global sourcing activities. We begin by focusing on firms with at least one foreign sourcing network (i.e., Import $\text{Intensity}_{f,97} > 0$) because the emission reduction effects were most pronounced at the intensive margin of importing activities in Table 4.[60] Column (1) of Table 5 indicates that non-importing establishments that belong to a firm with foreign sourcing networks began to source from abroad after PNTR. Conversely, Column (1) of Appendix Table B.17 shows that PNTR had no such effect for establishments that did not initially belong to an importing firm (i.e., Import $\text{Intensity}_{f,97} = 0$). Both results suggest that establishments with existing foreign

[60]As noted in footnote 57, the importance of intensive margin of the relationship is broadly consistent with the findings in Martin et al. (2021).

Table 6: PNTR and FDI into China vs. Other Countries, 1997 - 2017

	(1)	(2)	(3)	(4)
	Z = Num. Subsid. in China			
	I(Z> 0)		Log(Z)	
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.265	0.188	1.073*	1.173
	(0.260)	(0.193)	(0.611)	(0.920)
Establishment FE	✓	✓	✓	✓
County FE	✓	-	✓	-
Year FE	✓	-	✓	-
County x Year FE	-	✓	-	✓
Controls	✓	✓	✓	✓
Observations	12608	8346	6384	3067
	(5)	(6)	(7)	(8)
	Z = Num. Subsid. in Other			
	I(Z> 0)		Log(Z)	
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.126	0.090	-0.124	-0.005
	(0.215)	(0.148)	(0.682)	(0.654)
Establishment FE	✓	✓	✓	✓
County FE	✓	-	✓	-
Year FE	✓	-	✓	-
County x Year FE	-	✓	-	✓
Controls	✓	✓	✓	✓
Observations	12608	8346	11442	7298

Notes. This table investigates the effect of the conferral of PNTR to China on FDI activities. For each establishment-year pair, we assign yearly measures of FDI activities by its parent firm as dependent variables. Specifically, columns (1)-(2) consider a dummy variable that equals one if the establishment's parent firm has at least one subsidiary in China in year t (extensive margin). Columns (3)-(4) consider the log of the number of subsidiaries (of the establishment's parent firm) in China in year t (intensive margin). Columns (5)-(8) repeat Columns (1)-(4), where we consider the number of subsidiaries in other countries. Columns (1), (3), (5), and (7) separately include county fixed effects and year fixed effects, and columns (2), (4), (6), (8) include county-by-year fixed effects. The rest of the specifications are identical to those in Column (4) of Table 2: We include all controls and establishment fixed effects as in Column (4) of Table 2. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

networks, those who had already incurred the sunk investment costs, played a major role in global sourcing activities following PNTR, underscoring the importance of intensive margin adjustment.[61] Columns (2) and (3) of Table 5 further show that establishments that are most likely to be involved in high-polluting tasks engage more in importing activities than other establishments after PNTR. Specifically, we find that establishments are more likely to engage in importing activities if they faced stricter environmental regulation in the initial period under the Clean Air Act Amendments and for those that had higher initial pollution emission intensity. The results collectively suggest that US manufacturers sent (or sourced) a set of dirty tasks, thereby reducing toxic emissions.

Table 6 reports the estimates for FDI activities. In Columns (1)-(2) and (5)-(6), we consider a

[61] Similarly, we examine whether exporting activities respond to PNTR. Columns (2) and (3) of Appendix Table B.17 reveal that PNTR does not cause new exporting activities at the establishment level.

Table 7: Heterogeneous Treatment Effects:
PNTR and FDI into China, 1997 - 2017

	(1)	(2)
	Z = Num. Subsid. in China	
	I(Z> 0)	Log(Z)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.161	0.735
	(0.200)	(0.871)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Nonattainment}_{c,95-97}$	0.440	5.169***
	(0.461)	(1.102)
Establishment FE	✓	✓
County x Year FE	✓	✓
Controls	✓	✓
Observations	8346	3067

	(3)	(4)
	Z = Num. Subsid. in China	
	I(Z> 0)	Log(Z)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.872	12.871***
	(0.940)	(3.946)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(PM Emissions/Sales}_{p,97})$	0.057	0.938***
	(0.080)	(0.323)
Establishment FE	✓	✓
County x Year FE	✓	✓
Controls	✓	✓
Observations	4399	1372

Notes. This table investigates the heterogeneous treatment effects of the conferral of PNTR to China on FDI decisions in China. Specifically, Columns (1)-(2) and Columns (3)-(4) in this table, respectively, repeat the specifications in Columns (2) and (4) of Table 6, where we include triple interactions of a post-PNTR indicator, the NTR gap, and a given initial characteristic. Columns (1)-(2) consider a county-level measure of strict regulatory oversight under the Clean Air Act Amendments (CAAA). Specifically, we consider a nonattainment dummy variable that takes value one if a given county has a record of nonattainment during 1995-1997 to achieve the national standards for PM emissions under CAAA. Columns (3)-(4) consider a measure of the establishment's initial pollution emission intensity—measured by the log of PM_{10} emissions-to-sales ratio. All columns also include interactions of the column-specific initial characteristic with (i) post-PNTR indicator and (ii) NTR gap, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

dummy variable that equals one if firm f has at least one subsidiary in China (or other countries) in year t, thereby measuring the extensive margin; in Columns (3)-(4) and (7)-(8), we measure the log of firm f's number of subsidiaries in China (or other countries) in year t, thereby capturing the intensive margin. We find that the PNTR induced US manufacturing establishments to set up *more* subsidiaries, mainly at the intensive margin.[62] We further conduct a placebo test in Columns (5) through (8). As the change in PNTR status only concerns China, we do not expect the FDI effect to be significant for other destination countries. Consistent with this conjecture, the coefficients are all

[62] Due to the reduced number of observations in Column (4), we lose statistical power; nevertheless, the p-value is 0.204 and thus is statistically significant at the 21 percent level.

statistically insignificant and small in magnitude. In Table 7, we further find that establishments that are most likely to engage in high-polluting tasks increase their subsidiaries in China more than other establishments, an effect that mainly operated through the intensive margin (see Columns (2) and (4)), not the extensive margin. That is, we find that the number of subsidiaries in China increased more for establishments that faced stricter environmental regulation in the initial period and for those that had higher initial pollution emission intensity.

Dirty Product Imports from China to US We then test whether US manufacturers, in fact, increased imports of dirty products from China. If US manufacturers shifted high-polluting activities to China after the conferral of PNTR, and such a shift was driven by the offshoring mechanism, we would anticipate that US manufacturers will increase dirty product imports from China relative to other countries. To test this hypothesis, we use HS 10-digit product-by-year-level data from the UN Comtrade database and examine whether the share of US imports from China increased to a greater extent for products categorized under high-polluting industries using the same definition (criteria) of dirtiness as in the previous exercises. Table 8 shows the result. Column (1) confirms that, following the conferral of PNTR to China, the share of US imports from China increased. Column (2) shows the heterogeneity across products in the dirtiness of products where the effects are more pronounced for products that are produced by high-polluting industries. In Column (3), albeit only with a p-value of 0.258, we find that the increase in the share of US imports from China is more noticeable in upstream industries.

Discussion: Offshoring versus Import Competition The set of findings presented in this section collectively provides direct evidence supporting the offshoring channel as an important mechanism through which US manufacturers shifted high-polluting activities to China after the conferral of PNTR to China, resulting in a reduction in domestic emissions. We note that our results strongly indicate the *pollution offshoring hypothesis* in that progress toward trade liberalization induces firms in developed countries to avoid stringent environmental regulations by locating production in countries, typically developing countries, with laxer environmental standards.

Despite the extensive evidence supporting the offshoring mechanism, one may still argue that our results are potentially confounded by the import competition channel.[63] While we do not claim that the offshoring channel was the only contributor in a dichotomous way, we emphasize that there was ample evidence in favor of offshoring, but not import competition, as we illustrate below.

To reiterate, the conferral of PNTR to China is related to a reduction in trade policy uncertainty, not actual tariffs. Hence, a conventional tariff reduction channel—cheaper Chinese imported goods replacing US products—is not directly applicable to our empirical specification. Note that we do

[63]Here, import competition specifically refers to US firms' competition with Chinese firms that are unrelated to US firms' offshoring activities.

Table 8: Dirty Industries and Heterogeneity in Product-level Response of US Import Share from China, 1997 - 2017

	(1)	(2)	(3)
	Share of US Imports from China		
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	0.092^{**}	0.090^{**}	0.048
	(0.043)	(0.040)	(0.052)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Emissions of PM/Sales}_{i,97})$		0.074^{**}	
		(0.036)	
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Upstream}_{i,97}$			0.078
			(0.069)
Product FE	✓	✓	✓
Year FE	✓	✓	✓
Controls	✓	✓	✓
Observations	198716	170020	197905

Notes. This table investigates the heterogeneous treatment effects of the conferral of PNTR to China on product-level US import share from China, depending on (i) initial PM emission intensity and (ii) upstreamness. Observations are defined at HS 10-digit product-by-year level. The dependent variable is the share of imports from China to the US relative to total US imports. Column (1) considers the interaction of (i) post-PNTR indicator and (ii) NTR gap. Column (2) considers a triple interaction of (i) post-PNTR indicator, (ii) NTR gap, and (iii) log of initial PM_{10} emissions-to-sales ratio defined at the SIC 4-digit level, $\text{Log(PM Emissions/Sales}_{i,97})$. Column (3) considers a triple interaction of (i) post-PNTR indicator, (ii) NTR gap, and (iii) upstreamness dummy as in Column (3) of Table 4. To facilitate coefficient interpretation, we standardized $\text{Log(PM Emissions/Sales}_{i,97})$ so that the sample mean equals zero and the sample standard deviation equals one. Columns (2)-(3) also include interactions of the column-specific initial characteristic with (i) post-PNTR indicator and (ii) NTR gap, respectively. Additionally, all columns include time-varying industry-by-year variables—NTR tariff rates ($\text{NTR}_{i,t}$), MFA exposure ($\text{MFA Exposure}_{i,t}$)—as well as interactions of the post-PNTR indicator with time-invariant controls including the industry-level log of 1995 skill and capital intensity ($\text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$ and $\text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$, respectively), changes in Chinese import tariffs from 1996 to 2005 ($\Delta\text{Chinese Tariff}_i$), and changes in Chinese production subsidies per total sales from 1999 to 2005 ($\Delta\text{Chinese Subsidies}_i$). The sample period is from 1997 to 2017. Standard errors (in parentheses) are clustered at the SIC 4-digit industry level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

control for the NTR tariff rate in Equation (5.2). Admittedly, the reduction in uncertainty may induce Chinese firms to expand into the US market.[64] If the surge in imports from China operated as a competition mechanism, we would observe a reduction of production scale or establishment exits, which may reduce toxic emissions. However, our empirical results do not indicate the competition channel (see Figure 6, Table 3, and Figure A.9).

The offshoring channel, on the other hand, is strongly supported by the direct evidence shown in this section together with all other pieces of evidence presented in previous sections of the paper. Specifically, pollution emission reductions are more pronounced for establishments in upstream industries (see Table 4). This implies that the US may have sourced intermediate inputs, relative to final goods, from China, which resulted in the cleanup of US upstream industries relatively more. On top of that, the emission reductions along with sourcing/FDI activities are more substantial for establishments that already had foreign sourcing networks including China. As our analysis

[64] This channel is one of four potential mechanisms described in Pierce and Schott (2016).

is focused on manufacturing, plant-level sourcing activities indicate offshoring (not competition), thereby implying that US plants conduct more offshoring activities after PNTR. Furthermore, we observe a weak, but statistically insignificant, increase in sales after PNTR among establishments with positive emissions (see Figure A.9). This finding is again inconsistent with the competition channel but instead supports the productivity effect of offshoring—the cost-saving nature of offshoring allows firms to boost their productivity and increase sales (Grossman and Rossi-Hansberg, 2008).

