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

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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 further evidence that supports the pollution haven hypothesis whereby offshoring is central to the mechanism—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

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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., 2021). 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 haven hypothesis* 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-pollution activities to developing countries with laxer environmental regulations (Copeland and Taylor, 2004). Despite active discussions in 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 neglected in the discussion of the pollution haven 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 haven 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 pollution haven 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 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

[1] As Copeland and Taylor (2004) and Cherniwchan et al. (2017) 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. A notable exception is Cherniwchan (2017), who studies the impact of NAFTA on US manufacturing toxic emissions.

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 haven 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 haven 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 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.

[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.

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. First, we use time-varying importing status at the establishment level from the NETS database to proxy for global sourcing activity, and test whether PNTR induced US manufacturers to source from abroad. We find that US establishments initially associated with 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. Second, by combining Wharton Research Data Services (WRDS) Company Subsidiary Data with our main dataset, we use 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 (measured as above), thereby suggesting that US establishments sent high-polluting tasks to China and reduced domestic pollution emissions accordingly.

Finally, we directly assess whether US manufacturers increased imports of *dirtier* 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 expect that US manufacturers will increase dirty product imports from China relative to other countries. We test this hypothesis by 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 and that such an increase is more pronounced for products that are 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. 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 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

[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.

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), the US (Holladay, 2016; Cherniwchan, 2017), Mexico (Gutiérrez and Teshima, 2018), and China (Bombardini and Li, 2020; Rodrigue et al., 2020). Our paper contributes to the literature by providing direct establishment-level evidence of offshoring as an emission reduction mechanism for a developed country (United States). For example, Martin (2011) and Gutiérrez and Teshima (2018) both leverage tariff changes in developing countries, not developed countries. This is an important distinction in light of the finding in the literature that firms in developed economies are more able and prone than those in developing countries to leverage reduced trade barriers to conduct offshoring activities, which plausibly has environmental relevance. In this regard, our paper relates most closely to Cherniwchan (2017) who investigates the impact of NAFTA on US manufacturing emissions between 1994 and 1998.

Our work and Cherniwchan (2017) are complementary to one another but we depart from his paper in several dimensions. First, we study the environmental impacts of the China trade shock, which is widely accepted as one of the primary factors behind the massive US manufacturing employment decline and offshoring since the late 1990s (Autor et al., 2013, 2014; Acemoglu et al., 2016; Pierce and Schott, 2016). Importantly, we use the episode of eliminating the uncertainty of tariff hikes, not the actual tariff changes, which also allows us to contribute to the growing literature on firm-level impacts of trade policy uncertainty. Second, our setting based on PNTR singles out the impact of changes in import barriers rather than studying the effect of changes in both import and export barriers. Potentially because of this difference, we find a limited role of *trade-induced* clean technology adoption: The nature of our shock liberalizes imports, not exports through which the technology upgrading channel mainly operates (e.g., Bustos, 2011). Finally, in terms of mechanisms, we provide more compelling evidence of the pollution haven 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.

By providing establishment-level evidence supporting the pollution haven hypothesis, our paper also contributes to the literature that studies the pollution haven hypothesis or pollution haven effects.[7]

[6] While decomposition studies are useful for understanding the role of international trade in affecting the environment, the decomposition analysis alone cannot identify causality (Cherniwchan et al., 2017). 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] 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. Copeland and Taylor (2004) and Cherniwchan et al. (2017) 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 provides evidence in support of the pollution haven hypothesis.

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., 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).[8] 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 haven hypothesis. Our findings thus underscore the complexities in the relations among trade policy uncertainty, environmental regulations, and offshoring in determining the level of emissions in US manufacturing.

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.[9]

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 and descriptive patterns. Section 6 presents our main estimation results. Section 7 reports results on mechanisms and heterogeneous effects. Finally, Section 8 concludes the paper.

[8] Chung (2014) and Cole et al. (2014) find supporting evidence for pollution haven effects in Korea and Japan, respectively.

[9] 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.[10] Facilities that are noncompliant are subject to further investigation and possible enforcement actions by the EPA.[11] 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.

[10] 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.)."

[11] 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 (chemical name, acuteness in human health effects, carcinogenicity, the severity of environmental effects, etc.) 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.[12]

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.[13] 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 follow Cherniwchan (2017); Holladay and LaPlue III (2021) and apply a crosswalk obtained from the National Emissions Inventory (NEI) to map relevant chemicals to PM_{10}.[14] Throughout our analyses, we collapse the data and focus on facility-level waste production of this major pollutant.

National Establishment Time Series (NETS) To understand establishment-level responses in waste production relative to size (employment and sales) as well as important 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

[12] 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.

[13] 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

[14] 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

industry codes (SIC and NAICS codes),[15] 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 data through various records.[16] A number of studies have demonstrated the accuracy of information in NETS data (Neumark et al., 2006, 2011; Barnatchez et al., 2017).[17] 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.[18]

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.[19] 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).

[15]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.

[16]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.

[17]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).

[18]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)."

[19]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.

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).

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.[20]

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*.[21]

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)

[20] 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

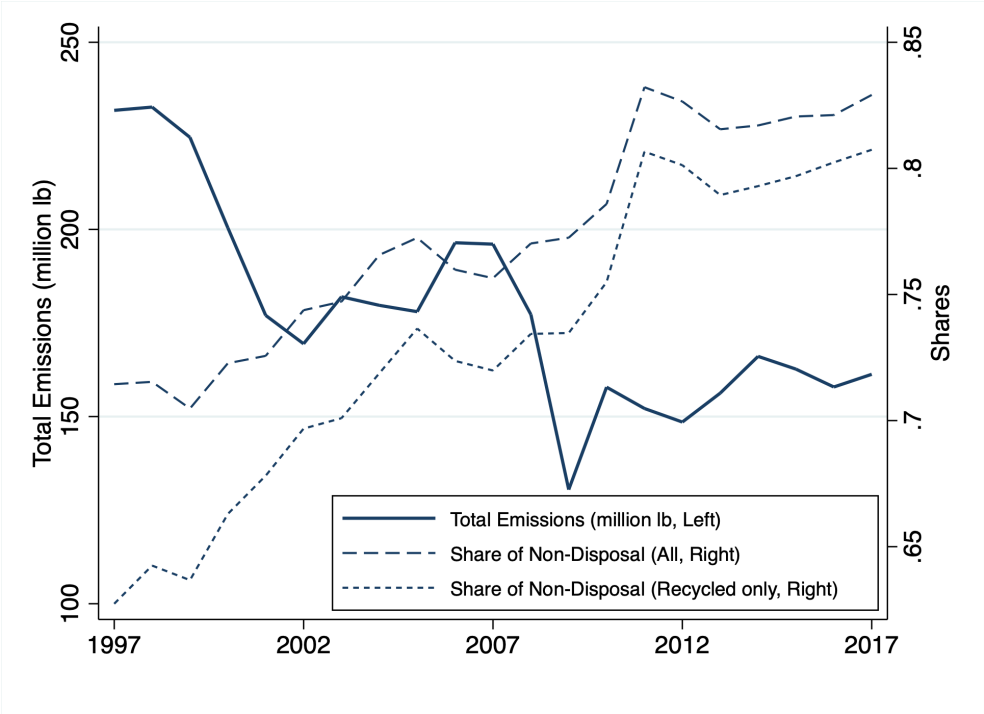
[21] Appendix Table B.1 provides further details related to these policy changes.