From a measurement perspective, one may still argue that the NTR gap could capture both output and input uncertainties within-establishment under a rather dichotomous assumption that output uncertainty captures the competition channel and input uncertainty reflects the offshoring channel. While plausible, we note that constructing separate within-establishment measures of output and input uncertainties is infeasible in our study as plant-level data on input purchases are not accessible. Typically, researchers have used an input-output table to infer plant-level production and sourcing structure, which necessarily generates measurement errors. Despite the limitation, as a robustness check, following Pierce and Schott (2016), we construct industry-level upstream- and downstream-specific NTR gap measures, respectively, and repeat the analysis. Reassuringly, in Appendix Table B.12, the main estimate of our interest, the first row, is still negative and statistically significant. Moreover, Bernard et al. (2020) finds that, after offshoring to low-wage countries, firms increase their imports of the same detailed six-digit HS goods that they continue to produce domestically. Offshoring firms then reallocate their tasks toward producing high-quality varieties within the same product category. This pattern contradicts the common assumption that offshoring necessarily entails imports of inputs.[65]

The remaining question is then how the cleanup of the US manufacturing has been possibly achieved via offshoring within the same 4-digit SIC industry. Similar to the "produced-good imports" and reallocation toward high-quality varieties through offshoring shown in Bernard et al. (2020), Hombert and Matray (2018) find that the China trade shock induced US firms, especially R&D-intensive firms, to increase product differentiation, and climb up the quality ladder. Just like their arguments, because PNTR facilitated offshoring (or FDI) activities to China, multi-stage (or multi-product) establishments, mostly producing differentiated steel or chemical products (see Figure A.2), may have differentiated their products (or production stages) by substituting high-polluting activities with low-polluting ones within establishments. Given that we find stronger responses for establishments located in upstream industries and owned by multisector firms (see Table 4), it is possible that these establishments are intermediate goods producers that shifted their tasks from dirty to clean activities and imported dirty products from China.

[65]Note that this is perfectly compatible with our findings that emphasize the role of establishments operating in upstream industries in general input-output structure (Table 4). It implies that even for establishments operating in upstream sectors, they tend to import back products sharing the same category as their own outputs instead of inputs.

Discussion: Pollution Offshoring and China We also emphasize that our findings do not necessarily imply that the level of pollution in China increased due to US manufacturers' offshoring activities. For example, it is possible that high-polluting tasks that the US offshored to China are still less pollution-intensive compared to those in local Chinese firms. In this case, the toxic emissions in both countries may as well decline.[66] Also, Chinese exporters may adopt environment-friendly technologies to comply with international environmental standards. In fact, there is mixed documentation on whether the expansion of Chinese exports resulted in higher pollution in China. For example, Bombardini and Li (2020) show that the rapid expansion of Chinese exports between 1990 and 2010 caused increases in local pollution and mortality in China, whereas Rodrigue et al. (2022) find that Chinese exporters are significantly less emission-intensive compared to non-exporters and that Chinese firms' exporting reduces their emissions. Our results suggest that the conferral of PNTR to China—followed by the offshoring of high-polluting tasks to China—resulted in increased reliance on imports from China, especially for products that are produced by dirty industries according to US standards.

7.2 Other Mechanisms: Clean Technology Adoption

We now examine the importance of the technology channel in understanding the PNTR-led reductions in PM_{10} emissions. If US manufacturers adopted clean technologies in response to PNTR, then the observed decline would reflect trade-induced advances in production or abatement processes rather than offshoring activities. Levinson (2009) finds that the majority of the pollution emission reductions in the US from 1987 to 2001 were attributable to technology adoption. Consequently, technology adoption is indeed crucial for understanding the emission reductions in the US and furthermore may be confounded with the offshoring channel.

To test for this possibility, we estimate Equation (5.2) using establishment-level pollution prevention (P2) activities—covering any practice that "reduces, eliminates, or prevents pollution at its source before it is created"—to construct outcome variables. Specifically, among the four broad categories of pollution prevention (P2) activities—*(i) material substitutions and modifications*; *(ii) product modifications, process and equipment modifications*; *(iii) inventory and material management*; and *(iv) operating practices and training*, we focus on *(i)* and *(ii)* to proxy for clean technology adoption. Appendix Table B.20 presents the estimated results. Column (1) uses an indicator variable for whether any clean-technology-related P2 activity is reported in a given year, and column (2)

[66] This idea is reminiscent of Feenstra and Hanson (1996) where outsourcing by Northern multinationals toward the South leads to an increase in South's capital stock relative to that in the North, which may increase the relative wage of skilled labor in both North and South simultaneously. In their model, the manufacturing activities outsourced to the South are ones that rely more on unskilled labor from the North's perspective, but are ones that rely more on skilled labor from the South's perspective, so that offshoring leads both countries to experience an increase in skilled labor premium. Likewise, it may be the case that the activities outsourced to the South are ones that rely more on dirty processes from the North's perspective, but are ones that rely more on clean processes from the South's perspective.

considers the number of chemicals that are associated with these P2 activities.[67] We find that neither the extensive nor intensive margin of pollution prevention (P2) activities respond to PNTR. That is, the trade liberalization associated with the conferral of PNTR have had a limited role in inducing clean technology adoption in the post-2000s period.

It is important to note that this result does not necessarily contradict the existing literature that highlights the role of technology (i.e., the *technique effect* shown in Section 4) in explaining the cleaning-up of manufacturing. In particular, the null findings could be attributed to a situation in which trade incentivizes establishments to adopt clean technologies, which to some extent serve as substitutes to offshoring, similar to the way that general process innovation—intended to reduce production costs—and offshoring were identified as substitutes in Bena and Simintzi (2022). Since clean technology adoption can be considered a form of cost-reducing process innovation when environmental regulations are present, the substitution channel might apply to our environmental context to neutralize any potential positive impact of PNTR on clean technology adoption.

8 Conclusion

Using the conferral of PNTR status to China as a quasi-natural experiment, we investigate the long-run environmental impacts of trade liberalization and provide support for the pollution offshoring hypothesis in US manufacturing. Data from TRI and NETS give us a unique longitudinal perspective over nearly two decades of the post-2000 period to observe how US manufacturers adjust pollution emissions to a reduction in trade policy uncertainty. The main driver of the reductions in pollution emissions was the abatement within an establishment, primarily a reduction in emission intensity. US establishment that were more able and willing to offshore production to China—in terms of (i) having existing foreign business relationships, (ii) moving away from stricter environmental regulations, (iii) operating in more upstream industries, and (iv) belonging to a multi-sector firm—indeed reduced pollution emissions more. We provide direct evidence that supports the pollution offshoring hypothesis. Specifically, we show that US manufacturers, especially those that emit pollutants more intensely, begin to source from abroad and establish more subsidiaries in China after PNTR.

The finding that reduced trade barriers induce US manufacturers to engage in offshoring activities implies that the extent to which differential environmental regulation between developed and developing countries creates a pollution haven depends on additional economic factors. In other words, US manufacturers take many other business environments into consideration when they decide to locate production facilities abroad. In our context, those were trade policy uncertainties that US firms would have faced had they decided to invest in China. More broadly, it could be institutional barriers that impede FDI, especially in developing countries. Thus, our work highlights the importance of nontrivial interactions among trade policy uncertainty, environmental regulations,

[67] Appendix Table B.21 shows that the results are similar using overall P2-related activity as a dependent variable.

and offshoring in teasing out the pollution haven or offshoring hypothesis.

While our work exploits a specific trade liberalization episode between the US and China, it could have broader implications for jointly explaining two salient global patterns since the late 20th century, namely, (i) the divergent paths of pollution emissions between developed and developing countries and (ii) offshoring production tasks from developed to developing countries. Our results indicate that these two global trends could be interpreted as a cause-and-effect relationship such that multinationals in developed countries relocated high-pollution production activities to low-income destination countries across the globe.

Also, our work implies that in devising a trade policy, the environmental impact should not be considered lightly given the recent studies that report significant adverse impacts of pollution on health and productivity (Chang et al., 2016; Deryugina et al., 2019). Despite the importance of adverse environmental consequences, however, the China trade shock literature has largely overlooked its impact on environmental outcomes in the US, let alone on the global distribution of pollution. This paper moves one step further in this direction and sheds light on the pros and cons of the effects of the China trade shock and, more broadly, trade liberalization. Further work along this line will deepen our understanding of the nexus between trade and the environment.

Lastly, our work has implications for the EU's Carbon Border Adjustment Mechanism (CBAM) which would mandate that imported goods face tariffs and requirements similar to those that would be applied if produced domestically in the EU. CBAM aims to prevent "carbon leakage" when EU-based firms shift carbon-intensive production abroad to countries with less stringent environmental policies. Since CBAM reduces the differential environmental regulation between developed and developing countries, we expect that it would counteract the offshoring of dirty production tasks from developed to developing countries. From this perspective, it may alleviate concerns that offshoring exacerbates environmental outcomes in developing countries. However, from another point of view, if multinationals in developed countries shift dirty tasks abroad and at the same time those tasks are still cleaner than the ones performed by local firms in developing countries, CBAM may hamper offshoring activities along with technology transfer, which is a key ingredient to economic growth in developing countries. Thus, our study lays out a more nuanced picture of the pros and cons of a currently debated topic on the US Carbon Border Adjustment proposals.

References

Aizenman, Joshua and Nancy Marion, "The Merits of Horizontal Versus Vertical FDI in the Presence of Uncertainty," *Journal of International Economics*, 2004, *62* (1).

Akerman, Anders, Rikard Forslid, and Ossian Prane, "Imports and the CO2 Emissions of Firms," *CEPR Discussion Paper No. DP16090*, 2021.

Antras, Pol, Davin Chor, Thibault Fally, and Russell Hillberry, "Measuring the Upstreamness of Production and Trade Flows," *American Economic Review: Papers & Proceedings*, 2012, *102* (3), 412–416.

Asquith, Brian, Sanjana Goswami, David Neumark, and Antonio Rodriguez-Lopez, "U.S. Job Flows and the China Shock," *Journal of International Economics*, 2019, *118*, 123–137.

Autor, David, David Dorn, and Gordon H Hanson, "The China Syndrome: Local Labor Market Effects of Import Competition in the United States," *American Economic Review*, 2013, *103* (6), 2121–68.

_ , _ , _ , Gary Pisano, and Pian Shu, "Foreign Competition and Domestic Innovation: Evidence from US Patents," *American Economic Review: Insights*, 2020, *2* (3), 357–74.

_ , _ , Gordon Hanson, and Kaveh Majlesi, "Importing Political Polarization? The Electoral Consequences of Rising Trade Exposure," *American Economic Review*, 2020, *110* (10), 3139–83.

Barnatchez, Keith, Leland Dod Crane, and Ryan Decker, "An Assessment of the National Establishment Time Series (NETS) Database," Technical Report 2017.

Barrows, Geoffrey and Hélène Ollivier, "Foreign demand, developing country exports, and CO2 emissions: Firm-level evidence from India," *Journal of Development Economics*, 2021, *149*, 102587.

Bartram, Söhnke M, Kewei Hou, and Sehoon Kim, "Real Effects of Climate Policy: Financial Constraints and Spillovers," *Journal of Financial Economics*, 2022, *143* (2), 668–696.

Behrens, Kristian, Brahim Boualam, Julien Martin, and Florian Mayner, "Gentrification and Pioneer Businesses," *accepted at Review of Economics and Statistics*, 2022.

Bena, Jan and Elena Simintzi, "Machines Could Not Compete with Chinese Labor: Evidence from US Firms' Innovation," *Available at SSRN 2613248*, 2022.

Bernard, Andrew B, Teresa Fort, Valerie Smeets, and Frederic Warzynski, "Heterogeneous Globalization: Offshoring and Reorganization," *NBER Working Paper #26854*, 2020.

Bloom, Nicholas, Mirko Draca, and John Van Reenen, "Trade Induced Technical Change? The Impact of Chinese Imports on Innovation, IT and Productivity," *Review of Economic Studies*, 2016, *83* (1), 87–117.

Bloom, Nick, Stephen Bond, and John Van Reenen, "Uncertainty and Investment Dynamics," *Review of Economic Studies*, 2007, *74* (2).

Bombardini, Matilde and Bingjing Li, "Trade, Pollution and Mortality in China," *Journal of International Economics*, 2020, *125*, 103321.

Burchardi, Konrad B, Thomas Chaney, and Tarek A Hassan, "Migrants, ancestors, and foreign investments," *Review of Economic Studies*, 2019, *86* (4), 1448–1486.

Caliendo, Lorenzo and Fernando Parro, "Trade Policy," *NBER Working Paper #29051*, 2021.

Chang, Tom, Joshua Graff Zivin, Tal Gross, and Matthew Neidell, "Particulate Pollution and the Productivity of Pear Packers," *American Economic Journal: Economic Policy*, 2016, *8* (3), 141–69.

Che, Yi, Yi Lu, Justin R Pierce, Peter K Schott, and Zhigang Tao, "Does Trade Liberalization with China Influence US Elections?," *NBER Working Paper #22178*, 2016.

Cherniwchan, Jevan, "Trade Liberalization and the Environment: Evidence from NAFTA and US Manufacturing," *Journal of International Economics*, 2017, *105*, 130–149.

_ , Brian R Copeland, and M Scott Taylor, "Trade and the Environment: New Methods, Measurements, and Results," *Annual Review of Economics*, 2017, *9*, 59–85.

Choi, Jaerim and Mingzhi Xu, "The Labor Market Effects of the China Syndrome: Evidence from South Korean Manufacturing," *World Economy*, 2020, *43* (11), 3039–3087.

_ , Jay Hyun, and Ziho Park, "Bound by Ancestors: Immigration, Credit Frictions, and Global Supply Chain Formation," Working Paper 2022.

_ , Seongin Hong, Jung Hur, and Manho Kang, "Product Scope Adjustment to the China Shock: Competition at Home and Abroad," *Available at SSRN 4289976*, 2022.

Chung, Sunghoon, "Environmental Regulation and Foreign Direct Investment: Evidence from South Korea," *Journal of Development Economics*, 2014, *108*, 222–236.

Cole, Matthew A, Robert JR Elliott, and Toshihiro Okubo, "International Environmental Outsourcing," *Review of World Economics*, 2014, *150* (4), 639–664.

Copeland, Brian R and M Scott Taylor, "North-South Trade and the Environment," *Quarterly Journal of Economics*, 1994, *109* (3), 755–787.

Copeland, Brian R. and Scott M. Taylor, "Trade, Growth, and the Environment," *Journal of Economic Literature*, 2004, *42* (1).

Copeland, Brian R, Joseph S Shapiro, and M Scott Taylor, "Globalization and the Environment," *Handbook of International Economics*, 2022, *5*, 61–146.

Deryugina, Tatyana, Garth Heutel, Nolan H Miller, David Molitor, and Julian Reif, "The Mortality and Medical Costs of Air Pollution: Evidence from Changes in Wind Direction," *American Economic Review*, 2019, *109* (12), 4178–4219.

Dix-Carneiro, Rafael and Brian K Kovak, "Trade Liberalization and Regional Dynamics," *American Economic Review*, 2017, *107* (10), 2908–46.

Dixit, Avinash and Robert Pindyck, *Investment under Uncertainty*, Princeton university press, 1994.

Dussaux, Damien, Francesco Vona, and Antoine Dechezleprêtre, "Imported carbon emissions: Evidence from French manufacturing companies," *Canadian Journal of Economics*, 2023.

Eskeland, Gunnar S and Ann E Harrison, "Moving to Greener Pastures? Multinationals and the Pollution Haven Hypothesis," *Journal of Development Economics*, 2003, *70* (1), 1–23.

Feenstra, Robert C, "Integration of Trade and Disintegration of Production in the Global Economy," *Journal of Economic Perspectives*, 1998, *12* (4), 31–50.

_ **and Gordon Hanson**, "Foreign Investment, Outsourcing and Relative Wages," *Political economy of trade policy: essays in honor of Jagdish Bhagwati*, 1996, pp. 89–127.

Feenstra, Robert C., John Romalis, and Peter K. Schott, "U.S. Imports, Exports, and Tariff Data, 1989-2001," *NBER Working Paper #9387*, 2002.

Gabaix, Xavier, "The Granular Origins of Aggregae Fluctuations," *Econometrica*, 2011, *79* (3), 733–772.

Garetto, Stefania, "Input Sourcing and Multinational Production," *American Economic Journal: Macroeconomics*, 2013, *5* (2), 118–51.

Gray, John V, Enno Siemsen, and Gurneeta Vasudeva, "Colocation Still Matters: Conformance Quality and the Interdependence of R&D and Manufacturing in the Pharmaceutical Industry," *Management Science*, 2015, *61* (11), 2760–2781.

Greenland, Andrew, John Lopresti, and Peter McHenry, "Import Competition and Internal Migration," *Review of Economics and Statistics*, 2019, *101* (1), 44–59.

Greenstone, Michael, "The Impacts of Environmental Regulations on Industrial Activity: Evidence from the 1970 and 1977 Clean Air Act Amendments and the Census of Manufactures," *Journal of Political Economy*, 2002, *110* (6), 1175–1219.

Grossman, Gene M and Alan B Krueger, "Economic Growth and the Environment," *Quarterly Journal of Economics*, 1995, *110* (2), 353–377.

_ **and Esteban Rossi-Hansberg**, "Trading tasks: A simple theory of offshoring," *American Economic Review*, 2008, *98* (5), 1978–1997.

Gutiérrez, Emilio and Kensuke Teshima, "Abatement Expenditures, Technology Choice, and Environmental Performance: Evidence from Firm Responses to Import Competition in Mexico," *Journal of Development Economics*, 2018, *133*, 264–274.

Hakobyan, Shushanik and John McLaren, "Looking for Local Labor Market Effects of NAFTA," *Review of Economics and Statistics*, 10 2016, *98* (4), 728–741.

Haltiwanger, John, Ron s. Jarmin, and Javier Miranda, "Who Creates Jobs? Small versus Large Versus Young," *Review of Economics and Statistics*, 2013, *95* (2), 347–361.

Handley, Kyle and Nuno Limao, "Trade and Investment under Policy Uncertainty: Theory and Firm Evidence," *American Economic Journal: Economic Policy*, 2015, *7* (4), 189–222.

_ **and Nuno Limão**, "Policy Uncertainty, Trade, and Welfare: Theory and Evidence for China and the United States," *American Economic Review*, 2017, *107* (9), 2731–83.

_ **and** _ , "Trade Policy Uncertainty," *forthcoming in Annual Review of Economics*, 2021.

Hanna, Rema, "US Environmental Regulation and FDI: Evidence from a Panel of US-Based Multinational Firms," *American Economic Journal: Applied Economics*, 2010, *2* (3), 158–89.

Hanson, Gordon H, Raymond J Mataloni Jr, and Matthew J Slaughter, "Vertical Production Networks in Multinational Firms," *Review of Economics and statistics*, 2005, *87* (4), 664–678.

Holladay, J Scott, "Exporters and the Environment," *Canadian Journal of Economics*, 2016, *49*

(1), 147–172.

— **and Lawrence D LaPlue III**, "Decomposing Changes in Establishment-Level Emissions with Entry and Exit," *Canadian Journal of Economics*, 2021, *54* (3), 1046–1071.

Hombert, Johan and Adrien Matray, "Can innovation help US manufacturing firms escape import competition from China?," *Journal of Finance*, 2018, *73* (5), 2003–2039.

Hummels, David, Jun Ishii, and Kei-Mu Yi, "The Nature and Growth of Vertical Specialization in World Trade," *Journal of International Economics*, 2001, *54* (1), 75–96.

— **, Rasmus Jørgensen, Jakob Munch, and Chong Xiang**, "The Wage Effects of Offshoring: Evidence from Danish Matched Worker-Firm Data," *American Economic Review*, June 2014, *104* (6), 1597–1629.

Hyun, Jay and Ryan Kim, "Spillovers through Multimarket Firms: The Uniform Product Replacement Channel," 2022. working paper.

— **, Ziho Park, and Vladimir Smirnyagin**, "Shock Propagation within Multisector Firms," *Available at SSRN 3882794*, 2022.

Kahn, Matthew E., "The Geography of US Pollution Intensive Trade: Evidence from 1958 to 1994," *Regional Science and Urban Economics*, 2003, *33* (4), 383–400.

Khanna, Neha, "A Narrative on Two Weaknesses of the TRI for Research Purposes," *Working Paper*, 2019.

Kim, Gueyon, "Trade-Induced Adoption of New Work," Technical Report, Human Capital and Economic Opportunity Working Group 2022.

Kim, Ki-Hyun, Ehsanul Kabir, and Shamin Kabir, "A Review on the Human Health Impact of Airborne Particulate Matter," *Environment International*, 2015, *74*, 136–143.

Leisner, Jonathan, Jakob R. Munch, August Twile Nielsen, and Georg Schaur, "The Impact of Offshoring and Import Competition on Firm-level Carbon Emissions," *Working Paper*, 2023.

Levinson, Arik, "Technology, International trade, and Pollution from US Manufacturing," *American Economic Review*, 2009, *99* (5), 2177–92.

— **and M Scott Taylor**, "Unmasking the Pollution Haven Effect," *International Economic Review*, 2008, *49* (1), 223–254.

List, John A, Daniel L Millimet, Per G Fredriksson, and W Warren McHone, "Effects of Environmental Regulations on Manufacturing Plant Births: Evidence from a Propensity Score Matching Estimator," *Review of Economics and Statistics*, 2003, *85* (4), 944–952.

Martin, Julien, Isabelle Mejean, and Mathieu Parenti, "Relationship Stickiness, International Trade, and Economic Uncertainty," *Working Paper*, 2021.

Martin, Leslie A, "Energy Efficiency Gains from Trade: Greenhouse Gas Emissions and India's Manufacturing Sector," *Mimeograph, Berkeley ARE*, 2011.

Melitz, Marc J. and Saso Polanec, "Dynamic Olley-Pakes Productivity Decomposition with Entry and Exit," *RAND Journal of Economics*, 2015, *46* (2), 362–375.

Monarch, Ryan, Jooyoun Park, and Jagadeesh Sivadasan, "Domestic gains from offshoring? Evidence from TAA-linked U.S. microdata," *Journal of International Economics*, 2017, *105*, 150–173.

Najjar, Nouri and Jevan Cherniwchan, "Environmental Regulations and the Cleanup of Manufacturing: Plant-Level Evidence," *Review of Economics and Statistics*, 07 2021, *103* (3), 476–491.

Neumark, David, Brandon Wall, and Junfu Zhang, "Do Small Business Create More Jobs? New Evidence for the United States from the National Establishment Time Series," *Review of Economics and Statistics*, 2011, *93* (1), 16–29.

_ **, Junfu Zhang, and Brandon Wall**, "Where the Jobs Are: Business Dynamics and Employment Growth," *Academy of Management Perspectives*, 2006, *20* (4), 79–94.

Oberfield, Ezra, Esteban Rossi-Hansberg, Pierre-Daniel Sarte, and Nicholas Trachter, "Plants in Space," *Mimeo*, 2022.

Olley, G. Steven and Ariel Pakes, "The Dynamics of Productivity in the Telecommunications Equipment Industry," *Econometrica*, 1996, *64* (6), 1263–1297.

Pierce, Justin R and Peter K Schott, "A Concordance Between Ten-Digit U.S. Harmonized System Codes and SIC/NAICS Product Classes and Industries," *NBER Working Paper #15548*, 2009.