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 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, etc.) 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.[22] In fact, these two industry categories represent a predominant share of the initial PM_{10} emissions from manufacturing establishments.[23] 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.[24] 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).[25] 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, we explore how the conferral of PNTR to China interacts with various initial characteristics and affects establishments' waste management efforts and PM_{10} emissions.[26]

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

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

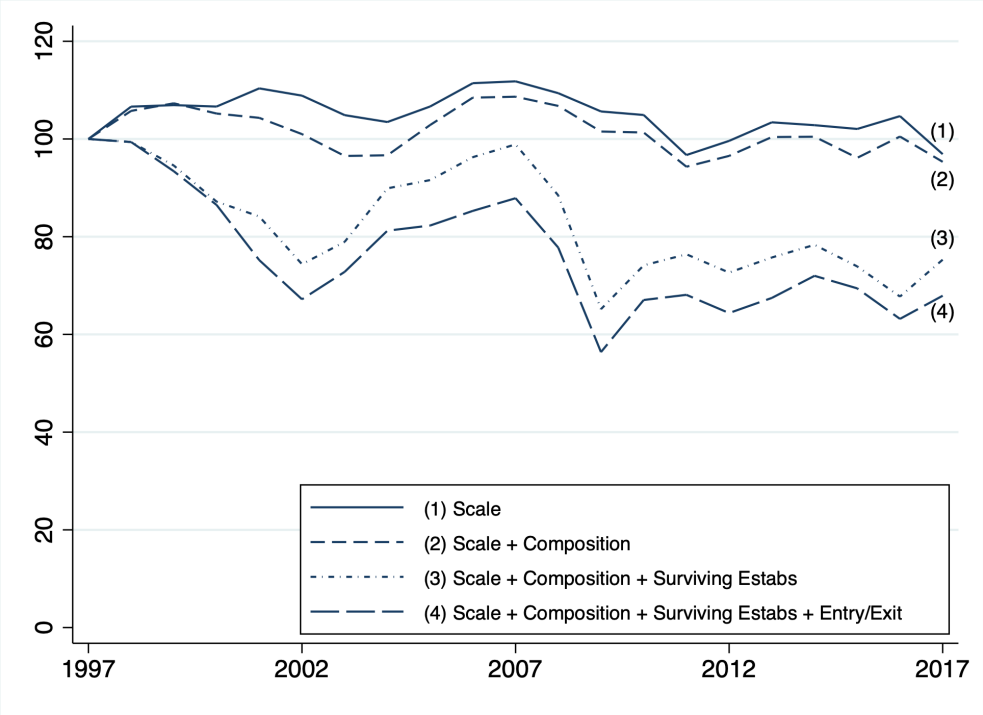
[23] Appendix Figure A.2 provides the industry distribution of our data using employment and total PM_{10} emissions.

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

[25] 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

[26] 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.

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 ($\sum_i \theta_{i,t} z_{i,t}$).

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

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.[27] Thus, the average emission efficiency of industry i in the baseline year and in year t are given as follows:

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

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

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.[28] 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).

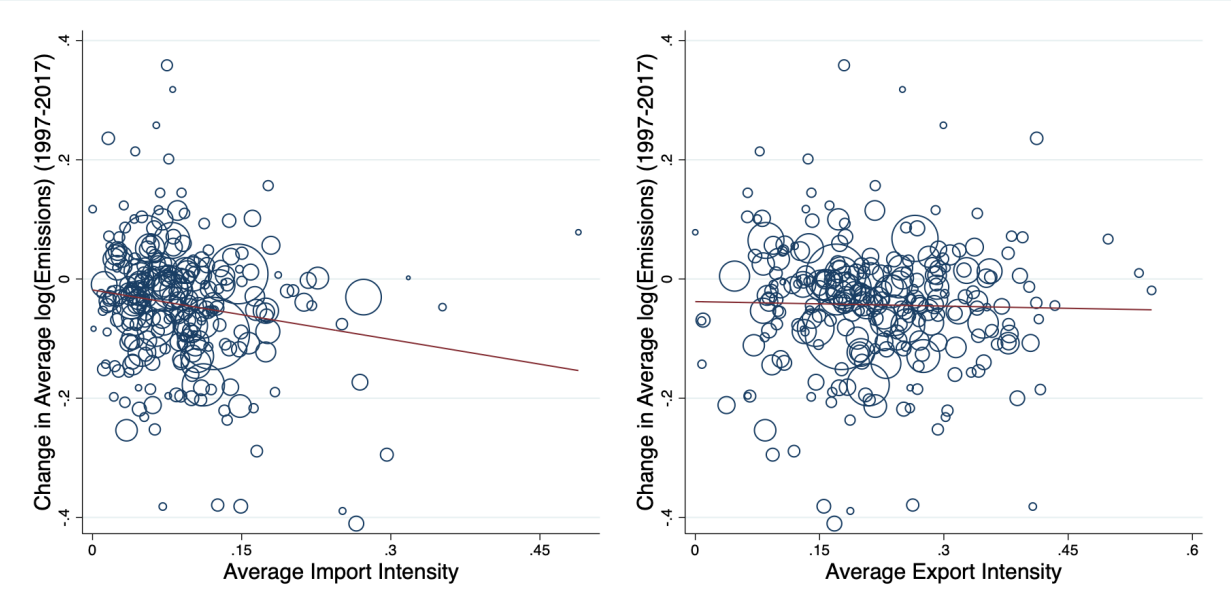
Figure 2 shows the decomposition results tracking changes in total manufacturing emissions of PM_{10} relative to 1997 for 20 years and the contribution of each channel. The aggregate change, which exhibits a downward trend in total manufacturing emissions of PM_{10} over time, is captured using line (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, etc.), 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

[27] We categorize firms that enter and exit between year t_0 and year t in the exit group.
[28] 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.