_ **and** _ , "The Surprisingly Swift Decline of US Manufacturing Employment," *American Economic Review*, 2016, *106* (7), 1632–62.

_ **and** _ , "Trade Liberalization and Mortality: Evidence from US Counties," *American Economic Review: Insights*, 2020, *2* (1), 47–64.

Rodrigue, Joel, Dan Sheng, and Yong Tan, "Exporting, Abatement, and Firm-Level Emissions: Evidence from China's Accession to the WTO," *Review of Economics and Statistics*, 2022, pp. 1–45.

Rossi-Hansberg, Esteban, Pierre-Daniel Sarte, and Nicholas Trachter, "Diverging Trends in National and Local Concentration," *NBER Macroeconomics Annual*, 2021, *35* (1), 115–150.

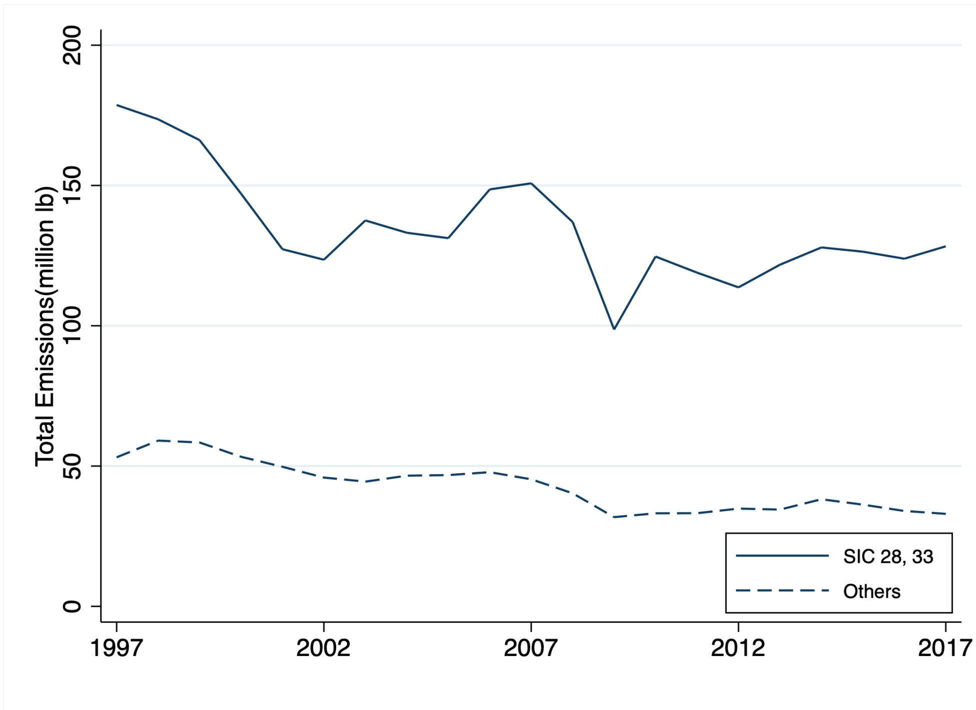
Shapiro, Joseph S and Reed Walker, "Why is Pollution from US Manufacturing Declining? The Roles of Environmental Regulation, Productivity, and Trade," *American Economic Review*, 2018, *108* (12), 3814–54.

Tanaka, Shinsuke, Kensuke Teshima, and Eric Verhoogen, "North-South Displacement Effects of Environmental Regulation: The Case of Battery Recycling," *American Economic Review: Insights*, 2022, *4* (3), 271–88.

Tintelnot, Felix, "Global Production with Export Platforms," *Quarterly Journal of Economics*, 2017, *132* (1), 157–209.

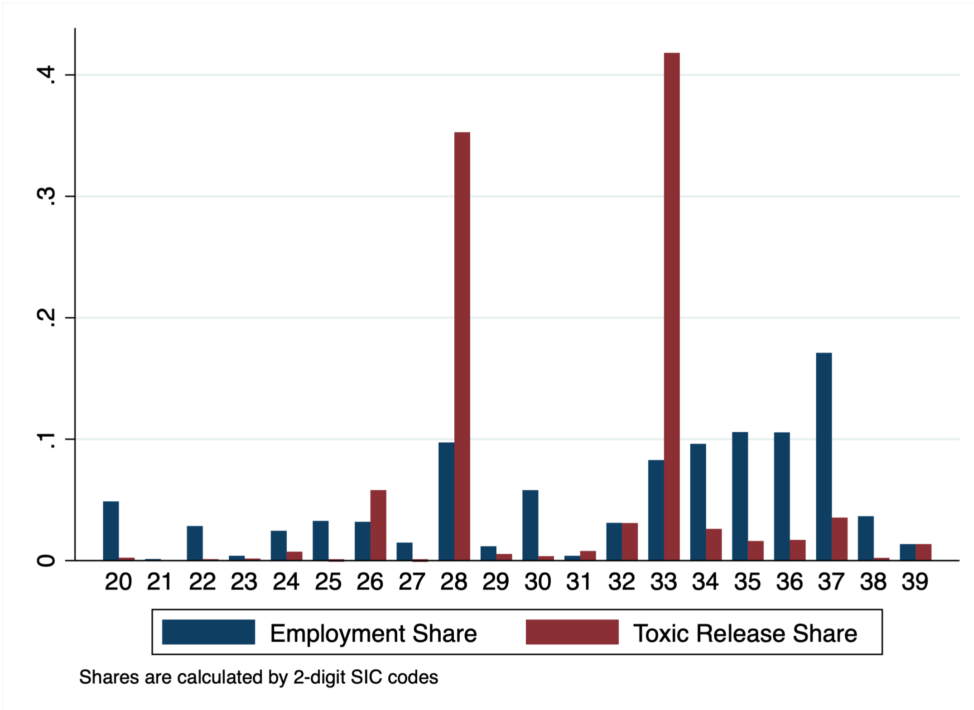
Appendix A Additional Figures

Figure A.1: PM_{10} Emissions Trends: 2-digit-SIC 28, 33 versus Other Industries



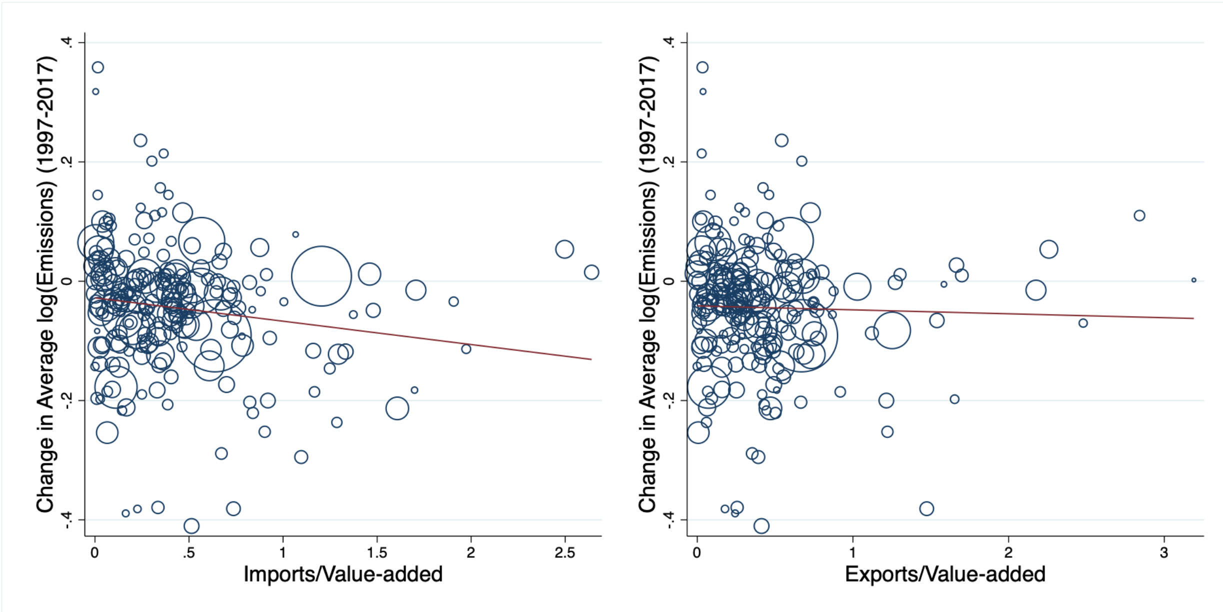
Notes: This figure displays PM_{10} emissions trends for (i) 2-digit-SIC 28 and 33 and (ii) all other industries for 1997-2017.

Figure A.2: Employment and PM_{10} Emissions Shares by 2-digit-SIC Industry



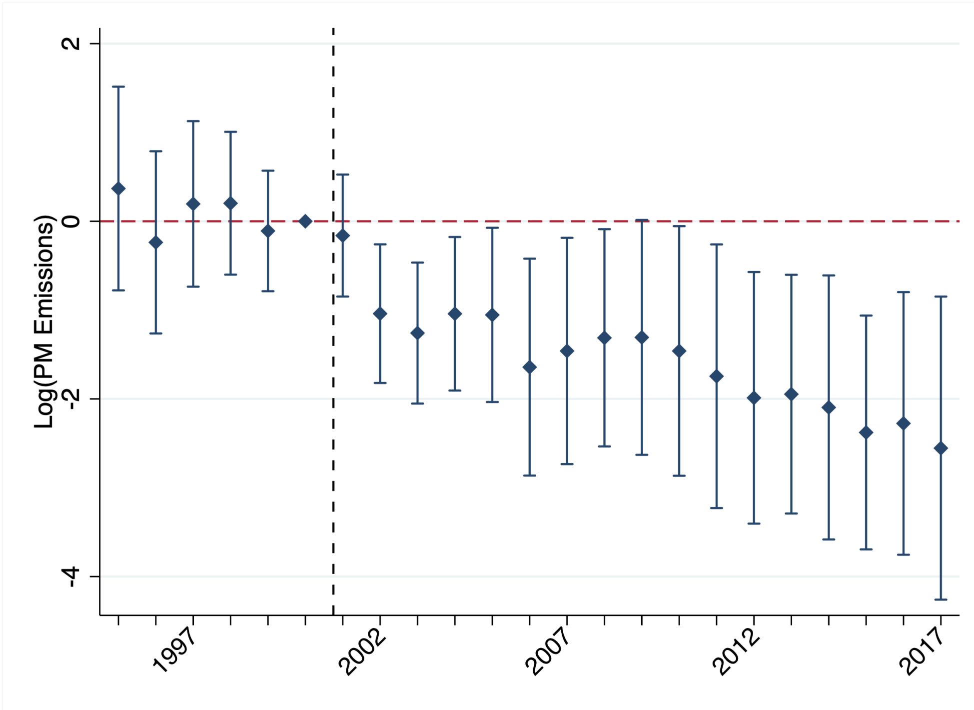
Notes: This figure displays employment (navy bars) and PM_{10} emissions (red bars) shares in 1997 by 2-digit-SIC industry. SIC 2-digit 28 indicates "Chemicals and Allied Products"; SIC 2-digit 33 indicates "Primary Metal Industries".