(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 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.[29] 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.

[29] 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.

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. 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.[30]

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,

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

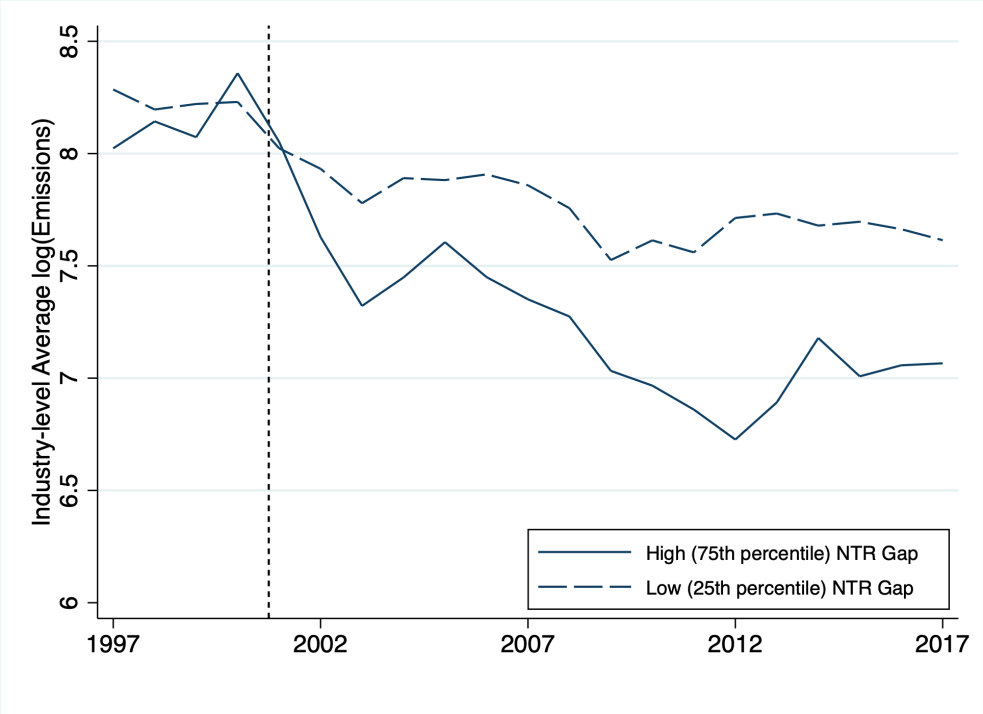
As summarized in Panel (B) of Appendix Table B.3, we observe sufficient variation in industry-level *NTR Gap* in our sample.[31] Note that the differences in the tariff rates faced by Chinese firms due to the policy change are mainly driven by the initial rates set under the Smoot-Hawley Tariff Act of 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

[30] See Pierce and Schott (2016, 2020) for a comprehensive description of the policy background.
[31] The average is 0.329 and the standard deviation is 0.142.

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.

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.

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

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

where the dependent variable is the log of PM_{10} emissions from establishment p in industry i in year t.[32] 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,

[32]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, although the estimated coefficients are not statistically significant.

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 the time-varying local environmental regulatory conditions and unobserved local demand or supply shocks that potentially generate confounding effects on the outcome variable. 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

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

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.[33] 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.[34]

[33]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).

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

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 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.

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.[35] 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.[36] 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).[37] 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.[38]

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

[36] 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.

[37] 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.

[38] 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 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.

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.

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 × (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).

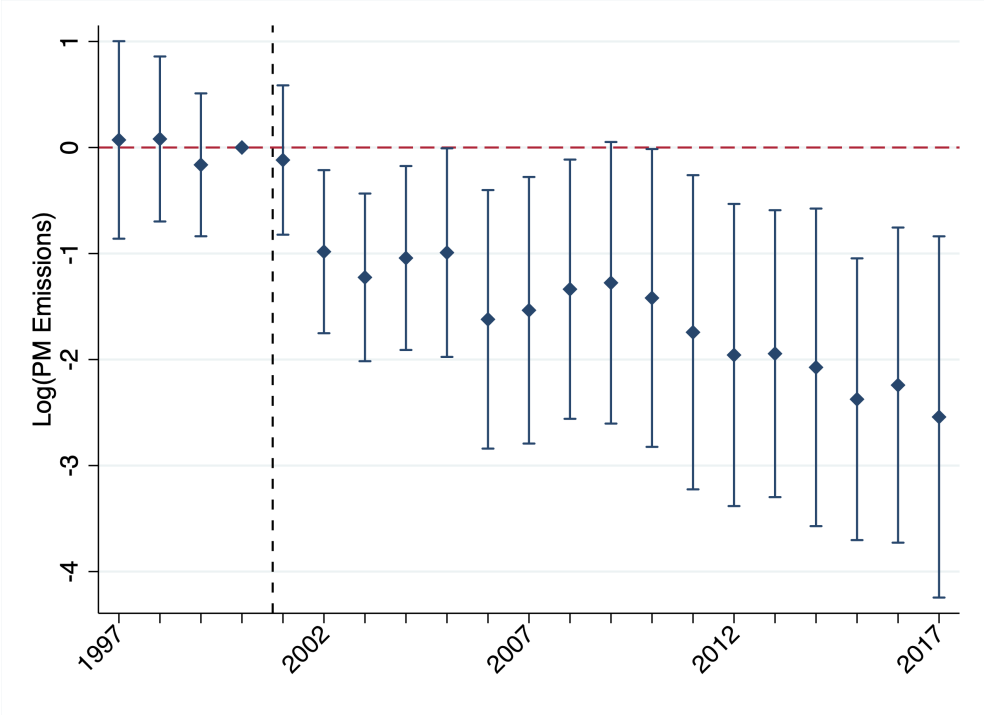
Pre-Existing Trends and Dynamic Treatment Effects Figure 5 plots the coefficient estimates, along with their 95 confidence intervals, from the regression in Equation (5.3). We do not detect any differential pretrends in that point estimates are statistically indistinguishable from zero leading up to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

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.[39] 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.

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

[39] 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.

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.