Figure A.3: Correlations between Changes in Average PM_{10} Emissions and Initial Industry Trade Intensity



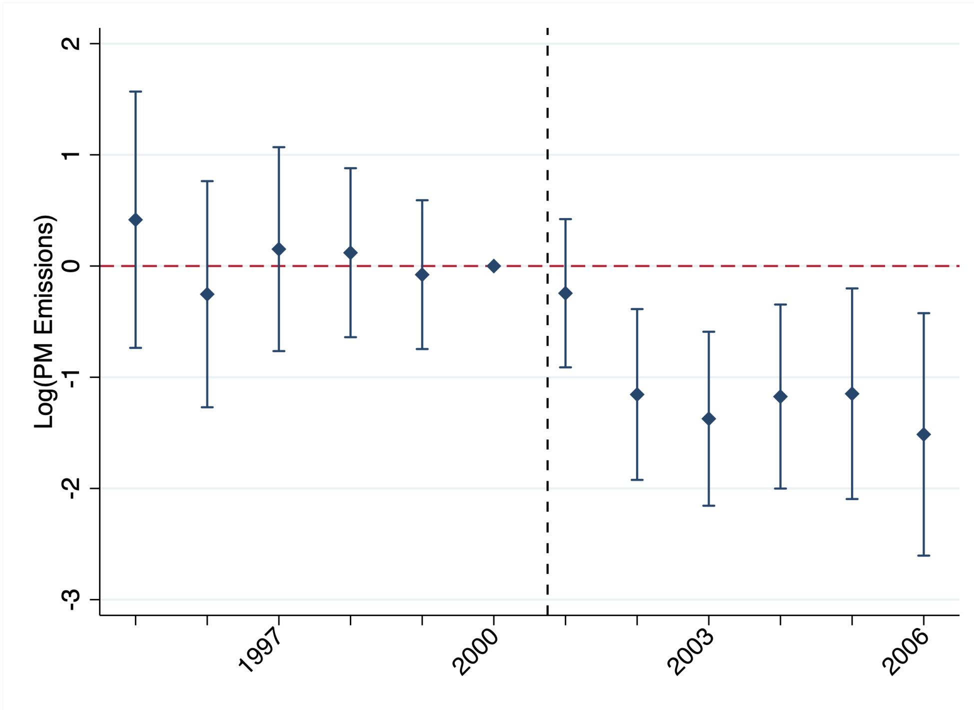
Notes: The graph on the left (right) illustrates the correlations between the industry-level averages of changes in the within-establishment log(emissions) of PM_{10} from 1997 to 2017 and the industry-level import (export) intensity constructed using the value of imports (exports) relative to value-added in 1997. The sizes of the circles are proportional to the industry-level log(employment) in 1997.

Figure A.4: Robustness: Dynamic Treatment Effects at the Establishment Level, 1995-2017



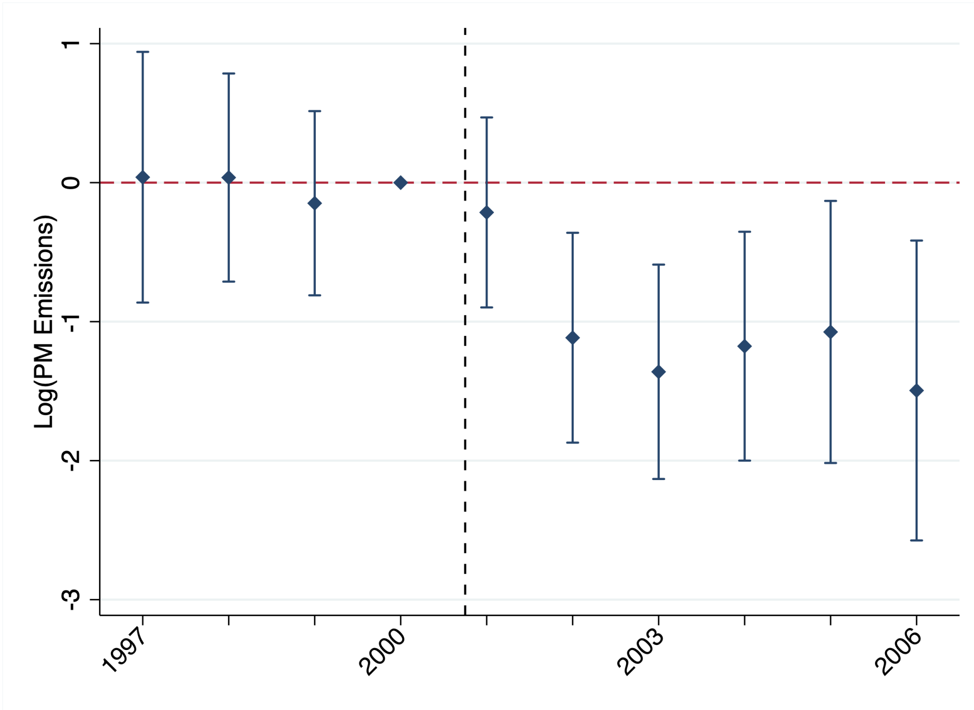
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1995 to 2017. All other specifications are identical to those in Equation (5.3).

Figure A.5: Robustness: Dynamic Treatment Effects at the Establishment Level, 1995-2006



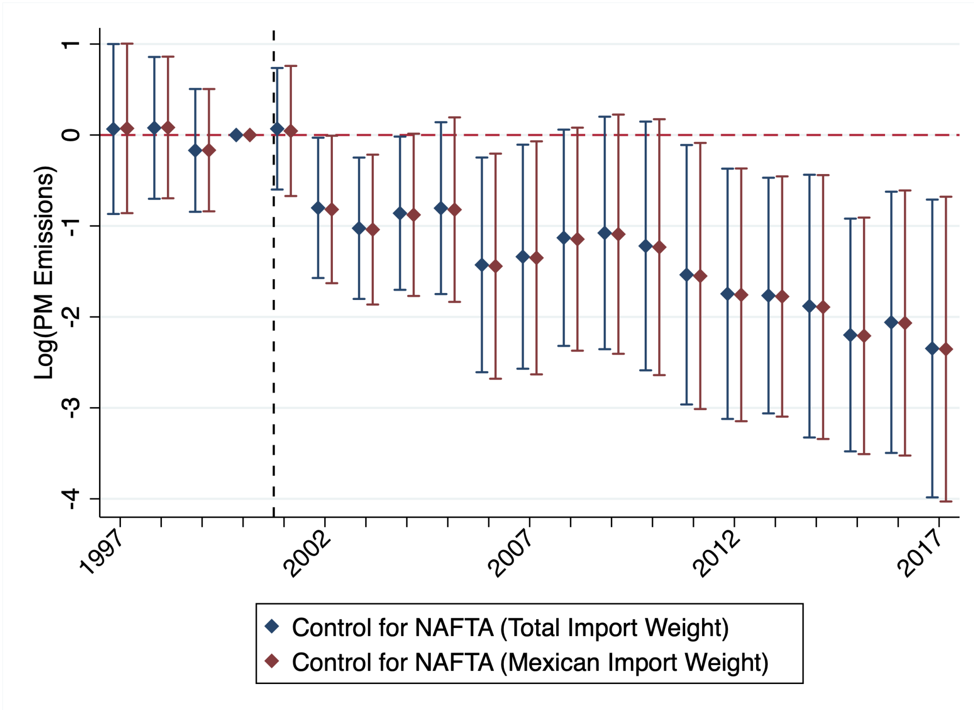
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider the sample period from 1995 to 2006. All other specifications are identical to those in Equation (5.3).

Figure A.6: Robustness: Dynamic Treatment Effects at the Establishment Level, 1997-2006



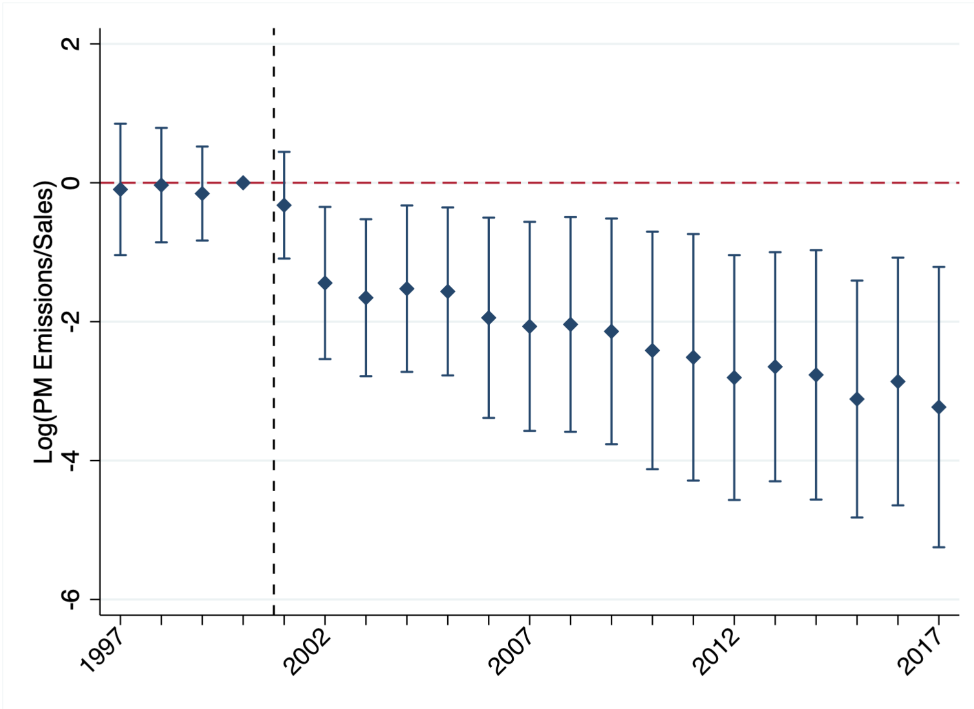
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1997 to 2006. All other specifications are identical to those in Equation (5.3).

Figure A.7: Controlling for NAFTA: Dynamic Treatment Effects at the Establishment Level



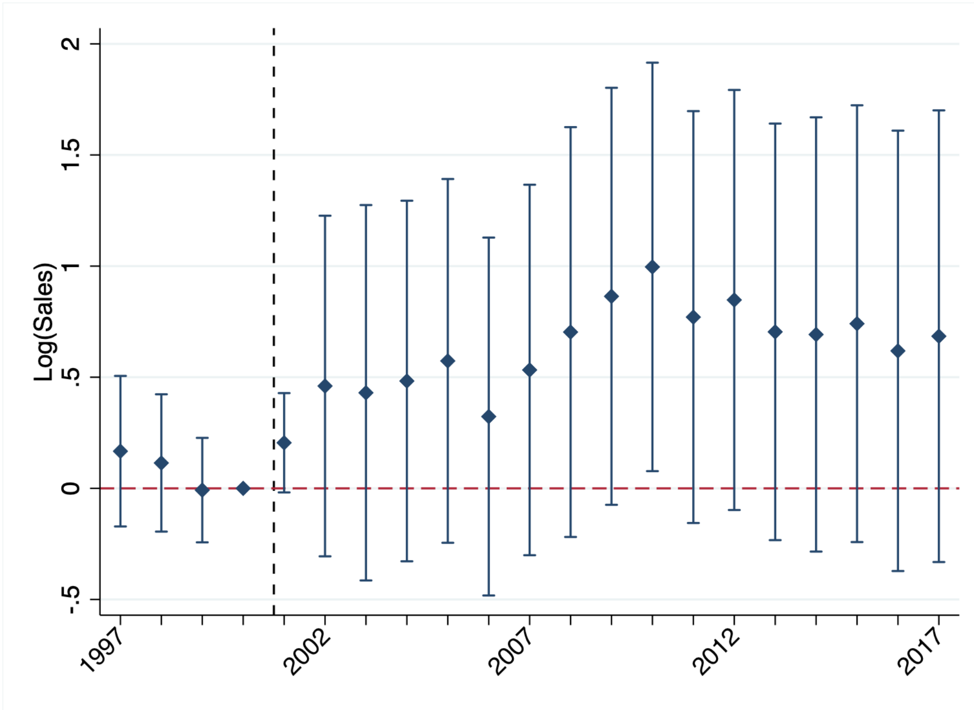
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we additionally control for the interaction of the post-PNTR indicator and the industry-level NAFTA tariff changes. Blue dots use US total imports as trade value weights in measuring industry-level NAFTA tariffs; red dots use US imports from Mexico as trade value weights. All other specifications are identical to those in Equation (5.3).

Figure A.8: Dynamic Treatment Effects of Pollution Emission Intensity at the Establishment Level



Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we use a measure of establishment-year-level pollution emission intensity—measured by log of PM_{10} emissions-to-sales ratio—as a dependent variable. All other specifications are identical to those in Equation (5.3).