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:[40]

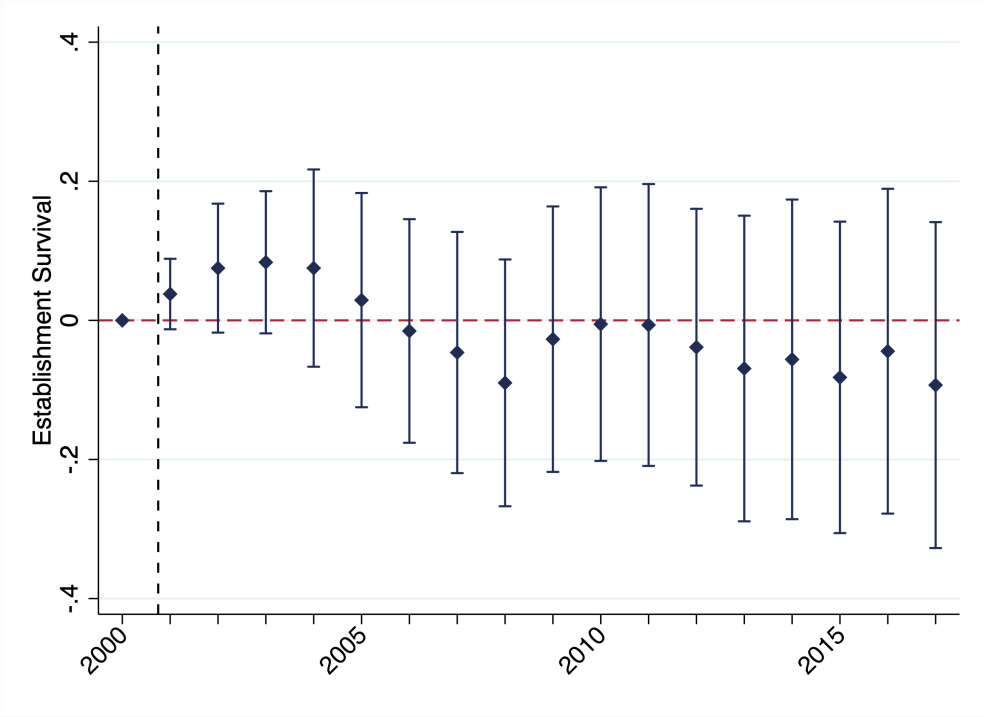
$$\mathrm{y}_{p,t} = \beta_t NTRGap_i + \alpha V_p + \gamma X_i + \delta Z_i + \eta_c + \varepsilon_{p,t}. \tag{6.1}$$

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

[40]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.

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).[41] 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: 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.

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

[41] These industry controls include the industry-level NTR tariff rates in 2000 ($NTR_{i,00}$), the industry-level MFA exposure in 2000 ($MFA\ Exposure_{i,00}$), the industry-level log of 1995 skill and capital intensity ($Log(NP_{i,95}/Emp_{i,95})$ and $Log(K_{i,95}/Emp_{i,95})$, respectively), changes in Chinese import tariffs from 1996 to 2005 ($\Delta Chinese\ Tariff_i$), and changes in Chinese production subsidies per total sales from 1999 to 2005 ($\Delta Chinese\ Subsidies_i$).

amounts of PM_{10} in 2000 to exit the market. Therefore, establishment exits do not appear to explain the reduction in pollution emissions in US manufacturing.[42]

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)			
$Post_t \times NTR\ Gap_{i,99}$	-1.743***	-1.621***	-1.595***	-1.635***
	(0.514)	(0.597)	(0.544)	(0.535)
$NTR_{i,t}$			0.013	0.041
			(0.042)	(0.045)
MFA $Exposure_{i,t}$			-0.010	-0.008
			(0.018)	(0.018)
$Post_t \times Log(NP_{i,95}/Emp_{i,95})$				0.312**
				(0.155)
$Post_t \times Log(K_{i,95}/Emp_{i,95})$				0.172***
				(0.062)
$Post_t \times \Delta$Chinese $Tariff_i$				-0.855
				(0.572)
$Post_t \times \Delta$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.

[42] Note that this result does not necessarily mean that the imposition of PNTR did not induce US manufacturers to leave the market because the manufacturing establishments in our sample (i.e., NETS-TRI matched dataset with positive emissions) are far larger in size (e.g., employment, sales) than the manufacturing establishments in the NETS data (see Appendix Table B.4). The question—whether the imposition of PNTR induced US manufacturers to exit the market—is indeed important to understand; however, it is beyond the scope of this paper.

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. 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:[43] 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.15 (off-site non-disposal) and B.16 (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, relocating high-polluting tasks abroad, etc. We revisit this discussion in Section 7 where we study mechanisms in detail.

[43] 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.

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 (Cherniwchan, 2017), 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.[44] 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.

Controlling for NAFTA A more direct way to address concerns related to the lagged responses

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

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.[45] Appendix Table B.8 presents the estimation results.[46] 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) establishment size, respectively. The results are barely affected by dropping those outliers.

[45] 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.

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

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.[47] Our results are robust to the alternative specifications.

7 Offshoring and Pollution Haven Hypothesis

In this section, we present evidence in support of the offshoring mechanism that explains our main findings on the PNTR-induced reductions in the PM_{10} emissions within US manufacturing. We begin by examining the heterogeneity in the effects of the PNTR across establishments. We then conduct additional analyses to directly assess the importance of two competing hypotheses: (i) global sourcing and FDI activities, and (ii) the adoption of environment-friendly practices.

Heterogeneous Treatment Effects 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.[48] 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.[49]

[47] 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.

[48] 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.

[49] 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.12.

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)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$	-4.452*									-10.944***
$\times \text{Import Intensity}_{f,97}$	(2.365)									(3.081)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$		-2.316***								-3.995***
$\times \text{Nonattainment}_{c,95-97}$		(0.772)								(0.986)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$			-2.187**							-3.172**
$\times \text{Upstream}_{i,97}$			(0.959)							(1.596)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$				-0.105						-2.934**
$\times \text{Log(Num. 4-digit Sectors}_{f,97})$				(0.486)						(1.355)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$					-0.454					-5.922
$\times \text{Export Intensity}_{f,97}$					(1.358)					(4.367)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$						0.057				0.397
$\times \text{Log(Num. Establishment}_{f,97})$						(0.179)				(1.125)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$							0.076			0.786
$\times \text{Log(Firm Employment}_{f,97})$							(0.170)			(0.860)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$								-0.002		0.007
$\times \text{Age}_{p,97}$								(0.009)		(0.010)
$\text{Post}_t \times \text{NTR Gap}_{i,99}$									0.532	0.977
$\times \text{I(Num. P2}_{p,95-97} > 0)$									(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 (Import $\text{Intensity}_{f,97}>0$) 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 (Export $\text{Intensity}_{f,97}>0$) 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.

There are four notable results in Table 4, which, for visibility, we placed in the first four columns 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.[50] 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.[51]

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 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

[50] 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.13, we consider the unconditional import intensity that includes non-importers. We obtain negative coefficients, but the estimate is less precise.

[51] 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.15). 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.16 presents the results for on-site non-disposal, where we do not find analogous patterns.

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.

Global Sourcing and FDI Activities Motivated by the suggestive evidence in support of the offshoring mechanism in the heterogeneous treatment effect analyses, we now directly assess the importance of (i) global sourcing and (ii) FDI, in turn, as channels through which US manufacturers adjust and reduce domestic pollutant emissions. Conceptually, 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. Therefore, we use two separate measures constructed from NETS and WRDS Company Subsidiary data as proxies.[52]

Specifically, we first use time-varying importing status at the establishment level from the NETS database to proxy for global sourcing activity, and test whether PNTR induced US manufacturers to source from abroad. Next, we use WRDS Company Subsidiary data linked to our main dataset and count the number of foreign subsidiaries in China (or other countries) to measure US multinationals' FDI activities at the establishment-year level. Using these two measures—importing status and FDI—as dependent variables, we estimate the main equation (5.2). In each case, we further 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. Given that a nonattainment designation for a given pollutant is granted to counties whose air pollution concentrations of that pollutant exceed federal standards (Hanna, 2010), establishments located in these counties are likely those that heavily emitted pollutants during the initial period.

Table 5 reports the estimates for global sourcing activities. We begin by focusing on firms with at

[52]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.

Table 5: PNTR and Import Status, 1997 - 2017

	(1)	(2)	(3)
	Import	Import	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 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.

least one foreign sourcing network (i.e., Import Intensity$_{f,97} > 0$) because the emission reduction effects were most pronounced at the intensive margin of importing activities in Table 4. Column (1) of Table 5 indicates that PNTR induced US manufacturing establishments to begin importing.[53] Conversely, Column (1) of Appendix Table B.14 shows that PNTR had no such effect for establishments that did not initially belong to an importing firm (i.e., Import Intensity$_{f,97} = 0$). Both results suggest that establishments with existing foreign networks, which had already paid sunk investment costs, played a major role in global sourcing activities after PNTR, underscoring the importance of intensive margin adjustment.[54] Columns (2) and (3) of Table 5 further show that establishments that are most likely associated with high-polluting tasks engage more in importing activities than other establishments

[53] In other words, non-importing establishments that belong to a firm with foreign sourcing networks began to source from abroad after PNTR.

[54] Similarly, we examine whether exporting activities respond to the conferral of PNTR to China. Columns (2) and (3) of Appendix Table B.14 reveal that the imposition of PNTR does not bring about new exporting activities at the establishment level. This is also consistent with the nature of the trade shock that liberalizes imports, not exports.

after PNTR. Collectively, the results indicate that US manufacturers sent (or sourced) a set of dirty tasks abroad within establishments and thus were able to reduce toxic emissions.

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.

Table 6 reports the estimates for FDI activities.[55] Columns (1)-(2) investigate whether the imposition of PNTR induced US manufacturers to set up more subsidiaries in China at the extensive margin, while columns (3)-(4) explore the intensive margin. We find that the imposition of PNTR induced US manufacturing establishments to set up more subsidiaries, mainly at the intensive

[55] In Columns (1)-(2) and (5)-(6), we consider a 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.

margin.[56] 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 statistically insignificant and small in magnitude. In Table 7, we further find that establishments that are most likely associated with high-polluting tasks increase their subsidiaries in China more than other establishments, which operated mainly along the intensive margin (see Columns (2) and (4)), not the extensive margin.

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.

[56] 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.

Taken together, we have provided multiple pieces of evidence of the offshoring channel through which US manufacturers shifted high-polluting activities to China after the conferral of PNTR to China, thereby reducing emissions domestically. Our results corroborate the pollution haven 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.

Dirty Product Imports from China to US We directly 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 expect that US manufacturers will increase dirty product imports from China relative to other countries. We test this hypothesis by using HS 10-digit product-by-year-level data from the UN Comtrade database. Specifically, we test whether the share of US imports from China increased more for products produced by high-polluting industries.[57] 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. We find that such an increase is more pronounced for products that are produced by high-polluting industries. In Column (3), albeit only with a p-value of 0.258, we further find that the increase in the share of US imports from China is more noticeable in upstream industries.

We 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.[58] 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. (2020) find that Chinese exporters are significantly less emission-intensive compared to non-exporters. 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

[57] We use the industry-level pollution emission intensity in the US to measure the "dirtiness" of each industry.

[58] 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.

standards.

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, Log(PM Emissions/$\text{Sales}_{i,97}$). 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 Log(PM Emissions/$\text{Sales}_{i,97}$) 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 (Log($\text{NP}_{i,95}/\text{Emp}_{i,95}$) and Log($\text{K}_{i,95}/\text{Emp}_{i,95}$), 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 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.

Pollution Prevention Efforts 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.17 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) considers the number of chemicals that are associated with these P2 activities.[59] We find that neither the extensive nor intensive margin of pollution prevention (P2) activities respond to PNTR. That is, we do not find support for the *PNTR-induced* clean technology adoption channel.

Note that this result does not necessarily contradict the existing literature that emphasized the role of clean technology adoption. Our result implies that the trade liberalization associated with the conferral of PNTR might have had a limited role in inducing clean technology adoption in the post-2000s period.[60] Aside from the *trade-induced* channel, it is still possible that a nationwide trend of green technology adoption is responsible for the emission reductions in US manufacturing. Moreover, even if there exists a mechanism of trade-induced clean technology adoption, there might be a counteracting force in our context, leading to the null result that we obtain. One such possibility is that clean technology adoption and offshoring were substitutes in our context, in a similar manner in which general process innovation and offshoring were found to be substitutes in Bena and Simintzi (2022). They find that a policy change that allows US firms to produce in China at lower costs, such as PNTR, decreases their process innovations intended to reduce production costs. 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 haven 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

[59] Appendix Table B.18 shows that the results are similar using overall P2-related activity as a dependent variable.

[60] Cherniwchan (2017) finds a positive impact of NAFTA on clean technology adoption in the pre-2000 period. One key difference is that our setting based on PNTR singles out the impact of changes in import barriers rather than changes in both import and export barriers. Given that the technology upgrade channel mainly operates through export liberalization (e.g., Bustos, 2011), this may explain the limited role of trade-induced clean technology adoptions in our setting.

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 further evidence that supports the pollution haven hypothesis whereby 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, and offshoring in teasing out the pollution haven 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.