Figure A.9: Dynamic Treatment Effects of Sales at the Establishment Level



Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we use a measure of establishment-year-level sales—measured by log of sales—as a dependent variable. All other specifications are identical to those in Equation (5.3).

Appendix B Additional Tables

Table B.1: Important Changes to TRI Program over Time

Time	Changes
Dec 1993	21 Chemicals and 2 Chemical Categories added
Nov 1994	286 Chemicals added
May 1997	Seven Industry Sectors (metal and coal mining facilities, electric power generators, commercial hazardous waste treatment operations, solvent recovery facilities, petroleum bulk terminals, and wholesale chemical distributors) added
Oct 1999	7 PBT Chemicals and 2 chemical categories added
Jan 2001	Lead and Lead Compounds designated as PBT chemicals
Dec 2006	TRI Burden Reduction Rule allowed the expansion of eligibility for using Form A
May 2007	TRI Dioxin Toxic Equivalency Rule
April 2009	Omnibus Appropriations Act restored the TRI reporting requirements that were effective before 2006
Nov 2010	National Toxicology Program Chemicals added
April 2012	Increasing Tribal Participation in the TRI Program
Nov 2015	1-Bromopropane added
Nov 2016	Hexabromocyclododecane (HBCD) Category added

Notes: The table mainly lists institutional changes that are relevant to our analysis. See the following link for a comprehensive list of changes to the TRI program: https://www.epa.gov/toxics-release-inventory-tri-program/history-toxics-release-inventory-tri-program

Table B.2: Top and Bottom 5 Industries in PM_{10} Emissions

Top 5 Industries in PM_{10} Emissions		Bottom 5 Industries in PM_{10} Emissions	
3313	Electrometallurgical Products, except Steel	2254	Knit Underwear and Nightwear Mills
3321	Gray and Ductile Iron Foundries	2591	Household Furniture, N.E.C.
2816	Inorganic Pigments	2047	Dog and Cat Food
2819	Industrial Inorganic Chemicals, N.E.C.	3489	Ordnance and Accessories, N.E.C.
3312	Steel Works, Blast Furnaces, and Rolling Mills	2043	Cereal Breakfast Foods

Notes: The table lists top and bottom five industries in PM_{10} emissions in 1997. Each industry title is preceded by the corresponding 4-digit-SIC code

Table B.3: Additional Summary Statistics

(A) Industry-Year Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
NTR $\text{Gap}_{i,99}$	5008	0.319	0.131	0.138	0.336	0.450
$\text{NTR}_{i,t}$	5008	2.457	2.658	0.000	2.122	5.067
MFA $\text{Exposure}_{i,t}$	5008	0.432	3.349	0.000	0.000	0.000

(B) Industry Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
NTR $\text{Gap}_{i,99}$	287	0.329	0.142	0.135	0.339	0.473
$\text{NP}_{i,95}/\text{Emp}_{i,95}$	287	0.295	0.115	0.173	0.266	0.452
$\text{K}_{i,95}/\text{Emp}_{i,95}$	287	94	102	27	60	218
ΔChinese Tariff_i	287	-0.122	0.105	-0.264	-0.092	-0.020
ΔChinese Subsidies_i	287	-0.000	0.002	-0.002	-0.000	0.001

(C) Firm Level: A Total of 3666 Unbalanced Firms						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
Import Intensity $(\text{Unconditional})_{f,97}$	2294	0.096	0.211	0.000	0.000	0.346
Import $\text{Intensity}_{f,97}$	703	0.289	0.275	0.029	0.200	0.762
Export Intensity $(\text{Unconditional})_{f,97}$	2294	0.337	0.387	0.000	0.144	1.000
Export $\text{Intensity}_{f,97}$	1485	0.501	0.374	0.049	0.422	1.000
Firm $\text{Employment}_{f,97}$	2294	5566	70366	40	388	8636
Num. $\text{Establishment}_{f,97}$	2294	50	407	1	4	84
Num. 4-digit $\text{Sectors}_{f,97}$	2294	9	17	1	2	24

(D) Establishment Level: A Total of 4946 Unbalanced Establishments						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
PM $\text{Emissions}_{p,97}$	3858	41262	472714	0	15	17422
PM $\text{Emissions}_{p,97}/\text{Sales}_{p,97}$ (lb/million dollar)	3858	2354.7	33172.9	0.0	0.6	577.9
I(Num. $\text{P2}_{p,95-97}$>0)	3858	0.260	0.439	0	0	1
I(Num. P2 $\text{Clean-Tech}_{p,95-97}$>0)	3858	0.130	0.336	0	0	1
Establishment $\text{Employment}_{p,97}$	3858	410	916	28	160	900
Establishment $\text{Sales}_{p,97}$	3858	91	245	4	25	189
$\text{Age}_{p,97}$	3858	55	42	9	50	109

(E) County Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
CAA $\text{Nonattainment}_{c,95-97}$	841	0.045	0.208	0	0	0

Notes. This table groups each variable based on its observation level and separately presents summary statistics by each group. Panel (A) presents summary statistics of industry-year-level variables; panel (B) presents summary statistics of industry-level variables; panel (C) presents summary statistics of firm-level variables; panel (D) presents summary statistics of establishment-level variables; panel (E) presents summary statistics of county-level variables. Subscripts t, p, f, i, and c indicate year, establishment, firm, SIC-4-digit industry, and county, respectively.

Table B.4: Summary Statistics: Compare Final Sample with NETS Manufacturing

(A) Establishment Level (1997)								
	1. Final Sample				2. NETS (Manufacturing)			
Variable	Obs.	Mean	Std. Dev.	P50	Obs.	Mean	Std. Dev.	P50
Establishment Employment$_{p,97}$	3858	410	916	160	748519	31	174	5
Establishment Sales$_{p,97}$	3858	91	245	25	748519	5	47	0.4
(B) Firm Level (1997)								
	1. Final Sample				2. NETS (Manufacturing)			
Variable	Obs.	Mean	Std. Dev.	P50	Obs.	Mean	Std. Dev.	P50
Import Intensity (Unconditional)$_{f,97}$	2294	0.096	0.211	0.000	649439	0.008	0.086	0.000
Import Intensity$_{f,97}$	703	0.289	0.275	0.200	8496	0.648	0.387	0.857
Export Intensity (Unconditional)$_{f,97}$	2294	0.337	0.387	0.144	649439	0.079	0.262	0.000
Export Intensity$_{f,97}$	1485	0.501	0.374	0.422	58484	0.874	0.261	1.000
Firm Employment$_{f,97}$	2294	5566	70366	388	649439	74	4551	5
Num. Establishment$_{f,97}$	2294	50	407	4	649439	2	47	1
Num. 4-digit Sectors$_{f,97}$	2294	9	17	2	649439	1	2	1

Notes. This table compares a snapshot of the 1997 distribution of establishment- and firm-level variables between the final sample (the NETS+TRI with positive emissions) and the original NETS data. We restrict establishments to those operating in manufacturing establishments (i.e., SIC-4-digit 2000-3999). Firm-level variables are calculated by including all establishments (i.e., manufacturing and non-manufacturing) within each firm that has at least one manufacturing establishment. Panel (A) presents summary statistics of establishment-level variables in 1997; panel (B) presents summary statistics of firm-level variables in 1997. Subscripts p and f indicate establishment and firm, respectively. P50 denotes 50th percentile (median).

Table B.5: SIC-2-digit 28, 33 versus Others:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)
	Log(PM Emissions)	
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-3.379**	-1.334***
	(1.397)	(0.441)
$\text{NTR}_{i,t}$	-0.099	-0.017
	(0.146)	(0.039)
MFA $\text{Exposure}_{i,t}$	0.198	-0.019
	(0.475)	(0.015)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.576***	0.116
	(0.191)	(0.150)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.161	0.010
	(0.204)	(0.066)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-3.547*	-0.448
	(1.891)	(0.557)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	45.575	-17.617
	(196.566)	(22.921)
Establishment FE	✓	✓
County x Year FE	✓	✓
Sample	SIC2: 28,33	SIC2: Others
Observations	9882	31414

Notes. This table repeats the specification in Column (4) of Table 2, where we run separate regressions for two sample groups. Column (1) considers establishments that operate in SIC-2-digit 28 or 33, whereas Column (2) considers the rest of the manufacturing establishments. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.6: PNTR and Establishment-level Pollution Emissions, 1997 - 2017: Other Chemicals - SO2 and VOC

	(1) Log(SO2 Emissions)	(2) Log(VOC Emissions)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.388	-0.151
	(0.580)	(0.375)
$\text{NTR}_{i,t}$	0.010	0.008
	(0.025)	(0.036)
$\text{MFA Exposure}_{i,t}$	0.009	0.012
	(0.028)	(0.026)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	-0.278	0.282^{**}
	(0.187)	(0.140)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	-0.061	0.087
	(0.113)	(0.061)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	1.990	0.681
	(1.221)	(0.595)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	46.444	-3.514
	(36.400)	(18.700)
Establishment FE	✓	✓
County x Year FE	✓	✓
Observations	10567	22036

Notes. This table repeats the specification in Columns (4) of Table 2, where we consider emissions of SO_2 and VOC, respectively, as dependent variables. Column (1) uses the log of establishment-year-level emissions of SO_2 and Column (2) considers the log of emissions of VOC. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.7: PNTR and Establishment-level Pollution Emissions, Alternative Sample Periods

	(1)	(2)	(3)	(4)
	Log(Emissions of PM)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.321***	-0.979***	-1.092***	-1.222***
	(0.375)	(0.339)	(0.343)	(0.382)
$\text{NTR}_{i,t}$	-0.012	-0.014	-0.017	-0.008
	(0.030)	(0.033)	(0.030)	(0.036)
$\text{MFA Exposure}_{i,t}$	-0.005	-0.005	-0.003	-0.009
	(0.016)	(0.011)	(0.011)	(0.016)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.314***	0.087	0.064	0.306***
	(0.110)	(0.121)	(0.116)	(0.114)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.043	0.027	0.023	0.043
	(0.058)	(0.042)	(0.048)	(0.052)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-0.629	-0.552	-0.436	-0.756*
	(0.476)	(0.428)	(0.449)	(0.457)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-37.084	-10.981	-11.668	-29.125
	(30.062)	(22.370)	(24.058)	(27.151)
Establishment FE	✓	✓	✓	✓
County x Year FE	✓	✓	✓	✓
Period	95-17	97-06	95-06	97-17 (drop 07-09)
Observations	51187	23071	27498	39913

Notes. This table repeats the specification in Column (4) of Table 2, where we consider alternative sample periods. Column (1) extends the pre-shock period and considers 1995-2017; Column (2) restricts the sample period after 2007 and considers 1997-2006, which allows us to exclude the Global Financial Crisis and the Great Trade Collapse period as well as the relaxation in reporting criteria during 2007 and 2009; Column (3) considers 1995-2006 as a robustness check; Column (4) considers 1997-2017, where we drop years corresponding to 2007, 2008, and 2009. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.8: Controlling for NAFTA:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)
	Log(PM Emissions)	
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.016***	-1.024***
	(0.356)	(0.379)
$\text{NTR}_{i,t}$	-0.027	-0.026
	(0.036)	(0.035)
MFA $\text{Exposure}_{i,t}$	-0.003	-0.005
	(0.016)	(0.016)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.235**	0.266**
	(0.115)	(0.116)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.080	0.073
	(0.055)	(0.057)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-0.995**	-0.883*
	(0.469)	(0.463)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-31.365	-31.691
	(27.075)	(27.074)
$\text{Post}_t \times \Delta\text{NAFTA Tariff}_i$ (Tot.Imp.Wt)	5.205**	
	(2.537)	
$\text{Post}_t \times \Delta\text{NAFTA Tariff}_i$ (MEX.Imp.Wt)		3.074
		(2.191)
Establishment FE	✓	✓
County x Year FE	✓	✓
Observations	46644	46644