Finally, 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.

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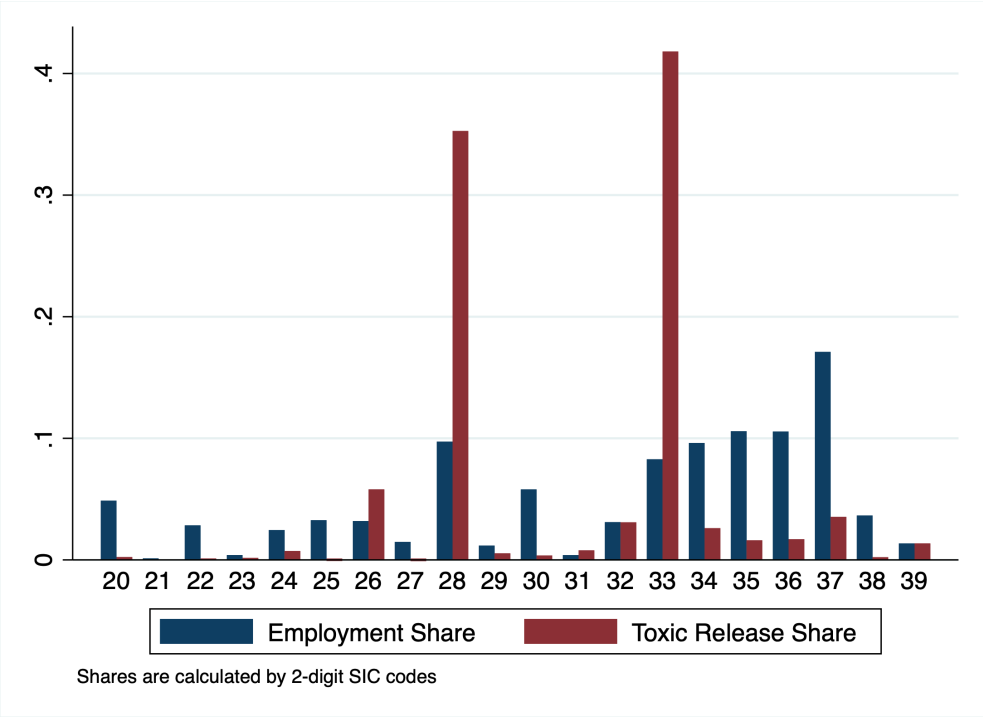
Appendix A Additional Figures

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



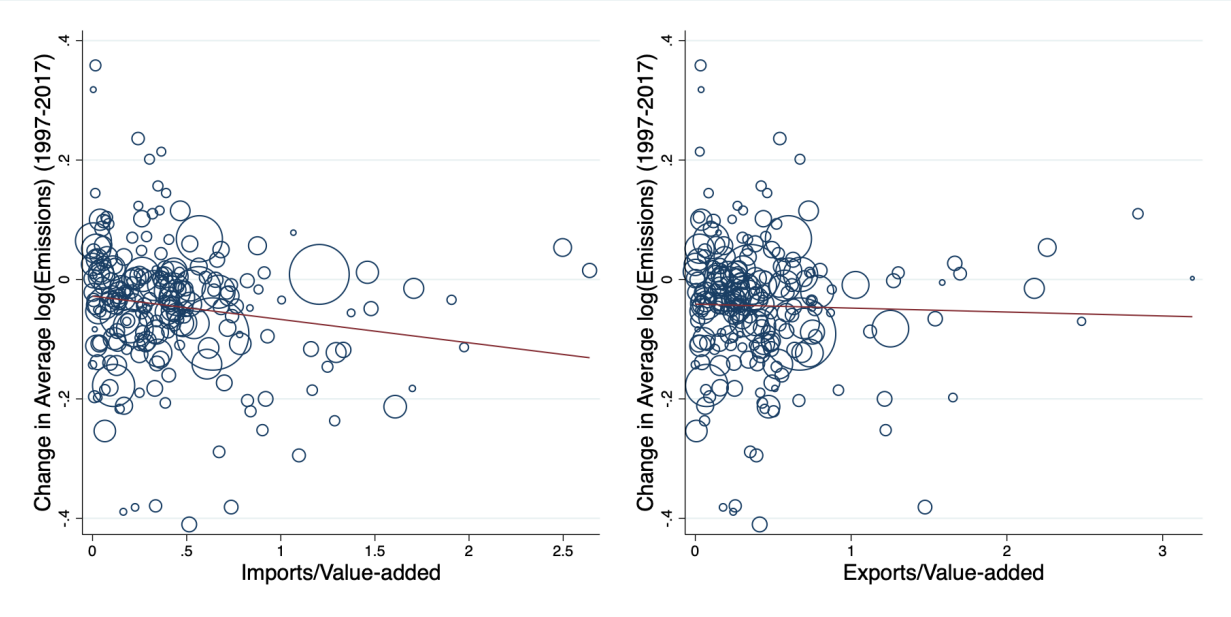
Notes: This figure displays PM_{10} emissions trends for (i) 2-digit-SIC 28 and 33 and (ii) all other industries for 1997-2017.

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



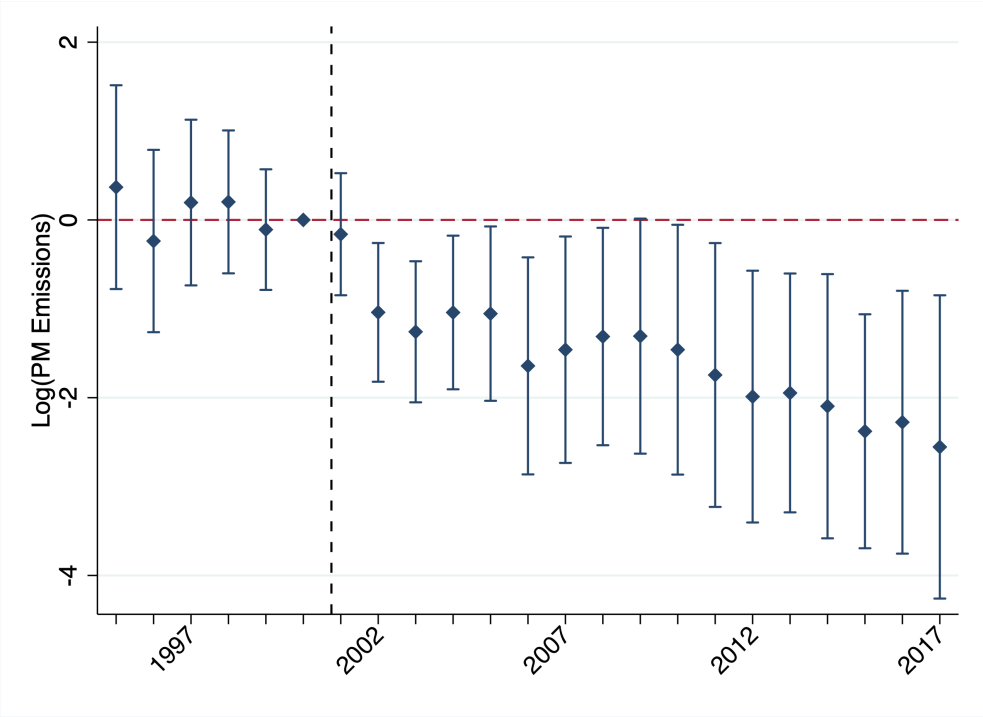
Notes: This figure displays employment (navy bars) and PM_{10} emissions (red bars) shares in 1997 by 2-digit-SIC industry.

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



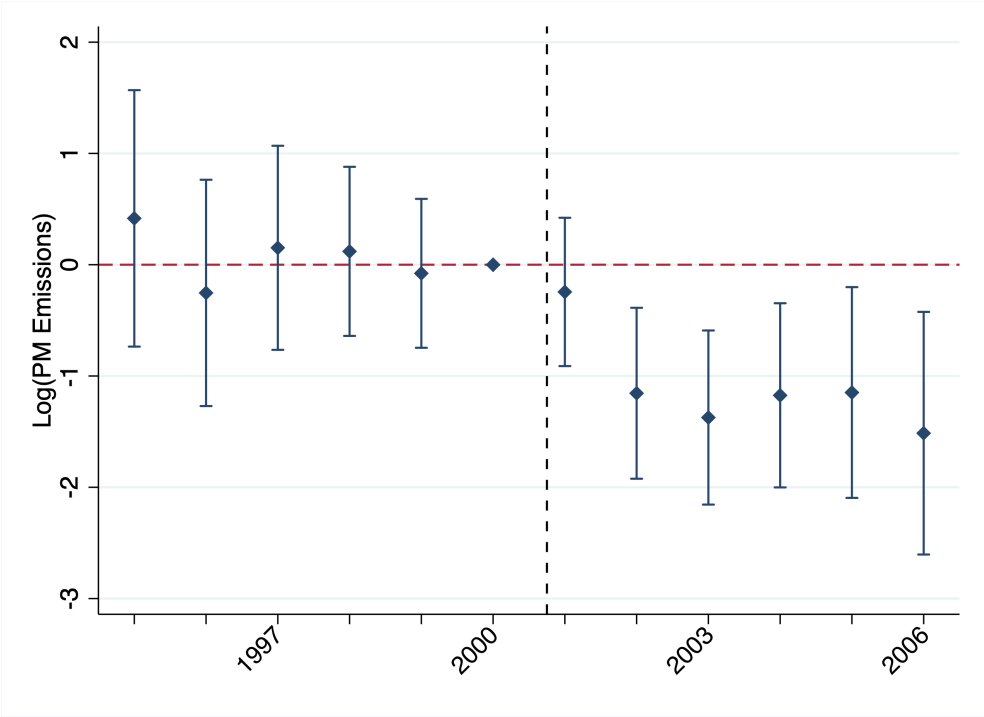
Notes: The graph on the left (right) illustrates the correlations between the industry-level averages of changes in the within-establishment log(emissions) of PM_{10} from 1997 to 2017 and the industry-level import (export) intensity constructed using the value of imports (exports) relative to value-added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



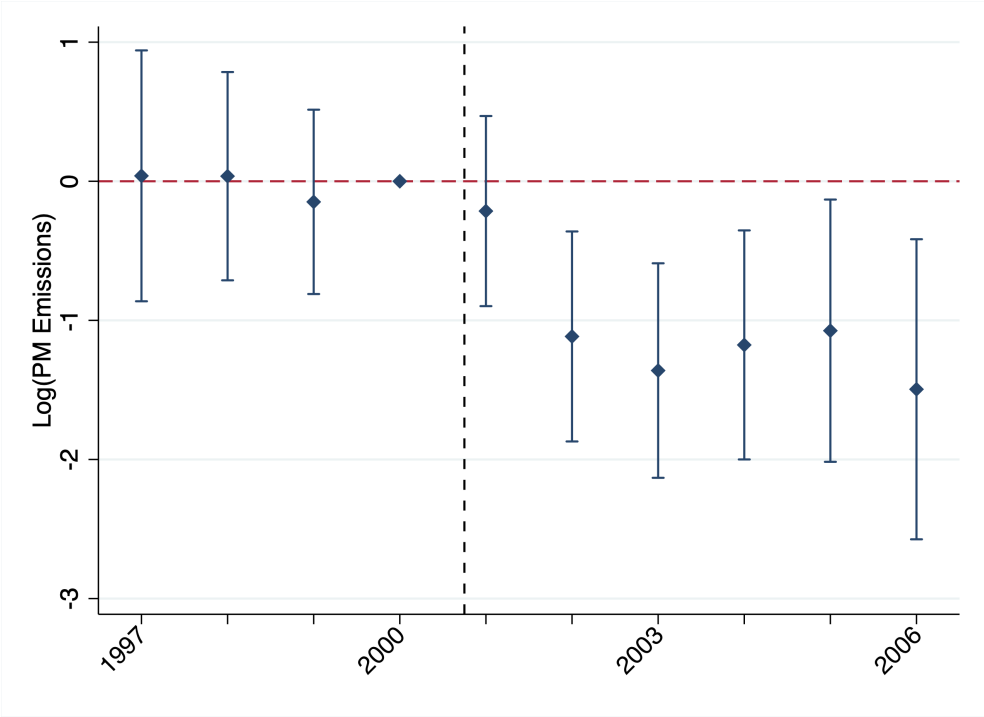
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1995 to 2017. All other specifications are identical to those in Equation (5.3).