Notes. This table repeats the specification in Column (4) of Table 2, where we additionally control for the interaction of the post-PNTR indicator and the industry-level NAFTA tariff changes. Column (1) uses US total imports as trade value weights in measuring industry-level NAFTA tariffs, and Column (2) uses US imports from Mexico as trade value weights. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.9: Excluding Establishment Entry and Exit:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)	(4)
	Log(PM Emissions)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.430***	-1.478***	-1.440***	-1.569***
	(0.442)	(0.487)	(0.491)	(0.520)
$\text{NTR}_{i,t}$			-0.012	0.003
			(0.041)	(0.044)
$\text{MFA Exposure}_{i,t}$			-0.017	-0.015
			(0.019)	(0.019)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				0.196
				(0.157)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$				0.070
				(0.067)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$				-0.342
				(0.574)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$				-49.783**
				(25.214)
Establishment FE	✓	✓	✓	✓
Year FE	✓	-	-	-
County x Year FE	-	✓	✓	✓
Observations	29049	29049	29049	29049

Notes. This table repeats the specifications in Columns (1)-(4) of Table 2, where we exclude establishments that entered or exited between 1997 and 2017. Therefore, the sample consists of establishments that existed throughout the sample period. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.10: Dropping Outliers:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)
	Log(PM Emissions)		
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.152***	-1.044***	-1.102***
	(0.371)	(0.400)	(0.401)
$\text{NTR}_{i,t}$	0.012	0.008	-0.008
	(0.033)	(0.036)	(0.036)
$\text{MFA Exposure}_{i,t}$	-0.010	-0.009	-0.006
	(0.014)	(0.017)	(0.017)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.222*	0.359***	0.294**
	(0.116)	(0.128)	(0.128)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.041	0.057	0.056
	(0.053)	(0.058)	(0.058)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-0.489	-0.705	-0.915
	(0.498)	(0.584)	(0.573)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-45.713*	-32.888	-34.000
	(26.913)	(27.369)	(26.585)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Drop Extreme	Emissions	Firm Size	Estab. Size
Observations	43925	44012	44260

Notes. This table repeats the specification in Column (4) of Table 2, where we drop outliers. Columns (1)-(3) drop the top and the bottom 2.5 percent of the distribution of (i) PM_{10} emissions, (ii) firm size, and (iii) establishment size, respectively. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.11: PNTR and Establishment-level Pollution Emissions, 1997 - 2017: Allowing Various Weighting Schemes

	(1)	(2)	(3)
	Log(PM Emissions)		Log(Toxic-Wt. PM)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-2.347***	-1.652***	-3.582**
	(0.558)	(0.589)	(1.566)
$\text{NTR}_{i,t}$	-0.047	-0.009	0.259**
	(0.063)	(0.064)	(0.105)
$\text{MFA Exposure}_{i,t}$	-0.054***	-0.012	-0.014
	(0.011)	(0.021)	(0.018)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.670**	0.232	0.049
	(0.328)	(0.172)	(0.324)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.180*	0.064	0.197
	(0.104)	(0.081)	(0.169)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-1.293	-0.836	2.170
	(1.135)	(0.534)	(2.025)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-99.705	-49.568	-134.935**
	(79.902)	(34.185)	(63.394)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Weights	Init. Release	Init. Employment	Init. Release
Observations	21783	37763	21573

Notes. This table repeats the specification in Columns (4) of Table 2, where we consider various weighting schemes in the regression. In Columns (1)-(2), we run weighted regressions weighted by establishment's initial PM_{10} emissions and initial employment, respectively. In Column (3), we consider as a dependent variable the log of establishment-year-level *toxicity-weighted* PM Emissions_{10} (Log(Toxic-Wt. PM)), and further weight the regression using the initial toxicity-weighted PM_{10} emissions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.12: Controlling for the Indirect Impact through Input-Output Linkages: Own, Upstream, and Downstream PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)
	Log(PM Emissions)		
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.166***	-1.275***	-1.236***
	(0.389)	(0.403)	(0.409)
$\text{Post}_t \times \text{NTR Gap}^{\text{Up}}_{i,99}$	-1.594		-1.137
	(1.641)		(1.710)
$\text{Post}_t \times \text{NTR Gap}^{\text{Down}}_{i,99}$		0.821	0.619
		(0.748)	(0.774)
$\text{NTR}_{i,t}$	-0.006	-0.008	-0.007
	(0.036)	(0.036)	(0.036)
$\text{MFA Exposure}_{i,t}$	-0.009	-0.010	-0.010
	(0.016)	(0.016)	(0.016)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.297**	0.324***	0.313**
	(0.117)	(0.122)	(0.122)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	0.019	0.019	0.005
	(0.059)	(0.060)	(0.063)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-0.859*	-0.862*	-0.917*
	(0.491)	(0.467)	(0.490)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-32.346	-28.517	-29.104
	(27.085)	(27.004)	(27.127)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Observations	46753	46753	46753

Notes. This table repeats the specifications in Column (4) of Table 2, where we additionally include upstream and downstream measures of NTR gap. These measures are constructed by using the industry-level input-output table following Pierce and Schott (2016). Upstream (downstream) NTR gap indicates the average NTR gap each SIC 4-digit industry faces from the upstream (downstream) industries in input-output networks. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.13: Controlling for Upstream-Specific Time Trends:
PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)	(4)
	Log(PM Emissions)			
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.232***	-1.245***	-1.221***	-1.407***
	(0.431)	(0.437)	(0.439)	(0.395)
$\text{NTR}_{i,t}$			-0.025	-0.012
			(0.035)	(0.036)
$\text{MFA Exposure}_{i,t}$			-0.015	-0.013
			(0.017)	(0.017)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$				0.281**
				(0.124)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$				0.051
				(0.058)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$				-0.600
				(0.512)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$				-46.052*
				(27.890)
Establishment FE	✓	✓	✓	✓
Year FE	✓	-	-	-
County x Year FE	-	✓	✓	✓
Upstream x Year FE	✓	✓	✓	✓
Observations	39219	37701	37701	37701

Notes. This table repeats the specifications in Columns (1)-(4) of Table 2, where we additionally include Upstream Indicator-by-Year fixed effects. Following Burchardi et al. (2019), upstream indicator is a binary indicator that takes value one if the upstreamness index (Antras et al., 2012) is larger than 2 and zero otherwise. Therefore, Upstream Indicator-by-Year fixed effects control for any upstream-specific time trends in pollution emissions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.14: Accommodating Observations with Zero Emission by using PPML Regression: PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1)	(2)	(3)
	PM Emissions		
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-2.025**	-2.319***	-2.080***
	(0.830)	(0.701)	(0.753)
$\text{NTR}_{i,t}$	-0.317	-0.052	-0.029
	(0.200)	(0.049)	(0.041)
MFA $\text{Exposure}_{i,t}$	-0.009	-0.012	-0.031**
	(0.020)	(0.010)	(0.014)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	-0.677	-0.016	0.474*
	(0.763)	(0.236)	(0.254)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	-0.105	0.048	0.024
	(0.125)	(0.090)	(0.090)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	-1.993	-1.349	-1.590
	(2.300)	(1.344)	(1.001)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-60.606	-64.132*	-73.524***
	(59.351)	(33.692)	(23.867)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Sample	All	Surviving Estab.	Emission> 0
Observations	118258	94431	46753

Notes. This table repeats the specifications in Column (4) of Table 2, where we accommodate observations with zero reported emission by using the Poisson Pseudo Maximum Likelihood (PPML) regression. Column (1) considers all establishments accommodating observations with zero emission associated with establishment entry and exit; Column (2) restricts the analysis to surviving establishments but accommodates zero emission cases; Column (3) restricts the analysis to observations with positive emissions. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.15: Heterogeneous Treatment Effects:
PNTR and Establishment-level Pollution Emission Intensity, 1997 - 2017,
Log(PM Emissions/Sales)

	(1) Log(PM Emissions/Sales)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.895
	(7.233)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Import Intensity}_{f,97}$	-14.448***
	(4.649)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Nonattainment}_{c,95-97}$	-3.801**
	(1.706)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Upstream}_{i,97}$	-3.841*
	(2.301)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. 4-digit Sectors}_{f,97})$	-2.801
	(2.127)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Export Intensity}_{f,97}$	-6.305
	(5.472)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. Establishment}_{f,97})$	-1.289
	(1.509)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Firm Employment}_{f,97})$	2.271*
	(1.207)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Age}_{p,97}$	-0.001
	(0.013)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{I(Num. P2}_{p,95-97} > 0)$	2.434**
	(1.162)
Establishment FE	✓
County x Year FE	✓
Controls	✓
Observations	15611

Notes. This table repeats the specification in Column (10) of Table 4, where we use an establishment-year-level pollution emission intensity—measured by the log of PM_{10} emissions-to-sales ratio—as a dependent variable. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.16: Heterogeneous Treatment Effects and the Unconditional Import Intensity: PNTR and Establishment-level Pollution Emissions, 1997 - 2017

	(1) Log(PM Emissions)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-1.147***
	(0.427)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Import Intensity (Unconditional)}_{f,97}$	-1.732
	(1.767)
Establishment FE	✓
County x Year FE	✓
Controls	✓
Observations	37763

Notes. This table repeats the specification in Column (1) of Table 4, where we consider unconditional import intensity that incorporates non-importers. The regression includes all controls in Column (1) of Table 4, including the interactions of import intensity with the post-PNTR indicator and the NTR gap (where, in fact, the latter is automatically dropped due to a perfect multicollinearity). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.17: PNTR, Import Status, and Export Status, 1997 - 2017

	(1) Import	(2) Export	(3) Export
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.027	-0.022	-0.028
	(0.131)	(0.170)	(0.085)
Establishment FE	✓	✓	✓
County x Year FE	✓	✓	✓
Controls	✓	✓	✓
Margin	Extensive	Extensive	Intensive
Observations	15525	8206	20189

Notes. This table investigates the effect of the conferral of PNTR to China on establishment-level import status (extensive margin) and export status (extensive and intensive margins). The dependent variable, Import (Export), is a dummy variable that equals to one if establishment p engages in importing (exporting) activities in year t. Column (1) focuses on the extensive margin adjustment of importing activities within a firm by restricting the sample to establishments that did not belong to importing firms in 1997 (i.e., $\text{Import Intensity}_{f,97} = 0$). Column (2) focuses on the extensive margin adjustment of exporting activities within a firm by restricting the sample to establishments that did not belong to exporting firm in 1997 (i.e., $\text{Export Intensity}_{f,97} = 0$). Column (3) focuses on the intensive margin adjustment of exporting activities within a firm by restricting the sample to establishments that belonged to exporting firm in 1997 (i.e., $\text{Export Intensity}_{f,97} > 0$). The rest of the specifications in Columns (1)-(3) are identical to Column (4) of Table 2. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.18: Heterogeneous Treatment Effects:
PNTR and Establishment-level Log of Off-Site Non-Disposal, 1997 - 2017

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Log(Off-Site Non-Disposal of PM)										
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.136	-2.571	-0.129	-1.559	0.139	-0.244	-0.003	-0.232	-0.815	0.349	16.563
	(0.720)	(2.029)	(0.682)	(0.999)	(3.837)	(1.194)	(1.164)	(2.644)	(1.304)	(1.018)	(10.278)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Import Intensity}_{f,97}$		10.484**									14.160**
		(5.106)									(6.850)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Nonattainment}_{c,95-97}$			1.003								2.778
			(2.039)								(3.099)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Upstream}_{i,97}$				2.143							-1.696
				(1.348)							(2.704)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. 4-digit Sectors}_{f,97})$					0.136						4.180*
					(0.697)						(2.171)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Export Intensity}_{f,97}$						0.398					6.454
						(2.067)					(6.732)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. Establishment}_{f,97})$							-0.001				1.286
							(0.296)				(2.009)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Firm Employment}_{f,97})$								0.028			-4.346***
								(0.327)			(1.553)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Age}_{p,97}$									0.013		0.030
									(0.016)		(0.026)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{I(Num. P2}_{p,95-97} > 0)$										-1.044	-1.387
										(1.453)	(1.851)
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County x Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	26301	8949	20928	20892	20928	15787	20928	20928	20928	20928	7992