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



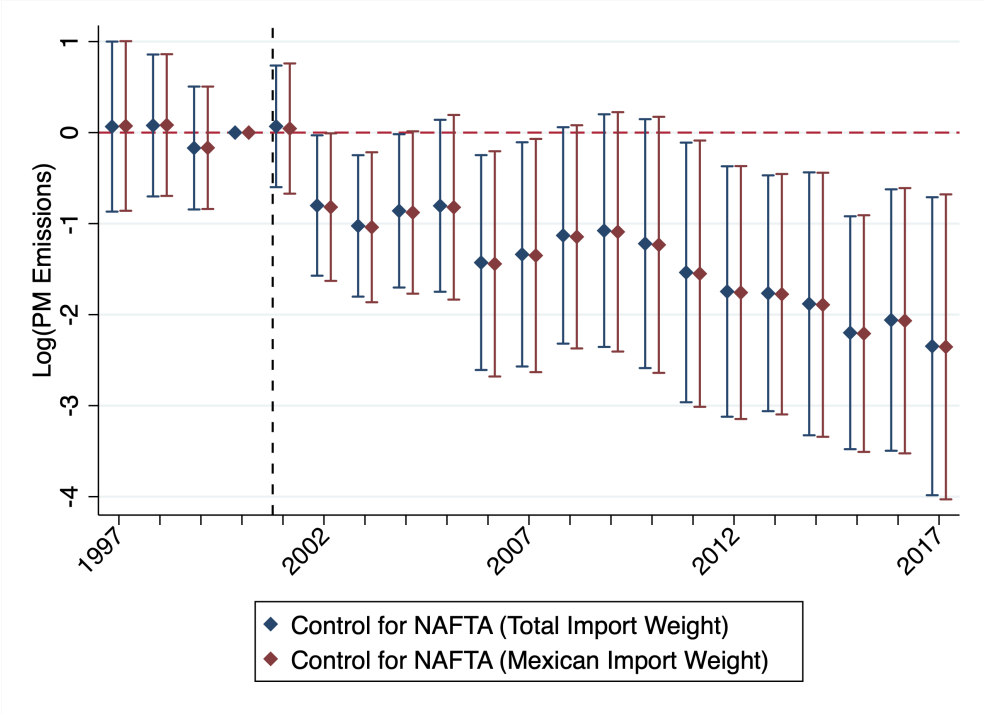
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider the sample period from 1995 to 2006. All other specifications are identical to those in Equation (5.3).

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



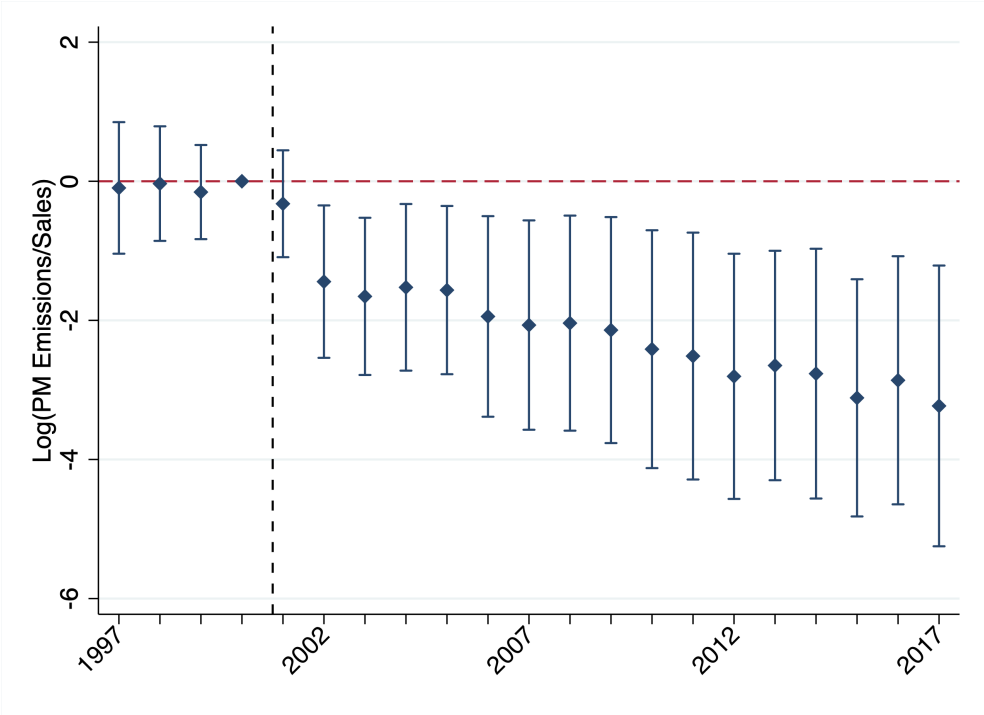
Notes: This figure displays the estimated difference-in-differences (DID) coefficients with their 95 percent confidence intervals, where we consider an extended sample period from 1997 to 2006. All other specifications are identical to those in Equation (5.3).

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).

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 Gap$_{i,99}$	5008	0.319	0.131	0.138	0.336	0.450
NTR$_{i,t}$	5008	2.457	2.658	0.000	2.122	5.067
MFA 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 Gap$_{i,99}$	287	0.329	0.142	0.135	0.339	0.473
NP$_{i,95}$/Emp$_{i,95}$	287	0.295	0.115	0.173	0.266	0.452
K$_{i,95}$/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 (Unconditional)$_{f,97}$	2294	0.096	0.211	0.000	0.000	0.346
Import Intensity$_{f,97}$	703	0.289	0.275	0.029	0.200	0.762
Export Intensity (Unconditional)$_{f,97}$	2294	0.337	0.387	0.000	0.144	1.000
Export Intensity$_{f,97}$	1485	0.501	0.374	0.049	0.422	1.000
Firm Employment$_{f,97}$	2294	5566	70366	40	388	8636
Num. Establishment$_{f,97}$	2294	50	407	1	4	84
Num. 4-digit 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 Emissions$_{p,97}$	3858	41262	472714	0	15	17422
PM Emissions$_{p,97}$/Sales$_{p,97}$ (lb/million dollar)	3858	2354.7	33172.9	0.0	0.6	577.9
I(Num. P2$_{p,95-97}$>0)	3858	0.260	0.439	0	0	1
I(Num. P2 Clean-Tech$_{p,95-97}$>0)	3858	0.130	0.336	0	0	1
Establishment Employment$_{p,97}$	3858	410	916	28	160	900
Establishment Sales$_{p,97}$	3858	91	245	4	25	189
Age$_{p,97}$	3858	55	42	9	50	109
(E) County Level						
Variable	Obs.	Mean	Std. Dev.	P10	P50	P90
CAA 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)
$\text{MFA Exposure}_{i,t}$	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)
$\text{MFA Exposure}_{i,t}$	-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: 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.13: 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.14: PNTR, Import Status, and Export Status, 1997 - 2017

	(1)	(2)	(3)
	Import	Export	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 sample to establishments that did not belong to importing firm 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 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 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.15: 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.16: 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.17: 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.18: 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.