Notes. This table repeats the specifications in Column (4) of Table 2 (for Column (1)) and Columns (1)-(10) of Table 4 (for Columns (2)-(11)), where we consider the log of establishment-year *off-site* non-disposal of PM_{10} as the dependent variable. Off-site non-disposal of PM_{10} measures the amount of PM_{10}-containing wastes that were transferred to off-site facilities that are geographically or physically separate from the facility reporting under TRI for non-disposal purposes—recycling, energy recovery, or treatment. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.19: Heterogeneous Treatment Effects:
PNTR and Establishment-level Log of On-Site Non-Disposal, 1997 - 2017

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Log(On-Site Non-Disposal of PM)										
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	1.284	-1.716	2.602*	12.451	2.596	0.946	1.551	2.675	3.730	-0.689	-80.852***
	(1.137)	(2.999)	(1.526)	(14.238)	(4.424)	(1.887)	(2.471)	(4.545)	(2.336)	(1.894)	(23.823)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Import Intensity}_{f,97}$		3.443									-15.593
		(8.827)									(13.842)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Nonattainment}_{c,95-97}$			-3.353*								1.205
			(1.934)								(2.949)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Upstream}_{i,97}$				-11.153							30.158**
				(14.343)							(12.293)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. 4-digit Sectors}_{f,97})$					-0.419						5.085
					(1.291)						(4.501)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Export Intensity}_{f,97}$						-0.801					-11.867
						(4.664)					(9.257)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Num. Establishment}_{f,97})$							-0.098				-24.475***
							(0.912)				(3.768)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Log(Firm Employment}_{f,97})$								-0.172			16.423***
								(0.651)			(3.790)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{Age}_{p,97}$									-0.054		-0.097*
									(0.044)		(0.057)
$\text{Post}_t \times \text{NTR Gap}_{i,99} \times \text{I(Num. P2}_{p,95-97} > 0)$										4.100	12.221***
										(2.654)	(3.126)
Establishment FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
County x Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2754	1032	2358	2345	2358	1559	2358	2358	2358	2358	819

Notes. This table repeats the specifications in Column (4) of Table 2 (for Column (1)) and Columns (1)-(10) of Table 4 (for Columns (2)-(11)), where we consider the log of establishment-year *on-site* non-disposal of PM_{10} as the dependent variable. On-site non-disposal of PM_{10} measures the amount of PM_{10}-containing wastes that underwent through non-disposal process—recycling, energy recovery, or treatment—in the facility reporting under TRI (i.e., on-site). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.20: PNTR and Establishment-level Number of Chemicals with Clean Technology Adoption-Related Pollution Prevention (P2) Activities, 1997 - 2017

	(1)	(2)
	Z = Num. P2 Clean-Tech	
	I(Z> 0)	Log(Z)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.060	0.453
	(0.071)	(0.518)
$\text{NTR}_{i,t}$	-0.011**	0.002
	(0.005)	(0.019)
$\text{MFA Exposure}_{i,t}$	-0.000	-0.003
	(0.004)	(0.003)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	-0.041**	0.078
	(0.019)	(0.188)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	-0.020*	0.128*
	(0.010)	(0.066)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	0.117*	-0.026
	(0.068)	(0.881)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	-2.386	-17.978
	(2.917)	(36.547)
Establishment FE	✓	✓
County x Year FE	✓	✓
Observations	46753	605

Notes. This table investigates the effect of the conferral of PNTR to China on establishments' clean technology adoption-related pollution prevention (P2) activities. Specifically, the table repeats the specification in Column (4) of Table 2, where we consider establishment-year-level measures of clean technology adoption-related P2 activities as dependent variables. Column (1) uses a dummy variable that equals one if there is at least one toxic chemical in year t that establishment p has taken any clean technology adoption-related P2 activities (extensive margin). Column (2) uses the log of the number of toxic chemicals in year t that establishment p has taken any clean technology adoption-related P2 activities (intensive margin). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table B.21: PNTR and Establishment-level Number of Chemicals with Overall Pollution Prevention (P2) Activities, 1997 - 2017

	(1)	(2)
	Z = Num. P2	
	I(Z> 0)	Log(Z)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.118	-0.047
	(0.080)	(0.481)
$\text{NTR}_{i,t}$	-0.009	-0.014
	(0.006)	(0.025)
$\text{MFA Exposure}_{i,t}$	0.005**	0.027***
	(0.002)	(0.006)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	-0.019	-0.138
	(0.028)	(0.107)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	-0.028**	0.005
	(0.011)	(0.068)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	0.069	0.103
	(0.091)	(0.768)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	1.033	-11.791
	(4.241)	(21.659)
Establishment FE	✓	✓
County x Year FE	✓	✓
Observations	46753	2727

Notes. This table investigates the effect of the conferral of PNTR to China on establishments' overall pollution prevention (P2) activities. Specifically, the table repeats the specification in Column (4) of Table 2, where we consider establishment-year-level measures of P2 activities as dependent variables. Column (1) uses a dummy variable that equals one if there is at least one toxic chemical in year t that establishment p has taken any P2 activities (extensive margin). Column (2) uses the log of the number of toxic chemicals in year t that establishment p has taken any P2 activities (intensive margin). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

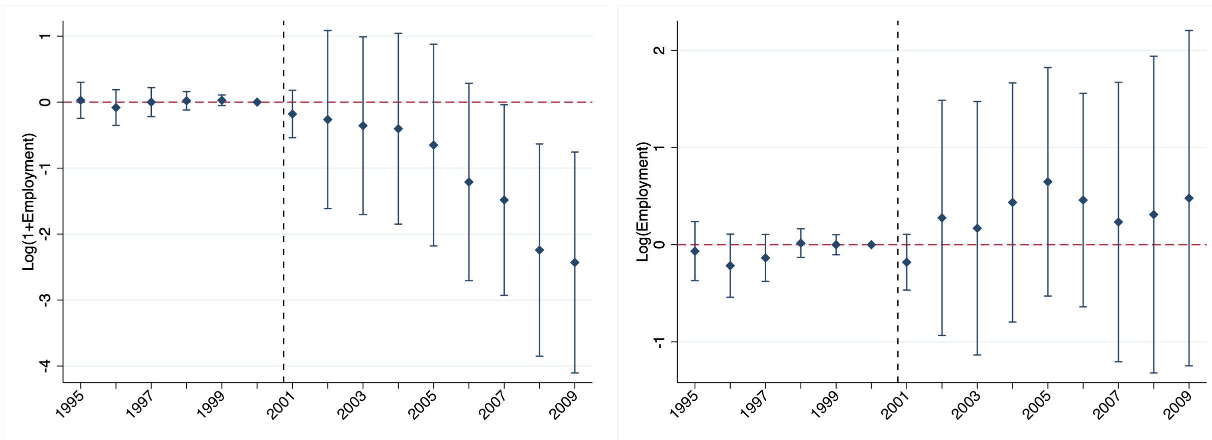
Table B.22: PNTR and Establishment-level Employment, 1997 - 2017

	(1)	(2)
	Log(1+Employment)	
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-0.608**	-1.185*
	(0.285)	(0.628)
$\text{NTR}_{i,t}$	-0.017	-0.029
	(0.023)	(0.057)
$\text{MFA Exposure}_{i,t}$	-0.028*	-0.058***
	(0.015)	(0.017)
$\text{Post}_t \times \text{Log}(\text{NP}_{i,95}/\text{Emp}_{i,95})$	0.410***	0.273
	(0.104)	(0.195)
$\text{Post}_t \times \text{Log}(\text{K}_{i,95}/\text{Emp}_{i,95})$	-0.170***	-0.165*
	(0.047)	(0.092)
$\text{Post}_t \times \Delta\text{Chinese Tariff}_i$	0.325	-0.253
	(0.391)	(0.844)
$\text{Post}_t \times \Delta\text{Chinese Subsidies}_i$	19.986	18.957
	(14.084)	(34.295)
Establishment FE	✓	✓
County x Year FE	✓	✓
Weights	Unweighted	Weighted
Observations	121149	121149

Notes. This table repeats the specification in Column (4) of Table 2, where we use log of one plus employment as a dependent variable. The sample consists of establishment-year-level observations from NETS-TRI matched data, where each establishment has at least one year of positive emissions reported to TRI during the sample period. We restrict the sample to establishments that existed in 1997. Column (1) considers unweighted regression and Column (2) considers weighted regression weighted by initial employment. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Appendix C PNTR and Employment Responses

Figure C.1: Dynamic Treatment Effects of Employment at the Establishment Level:
(i) Full NETS-TRI Matched Establishments (Left);
(ii) NETS-TRI Matched Establishments with Positive Initial Emissions (Right)



Notes: These figures display the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we use measures of establishment-year-level employment—measured by (i) log of one plus employment (Left) and (ii) log of employment (Right)—as dependent variables. The sample consists of establishment-year-level observations from NETS-TRI matched data, where each establishment has at least one year of positive emissions reported to TRI. In the left panel, we restrict the sample to establishments that had ten or more workers during the initial period (1995-2000). In the right panel, we further restrict the sample to those that had positive initial emissions. All other specifications are identical to those in Equation (5.3).

The left panel of Figure C.1 shows the dynamic treatment effects of employment at the establishment level using the full NETS-TRI matched establishments, where each establishment has at least one year of positive emissions reported to TRI. To make sure that these establishments satisfy the TRI-reporting criteria in terms of establishment size in the initial period, we restrict the sample to establishments that had ten or more workers during 1995-2000. The key departure from our baseline sample is that we include establishments with zero reported emission (conditional on survival) and accommodate establishment exits. Given that these establishments have ten or more employees (and thus satisfy the TRI-reporting criteria in terms of establishment size), zero emission implies that they do not produce toxic PM chemicals or produce them but below the reporting threshold level (i.e., negligible amount). We include establishment exit margin because it is well-documented in the literature that employment impact of import competition from China is most significant at the exit margin (e.g., Asquith et al., 2019).

With this extended sample, we find a significant decline of employment in response to the reduction of trade policy uncertainty, a consistent result with Pierce and Schott (2016) and Asquith et al. (2019). In particular, Asquith et al. (2019) find that PNTR led to manufacturing employment declines and establishment exits using the entire NETS sample.

The result exhibits a clear contrast with the right panel of Figure C.1, where we restrict the sample to establishments that had positive initial emissions. Once we restrict the sample to those with positive initial emissions, we find an insignificant response of employment. This is also consistent with Figure A.9, where we observe a mild—but insignificant—increase of sales following the PNTR when we focus on establishments with positive emissions.[68]

These exercises suggest that our result that attributes within-establishment emission abatement to surviving establishments is not a spurious result driven by the restriction of sample to those that satisfy the TRI-reporting criteria (i.e., relatively larger establishments). Instead, it shows that establishments that generate positive amounts of emissions are fundamentally different from those with zero emission. This is also consistent with Figure A.2, which shows that the most important industries in terms of toxic emissions—SIC 2-digit: 33 (Primary Metal Industries) and 28 (Chemicals and Applied Products)—are different from those in terms of employment—SIC 2-digit: 37 (Transportation Equipment) and 35 (Industrial and Commercial Machinery and Computer Equipment).

[68]This is, in fact, consistent with the literature because the previous studies suggest that the direction of sales and employment response to the China shock may be heterogeneous across businesses with different characteristics. For example, Bloom et al. (2016) find that the industry-level growth of import penetration from China has an insignificant yet *positive* association with sales and employment of large US public firms, but has a negative association with sales of small US public firms.